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PREFACE

With the publication of this volume, the Annual Review of Anthropology takes its place among the distinguished series of Annual Reviews of current scientific literature. Anthropology is the second behavioral science to become a part of this enterprise—the Annual Review of Psychology has been published since 1950—and is a logical outgrowth of the Biennial Review of Anthropology, which terminates with the 1971 volume.

There are certain distinct advantages to the profession in having a more frequent review of output in the field, as well as in its incorporation within a publishing venture devoted exclusively to the communication of scientific information. Under the Annual Reviews format, we divide each volume into more chapters of greater or lesser scope, some substantial in coverage, others very narrow. Without committing ourselves in every issue to a single chapter review of all the diverse kinds of work in a subfield as complex as prehistory, the editors can request a brief but useful chapter on important new dating techniques, as well as a much longer one on such a broad topic as environment and adaptation.

The resources of Annual Reviews Inc., while modest, nevertheless permit the establishment of a rotating editorial committee and group of editors drawn from the general anthropological community. In this way it is hoped to maintain a changing staff, composed of both generalists and specialists, who will make every effort to cover the cutting edges of the discipline as well as the major areas of output.

As we sought to decide upon an appropriate selection and organization of chapters for this first volume of the Annual Review of Anthropology, the editorial committee necessarily entered into philosophical considerations of the nature and state of the discipline. This proved to be extremely interesting, among other things because of a substantial meeting of the minds despite differences in the background, experience, and special competences of its members. One consequence of this discussion was the decision, for the initial volume at least, to avoid broad conventional headings—linguistics, social anthropology, or physical anthropology, for example—in favor simply of serially titled chapters that tended to follow more or less implicitly such categories. This permitted on occasion a logical if somewhat unorthodox grouping according to subject matter that cross-cut conventional breakdowns. A prehistorian and an ethnologist thus might contribute jointly, in contiguous but separate chapters, to a review of settlement pattern studies. In the future this procedure could be followed in other ways, in the review of regional studies, for example.

The editorial committee must cope in subsequent volumes with the problem of frequency with which a given topic or field should be reviewed, and
they will continue to concern themselves with new directions, such as innovative work at the interface of anthropology and other disciplines. In addressing itself to the communication needs of the profession, the Annual Review of Anthropology will depend for its continuing success upon the collaboration of colleagues, a collaboration already successfully begun in the Biennial Review.

The Editors and the Editorial Committee
CONTENTS

CULTURE AS BEHAVIOR: STRUCTURE AND EMERGENCE, Conrad M. Arensberg 1

CONCEPTUAL PROGRESS IN PHYSICAL ANTHROPOLOGY: FOSSIL MAN, Bernard
G. Campbell ........................................... 27

STUDIES OF MODERN MAN, D. F. Roberts and J. C. Bear ............... 55

DATING METHODS, Joseph W. Michels ........................................ 113

ARCHAEOLOGICAL SETTLEMENT PATTERNS, Jeffrey R. Parsons .......... 127

DEMOGRAPHIC STUDIES IN ANTHROPOLOGY, Paul T. Baker and William T.
Sanders ...................................................... 151

ENVIRONMENT, SUBSISTENCE, AND SOCIETY: THE CHANGING ARCHAEO-
LOGICAL PERSPECTIVE, Ezra B. W. Zubrow ................................. 179

ENVIRONMENT, SUBSISTENCE, AND SOCIETY, Karl G. Heider ............ 207

ETHNOHISTORY: A REVIEW OF ITS DEVELOPMENT, DEFINITIONS, METHODS,
AND AIDS, Robert M. Carmack ............................................. 227

SOCIAL STRATEGIES AND SOCIAL RELATIONSHIPS, Norman E. Whitten, Jr.
and Dorothea S. Whitten .................................................. 247

ETHNOSCIENCE 1972, Oswald Werner ......................................... 271

KINSHIP SEMANTICS, Harold W. Scheffler .................................... 309

STRUCTURALISM IN CULTURAL ANTHROPOLOGY, Pierre Maranda ........... 329

LINGUISTIC THEORY: SYNTAX, SEMANTICS, PRAGMATICS, Michael Silverstein 349

LINGUISTIC MODELS IN ANTHROPOLOGY, Mridula Adenwala Durbin ........ 383

AMERICAN INDIAN LINGUISTICS, Karl V. Teeter ............................ 411

AUTHOR INDEX ............................................................................. 425

SUBJECT INDEX ........................................................................... 436
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CULTURE AS BEHAVIOR: STRUCTURE AND EMERGENCE

CONRAD M. ARENSBERG

Department of Anthropology, Columbia University, New York, NY

CULTURE, CULTURAL FORMS, AND THE SEARCH FOR STRUCTURE

The annual review of the fluid and protean science of anthropology which is to occupy this book reveals a discipline with visible progress in many empirical discoveries. Its continued devotion to fact-gathering and its careful observation of the human realities of cultural variation attest to its enduring tradition. Anthropology is still the proper heir of natural science and natural history. Its data are those that come, in one way or another, from recording past and current events in human and hominid lives, whether in the testimony of fossil and artifact or in the protocols of the fieldworker’s notes and tapes. Its coverage is still immense, indeed encyclopedic. It ranges from the diaries of the ethologist animal watchers to computer-sorting of worldwide processes of evolutionary advance, from word uses and color categories of tribesmen to the emergence of empires or the current fading away in the world of the rural-urban distinction. It remains the one science bridging the gap between the “hard” and the “soft” sciences, uniting the Two Cultures of Sir Charles Snow.

Yet the progress of anthropology displays a proliferation and sprawl that threatens its advance. There are, of course, master concepts which offer some unity. But often these are merely imported from outside, like evolution and ecology. Some parts, chapters here, show order in classification and clarity in identifying tested processes. There are established mutual dependencies among data already firm. Archaeological sequences and language families can be cited. In other parts, particularly in cultural and social anthropology, unity seems much less. The replication and the cumulation that in science depends upon agreed definitions and shared paradigms of method and inference is too little.

An introductory essay on the progress here recorded, then, should devote itself perhaps less to summation, a task already in the hands of the capable chapter writers, than to uncertainties. Obviously it is culture which provides the theme which unifies, if tenuously, the great sprawl of the science. Culture, as man’s special behavior, remains the theme still implicit in the background of all the subdisciplines. Perhaps it would be most useful to treat the nature, emergence, variability, and relationships of culture.
It is useful to remember this centrality of man's cultural behavior. That is what we all watch and all seek to connect our data to. Human evolution, after all, treats the bearer of culture for his behaviors and capacities in the light of their growths into technics, social systems, and languages. Archaeology treats the relics of those growths. Linguistics explores the languages that symbolize and communicate them. Cultural anthropology, however defined, compares aspects of culture, explores man's experience with them and with his fellows by means of them. All four rest on the observation, direct or indirect, of man's immense diversity in his endless restructuring of his cultural world. It is to the ordering and the restructuring of the data of culture that we should turn in any quest for unity. This is where the least unity prevails.

Efforts at advance abound. Some of them are still programmatic, others are fruitful. With the attainment in the Human Relations Area File and the World Ethnographic Atlas of a compendium of ethnographic fact about man's cultures, with the gradual acceptance of a world classification (Murdock's) of at least some of our cultural data, as in the case of the nomenclatures of kinship systems, there are already generalizations at hand capable of revealing wide correlations. There are already attempts at exploring systemic and structural interconnections of such nomenclatures and at giving reasons for their existence, emergence, and form.

In another review of anthropology, Henry Selby reminded us (17) of the recent sway of British and American functionalism: sociological and psychological. These provided, he says, "the macrotheorist with a guiding design to the construction of theory," one in which social structure either as service to the collectivity or support to the individual was the point of departure for cultural analysis. This sway arose only to be challenged—first, by the formal structuralist models of Lévi-Strauss and his successors; second, by the cognitive models of componental analysis and the depth structures of the linguists. In turn, this search for abstract structure has been joined by a still more recent "structural formalism," in which game-theory, decision-making, and mathematical programming have been put to work on cultural data. These are the structuralists of the moment, as Selby identifies them. We can expect more.

It is plain that efforts at ordering cultural data into larger and wider structures, whether empirical, stochastic, or abstract and formal, are strong and growing. There is also a neofunctionalist, empiricist, "materialist" reaction to these structuralisms. Selby duly notes it in the persons of the evolutionists and the ecologists of culture who most recently in anthropology have reasserted priority in the analysis of cultural data to lie "with biological, nutritional, technological, and economic data." Yet they too seek structure. They seek to structure the two kinds of data in the new ways of general systems theory, in open equilibria and in feedbacks both adaptive and destructive. Model, structure, system, and form in statics, and trend, drift, change, emergence, and devolution in dynamics, are all invoked in the search for
newer order in the study of cultural forms in the fluid if nervous anthropol- 
gogy of 1972.

In 1970, much as today, Selby (17) saw us as concerned with three 
large themes:

1. "White-box cognition." Selby used this name to designate a search 
for a language for cultural study with which to describe behavioral processes, 
one that would put the human being back into central place—in short, a be-
havioral anthropology.

2. "Unconscious" models of social structure and process. Here we must 
remember with Lévi-Strauss (11) in his seminal article, Social Structure, of 
1953 that these are the anthropologists' constructs, not the natives' folk-soci-
ology.

3. Priority to technology and ecology.

Selby ends his 1970 review with part, but not all, of the same hope that 
will infuse this Introduction. First, he says of the many schools, the "cognitiv-
ists," the "materialists," the "empiricists," and the "structuralists" that they 
"appear to compete, whereas they are surely complementary." We have mined 
our traditional constructs, he says, almost as thoroughly as we can. It is time 
instead, he insists, to "make an effort to re-conceptualize the central subject-
matter of our discipline." He then proceeds to bring into use in anthropology 
from outside the discipline three concepts that have been developed else-
where but that might fit our purposes: game-theory, maximization, and sto-
chastic process, only to concede that the problem of fit still remains. In cul-
tural behaviors "men do not (only) play games, neither do they consciously 
maximize, nor engage in random association" (17). So Selby concedes, "the 
language is untrue to the phenomenon of study." He pleads our hope for 
"new more general constructs that will amplify the scope of our queries and 
generalize our results." He argues most strongly, with good new work of his 
own and others to back him, that only with new models of underlying reality 
drawn from bold analogy can we advance. He seems not to share our hope of 
this Introduction that such generalization can be done, not by analogy from 
outside, but by further ordering from within.

Fredrik Barth has brought our attention to the fact that these schools of 
ours use alternative models of reality incorporating different, unreexamined 
premises. He has tried another reconciliation, though it remains short of the 
generalization we seek. Even in 1972 the three large themes are not yet rec-
ognized, but the "central subject matter" had indeed been readdressed just 4 
years before Selby wrote. Barth, in his distinguished second Royal Society 
Nuffield lecture entitled Anthropological Models and Social Reality (2), had 
given a very similar review of social anthropology. A Cambridge man and a 
Chicago student, but a national of neither Britain nor America, Barth bridged 
the American and the British traditions in social and cultural anthropology. 
He thereby came closer to our central subject matter. The structuring we all 
seek, he said, came from trying "to collect and to systematize observations
that depict the reality that confronts us: the regularities of life in human communities.” He identified three kinds of model whereby anthropologists today “depict the social order.” There were jural rules (often the British attempt), cognitive categories (often the American one), and an interactional system (the one he favored), a model of the “constraints on individuals that arise from the behaviors of others in a social system.”

Barth thus returned us to an older line of development that does indeed readdress the central subject matter of culture. It is one that is not analogizing at all, nor limited in fit. He identifies this subject matter in the “social reality” of man. Whether primate, or paleontological, or conjectured from artefact and settlement, or enshrined in symbol or residual value, or watched with field notebook or film, that is what we see or infer we see. There have been others who have found in social interaction the nexus of culture, behavior, meaning, and society. The path to wider ordering of structure and process, taking off but hewing close to our common but many-tiered central subject matter, will be the subject of this paper.

Barth himself has not provided us with new and unifying models. Like Selby, he evaded this issue. Rather than placing modeling rules, cognitive categories, and “an interactional system,” together into any embracing or generalizing structure, he slid off into an exclusive devotion to the third of his kinds, translating the social into a calculus of individual strategies. With ready analogies at hand from studies in economics of personal economizing, competition, and manipulation of strategies, he could and did at once move into many fruitful field studies of roles and choices, first in entrepreneurship, latterly in ethnic identities. But the doctrine of “transactions” he espoused, like exchange theory in sociology (to which we shall come later), soon lost itself in petty calculations as to why individuals made the choices that they made. There is, of course, much to be learned in human losses and gains, in maximizings and satisficings, but such studies do not tell us why the roles exist to be exploited nor why arenas of contention are so different. A hedonistic calculus is open to the same criticism of lack of fit that Selby concedes.

First, a hedonistic calculus, a rationalist reduction of cultural behaviors, is untrue to the phenomena. It is too limited, too monadic, even when the dyad of transaction is explored. Cultures shape numbers far larger than pairs of persons however shrewd they may be, and we need to specify the way up to group activities. Of course, people calculate their advantage or security as animals calculate their survival, but that is not all they do. Nor does their doing it by itself account for the cultural and social field or setting which makes the choices real ones or the array of choices present to be decided upon. Powerful as the analogy from formal economics is, it neither can generalize nor unify our field. We are left with no picture as to why the choices exist, nor whence the jural rules or social roles emerged (since there are many, the incumbents changing, the games various from culture to culture), nor why the culturally diverse cognitive categories take the form they take.

Even in the economics from which the calculus comes its sufficiency as a
CULTURE AS BEHAVIOR

generator of structure and system is today suspect. It would be unwise of us to use a formal economics "to depict" our "social reality" at the very time when its devotees doubt its sufficiency for their own economic reality. We no longer so easily believe that economic realities, either primitive or modern, fall into an order in which individual supplies and demands add up at once to an "all-regulating invisible hand." Neither in economics any longer, nor certainly in the study of culture and institution, can we move directly from personal choices to emerged cultural forms.

In this, we are like other sciences. We cannot rely on analogies from outside. We must identify for ourselves the common properties of our subject matter; we must follow in fact the arrangements and orders that yield the structurings and the systemic relationships of our own data. We can turn to other sciences, certainly, for their experience and learn from it, but we must build our own generalizations.

In this spirit, Valerie V. Hunt (9) reminds us what that experience, most broadly taken, has been in our century. She writes:

For over a hundred years, science was primarily involved in the manipulation of exact measurements, attempting to discover the very elemental facts and properties of matter. In the twentieth century the emphasis has shifted. Measurement and precision, although necessary, must be recognized as only giving raw materials for science. The aim of science is now primarily to find relationships which give order to the raw material discovered throughout the years, to detect patterns into which these measurements fit. . . . Science is not constantly preoccupied with mere facts, but rather with the relationship these facts have to one another and with the whole that they form.

And further, this whole, wherever it occurs, is structure

. . . not a thing in space (but) a series of relationships, and these relationships have order. Structure appears whenever elements combine into meaningful wholes whose arrangements provide definite and significant laws. 1 The ordering principles that govern these laws (outcomes) are principles having to do with the wholeness of the structure and not just its isolated parts. In science, then, such an approach is replacing older concepts of what constitutes the nature of man and material. This is the approach from science which I think we must listen to.

It will be the purpose of this Introduction, in the spirit of this modern effort in science, to offer generalizing models of structure, system, and process for ordering relationships among the cultural data we have already amassed so abundantly. These will come from inside, not from outside our field. They will build upon our observed facts or cognitive, jural, and transactional behaviors, try to follow out the attested connections between them, push toward the wholenesses and outcomes or products they seem empirically to manifest, the while keeping systematically and controlledly in mind the operations of fact finding and fact testing which govern our field.

1 (Sic, she might as well have said "outcomes.")
Unifying models structuring cultural data can now be achieved that address our central subject matter: social and interpersonal behavior regularized into culture from the start. They need not be imported analogues. They certainly are not egocentric or reductionist; they may thus have a good fit and the widest scope from the start. I will argue that we can already unite most of schools on the high road of natural science and advance with the materials we already have. We can achieve a new and combinatorial inference of structure, a different kind of modeling and processing of our data, their orders, and the outcomes emergent from their orders. I will try to demonstrate that what I shall call minimal-sequence modeling of human social interactional behavior—a human ethology, if you will, of variously patterned and concatenated social repertories—can both generate cultural forms and their strategic arenas and account for the disputed formal and other "deep structures of the mind" that seem to so many observers to underlie cultural behavior.

Let me point here to the small but growing tradition within anthropology constructing behavioral, not formal or mental, structures and processes for the social reality of culture, operational in defining and empirical but generative in ordering our data. The existence of a tradition within our ranks of an operational generalization of social behavior into cultural forms may come as a surprise to many anthropologists. It is not well reported or understood. Its promise may seem to some unlikely but it has not been without its successes.

What will be most surprising, perhaps, will be its scope, the scope for which Barth and Selby asked. For it offers to unite cognitive maps, jural rules, and manipulable roles by bringing them back into relationship with ethology and communication theory. In this it reasserts our identity with sociology and returns us to the realization that anthropological data are also generalizations from history, events which we watch unfold in time whether to comparable outcomes or to unique ones. Timing and ordering in time become conscious tools in this tradition, once our data become events and outcomes of events. With timing naturalized in cultural studies and social behaviors, as they already are in evolutionary and archaeological ones, problems of generative emergence fuse structure and process. New paradigms are possible more dynamic than before.

Measuring and Ordering Observations of Interactive Behavior

With the recent publication of Chapple's book, *Culture and Biological Man* (5), it is clear that ever more systematic and quantitative observation of human interpersonal interaction has won through to remarkably rich results. These have grounded both normal human social organization and deviant disorganization in the variably achieved or failed actualizations of the interpersonal behavior sequences characteristic of a biologically evolved and phys-

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³ Parts of this section repeat and abridge my Introduction to the forthcoming book *Interaction and Social Structure* by Orvis and June Collins, which consult for a fuller account. (University of Indiana Press, 1972. In press.)
iologically rooted human ethnology. Chapple’s careful, microlevel hospital studies at Rockland State Hospital and elsewhere have corroborated and extended ever more precisely, in case observation and therapeutic experiment, the covariances of emotional states, personal and interpersonal equilibria of initiatives, in formal and informal flows of activity, and performance behaviors. Whether in work productivity, role maintenance, or acceptance of innovations, as in the factory workrooms or hospital offices or peasant villages in transfer of technique efforts, documented and analyzed by William Whyte, Frederick Richardson, Conrad Arensberg, Solon Kimball, Robert Guest, these covariances were demonstrated during the hey-day of “in plant research,” “human relations research,” and “applied anthropology” of the 1950s. Despite turns of trend and fashion in social science, these mutual dependencies in behavior and emotion appear proved, and the processes channeling them are becoming predictable.

At much the same time as Chapple’s new book, the joint efforts of Alan Lomax and myself, with the others of the Cantometrics Project of Columbia University, resulted in our announcing in Folk Song Style and Culture (12) the successful discovery of panhuman, worldwide correlations between culturally various culture-characterizing styles of folk music and dance, expressive arts of every human culture, on the one hand, and highly specific cultural variations in the social relationships of each human community’s ordering of its preferred and customary male-female, work, familial, and public decision-making behaviors, on the other hand. These correlations arose out of computer comparison of world ethnographic data with world musical trait distributions. They emerged to view, in the computer analysis, not through easy first-order association, but only with systematic cross-cultural measurement of both musical and nonmusical cultural data in behavioral terms. They arose when the researchers (Lomax and Arensberg) transposed and scaled for each trait, whether musical performance or social-structural rule or activity, the behavior, especially the interactive behavior, reported and described in the ethnographers’ or music-judges’ record of what they saw and heard the actors do with and to one another. [For an account of the process of translation, measurement, and scaling, see Lomax et al (12) op. cit. passim, and Chapter 4 by Arensberg.]

These developments, seemingly so diverse, are in fact outcomes of the same stream of thought and method, however different the observational data and the mathematics of their ordering may seem. They come from a common innovation in social and human science. They both use operational procedures and definitions invented for the study of human social and cultural behaviors—from the most ephemeral to the most formal and enduring—and they both do this through the discrimination, measurement, and sequencing of interpersonal interactions. They draw this explicitly upon a method outlined first in 1940. They are parts of a stream of scholarly work flowing from that beginning.

This new and growing stream of inquiry and discovery has no agreed-
upon name, other than perhaps "interaction theory." Its output seems to catch attention piece by piece, but its unity and consistency in method and theory, perhaps because of the lack of a catchy name, seems to escape its readers. A new book to appear this year, *Interaction and Social Structure*, by Orvis and June Collins (6), two scholars' recapitulation of the basic paradigm of the theory, may help its identification, examination, and acceptance as a whole, just as it may perhaps inspire some readers to replicate discoveries of the stream and to push it forward.

In all this, of course, interaction theory, a behavioral anthropology and sociology, must build upon what is continuously being learned in its chosen realm. Just now the learning comes from semiotics and communication study; from that of language and paralanguage; from kinetics of movement and performance in work, art, play, and other shared activities; from analysis of ritual and ceremony and of the other evocative, supportive, or decision-making activities human beings share; from work on roles, role-playing, small-group dynamics, and on other dramas and scenarios of "symbolic interaction" and on the social processes of institutional stasis and institutional change.

Reciprocally, too, all these lively inquiries of many workers of today stand to gain from systematic observation and ordering of the interpersonal dimensions and sequences of their data, some of which can soon be made explicit. Similarly, the still too often left implicit (for lack of a method) interpersonal processes and structures lying beneath and maintaining, perhaps determining, the human parts of the web of life in the ecological relationships so diverse in human culture can come under direct study. Likewise the regular patternings of interpersonal action useful for understanding through "generative" and "open-ended" models, the emergences, structurings, and symbolizations of alternative production and distribution systems and of diverse and culturally specific institutions of familial, political, and other kinds shaping man's evolving and adapting customs, values, and groupings, can be identified. Cross-cultural understandings of, let us say, work patterns, or political process or healing practice, already each a growing part of comparative sociology and anthropology, will gain in precision and power when common terms of description of the constituent roles in each are achieved, tested, and compared.

The theory insists, thus, on the redefinition of all received and conventional constructs. Native terms and concepts in each culture, the "folk sociology" of Lévi-Strauss, are good evidence as glosses on natural events, but they need translation. All too many current scholarly concepts are still culture bound, deposits of old scholasticisms, or drawn from axioms untested by observation or chosen to fit methods, alas, never selected for relevance and economy. Both the natives' and the scholars' constructs glossing and interpreting social and cultural data must be redefined downward into the common and universal terms referrable to the operations evolved in the ordering of our observations of real events of real persons, as "force," "work," "com-
bustion," once mere words of lanugage, have progressively been so redefined against quantitative measures and universal identifications of tested agreement. Further, the new stream of inquiry follows the natural sciences in restating empirically discovered mutual dependencies among the so-redefined phenomena of its realm in the demonstrable conditions of their observed recurrences. That is, its aim is to specify the order, the limits, the resultants of these recurrences in shortest and most precise form, to establish and predict process and outcome, to push to the discovery and verification of the law that Valerie Hunt invoked above.

The new stream of inquiry and theory described here starts from observation of interpersonal events. From its study of these it has provided a growing corpus of research discovery, as we have said, though as yet without little wider general statement. The diverse works of the many people over a generation since its inception have already been mentioned: Chapple, Whyte, Kimball, Richardson, Arensberg, Oliver, Guest, Leonard, Sayles, Orvis Collins can all be cited. They have paralleled the growing company of the many other researchers in social psychology, like Muzaffer Sherif or Alex Bavelas or Mason Haire, who also study, with different methods, the dynamics of social action. They parallel as well the still greater company, already identified here, of researchers in comminucative, paralinguistic, semiotic, symbolic-interactional, and group-problem-solving and small-group-dynamics studies, much too numerous to name here.

The people of this stream, unlike their paralleled colleagues, have hewed to their own common logic and procedure. They have kept to a rigorously operational method for identifying, comparing and measuring, structuring, sequencing, and generalizing observed runs of real events among real persons.

The method was first introduced by Eliot D. Chapple, in collaboration with me, Conrad M. Arensberg, in 1940 in a monograph reporting 2 years of joint exploration we carried out in long discussions and trial formulations leading to a solution of the problem of the monograph's title: *Measuring Human Relations* (3). The publication lead at once to a flow of articles in our separate empirical studies, the hospital studies of Chapple and Lindemann and the factory and community project studies of Arensberg and Macgregor, Horsfall and Arensberg, William Foote Whyte, Frederick J. W. Richardson, and others. These marked themselves off from the work of the time, excellent work by other students of Lloyd Warner and Everett Hughes, who imported into sociology the anthropological canons of field work, participant observation, and open-ended interviewing, by explicitly sequencing, counting, and timing interaction flows. The fullest statement of the new method, as a paradigm, came quickly in 1942. Chapple and Carleton S. Coon, a superlative ethnographer among his other talents, attempted a full recasting of anthropology, perhaps prematurely, in a brilliant book entitled *Principles of Anthropology* (4). The book remains a classic.

There have been other assertions of the need for operational definitions
of cultural and social phenomena, and they still appear. Ours proposed the conscious and controlled use of the universally shared operations of description natural to human observers of other human beings in the events of their action with one another. It identified these and suggested how they are to be made precise, replicable, and conventional from observation to observation and general for the full range of the data, thus universal to the class of phenomena. It anticipated S. F. Nadel in showing scholarly experience to have already demonstrated that culture and social structure are emergents from "recurrent regularities of the behavior of persons with and upon one another and in respect to one another." (See Nadel's *Foundations of Social Anthropology* 14.) It held that the events in which these recurrences appear can be identified, followed, and compared most meaningfully by coding them naturalistically in the very broadest and most general terms applicable to any and all of them, thus with operations of description and classification potentially if not yet consciously as universal, as replicable, and as verifiable as those of natural science. If universal enough, these operations can be used at various levels of precision to render comparable and measurable recurrences and regularities in records of observed events. With them the protocols of direct observers—the evidence of the eye—can be coaxed into the same patternings and structurings as can the interviews and histories of secondary witnesses—"native accounts" or "hearsay" to the anthropologist and the lawyer—the evidence of the ear. What we see people do can at last be compared with what they say they do. At last, as in Bridgman's examples, the "length" of the yardstick, in inches on a table, can be compared with (and is the 'same' length as) the yards of the theodolite and the light years of the telescope.

The operations we suggested in the monograph that are already in use and can be so precisioned and so used, and on which a new paradigm of basic definitions for social and cultural phenomena can be built, were simple enough. They are indeed universal and they were enough to make a start, as the flow of subsequent work has witnessed. They were: (a) The identification, specification (which opens the enumeration and the discrimination) of the persons acting. The first question is Who? (b) The specification and comparison of the order in which they act, the order of action. The second question is When? In what order? This opens sequences, directions, and a myriad of structural interpretations. (c) The frequency, the time rates, the other discriminations of recurrence in person and order in action and interaction. The question is how often? This opens the door to empirical comparison and measurement.

It was surprising but it is clear that these few were the initial universal operations—they are already questions every observer puts. Surprising indeed, because What? Why? and Where? even How? were not universal, being either inferential or already subsumed in the others. A rereading of our monograph or a reading of the recapitulating paradigm of the Collins book will tell the reader why. Yet other observers have worked very well with them. Edward T. Hall with his proxemics, and ethologists with their territoriality,
show spacing to be a universal variable powerful as the timing that opened
the way for us. But we did not claim to have found the only path to a human
science. We claimed only to have discovered a rewarding one. Others can add
universal operations handling these further questions.

That was the beginning. The mapping of changes in the frequencies of
interaction has led, in the subsequent work of Chapple, Chapple and Sayles,
Richardson, Sayles, and Guest in organizations, to a much more precise un-
derstanding and predictability of the shifting pressures, failures of com-
unication, and necessities for opening of contracts and clearing of channels,
and has made clear their crucial role in the generation and in the allay-
ment of malfunctions, conflicts, and stresses, and failures of performance in work
and in institutional relationships. Karl Deutsch, the political scientist, has
even naturalized such a concept of relative volumes of communication flow
to account for the cohesion of provinces into nations when volume is high,
and their separation, even back to back, when volume is less between them
than with regions afar. The counting of social acts is an idea widespread
enough to win acceptance, especially as other paradigms admit it easily.

The use of the order of action and the discrimination of the direction of
flows of interpersonal action has won far less understanding. Yet Chapple
achieved redefinition of dominance in men and animals into measures of ini-
tiative and response and used it to effect in psychiatry. And restatement of
authority, workflow, grievance, reporting, and other formal and informal
components of organizational behavior into flows of interpersonal action
moving fatefully through the personnel of the field of action in different di-
rections at different rates, codifying the up-the-line, down-the-line, in-chan-
nels and out-of-channels and other folk classifications of pressures and reliefs
in the activities of their lives that the military or business people perceive was
achieved and used empirically and effectively for the explanation and the
prediction of a host of human outcomes in institutional systems. This indeed
was reported often by Arensberg, Whyte, Richardson, Guest, Chapple again,
and others. But it has been widely unrecognized or misunderstood.

The difficulty seems to be a continuing inability to treat men as data in a field. Perhaps people squirm away from it through empathy and fellow-feeling or perhaps from pride in free will. But also they lack the concepts which would carry them up to the necessary abstraction and synthesis. These have been slow to appear in social science. Despite such obvious everyday experience of directional lineup in activities as simple as bucket lines and traffic queues and pecking orders or dinner-party precedence, or routing office memoranda, social science still seeks to derive its constructs from purposes and essences rather than from behaviors. Dynamics and process are still looked at as successions of nebulous states rather than as vectors of action in a human field. Systems are all too often invoked in which every other factor is coded, but the flows of action among the system-elements which are live persons is ignored.

The level of comparative analysis and empirical syntheses that can iso-
late, compare, and model differences in form in institutions arising from the ordering and the deployment and the sequencing of the human action actualizing the institutions seems too high for most analysts. Yet we can all perceive intuitively with the historians that democracies allow frequent initiative from below and tyrannies push to total dictation from above. Is the quantification of the insight just suggested beyond attempting? Or are the consequences of muddling or of hybris beyond our daring to predict?

Yet exactly reaching that level helped Whyte (20), in *Patterns for Industrial Peace*, to give examples, to show strikes to arise in the misequilibration of human pressures running between authority and flow of grievances. It also enabled Arensberg to distinguish the diverse, ethnographically attested primitive, archaic, and modern distributive economic systems, the reciprocative, and redistributive, and the market, reported in *Trade and Markets in the Early Empires* (15, see Introduction). We have seen already how Arensberg and Lomax could use it to scale and to correlate cultural styles in song and in male-female relationships in *Folk Song Style and Culture* (12; see passim, the treatment of Complementarity and Polyphony.) A relook at Richardson's mapping of hospitals (16) would also show the way.

More recent observation studies, it is encouraging to note, occasionally return to the theme, though not yet drawing well upon its replicative power. Marvin Harris, at Columbia University, will soon report videotapes of households of ethnic subculture, work in which A. L. De Havenon firmly grounds household "authority" in operational and quantitative discriminations of her data. A group reports from California on everyday social interaction, among them Adam Cicourel seeking out temporal parameters, but without such rigorous operational redefinition as the subject requires for generalization (*Studies in Social Interaction* 18). Restating the theme now can only hasten the unification we seek.

**Comparison and Generalization: Minimal Sequencing**

Can such a method, qualitative in operational redefinitions, quantitative in the specification, comparison, and ordering of our common data, really unify? That is the next question, and the crucial one.

Let us now, as diffidently as we can in such an ambitious effort, try the unification first in our sister science, sociology. There at least the centrality of "social reality" is less challengeable. Thereafter, if the method yields some satisfaction, let us go on to try it on some of our familiar cultural forms from cultural and social anthropology.

First, then, where is sociology today? *Divisa est in tres partes*: exchange theory, consensus theory, and conflict theory. There are strong echoes of all three in social anthropology. A new and authoritative summary review of sociological theory by Geoffrey Toetell, entitled *Major Convergences among General Social Theories* (19), does the office of summary survey of Selby, Barth, and this chapter for that divided sister country. Tootell peoples the country a little more profusely, but he shows that the three theories (and
kinds of observation supporting them) are the main divisions. He makes a plea parallel to those of Selby and of Barth and of myself, a plea for wider scope and better fit in newer theories:

Sociologists commonly divide general social theory into competing types, such as consensus... conflict, exchange, institutional, and evolutionary. However, important convergences can be found among these theories: (the use of) sanction, psychological intensity and internality (in actors)... media of interaction... adaptive and cumulative effects of cultural build-up, and, most basically, a common origin in Aristotelian thought and (in) assumptions, generally tacit, retained from 19th century economics or philosophy... Given these convergences, still each of these theories covers certain social phenomena better than others. Thus is seems highly rewarding to treat them as complementary parts of a broader, common, general social theory which is coherent at a deeper level.

Tootell's isolation of the logics of these theories, especially the trinity of exchange, consensus, and conflict, and his demonstration that they do indeed share the perceptions of the presence of behaviors of sanction, enculturation, etc that he lists above, and that they remain caught in an Aristotelian and sometimes even Hegelian language of received, nonoperational, and pre-scientific premises and semantic form, are contributions very well worth attention, if too long in the exposition even to be summarized here. So is his plea for restatement and new and wider concepts. He does not, alas, take the step of redefinition into the common and replicable procedures of the observers. Induction, deduction, and verifications in gross are already at home in sociology, as with Tootell. It remains only to find terms capable of running through all three, terms still as missing in sociology as in our own anthropology. Tootell is quite right that we will not find it any longer in any received language or logic, either Indo-European or Aristotelian.

The new language must serve all of us of every inheritance who observe the rich Freudian or Adam-Smithian individuals, the many-leveled symbols and expressions and evocations of any one's content analysis of what they say against the sentiments, Parebian residui, they feel, but always against the recurrent regularities in what we see them do. They exchange, pair by pair; they consene, in bigger clumps than pairs; they conflict, one against another (sometimes), but far oftener clump against clump. Who are they? How do we relate who is doing what to exchange, conflict, consensus?

The language must not be about words, we know. It must be about events or acts of people. We must pick out from all languages and all representations, whether language or not, what we need for the job. We need no language of verbal statements and sentence-making, not even a logical operationalism that is merely linguistically such, however universal to human minds. Chomsky may well be right; there are undoubtedly panhuman deposits-in-the-mind of “deep structure,” like Lévi-Strauss's famous oppositions. But that deep structure and those oppositions may be social experience itself. Durkheim said it first; I see no reason to think him wrong. The language we
need is *not* that of the mind alone. It must be instead perceptually, manually, and kinesthetically capable of mapping the external reality of what *they* do.

Take the notation I suggest. Tootell is quite right that exchange theory, strong today, is not enough. Exchange, consensus, conflict all exist; they are indeed human events; the representation of one will hardly suffice to cover the other two. How can we most sparely, minimally, represent exchange? This was already done, in *Trade and Markets in the Ancient Empires* (15, see Preface), as follows:

A and B are persons. We write the order of action from left to right: \(A_p B_q\), where \(p\) and \(q\) are quanta or durations of action. We write sequent events down the page, or across it, in order of their occurrence. We subsume these persons’ words, doings, states, values, etc to their places in the sequent strings and treat the outcome as it results. In an exchange system of economic distribution, then, like the *kula* ring:

\[
\begin{align*}
AB & \quad \text{A gives to B} \\
BA & \quad \text{B gives to A} \\
AB & \quad \text{A gives again to B} \\
BA & \quad \text{B gives again to A} \\
\end{align*}
\]

The sequence is \(AB\ BA\ BA\ BA\ AB\ BA\ etc\)

The last \(BA\) is a “fitting” pay-off, an “honorable” settlement of the “obligation” which arises at the prior point \(AB\ BA\ AB\ BA\ AB\ . . .\), where the last \(BA\) has not yet occurred to balance the string and complete its form out of the process unfolding and where \(B\) is (subjectively) under the tension of still “owing.”

The *kula* ring or structure thus simply adds individuals in the same form. It makes a ring of the reciprocal exchanges. These individuals added, the ring of sequent events has immediate newly emergent features. It has a direction of flow. \(AB\ BC\ CA\) is one direction and \(CB\ BA\ AC\) is another. Anything having made the flow is a mnemonic of the exchanges and of the flow direction it passed from one person to the next. “Valuables” (durables) are born. Longer strings soon exist, \(AB\ BC\ CD\ DA\) and its reverse, and the process is “instituted” (established by recurrence), as Polanyi would have said. Note that now the structure, or system, is fuller, more regular in each repetition, and that it has its formal consequences for all to see. Any dyadic event, any exchange, say \(D_q C_r\), or \(DC\) is a role of which \(q\) and \(r\) are attributes and any relationship, \(DC\) and \(CD\), is a sum of past events of the action of \(C\) and \(D\) on one another in the context of the ring made by the rest of the persons. Relationships are thus reciprocals (as Warner long ago called “kintypes”) or reciprocal roles, if you like, but both are emergents, not primal postulates. They are not figments of the mind, but perceptions of experience, products of history for all to see, native and scientist alike. Let history regularize such events among enough such persons and an organization of behaviors through time arises and a *form* appears as tangible as any other. By regularizing and positioning added links the ring is built, and the full process soon has
its own products. Some of these appear to us as cultural values and in personal and motivational logics, like the obligation here to pay off. Mauss’s lovely primal principle of Reciprocity was this obligation. But it is not an axiom, it is clear. It is a systems-product. Other outcomes are more tangible: “artefacts” such as, here, the mnemonic valuables. System, structure, process, emergent form, and resultant product come properly from the arrangement and the linkage of persons and thus behaviors in the repeated and the ordered ways of the sequences they follow.

Thus exchange theory, the perception of exchanges, is powerful but partial, neither primal nor sufficient, but still much needed in larger unification in social science. Some of our cultural data do indeed, like the kula ring, represent exchanges and systems of exchanges. They can be distinguished from others which are not easily so represented. We can go on to explore the usual parameters of their spacing, timing, limits, to ascertain their ecological or other enabling conditions, and to follow their energy-flow inputs, throughputs, and outputs as natural systems of ordered behaviors. But even these systems, so strong in dyadic exchanges, are characterizable empirically only by further defining exchange itself through specifying persons and the orders in which they act. The exchanges alone do not make the system; the stringing of exchanging pairs into rings is obviously equally crucial; the relative absence of other groupings than the pairs is again obviously of noteworthy force.

Consensus theory, Parsonian and other, is another strong part and enduring theme of sociology. Men accept leaders, agree on equities of distribution and allocation, comply with power or convention, sanction nonconformables. Anthropology is equally concerned with such realities. In ritual, in politics, in rules and manipulations of rules, in law, custom and religion, consensus, compliance, conformity, sanction are all also themes of ours. Consensus also requires redefinition in kinesthetically operational language, both for itself, to put it into usable general and common terms for all observers, and also, for comparison, to distinguish it from exchange above.

Note at once, then, that exchange, as we represented it, did not need to note events beyond the pairs and the strings of pairs of acting persons. It was a calculus of dyads and the addition of dyads. Empirically, of course, all observers see some events in cultural and social life more clumpy of individuals than interacting dyads, events in which some persons seem to act together at the same time. Sometimes their actions cannot, perhaps, be usefully treated as individually sequent but instead as concurrent.

Many of the events we observe in watching the behaviors of consensus are of this kind. Can we not take account of the simultaneous or near-concurrent action of persons in our notation of our operational description of interactional events?

In the many empirical studies of leadership in anthropology and sociology, consensus arises in the led as compliance by most of them to the decision-making initiative of their leader. If we reorder the accounts of such lead-
ership and compliance, we can isolate another structure again and again, without regard to situation or particularity of decision and activity complied with. The accounts show us comparable, empirically attestable, similar sequences of actors and actions. Proofs of the attestation will have to be presented in another place. With very different timing and variable lengths of strings of actors acting, these patterns recur in the published accounts, rising to view when we perform our operations on the run of the events recounted.

For example, for shamanism (7), in accounts of cult leadership in group-healing or for crisis-handling rituals of intensification, in Omar Khayyam Moore's account of divination (13) the process of leadership and compliance-winning stands out. Again let letters be people. Let the notation / (bar) separate actors, as A/B, for an event, the initiants of the sequence, and let the notation // (double bar) separate, for other events, an initiants and another B, addressed but uncompliant, failing to act sequently. Indeed A //B, or blocked or failed coaction, is frequent enough. Further, let it now be the convention that two or more actors seeming to act, or to fail to act together at the same time and in no distinguishable order as between themselves shall be written together on the appropriate side of //, so that one /many and many/one interactions may be noted where they occur. Thus A/BC etc, where A acts first and B and C respond in unison. Then shamanistic leadership goes like this:

\[
\begin{align*}
A/D & \quad \text{A client seeks the shaman; he notes and waits.} \\
B/D & \quad \text{Another concerned member of the client group does same; he waits.} \\
C/D & \quad \text{Others still, perhaps; he waits . . .} \\
\ldots & \\
D/ABC & \quad \text{He acts now, to assemble them.} \\
D/ABC & \quad \text{He fixes their attention and responses, with tic, rattle, trance.} \\
D/ABC & \quad \text{He builds their tension with these theatrics and rhythmics.} \\
\ldots & \\
D/ABC & \quad \text{He divines at climax, reads a random decision; they go off in unison, to find game or kill a witch . . . etc.}
\end{align*}
\]

Formally and minimally generalized, then, the pattern, in real life from case to case in various quantifications of the persons and the strings of these sequent and concurrent actions, is a run of events A/D, B/D, C/D followed by a run D/ABC climaxing in a leadership act of "joint decision" (our wording) and a communication of supernatural power (perhaps theirs) accepted in consensed, compliant joint activity. The early events can well be thought of by us as information inputs, of many persons to one person of their group. The later events we can see as decision making after information sorting and storing in that same one person for the many, a shared decision-making "mechanism" in the natural social behaviors of the human band. This may
well be a mechanism adaptive in the saving of energy and time and information processing certainly superior to random and individual search. The mechanism is a structure, like reciprocity, with cultural products in the beliefs that explain it to the participants and with the artefacts and rituals well evolved to heighten the motivation for consensus and joint action.

Once again a cultural form emerges from recurrent and ordered behaviors. But these make a process ordered very differently and productive of a very different form and outcome. Consensus and exchange are indeed different emergent structurations of human social life, and the cultural and institutional products to which they give rise are very different ones. But they yield to a common, operational analysis.

Let me remind the reader that human sharing, redistribution, an economic system of a different form from that of reciprocal exchanges and a system found in temple economies and in kingdoms of the human record, climaxed in “oriental despotisms,” takes this form. Leadership from Lévi-Strauss’ (10) Nambikwara chief upward to empire and outward to modern formal organizations, shows a pyramidal combination of such events and structures. These order prior flows of information reporting and stores-assembly upward and inward on a center, and they link to them subsequent set events, in pyramidal form, coordinations, decisions, and doles reaching downward to the ranks. These lowest ranks and broadest groups of the coordinated rank-and-file are the ones who consense and coact in concurrent compliances.

The discovery of the pyramidal hierarchies of coaction structure was already announced in the identification of “set-events” by Chapple & Arensberg and Chapple & Coon (3, 4), set forth in the second section of this chapter. The structures of larger and larger institutions are conformable and isomorphic, and despite their great complexities and many-stepped echelons, they now seem chartable and reckonable. There is even hope that social structure, like atomic, organic, physiological, and phylogenetic structures in other sciences, will disclose further common isomorphies of coaction reaching throughout the natural world and including in it the kingdom of man (8).

Let us likewise remember that such an operational notation for leadership events and for consensing and coordinating structures is not simply a family of definitions. The terms of the operational language, if they are to work, are quantifiable. In some real cases, the specification of the nodal person, center of pooling resources or judge of reported events and their resolution, is unique and sole. In others it is variable and fluctuant, as shamans and cult-leaders or hegemons wax and wane and take on or lose ranks among their competitors in the eyes of followers free to move from one to another. In others, Quaker meetings and Akan kingdoms and Mesoamerican civil-religious hierarchies, headship is rotated regularly, and the centering role moves about. A/BC and C/BA and B/AC are set-events, one/many coordinating initiatives to be compared and to be counted. They are to be celebrated not for their logical sufficiency but for their comparative empirical occurrence.
The properties of systems and structures are variant and quantitative. We must use terms to define them capable of such measurement; the orders, the directions, the boundaries we find in them are no less concrete. There is plenty of mathematics to fit them, whatever their constants and their topologies of concretely invariant form. But it is not analogues we seek, but eventually calculable models of best fit. Strings and pyramids, for example, are such models, though simple ones, if we remember we are not speaking in metaphors. We are making empirical discriminations. Are there or are there not in exchange systems many dyads linked in events? How many in this and how many in that one, with what consequences, are verifiable questions, with good data. Are there or are there not inflows and poolings followed by doles and consensed decisions in our data? The test is that separate observers can find them again when we check.

Let us remember one more thing before we proceed. Data discriminated in one place in real life turn up again combined together in another. Structures and systems are combinations of properly and usefully distinguished parts. Thus it was in Polanyi and Arensberg's work on markets (15), and in Arensberg's assessment of the guaranteed income (1), both of which brought the empirical data of economic behaviors from anthropology into juxtaposition with the formalities of economics. The market system of the Great Transformation turned out to be a new institutional emergence, an evolved cultural form sweeping the earth for a time rather than an eternal verity of man. It was unlike reciprocal exchange and unlike redistribution in that it combined them in its own special way. The argument can be followed in full elsewhere. Quickly and formally stated, it was simply that the market was an arena open to all comers, where any contestant could take up either role of supplier or demander as his information on prices and his interest moved him. He was no longer fixed partner to reciprocating others nor bound deliverer to a fixed center of doling outward among deliverers.

The form of the distributive mechanism called the market was different. A's offer to B and B's refusals were many and frequent, as were A/C and C/A and C/B and B/C, where any comer could and did reverse his roles. Limits existed, not in centering, but in freedom to withdraw on the one hand, and in "cornering the market," on the other. In that last phenomenon A/BC... and B/AC... and C/AB... out to A/BC... N could and did occasionally take place, and formally at least "any comer" could make all the others, the "whole market," dance to his tune (pay his price). The market is thus both exchange and consensus, but in combination and with much more in ordered and structured, quantitatively distinguishable coaction. Certainly, as we have already said, to say that the market, that iron "invisible hand," comes only from the exchanges of A/B and B/A and C/A and B/C etc is to falsify the facts of observation.

All this leaves us with conflict and conflict resolution, the third and last part of the omnis sociologia of today. We have learned a great deal about it in cultural anthropology in recent decades, through Gluckman, Beals and Sie-
gel, and many others. In segmentary oppositions of acephalous polities, in the recurrent class and ethnic group combines of Warner's *Yankee City* and Gerald Suttles' *The Social Order of the Slum*, progress toward the dynamics of the phenomena is clear. Though terms remain undefined and operational redefinition is barely begun, still it seems clear not all conflicts go unresolved nor end in massacre and flight (though many do). Recurrent conflicts and partial resolutions end in the truces of relative ranking and in the compliances that mark submission to power. Once again the behaviors are those of individuals and clumps of individuals and we can note them, tentatively, simply enough:

<table>
<thead>
<tr>
<th>Conflict</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>A/BC</td>
<td>A wants an outcome (policy); rallies his men.</td>
</tr>
<tr>
<td>D/EF</td>
<td>D wants something else; he rallies his.</td>
</tr>
<tr>
<td>A/D/EF</td>
<td>A &quot;forces&quot; D to comply; his followers comply.</td>
</tr>
<tr>
<td>D/A/BC</td>
<td>Next time, perhaps, D &quot;forces&quot; A; his men comply.</td>
</tr>
<tr>
<td>or else</td>
<td>A counter-victory, or a compromise.</td>
</tr>
<tr>
<td>D/A</td>
<td>D tries and loses; A will not.</td>
</tr>
<tr>
<td>and next again</td>
<td></td>
</tr>
<tr>
<td>A/DEF or A/D/EF</td>
<td>once more. A has power, in a second trial of initiative, or strength.</td>
</tr>
</tbody>
</table>

Let the reader try noting the runs of the events himself. Conflict and conflict resolution are also events of social action and can be treated like the other kinds and distinguished from them. We now have a common language for all of them: exchange, consensus, and conflict (and its resolutions).

**Minimal-Sequence Models: Culture as Emergent Form**

Lastly, can unification clarify our view of culture itself? If ordering the data of observation into operationally comparable and contrastable sequences of interpersonal acts of the actors can give us summary and distinguishable redefinitions of the theories of modern sociology, can similar reordering aid our understanding of cultural forms familiar to us in social anthropology? Can it both return us to the basic common matter of our science and bring together the cognitive, jural, and strategic models to Barth with the "surely complementary" white-box conditions, the unconscious (but formal) models of social structure and process, and with the technics and ecological adaptations of the schools named and celebrated by Selby? The answer is yes. They can put us on the way to such generalizing power.

The way to such power leads through a better understanding of modeling procedure. We are in need of a much sparer and more operational notation than we now possess. The first reformation, in modeling, is already introduced to the reader: sequencing. It requires we model the data in the natural order or sequence of their most frequent occurrence (chaining or tree-diagramming them in the sparsest way we can). With Jack Goody's domestic cycle and Norman Whitten's three generations of strategies uniting Afro-his-
panic leaders to Highland white aristocrats and politicos in Columbia, or Wittfogel's 300-year cycles of dynastic health and decay in the hydraulic empire of China, we have already begun to discover process empirically. But we do not use it systematically as yet in theory and model building, especially not for social structure.

The next requirements, already discussed as operational procedures, demand we translate our ethnographic data into regular recurrences of specified interpersonal coactions. We must do this for all of them. We must do this in minimal terms, with Occam, only once for each occurrence of the order we note as we proceed to preserve and record the recurrences. We string the rest of the data, not so translatable, into their places in this order.

For example, we already know from the biogram of the human community that parents precede children. In kinship, of course, they are parented to parents themselves before they are married to a spouse, mated with that spouse before their children appear, and are affiliated to them in term. Our genealogical charts, refined as kinship diagrams, mark such sequences faithfully enough for successive generations, but our treatments of nonkinship do not. Instead they often use correlation models in one form or another, seeking to relate to kinship data a kind of such nonkinship data: household, residence, access to fields, gifts and presentations, etc without regard to sequential occurrence, but timeless, simultaneously. There is much power in this; and we have learned a great deal from it, as we have also learned from the uncovering, by componental and formal analyses, of the timeless internal logics of kinship nomenclature systems. But we do not yet use sequential, processual models which will state the relationships among all these things in summary form.

If social structure is a regularization of behaviors and a system interrelating them, then it is also a process, relating them in some order and at some rates.\(^2\) A minimal, most general but empirical representation—a processual model—will show the repetition of the relationships between elements or factors of the system which occurred most frequently, in which the order of the occurrence is most established and the outcome most constrained. The outcome, the combined or synergetic product, is an emergence, a "new" thing or

\(^2\) Lévi-Strauss' classification of culture into three kinds of "exchange" or communication (of messages, of goods, and of women) was a neat uniting of language, economics, and formal social structure, but more philosophical than empirical. It is equally easy to think of them as social behaviors at three speeds of regularly ordered recurrences of human coaction in their resultant cultural forms; very fast, second by second (language and communication); middling rhythms, daily to annual, from meal to meal and crop to crop; and long beats of 15 to 30 or more years, generational time-rates, between assignments to mates and parts in inheritances. Women students have complained that social structure is also exchanging men, perhaps an un-Gallic notion. Out of deference, however, to Lévi-Strauss and his more recent French titles, I will be happy to think of the economic exchanges as "events in gastronomic time."
state. Let us look at familiar kinship systems in this light. Particular cultural forms, even in kinship, may be such new things.

Such a structure, or system, rendered as a minimal-sequence model, is a "mechanical model" in the sense of Lévi-Strauss' classical article (11). Yet it is not a formal or logical model. We need not think it exists only in the heads of the natives or on the drawing-boards of the formalists. There is a real structuration to "cognize," as real as a cloud, a melody, a dance, a hurricane, a harvest, or a winter. All of them are real conjugations of events in time and space among elements we can recognize. Structural arrangements as they are, they have occurred and will recur again, like DNA. If they play out their forms they will bear their fruits.

Take the familiar Dravidian kinship system. What are the factors and attributes, in quick summary, we know it to have? Like all kinship systems anywhere, it is a kinship nomenclature, a cognitive map of "kintypes," a special mapping of biological, marital, and group-membership offices and categories of persons. It is also a code or table of interlaced rules of residence and filiation, household compositions and settlement rights. It is a roster of rules of incest prohibition and mate choice, prohibiting some matings and marriages and preferring or prescribing some others. It is a rulebook of rights to properties and alliances, ascriptions to groups and exclusions from others, a code of inheritance and property divisions. It is further, of course, a set of roles, obligations to share, rights to parts or supports, in shelter, food, or protection, in solidarities and identifications. It provides each ego of the culture with a place and a stance from which to manipulate, manage, or revamp these things as he can, within the sanctions of his fellows. And it endures so through generations.

But most kinship systems are all these things. The question is not only why do they exist in all cultures, but also why this one rather than another? And why does this one have the form it has? The Dravidian has been reported in many places, from modern Madras, a capital of millions, to Amazonian hamlets of minute size. It has special features: in nomenclature, for example, an equation of father-in-law and mother's brother; in incest-prohibition a division of one's own generation into potential spouses and non-spouses; in group-building and affiliations, both patrilineal and matrilineal, and a wobble between them, stressing now one and now the other in "dysharmonic regimes"; in addition to all this it has as well a preferred rule of marriage, that with a "symmetrical cross-cousin" either fa si da or mo br da (for men) either real or analogous. What cultural or social system can combine so much so distinctively?

We are ready to model it. We can now try our hand at generating the whole system: its attributes, the nomenclature, the categories, and these rules just named. Let us do it from the behaviors reported in ethnography, not from a componental analysis of the logic of the nomenclature alone. Let us try to generate that nomenclature too. Componental analysis has been powerful, as we know. With it, in any one nomenclatural system and with the
assistance of some panhuman facts of biology, given one term one can deduce the rest. But it does not generate any system except from its own rules. It tells us little about why there are alternate systems. Nor has functional explanation, alliance, through marriages and exchange of women, nor descent, allocation of differential rights to livelihoods, given the final word. Indeed, it grows ever plainer that all kinship systems provide not so much descent rather than alliance or vice versa as instead that they all provide both. Why then so many ways to ally over marriages and allocate over memberships? And why the Dravidian rather than another?

Here is why:

Let letters be people: A, B, C.

Dravidians distinguish sexes. We shall use majuscules A, B, C for males; minuscules for females, a, b, c. The distinction seems panhuman.

Ecologically, Dravidians are often (even in India) small-family and smaller household people, but in any event, they live in dispersed gardens or houses on streets as couples (except Nayars). We can count, minimally, to begin with, two such pairs

\[ Aa \quad Bb \quad \ldots \ldots \]

In the biogram, identifying these people, we see these pairs have children, and these are usually kept in and raised in the houses of their immediate parents—"affiliated" there, if you like. They also come in two sexes, and we can so distinguish them once again.

\[ Aa_1 \quad Ba_1 \]
\[ Aa_2 \quad Ba_2 \]

These children grow up and marry soon. The rule of incest requires that these children do not mate together but with another available person. It is usual to move that person in, usually the girl being brought to the boy, who stays with his parents: virilocal and especially patrivilocal residence being most frequent. So we move the girls:

\[ Aa_2 \quad Ba_2 \]

Soon the pairs have children. These also are patrificial, grow up with parents again:

\[ Aa_2 \quad Ba_2 \]
\[ Aa_3 \quad Ba_3 \]

These new ones mate and move as before:

\[ Ab \quad Ba \]
\[ Ab \quad Ba \]

This repeats itself; it is now a regular rule:

\[ Ab \quad Ba \]
\[ Ab \quad Ba \]
\[ Ab \quad Ba \]
When three generations have done the same things in each generation, the full attributes of the system have emerged. They now practice a set of kin-types and a nomenclature of 12 terms: in which, note, for any ego, in third generation $A_3$, shall we say, his wife's fa $B_3$ is indeed his mother's brother, all the women of his own generation are either wife $b_3$ or sister $a_3$ etc, and the 12 formal kintypes are the 12 persons represented minimally in the diagram. Note that we have represented people only minimally, one "sibling" say when in any real case there might have been more, but this is what Radcliffe-Brown called "the equivalence of siblings." Neither we nor they need to know the real and variant number because the limiting regularity is that there be one. Note at once as well that a rule of preferred marriage has arisen: every person of generation 3 is now married to his cross-cousin. Note that there are groups: those designated the A's are a descent line of males, the B's are another of males, and there is indeed, pace Lévi-Strauss, a "reciprocal exchange," an "échange restreint" of women. But note at once also that the new diagram is a simple permutation matrix, a well-known algebraic form. Established in three generations, with these regularities recorded now become rules, it can be repeated in same form, with corrective sanctions, for as many generations and as large numbers as one cares to count.

I will demonstrate later that all kinship systems are members of a family of such matrices. This is the simplest one, as if two "families" were exchanging forever their sons and their daughters. Other systems are periodically different: 3, 4, 5, 6, etc up to 12. But that is a book.

The Dravidian system, a cultural form, is thus most sparely stated as a permutation. This is a minimal summary of the human interactions: moving children, deploying households, etc most regularly recurrent through the relevant time, here biogrammatic and generational time. It is not a formal structure in the cognitive sense, or even the functional sense, since the nomenclature is generable from the behaviors, which are local and specific, not from the biology, which is panhuman and can thus account only for the necessary conditions of all kinship rather than for the form of one system rather than another. It is combinatory, not only in an exchange of women, but also a chart of rules for households, for affiliations, for descents and inheritances as well. It is perfectly maneuverable for ego, of course, but that fact does not make it. As always in sequences or in any relational or topological system such as this, any element has a location. The link above ego and the link below him are separable from his vantage point, but the links are merely successive to an outsider. Put ego at coordinates-origin 0, then he had $-A$, a father, and $+A$, a son, but the three of them are three persons $A_1, A_2, A_3$ in the chain $A_n$. That each is a sentient human being is a necessary but hardly sufficient cause for a human chain.

Thus none of these attributes or functions or parts of the structure gives it its form or generates emergent products such as matrlines, patrlines, or the rule of preferential marriage. But their combination does. These things are
synergetic, emergent system-outputs or resultants. Form has emerged from process and process combines the factors here, the behaviors of ecological adaptation, settlement pattern, household composition, affiliation, rule of residence, etc, etc. Their occurrences are variable, of course, but their combination is a unique cultural form.

Let us try one more processual, minimal-sequence model, and then call halt. The Sudanese kinship system, especially in the Bedouin form, with its very special rule of preferential marriage with parallel cousins, is “opposite,” or seems thus to the Dravidian. It is another much discussed and analyzed cultural form. It appears among villagers and mountaineers through much of Islam, but it has been explored deeply for its putative relationships to Bedouin camp life and camel nomadism. Let us take no sides in the debate about it, but instead marshall the reported ethnography and model it. Why this cultural form?

Here is why, again:

The reader remembers that in the Sudanese nomenclature, all four first cousins have different designations. Marriage is patrilocal; there are minimal and maximal patrilineal branching segments from apical ancestors. Segmentary oppositions of senior and junior lines trace back to elder and younger brothers. Camps are fluid in composition, separable and regroupable at need, herds poolable but separable again, camp members are agnates if of full right, or noble, client and slave if not, of the patriline. Blood brotherhood binds agnates to five generations (third cousins), all called ikhwān or brothers, the vengeance group. All agnates inherit partibly; chiefly office goes by tanistry (brothers exhausted in choice of successor before sons). Households are variably composed of patrilineal agnates; women are moved about so that nomads carry them with them in camps; names recite agnates in order: A son of B son of C etc. Groups are sons of common apical males, at all levels. Teknonymy: A father of B, when B is born. Levirate, easy divorce, frequent remarriages, polygamy permitted, only prohibitions are daughter, full sister or mother (for man), preferred marriage with prescriptive right of first option as spouse man with fabrda, woman with fabrso (bint and ibn 'am). This of course is lineage endogamy. Our job is to put all these into the one structure or combination, the Bedouin effect: their kinship system.

<table>
<thead>
<tr>
<th>Gen. 1</th>
<th>Ab</th>
<th>A man and his woman, encamped.</th>
<th>Myth: Adam had 2 sons, they took wives;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gen. 2</td>
<td>Ab Ba</td>
<td>A man has two sons, who take women. They move about, separate, regroup.</td>
<td>Myth: Sisters or daughters of giants?</td>
</tr>
<tr>
<td>Gen. 3</td>
<td>Aa Cc Bb</td>
<td>Each has two sons in turn; they move about, separate, regroup. A₁ and C₁ are a segment, brothers as sons of A₂. B₁ in Gen. 3 and his brothers divide</td>
<td>All noble Arabs are either sons of Yarab or</td>
</tr>
</tbody>
</table>
CULTURE AS BEHAVIOR

off, may or may not oppose, may or of Qahtan, of Gen. 2, another segment.
may not go off; they are sons of (beni) etc.
B₁

Ab Ca Bc

Women are carried about, moved into any tent except that in which they grew up (incest prohibition of full sister, the one sharing one’s father and mother). This is not exchange but reshuffling.

Gen. 4 Ac Db Cd Ba

A and D are senior segment beni A₁, C and his brother, whom we need not record, form another segment also beni A₂ with them, but B and all his line are beni B₂ of beni A₁.

Gen. 5 Ad Eb Da Ce Bc

Repeat each man has two sons, repeat moving the women anywhere at all at random except that Aa Ee etc (brothers and sisters) must not mate, but that the women a, e, etc move to any other near male, except father (and grandfather; immediate agnates).

The full permutation matrix which now arises is:

\[
\begin{align*}
& \begin{array}{c}
\text{Ab} \\
\text{AbBa} \\
\text{AcCbBa} \\
\text{AbDcCaBd} \\
\text{AdEcDaCbBe}
\end{array}
\end{align*}
\]

In five generational recurrences, a minimal structure arises in which (note in generation 5) all of one’s generation are brothers and sisters (beni A₁) together, but segments sons of A,B,C,D, and now E are present or potential (when E₂ and A₂ have their sons), the fifths so often noted. Every one is descendant either of A or of B, the duality so famous. And in this last generation, the females, no matter how moved about in divorces and levirates, carried along with the tribe and the lineage, are for A, one can see: d, his ukht (my sister my bride, said the Song of Solomon), who is his fabrda, his wife; b, his classificatory sister (ukht) also as B’s daughter his classificatory fabrda, the remote not the immediate one, who is also his mother’s brother’s daughter (bint el khāl); a, his “real sister,” the ukht he may not mate with or marry; e, the other of the two daughters of his father, the same, and c, his classificatory sister, his other fabrda, not the immediate one whom he married but the other(s) of the tribe and lineage who are potential and preferred spouses with whom other marriages and alliances can be made beyond her to her father(s), as ‘am or father’s brothers. All older men of the lineage are thus such father’s brothers classificatory or real, just as all of one’s own generation are brothers.

This, then, is the model of the system. Once again, it has as many places of the matrix as there are kin types and categories. We have generated the nomenclature once again from sequencing the recurrent regularities of Bedouin behavior.

Let the reader try his own models next.
LITERATURE CITED


CONCEPTUAL PROGRESS IN PHYSICAL ANTHROPOLOGY: FOSSIL MAN

BERNARD G. CAMPBELL
Department of Anthropology
University of California, Los Angeles

The aim of science is to seek the simplest explanation of complex facts. We are apt to fall into the error of thinking that the facts are simple because simplicity is the goal of our quest. The guiding motto in the life of every natural philosopher should be, seek simplicity and distrust it.

Alfred North Whitehead (127, p. 163)

The strictures which encompass and define scientific method are highlighted in an observational science such as paleontology, which permits practically no experimentation. Progress in the study of human evolution based on the fossil record has been beset by nearly as many problems as it has solved. While today we know far more of the fossil evidence than those who wrote early in this century, we have also come to realize more clearly the theoretical difficulties which stand in our way. We know that we can never do more than present hypotheses on the basis of presently available evidence. As time-bound creatures, no ultimate truth about the origin and evolution of mankind can ever be known to us.

The recent discovery of so many fossil hominids has, as we shall see, opened up a wider range of hypothetical possibilities than have been appropriate in the past. Those fossils known earlier in this century, and indeed as late as 1955, could be fitted into a relatively simple and not very controversial phylogenetic lineage. The numerous fossils now known offer alternative interpretations (Figure 1). Since the number of possible hypotheses are both theoretically and practically unlimited, it is essential in our assessment of the present position to evoke the principle of William of Ockham that plurality should not be posited without need (essentia non sunt multiplicanda praeter necessitatem), but as Whitehead has pointed out, this does not mean that the truth itself is necessarily simple (127).

In this review we shall discuss first some of the most important fossil discoveries since 1955,1 and then consider their conceptual significance.

1Archaeological sites lacking fossil hominid remains are obviously relevant to an understanding of human evolution. In the present state of our understanding of cultural variation and the taxonomy of tool assemblages, however, it is most unwise to equate a particular culture with a particular hominid taxon. Because of limited
Figure 1. Diagrammatic representations of some hypotheses of human evolution. A after Clark (24); B after Campbell (16); C after Brace (9); D after Tobias (109); E after L. Leakey (61); F after Napier (82). Sap = Homo sapiens; N = H. neanderthalensis; P = H. erectus; H. = H. habilis; A.a = Australopithecus africanus; A.r = A. robustus; A.b = A. boisei; Pa = Paranthropus; R = Ramapithecus punjacicus; K.w = Ramapithecus wickeri; and K.a = Kenyapithecus africanus.
FOSSIL MAN

NEW FINDS: NEW DATES

Exploratory work, both in the field and laboratory, has given us not only many new fossil specimens, but a great deal of information about the culture, ecology, and behavior of early man. The significance of these factors will be briefly discussed in the second part of this chapter. No less important, we have new means of assessing their age (Oakley '83). Without knowledge of the antiquity of individual fossils, both relative and absolute, their significance as human ancestors is almost impossible to evaluate. Knowledge of their age is almost as important as the fossils themselves when we come to construct an hypothesis of man's evolution.

The year 1955 saw the final solution of the Piltdown problem and the disappearance of the notorious Piltdown skull from the fossil hominid record. This cleared up a real difficulty in understanding man's evolution (Weiner et al 124, 125). The way was now clear for a big step in palaeoanthropology. The opinions most generally accepted at that time are reflected in the book by le Gros Clark (24) which succinctly reviews the fossil evidence and presents a reasoned and simple interpretation of it. What controversy existed related to the status of Neanderthal man and the length of time attributed to a human lineage independent of the Neanderthal people (see, for example, Vallois 119). The stand taken by le Gros Clark was conservative and fell in line with the important studies of the Neanderthal question by Howell (38, 39). The situation at that time is roughly indicated in Figure 1 A.

The other area of controversy related to the South African Australopithecus fossils from the Transvaal. Most workers recognized two separate but related lineages (as genera or species), one of which (Australopithecus africanaus) led to modern man, and the second of which (A. robustus) became extinct. While their relative ages were generally agreed upon, their absolute ages were and are not certainly known. The final vindication of Dart's claim of their hominid status (29) had come with the discovery of the pelvic bones (Broom, Robinson & Schepers 12), and since that time only Zuckerman has remained in doubt (Zuckerman 136; see also Campbell 20). There were also doubts about the status of a third hominid ("Telanthropus") which Broom and Robinson claimed was present at the Swartkrans site. Le Gros Clark believed it was A. africanus (24), while later authors considered it a South African Homo erectus (e.g. 19, 92). As we shall see, some of these problems can now be resolved.

Since 1955 we have become familiar with many new fossil hominids, an entirely new species, together with a fossil genus of an earlier age which is claimed to be a hominid. Our review of all recent hominid discoveries will be broadly taxonomic. We will consider in turn the different species into which space, it has proved impractical to discuss cultural evidence in this review of fossil hominids, although some sites lacking hominids are mentioned briefly in the second part of this chapter.
the Hominidae have been divided, starting with the most ancient members. For these purposes we shall consider the following taxa, though they do not represent the author's final assessment of the material.

*Ramapithecus wickeri*
*R. punjabicus*
*Australopithecus africanus*
*A. robustus*
*A. boisei*
*Homo habilis*
*H. erectus*
*H. sapiens*

*Ramapithecus wickeri.*—This taxon, represented by the fragments of a single individual, is now generally recognized to be the earliest known hominid. The fossil remains consist of an incomplete upper dentition in two maxillary fragments and part of the left side of the mandible (Figure 2). They were found by L. S. B. Leakey in 1961 at Fort Ternan in Kenya, and the upper dentition was described by him as hominid and named *Kenyapithecus wickeri* (58). Soon afterward, Simons (100) demonstrated the close affinity of the maxilla with the fossil hominids from the Siwalik hills of northern India (discussed below). The lower jaw fragment from the same site was later recognized by Andrews (1) as belonging to the same individual. The volcanic deposits at Fort Ternan which overlie the bones have been dated by potassium/argon measurement to be 14 million years of age (33).

This is a fossil of unquestionable importance, bearing characters of both man and ape. On superficial examination it seems very ape-like—far more so than *Australopithecus*—but the total morphological pattern is not that of an ape and varies from it quite specifically in the direction of man. The features which distinguish it from a pongid are its broad, shortened, flat, compressed fourth premolar and molars, which are set in a relatively shallow mandible as in a gracile *Australopithecus*. The third lower premolar is semisectorial with incipient development of a second cusp. The molars are subequal in size, with simple hominin-like crowns. Canines and incisors are probably arcuate and

3 A list of fossil hominids, together with their best published dates and their geological, stratigraphical, archaeological, and faunal context, can be consulted in the *Catalogue of Fossil Hominids*, edited by K. P. Oakley, B. G. Campbell, and T. I. Mollison (84), for Africa (1967), for Europe (1971) and for America, Asia, and Australia (1972). A revised edition of the Africa volume is due to be published in 1973. All are available from the publisher: British Museum (Natural History), Cromwell Road, London S.W. 7, England.

In this chapter, references are given primarily to new discoveries, new dates, and new opinions. No attempt has been made to repeat the full selected bibliography which is published in the *Catalogue of Fossil Hominids*. Where dates are quoted in this chapter it has not seemed appropriate to give full details of all dating evidence, nor of the calculated error or laboratory number of the determination.
Figure 2. Photographs of the oldest hominid *Ramapithecus wickeri*, showing occlusion of the upper and lower jaw. A. Buccal view of left maxilla and mandible in occlusion. B. Lingual view of the same. C. Occlusal view of the left maxillary fragment. D. Occlusal view of right maxillary fragment. All printed to the same scale. Photographs kindly supplied by Peter Andrews.
smaller in relation to cheek teeth than in apes. No diastema is present. The face is shorter and deeper than in the Pongidae.

No one feature of this specimen can be characterized as hominid, as distinct from pongid, but the total morphological pattern is significant and falls very close to what might be predicted for a form intermediate between an early *Dryopithecus* ape and *Australopithecus*.

*Ramapithecus punjabicus.*—Soon after the discovery of *R. wickeri*, Simons, as we have said, demonstrated the close morphological affinity of the maxillary dentition with that of *R. punjabicus* from India (70). Specimens of this taxon were first found before 1910 (88), but only later were claimed to be a hominid by Lewis in his unpublished PhD thesis of 1937, and then by Simons (99). However, the mandibles of the Kenyan and Indian samples are quite distinct, and in combination with the upper dentition it is clear that the Indian form is more man-like. This fits in well with its more recent age which is probably less than 14 and possibly as low as nine million years BP (before present). The age estimate is at present based on faunal correlation and does not have the authority of an absolute age determination by K/Ar measurement. The Indian fossils come from a number of sites in the Siwalik hills near the Indo-Pakistan frontier. The material has been listed and described by Simons (102), and consists of five mandibular and two maxillary fragments with teeth from different individuals. It differs from the older African form by carrying the incisors in such a way that they form an almost vertically oriented cutting edge. Though the canines are not known, the fragmentary bones carry a parabolic dental arcade as in *Australopithecus*, which together with the size of the sockets, suggests that the canines were small. The third premolar unfortunately is not known, but it is predicted to be bicuspid.

These two early species of *Ramapithecus*, which in all probability date from the late Miocene and early Pliocene, have been widely recognized as hominids by paleoanthropologists. Some workers feel that the evidence is still too slight for any certainty in the matter, while others believe the fossils to be those of apes. Some claim that the biochemical evidence for the close similarity of man, gorilla, and chimpanzee excludes such a long independent lineage for man (Sarich 97). Others go to the opposite extreme and recognize hominid fossils from even more remote times, such as the early Miocene *Kenyapithecus africanus* (59), but the hominid nature of this fossil is not generally recognized (Pilbeam 87).

A fossil tooth from the Pliocene site Ngorora in Kenya has been classified as a hominid. Its age is in the region of 9 million years BP (Bishop & Chapman 7).

*Australopithecus africanus.*—Since 1955 there has been intermittent work at the South African sites of Sterkfontein and Makapan, especially in the last few years (114). New discoveries have been limited and do not add greatly to our knowledge of this group, though the recent announcement of
the discovery of a cranium is an important addition (Tobias 113). Finds from East Africa which many workers believe fall into this taxon are discussed below under *Homo habilis*.

The deposits at these sites have proved unsuitable for absolute dating up to now, so our estimates of the age of the sites are necessarily based on faunal correlations with well-dated sites in East Africa and elsewhere. Traditionally, the age of Sterkfontein and Makapan have been considered in the region of 1 to 1½ million years BP. Today the latest faunal work suggests a date twice as old—from about 2.5 to 3.0 million years BP (26).

* Australopithecus robustus.*—The fossils which traditionally fall into this taxon are from the Transvaal sites at Kromdraai and Swartkrans (11). Extensive work has been carried out at Swartkrans by Brain (10), and 25 fossil fragments have been added to the fossil sample from this site. Of considerable interest is a natural endocranial cast which shows that the specimen had an endocranial capacity of 530 cc (37). This is the only evidence that we have of the size of the brain of this group. There is a stone culture at both these sites (64).

The evidence of a second hominid ("Telanthropus")—a more gracile form—at Swartkrans has been enhanced with new small specimens and a new reconstruction of the face of a cranium (25). There are still those who believe that a more advanced hominid was present (25) and those who believe that these small specimens are merely small members of the population of *A. robustus* at this site (129). There is also unpublished evidence of small individuals at Kromdraai: this amount of variation may well include considerable sexual dimorphism.

The deposits at these sites also remain undated. The latest estimates based on faunal correlation suggest an age of 1.7 to 2.0 million years BP (26).

* Australopithecus boisei.*—The most important developments in African prehistory have occurred farther north. At Olduvai Gorge in Tanzania, Louis and Mary Leakey found their first Olduvai hominid remains in 1954—two milk teeth at site BK in Bed II (56). At the time their interpretation was controversial, but today they are generally accepted in this taxon. Further excavation followed, and in 1959 Mary Leakey discovered a very robust cranium (without mandible) which was published amid great publicity and called *Zinjanthropus boisei* (57) (site FLK, Bed I). In 1964 a very robust mandible was found at Lake Natron (at a site by the Peninj river) which clearly belonged to the same taxon (62). Tobias, who studied the Olduvai skull, concluded that this fossil represented a species of *Australopithecus* morphologically as different from *A. robustus* as that taxon is from *A. africanaus* (109).

The deposits of Bed I in Olduvai Gorge are perhaps the most securely dated in the entire Pleistocene sequence. Over 50 samples have been
dated and suggest an age range from 1.8 to 1.7 million years BP (Hay 36). The level of the cranium is dated as 1.75 million years BP.

The milk teeth and skull from Olduvai and the jaw from Lake Natron (with an age of ca 1.4 million years BP) were the first representatives of a species which has become very well known today. At Olduvai, as elsewhere, it occurs with a second, distinct species of hominid which has been called Homo habilis; in one instance they occur associated on the same living floor (site FLK, Bed I). Homo habilis, which is closely related to the Transvaal A. africanus, will be discussed in the following section. Meanwhile we shall review further discoveries of A. boisei.

In 1967 an international research expedition began work along the west bank of the Omo river in South Ethiopia, in a series of deposits now known as the Omo beds (13). Since that time, fragments of fossil hominids which fall into the taxon A. boisei have been found at many sites in this area in deposits dated from 3.7 to 1.85 million years of age. As at Olduvai, the remains are present with those of gracile hominids, though not associated with them on the same living floor. Approximately 150 teeth were found which belong to this taxon including many of great size, and there are more than 8 pieces of mandible, a complete ulna (a rare find), and the back of a juvenile skull associated with the shaft of a humerus (Howell 41, 42; Coppens 27, 28).

In 1968 and subsequent years Richard Leakey took a Kenya-based expedition to the desert area east of Lake Rudolf. More robust hominids were found here in somewhat similar deposits dating from about 2.5 to 1.5 million years BP. The dating and stratigraphy are still preliminary and remain under investigation (49). The hominids again include two kinds, one of which is extremely robust (67–69). The robust specimens include remains of 16 individuals, with one cranium, one half cranium, nine mandibles or mandible fragments; two femora, a tibia, and a humerus (all of which are incomplete). This site represents the richest source of A. boisei fossils known to date, and it will no doubt yield more. Of particular interest are the postcranial bones which are not yet fully published. The taxon shows much variability, including considerable sexual dimorphism, and can be recognized cranially by the characters of the A. robustus skull in much more extreme form. The face and jaws are immense; the whole masticatory apparatus is extremely powerful. The braincase carries a sagittal crest to anchor the immense temporal muscles indicated by very wide zygomatic bones. The molars are huge, as are both premolars, which are fully molarized; they are over half as big again as the robustus teeth. The whole masticatory adaptation and the rapid tooth wear suggest that the diet demanded very powerful and prolonged grinding. The only endocranial capacity calculated to date is 530 cc (Olduvai hominid 5, 109).

The postcranial skeleton is represented by some arm and leg bones.3

3 Discoveries of the 1971 season had not been announced at press time.
These have not yet been fully described, but they are characterized by great size and extreme robustness reminiscent of the gorilla. However, they do bear characters associated with the Hominidae, and certainly are not the bones of fossil apes, which are not represented in the East Rudolf area (77).

A fragmentary fossil cranium that has been discovered at Chesowanja in northern Kenya is attributed to this taxon (Carney et al 22; Szalay 106). It is dated at about 1.1 million years BP and is therefore the most recent specimen of the group.

_Homo habilis; East African gracile hominids._—Work at Olduvai (Beds I and II), at Omo, and East Rudolf has yielded fossil hominids which in their morphology and size are very different indeed from the robust *Australopithecus boisei*. These will be reviewed under the name *Homo habilis* without prejudice to the conclusions that are drawn in the second part of this chapter. The nomen was based on the type specimen found in 1960 at Olduvai Gorge, site FLK NN in Bed I (63). It consists of a broken mandible with teeth, parts of two parietal bones, and 13 hand bones. This specimen, and others associated with it in the same species, all have one thing in common: they are clearly morphologically intermediate between the Transvaal *A. africanus* and the later *Homo erectus* specimens from Africa and Asia, though some are almost indistinguishable from the one or the other.

Representatives of this intermediate group have been found in deposits at Olduvai dated between 1.8 and about 1.5 million years of age. Other specimens which belong to this taxon or to *A. africanus* have been found in Ethiopia and Kenya. At Omo they range from 3.5 to 1.9 million years BP, and at East Rudolf from approximately 2.5 to 1.5 million years BP. The number of fossils which can be attributed to this taxon may be in excess of 50, though many of these are isolated teeth. The two finest are a skull from Olduvai, site DK, Bed I (66), and one from Ileret, East Rudolf (68). None of these fossils has received detailed publication, and the taxonomic status of the majority is not yet clear. We refer only to announcements and very brief descriptions (Howell 41–43). Further fossils of gracile hominids that have been discovered include: a right temporal bone found at Chemeron (110); a humerus fragment uncovered at Kanapoi (85); and part of a mandible found at Lothagam (86). These three sites span a time range from about 5.5 (Lothagam) to 1.5 million years BP (Chemeron).

Soon after the discovery of the first *habilis* fossils at Olduvai, Tobias stressed the peculiar nature of their dentition, especially the premolars which he believed to be distinct from those of other hominids (94, 108). At present Tobias believes these differences to be of only minor importance (113), and indeed such variability proves little in view of the known variability of living species in characters of this kind. As a diagnostic feature the endocranial capacity is more important. The four specimens of this taxon of which the capacity has been calculated range from 600 to 684 cc (113), while those of *A. africanus* from the Transvaal range from 428 to 485 cc (37), and those of
*Homo erectus* from Java and China range from 750 to 1225 cc (112). The intermediate nature of this group in the evolution of the brain is clearly apparent. *Homo habilis* is in this respect a link between *A. africanus* and *H. erectus*, and its close relationship to these two well-known species is a clear indication of its morphological position in man's evolutionary lineage. While we await detailed publication of these important fossils, their character and dating is generally clear, and controversy ranges for the most part over their nomenclature.

*Homo erectus.*—In the last 15 years additions to this palaeospecies (32) have not been as extensive as to the last two that we have discussed. Nevertheless new discoveries from many parts of the world have added to our knowledge of the geographical range and morphological variation of this taxon. The first important postwar discoveries came from a sand pit at Ternifine in Algeria in 1954 and 1955, when Arambourg and Hoffstetter found three mandibles, a right parietal, and some isolated teeth with an Acheulian industry. Though named *A nthropus mauritanicus*, they clearly represented the local race of *Homo erectus*, and were very similar to the fossils from Peking (3).

During this period further discoveries were made at or near Sangiran in Java, from both the Trinil and Djetis levels. Here new finds of crania and mandibles added considerably to the known sample, but they gave us only a little new information about the taxon. From the Trinil level we have three new calvariae (making a total of five) and fragments of four others, together with a clavicle, maxillary fragment, and teeth (Jacob 50, 51, Sartono 98). From the Djetis level we have two new mandible fragments and a potassium/argon date associated with one of them of > 1.9 million years BP (52a). All these finds are listed by Jacob (52).

In China, excavation began again in the 1950s, and new sites were opened in addition to continuing work at Choukoutien near Peking (Woo & Chia 133, 134; see also 2). Their most important contributions were the discovery of a mandible and cranium at two neighboring sites at Chenchiaou in Shensi province. These come from a period much earlier than the Choukoutien deposits, perhaps even contemporary with the Djetis fauna in Java (Woo 131, 132). The skull had an endocranial capacity of 780 cc. A careful pollen analysis of the Choukoutien deposits showed that they span a long warm period between cold phases (Hsu 44) which are believed to be equivalent to the Mindel-Riss interglacial of Europe, although others feel that an earlier warm phase, the *InterMindel*, was more likely to be the period of deposition (Howell 40).

It is not surprising that the extensive deposits at Olduvai Gorge have also yielded remains of *Homo erectus*. From site LLK in Bed II we have an excellent calvaria (Tobias et al 116), and from site WK in Bed IV we have part of a pelvis and femur shaft (Day 30) together with an Acheulian industry. In
many features this material again compares closely with the remains from Peking, though it has not yet received very detailed study.

An important discovery was made in Hungary in 1965 in material excavated the previous year from a travertine quarry at Vertesszöllős. Some human milk teeth were found, together with the occipital bone of an adult male of surprisingly modern morphology. It was a large bone and the cranium to which it belonged was calculated to have had a cranial capacity of 1325 (130) to 1400 cc (117). The deposit contains hearths and a simple chopping tool industry comparable with the Oldowan and that from Choukoutien; it is of Mindel age. For this great age the bone is surprisingly modern and contrasts markedly with the skulls from Peking.

These recent discoveries have demonstrated the wide geographical and morphological range of the classic *Homo erectus* type. From Java to Algeria, and from Tanzania north to Hungary and Germany (Mauer, at 49° latitude), the species spread widely throughout the Old World. It is not known at present whether it reached the Indian subcontinent or western Asia, though an Acheulian industry of Mindel age is recorded in India (96), as well as both a developed Oldowan and Acheulian industry from Israel (105).

*Homo sapiens.*—We still have very few fossil representatives of early *Homo sapiens*. Best known are the two famous skulls from the end of the Mindel-Riss interglacial in Europe—from Swanscombe in England (Weiner & Campbell 126) and from Steinheim in Germany, both found in the 1930s. In 1949 a mandible of about the same age was found in France at Montmaurin (Vallois 120). Recent work in France has uncovered material from the beginning of the Riss period at Arago in the Pyrenees (Lumley 73). There is a cranium, mandible fragments, and isolated teeth; the cranium suggesting a fairly close relationship with the Peking fossils. Another group coming from La Chaise, in the Charente region of France, has been dated to the later phases of the Riss glaciation (89), as have some fragments from Lazaret (72).

Thus we have evidence of a wide range of morphological types in Europe from the more primitive (Arago) to the more modern specimens (Swanscombe). This variation persists as the classic European Neanderthal race and its more modern contemporaries such as those from Skhul and Djebel Kafzeh (Vandermeersch 121).

There have been no notable additions to the well-known Riss-Wurm fossils such as those from Weimar-Ehringsdorf and Saccopastore in Europe. From China we have the Mapa skull (Woo & Peng 135) which may date from this period, and of course the series from Ngandong, which may be older.

From Africa we have three dated finds from this period: that of Haua Fteah, which is about 40,000 BP (76); that from the Omo river region of
southwest Ethiopia, dated about 35,000 BP (15); and that from Florisbad, dated 39,000 BP (4). Dates are forthcoming from other South African sites including Saldanha and the Makapansgat Cave of Hearths (Protsch, personal communication).

From Asia we now have radiocarbon dates for Tabun of 40,900 BP (122), while the skull from the Niah Cave in Sarawak has a questionable date of 40,800 BP (35). Most interesting, perhaps, are the new finds from Australia. Remains of more than 15 individuals from Kow Swamp are nearly 10,000 years of age, and other remains from a living site on Lake Mungo have been dated to ca 30,000 BP (8).

Of special interest are the new dates of early man in America, which are presently being calculated by Berger and Protsch at the University of California at Los Angeles. Human cultures in Peru at Pikimachay have been dated at 19,600 BP (78), while human skeletal material has been directly dated from a number of sites in America, including the following:

<table>
<thead>
<tr>
<th>Site</th>
<th>Age</th>
<th>Units</th>
</tr>
</thead>
<tbody>
<tr>
<td>Los Angeles</td>
<td>23,600</td>
<td>BP (5)</td>
</tr>
<tr>
<td>Laguna Beach 1</td>
<td>17,150</td>
<td>BP (5)</td>
</tr>
<tr>
<td>Laguna Beach 2</td>
<td>14,000</td>
<td>BP (5)</td>
</tr>
<tr>
<td>Guitarrero Cave</td>
<td>12,610</td>
<td>BP (75)</td>
</tr>
<tr>
<td>Tepexpan 1</td>
<td>11,300</td>
<td>BP (95)</td>
</tr>
<tr>
<td>Marmes</td>
<td>11,000</td>
<td>BP (34)</td>
</tr>
</tbody>
</table>

In addition to these there are eight more that are older than 9,000 years. These dates, together with the palaeogeographical data, support the hypothesis that man first entered the New World at least 35,000 years ago and possibly earlier by crossing Beringia as the ice retreated but before the sea level rose. Further immigrants may have entered again since about 10,000 BP (Bryan 14).

**Conceptual Evaluation of Fossil Data**

Anatomy and variability.—One of the greatest problems in the evaluation of paleontological material is presented by the variability which is present in and between fossil samples. An understanding of variability in living populations, races, and species of animals and men, should enable us to avoid simplistic interpretations of fossil data based on very small samples. Within a single population we can always expect to find the variation due to genetic differences between individuals and the different environmental stresses to which they are exposed. Beyond this we may expect to find variability due to age and sex. There is considerable variation in the extent of sexual dimorphism in primates, and this may be an important factor in differences in size and robusticity among individuals of a single fossil population. When we come to consider a whole paleospecies lineage we have to take into account the variability that we expect to find at different time levels (which may represent evolution) and in different geographical areas. The expansion and contraction of the range of differently adapted geographical subspecies or races
may also give the appearance of very rapid change in morphology at a given site or suggest that more than one taxon is present. The importance of these considerations cannot be overstressed when the paleontologist is faced with taxonomic evaluation.

A second difficulty arises when we consider the methodology of paleontology. Paleospecies are sequent, continuous, and not discrete units, and they cannot be distinguished by morphological characters alone. Somewhere, many times, an *Australopithecus* gave birth to a *Homo* and they were indistinguishable at the taxonomic level. Nothing is to be gained by creating intermediate taxonomic categories (111), for neither morphological nor behavioral boundaries exist in reality, however hard we look for them (113). Simpson (104) has discussed this problem, and it is clear to anyone who faces the difficulties squarely that the boundaries of sequent taxa (be they genera, species, or subspecies) should be conventionally agreed time-lines rather than diagnostic morphological features (such as the famed cerebral rubicon of Keith). This means that both anatomy and dating are necessary to create the taxonomy of fossil lineages.

But morphology is not only of importance for taxonomy; its central interest is what it tells us about the path of human evolution and the anatomical and behavioral adaptations which characterized early man and his predecessors. In this respect the data are unambiguous, though in places rather sparse. We see that man’s evolution was indeed mosaic: different functional complexes evolved to their present state at different times; one anatomical adaptation overlapped and succeeded another.

Our knowledge of this succession has not been greatly altered by the latest discoveries. Bipedalism is not documented before the *A. africanus* stage, when it was fairly highly evolved. The change to bipedalism with upright posture had already been made, and it may well have been a lengthy process. No evidence of this process can be derived from the fragments of *Ramapithecus* at present. Later stages are well seen at Olduvai (site WK, Bed IV) where *Homo erectus* had a robust but otherwise practically modern pelvis as long as half a million years ago. New evidence from the postcranial skeleton other than the pelvis is scarce, but the femoral fragment found with the pelvis is very like the femur from Choukoutien, having some few primitive features which distinguish it from the very modern looking femora which Dubois found in Java. The antiquity of these should now be investigated afresh; their association with the skull cap found by Dubois is based on the fluorine analysis made in 1952 (6).

Olduvai Gorge has yielded us a fossil foot from a 1.75 million year level (site FLK NN in Bed I). It already shows most of the characters that we associate with a modern human foot though it has a few primitive features (Day & Napier 31).

From the available evidence the masticatory apparatus may well have evolved with the locomotor apparatus. Whether we look at the very ancient and fragmentary Lothagam mandible or the well-documented remains of *A.*
africanus and H. habilis, we find the typical hominid dentition with small front teeth and relatively large molars and premolars. There is no sign of the rectangular palate, the large canines and upper incisors, which we see among living apes. To add to this we now have the evidence of Ramapithecus. Whether or not this genus proves eventually to be a hominid, the dentition of such a link could have been predicted to be nearly exactly as it is.

Although the arcuate hominid dentition was already present in Ramapithecus and fully evolved in Australopithecus, further changes took place on the way to man. The large premolar-molar series was reduced in size, and the incisors and canines again assumed a greater importance. Teeth may have been used as implements, and later changes in diet and food preparation, including cooking, altered the selective pressures on the human dentition. As the dentition as a whole was finally reduced, together with the rest of the masticatory apparatus, the jaws and face became smaller in relation to the neurocranium, and the present proportions of the human head probably appeared between 100,000 and 50,000 years ago.

Compared with these features, the increase in cranial capacity was a relatively late event in human evolution. Always very variable, it already shows a slight increase over that of the chimpanzee in relation to body size in A. africanus, which appears to have been a small creature with a stature of 4 to 4½ feet. From this time onward there was a steady increase, which parallels the evidence for increased industrial activity, over 3 million years. The increase in cranial capacity was of course accompanied by changes in neural microstructure and organization which, although they are not recorded in the fossil record, are of fundamental importance in understanding the evolution of man's brain and behavior.

Manual skill, another functional complex which finds its anatomical correlates in the hand and brain, is still poorly represented in the fossil record. Beside two hand bones from Swartkrans (Napier 79) and a wrist bone from Sterkfontein, there are hand bones of two individuals from site FLK NN in Bed I, Olduvai Gorge. As a whole none of these bones differs very much from modern man, though without a complete thumb we cannot tell its length. It is likely that the Olduvai hand was not fully capable of the precision grip that we associate with Homo sapiens (Napier 80). A better indication of manual skill is perhaps the quality of stone tool (given the limitations of material) that we find in the archaeological record.

Ecology and behavior.—Beside the sites which have yielded hominid fossils, there are many others which tell us much about the ecology and behavior of early hominids. Some of these are mentioned by Howell (40), and others by J. D. Clark (23) and Isaac (45, 46, 48). This evidence is taken into account in the following paragraphs.

The earliest finds of Ramapithecus in Kenya are associated with forest fauna, and in the Siwaliks the bones are associated with forest, swamp, and riverine animals, and some open country species such as Gazella and Hyaena
(107). It is probable that these sites were riverine forests bordering more open country, and this indeed is the kind of situation in which one would expect to find the earliest hominids. At Fort Ternan a battered cobbled and depressed fractures on animal bones on an ancient land surface have been claimed as possible evidence of tool use (L. Leakey 60), but it is insufficient for present evaluation. Whether or not hominids were present at this time, we need further data of the evolution of the Hominoidea from the Miocene and Pliocene.

The most ancient evidence of tool use, and perhaps of hunting, comes from the site KBS at Koobi Fora, east of Lake Rudolf, excavated by Isaac (49). Here there is an occupation level containing stone artifacts and introduced broken-up bones in a freshwater stream bed near a lake shore. Mingled with the limbs of a single hippopotamus were 31 artifacts and manuports. In all probability the site represents a low-density home base briefly occupied. With a date in the region of 2.5 million years this is the oldest occupation level of any hominid, but older stone implements have been collected at Omo with a date of about 3 million years (Howell, personal communication).

As we know from many sites at Olduvai (65), hunting was a fairly regular occupation of the early hominids after 2.0 million years BP. Game varied from small reptiles, frogs, birds, and rodents to middle-sized antelopes and pigs, to giraffe, buffalo, and even large pachyderms. Some of the remains may have been hunted, some scavenged. Some sites look like living sites, some like butchery sites, and some like workshop sites (47). The full interpretation of this wealth of material, so recently published, will be very rewarding. The evidence of so much butchery, however, should not cause us to forget that hominids have almost certainly always eaten a preponderance of vegetable food. A broad subsistence pattern is more likely to be typical than full-scale meat eating, with a division of labor between male hunters and female gatherers.

Isaac (48) has reviewed the evidence of the early Pleistocene African sites and finds that they usually lie along sandy stream channels. At Olduvai Gorge, where the best examples occur, they also lie beside a saline lake. The ephemeral stream courses would carry strips of riverine bush as well as trees; shade and fruits would be available as well as essential drinking water. There is also less well documented evidence of settlement in montane forest. As Isaac says "the data could be made to fit any dietary hypothesis, but perhaps in truth it is the environmental diversity that should be stressed." We can certainly predict as well as recognize this characteristic among early Pleistocene hominids. An adaptability in this respect is a sine qua non of expansion into yet more varied environments including those of the north temperate regions.

The earliest sites from the Eurasian continents are tropical and found in Java, but they do not bear any archaeological evidence. The faunal context suggests forest and woodland. The first clear archaeological evidence comes
from the Vallonet Cave, Alpes-Maritimes, France, where there are some unquestionable artifacts with fractured and flaked animal bones (74). This site is at least of early Mindel age, and may possibly belong to the even earlier Gunz series of glacialiations. More important and better preserved are the sites at Terra Amata (71) and Vertesszöllös (54) which are of Mindel age. At both these sites are unmistakable hearths, and at Terra Amata there were huts or shelters marked by oval arrangements of stone blocks and postholes. The earlier date for the Choukoutien cave would be about this time, and it appears that by about half a million years ago *Homo erectus* was well capable of surviving the cold winters of the north temperate regions with the help of fire, and in most places, caves. From this point on, the story of man's adaptations to cold winters becomes more complex and has been reviewed elsewhere (21).

It seems clear that the demands made by such an environment were responsible for significant biological and cultural development in man's evolution. The adaptability already noted by Isaac in Early Pleistocene sites in Africa became the hallmark of hominids. By about 60,000 years BP man had come to occupy every biome from the tropical savanna to the arctic and outclassed every other mammal in his adaptability. The feedback between the evolving brain and the developing culture caused a rapid period of evolution during this era of expansion. Gene flow to the more tropical regions would have carried the biological improvements back to these more stable biomes. Man's brain and culture gave him astounding flexibility in his behavior. From hunting as an occasional variation in a vegetable diet (such as we see in the Tanzanian chimpanzee), man came to rely almost entirely on meat in the arctic winter. Improvement in hunting technology gave man the ability to survive seasons when vegetable food, which had always been his staple diet, was scarce or absent.

**Chronology.—** It has already been said that the development of a reliable chronology is one of the most important results of the most recent period of research. While it does not give us a lineage without morphological study, it does give us essential data with which to derive one. In terms of their recorded time span, the fossil taxa listed earlier can be approximately dated as follows:

<table>
<thead>
<tr>
<th>Fossil Taxa</th>
<th>Million Years BP</th>
</tr>
</thead>
<tbody>
<tr>
<td><em>Ramapithecus wickeri</em></td>
<td>14</td>
</tr>
<tr>
<td><em>R. punjabicus</em></td>
<td>10*</td>
</tr>
<tr>
<td><em>Australopithecus africanus</em></td>
<td>3.0–2.5*</td>
</tr>
<tr>
<td><em>A. robustus</em></td>
<td>2.0–1.7*</td>
</tr>
<tr>
<td><em>A. boisei</em></td>
<td>3.7–1.1</td>
</tr>
<tr>
<td><em>H. habilis</em> et al*</td>
<td>5.5–1.5</td>
</tr>
<tr>
<td><em>H. erectus</em></td>
<td>2.0–0.3</td>
</tr>
<tr>
<td><em>H. sapiens</em></td>
<td>0.3–present day</td>
</tr>
</tbody>
</table>

* Dating less reliable and based only on faunal correlation.
Apart from the doubts raised by dates based on faunal correlation with distant sites such as we find in the case of the Indian *Ramapithecus* and South African *Australopithecus* fossils, the overall framework is now becoming clearly established. It is unlikely that the Indian or Transvaal dates are very seriously in error. Our main difficulty arises from the use of the above taxonomic terms, which have not been defined. The composition of these will now be discussed.

**Taxonomy.**—Though its position in the Hominoidea is not established beyond doubt, the taxonomy of the two species of *Ramapithecus* presents no immediate problems. They are morphologically, geographically, and chronologically distinct, and we can postulate the succession *R. wickeri* → *R. punjabicus*, with a time-line of 12 million years BP between them. There is considerable evidence of faunal interchange between Asia and Africa in the Miocene (103), and we are reasonably justified in suggesting this connection between the two samples. For convenience the time-line between *Ramapithecus* and *Australopithecus* can be placed at 6 million years BP. Admittedly, the morphological distinction between these two genera is not known to be very great, and new samples of *Ramapithecus* may well lead us to drop it from the human lineage, or alternatively sink the generic name *Ramapithecus* (1934) in favor of *Australopithecus* (1925).

Is *Ramapithecus* a hominid? Considerable evidence has been published that points to this conclusion on the basis of the known samples (Simons 99–101). In contrast, the arguments against such a recognition are indirect and arise from the hypothesis that the hominid-pongid lineage split about 5 million years ago, and this would exclude *Ramapithecus* from its position as hominid ancestor (97, 123, 128). The value of the biochemical evidence on which this hypothesis is partly based is not in question—it very strikingly emphasizes the close relationship between *Homo* and *Pan*. It remains questionable, however, how much weight can be given to calculations of the time of divergence of these two lineages. The calculated figures are at present no more than probabilities which should be weighed against the more direct fossil evidence. The question has recently been reviewed by a biochemist and palaeontologist (Uzzell & Pilbeam 118), and cannot be discussed further here. An opinion can only be based on a personal assessment of very complex and conflicting evidence.

If we accept the evidence in favor of the hominid status of *Ramapithecus*, we can readily accept the sequence outlined above:

\[
\begin{align*}
12 \text{ m} & \quad R. \text{ wickeri} \quad \longrightarrow \quad R. \text{ punjabicus} \quad \longrightarrow \quad \text{Australopithecus}
\end{align*}
\]

*The opinions which follow are the author's own interpretation of the published data reviewed in the first part of this chapter. They represent the first reassessment of hominid phylogeny and taxonomy since the author's 1962 review (16). Various other recent hypotheses are diagrammatically presented in Figure 1.*
For the purposes of the first part of this chapter, all gracile East African hominids (not including those designated *Homo erectus*) were discussed under the heading *Homo habilis*. Many of these specimens have not been described nor have they been allotted to any existing taxon. In the absence of full descriptions it is not possible to come to any final decision about these finds. Nevertheless, on the basis of the published evidence, it is fairly clear that all the fossils from this area and time range which do not fall into the taxon *A. boisei*, as we have defined it, do fall into the human lineage and form a fairly continuous series up to *H. erectus*. It may prove appropriate to include the later of these in the taxon *H. erectus* and the earlier in the taxon *A. africanus*.

Present views of their age suggest that the Transvaal *A. africanus* group fall well before the vast majority of the E. African gracile fossils: they could indeed represent the South African race of an ancestral species. This economic proposal would then give us the sequence:

\[ A. africanus \rightarrow H. habilis \rightarrow H. erectus \]

It seems most improbable that two different species of such an adaptable hominin should have existed in the equivalent savanna areas of East and South Africa in isolation, and at the same time there is no good evidence of morphological differences during their period of overlap (3.0–2.5 million years BP) when the East African sample is very small. It is therefore reasonable and economic to postulate that *A. africanus* is indeed ancestral to *H. habilis* in the absence of evidence to the contrary, rather than to suggest a separate branch as Leakey, Napier, and others have done (Figure 1 D, E, and F). Time-lines may conveniently be drawn at 2.0 and 1.3 million years BP to separate these taxa. These boundaries more or less represent the traditional taxonomy of the group:

\[ A. africanus \rightarrow H. habilis \rightarrow H. erectus \]

The evidence that *A. boisei* formed a separate lineage to this one is strong. The taxon is morphologically distinct, contemporary and sympatric with the human lineage. Its sudden extinction at about 1.1 million years BP (Chesowanja is the last known occurrence) goes a long way towards demonstrating this hypothesis. Two hominids occupying sympatric, wide and rather similar niches would have eventually excluded one or the other through competition. We can conclude for the present that *A. boisei* is indeed a contemporary African species lineage alongside *A. africanus*, *H. habilis*, and *H. erectus*.

Is *A. boisei* a hominin? The evidence here is much stronger than for *R. mapithecus*. The dentition has typical, even exaggerated hominid characteris-
tics. The brain size (530 cc), however, is not especially large considering the size of the animal. The locomotion is not yet understood, but the postcranial bones do seem to carry some hominid characters. We might guess that it split from the early protohominid line to occupy a complementary niche on the African savanna as a primarily graminivorous and generally herbivorous protohominid (53). The anatomical divergence between A. boisei and A. africanaus implies a considerable period of geographical isolation from each other before they came together nearly 4 million years ago. The two lineages are not likely to have split much less than 7 million years ago, or possibly even earlier. Ramapithecus could have been a common ancestor.

The South African A. robustus fossils are less easily interpreted. There is considerable evidence that the differences between A. africanus and A. robustus emphasized by Robinson (93) and Napier (81) have been exaggerated (e.g. see Tobias 109). There is in fact considerable overlap in the morphology of the teeth as well as in other features. The locomotor adaptations of the taxa have much in common and little to separate them (only perhaps the length of the ischium). In short, the features of the robustus fossils are in concordance with an allometric size increase from an A. africanus ancestor half a million years earlier. What differences exist in the dentition and skull (including the sagittal crest) can all be seen as an expression of a larger masticatory apparatus. At the same time, there is no evidence that they ever lived as sympatric species at the same time (although the sites are within a mile of one another); estimates of their age have never suggested that they were contemporary. In short, their morphological similarity and successive dates suggest that the most economic hypothesis is the lineage A. africanus → A. robustus. The latter then turns out to be contemporary with H. habilis (as redefined) in East Africa. So we see that two divergent trends were occurring here at this period (2.0–1.5 million years BP): one towards gracility, and one towards robusticity. Perhaps the robust South African forms were beginning to fill the niche which A. boisei was at that time occupying in East Africa (the species is not known from South Africa). Unless isolation of the South African group resulted in the formation of a separate biospecies (which is not indicated by the anatomy nor suggested by the time interval), we have to suppose that H. habilis and A. robustus were two geographical subspecies of the same species. This means a major revision of nomenclature (see below).

Some 8,000 miles away, by land, we have a few hominid remains from the Djetis level in Java dated in the region of 2 million years BP and therefore contemporary with H. habilis and A. robustus. The nature of these finds—five mandible fragments (including Meganthropus palaeojavanicus) and two calvariae (named Pithecanthropus robustus and Homo modjokertensis)—has long been controversial. The heavy mandibles from the lowest levels (Sangiran 6 & 8) which carry the approximate 2.0 million BP date, have often
been claimed as an East Asian *Australopithecus* (90, 91), but with such limited material their status has never been quite clear. Nevertheless, similarities with *H. habilis* are well defined (Tobias & von Koenigswald 115). One calvaria (Sangiran 4) is a very primitive specimen of *H. erectus* with an endocranial capacity of 750 cc (112), and it is the most ancient cranium of that species. The child’s skull from Modjokerto and the cranium from Chen-chiaou probably belong to the same taxon and are probably of about the same age.

It is clear that unless morphological evidence can be produced that these fossils represent a distinct lineage from their contemporaries in Africa (a view held by L. Leakey, Figure 1 E), they should be placed in the same species and share the same species name. Thus we have arrived at the following classificatory scheme:

<table>
<thead>
<tr>
<th>E. Africa</th>
<th>S. Africa</th>
<th>Java &amp; China</th>
</tr>
</thead>
<tbody>
<tr>
<td><em>Homo erectus</em></td>
<td>—</td>
<td><em>Homo erectus</em></td>
</tr>
<tr>
<td>1.3 m</td>
<td><em>H. habilis</em></td>
<td><em>A. robustus</em></td>
</tr>
<tr>
<td>2.0 m</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6.0 m</td>
<td><em>A. africanaus</em></td>
<td><em>A. africanaus</em></td>
</tr>
</tbody>
</table>

**Ramapithecus**

*Nomenclature.*—Which genus is appropriate for the time zone 2.0–1.3 million years? This is a matter of opinion, and there is no simple methodological basis on which to make a judgement, though generic boundaries are usually used to connote very fundamental changes in adaptation. For the present I prefer to retain the generic name *Australopithecus* for this time zone, but only an international committee could remove controversy on this point by assessing the majority of informed opinion.

Which species name has priority for this time zone? The answer is *modjokertensis* (von Koenigswald 55).

What rank should be assigned to this group? Again this is a matter of opinion. I still prefer to assign it subspecific rank because I do not believe that the morphological distance between the mean of *A. africanaus* and *H. erectus* is sufficient to justify creation of another species between them in the lineage. This point I have argued elsewhere (17). We are now left with the following succession:

* A. africanaus africanaus ——> A. africanaus modjokertensis ——> Homo erectus

* For a complete review of hominid nomenclature up to 1964 and discussion of related problems, see Campbell (18).
This scheme, however, obscures regional differentiation which we both expect and find in fossil species. This is best shown by using more subspecific names to indicate geographical subspecies, as follows:

<table>
<thead>
<tr>
<th></th>
<th>E. Africa</th>
<th>S. Africa</th>
<th>Java &amp; China</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>H. erectus leakeyi</strong></td>
<td></td>
<td></td>
<td><strong>H. erectus erectus</strong></td>
</tr>
<tr>
<td><strong>A. africanus habilis</strong></td>
<td><strong>A. a. robustus</strong></td>
<td><strong>A. a. modjokertensis</strong></td>
<td></td>
</tr>
</tbody>
</table>

**A. africanus africanus**

Here we are dividing the existing sample of *A. africanus* into two chrono-subspecies and three geographical subspecies. This gives us an interpretation of the second hominin at Swartkrans, if it exists ("Telanthropus"). In this case it represents a sample of the East African race (*A. africanus habilis*) which expanded southwards at this time and perhaps began to replace the South African race, as it was later to replace *A. boisei*. It should be noted that if authors wish to give the *habilis* time zone the taxonomic rank species, then the correct name would be *Homo* or *Australopithecus modjokertensis* (von Koenigswald 55).

The nomenclature and taxonomy of *Homo erectus* broadly follows earlier schemes I have proposed (16, 19). It seems appropriate at present to divide the species into two chrono-subspecies and four geographical subspecies, with a 0.8 million years BP time-line:

<table>
<thead>
<tr>
<th></th>
<th>Europe</th>
<th>N. Africa</th>
<th>E. Africa</th>
<th>E. Asia</th>
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</thead>
<tbody>
<tr>
<td><strong>heidelbergensis</strong></td>
<td><strong>mauritanicus</strong></td>
<td>***</td>
<td><strong>pekinensis</strong></td>
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<tr>
<td><strong>leakeyi</strong></td>
<td></td>
<td><strong>erectus</strong></td>
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*** Hominid remains from Bed IV Olduvai Gorge.

*Homo sapiens* can be similarly divided, with a 0.05 million year time-line for its two chronological subspecies and a 0.3 million year time-line to separate it from *Homo erectus*.

<table>
<thead>
<tr>
<th></th>
<th>Europe</th>
<th>Africa</th>
<th>W. Asia</th>
<th>E. Asia</th>
</tr>
</thead>
<tbody>
<tr>
<td>Living subspecies of <em>Homo sapiens</em></td>
<td><strong>neanderthalensis</strong></td>
<td><strong>rhodesiensis</strong></td>
<td><strong>palestinus</strong></td>
<td><strong>soloensis</strong></td>
</tr>
<tr>
<td><strong>0.05 m</strong></td>
<td></td>
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<tr>
<td><strong>0.3 m</strong></td>
<td></td>
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</table>

Late subspecies of *Homo erectus*

The fossil evidence does not at present tell us how long early hominids were present in Asia. They appear to have been present about 10 million
years BP (*Ramapithecus punjabicus*) and again from more than two million years ago. The absence of evidence from the intervening period is inconclusive, and we must therefore recognize that hominid evolution may have occurred in both Africa and Asia over a long time span of at least 10 million years. The first species of the hominid family, Darwin’s third ape, was certainly a tropical animal and might have lived in either continent.

**Summary.**—It is a fact that a typological taxonomy based solely on morphological differences between fossil samples is still widely represented in the literature of paleoanthropology. It is essential to continue questioning the basic assumptions from which paleontologists operate, assumptions which are fundamental to the derivation of an acceptable taxonomy. Apart from the important fact that it is methodologically correct, the taxonomy of a fossil lineage divided by appropriately spaced time-lines removes many grounds for disagreement, and avoids the belief that what are really artificial boundaries have some biological reality. Our scheme depends more directly on chronological data, and as they become more accurately determined, disagreement about the boundaries of taxa should be reduced. The actual rank of taxa should finally be decided by the majority opinion of those best qualified to determine it.

There follows a classification of the Hominidae which reflects the substance of our conclusions. Though this minimal hypothesis may not turn out to be correct, it constitutes the optimum working hypothesis at the present time.

*Ramapithecus* Lewis, 1934
- *R. wickeri* (Leakey), 1962
- *R. punjabicus* (Pilgrim), 1910

*Australopithecus* Dart, 1925
- *A. africanus* Dart, 1925
  - *A. a. africanus* Dart, 1925
  - *A. a. robustus* (Broom), 1938
  - *A. a. habilis* (Leakey, Tobias & Napier), 1964
  - *A. a. modjokertensis* (von Koenigswald), 1936
- *A. boisei* (Leakey) 1959

*Homo* L., 1758
- *H. erectus* (Dubois), 1892
  - *H. e. erectus* (Dubois), 1892
  - *H. e. heidelbergensis* Schoetensack, 1908
  - *H. e. mauritanicus* (Arambourg), 1954
  - *H. e. leakeyi* Heberer, 1963
  - *H. e. pekinensis* (Black & Zdansky), 1927
H. sapiens L., 1758
H. s. neanderthalensis King, 1864
H. s. palestinus (McCown & Keith), 1932
H. s. rhodesiensis (Woodward), 1921
H. s. soloensis (Oppenoorth), 1932
H. s. sapiens L., 1758 and other living subspecies
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STUDIES OF MODERN MAN

D. F. Roberts
AND J. C. Bear

Laboratory of Human Genetics
University of Newcastle upon Tyne, England

STUDIES OF MODERN MAN

This review of studies in the physical anthropology of modern man concerns papers that have appeared in the period from mid-1969 to mid-1971. Though far from comprehensive it aims to show how much activity there is in each major topic of investigation in the biological study of populations of man that exist today, and to pick out any trends in investigation that may be detectable. Several topics have been intentionally excluded, e.g. behavior studies, psychometric investigations, descriptive osteometry, and epidemiological works relating to particular disease states.

DEMOGRAPHY

Descriptive.—There have been relatively few descriptive demographic studies of anthropological units of population or single communities—e.g. studies on the demography of an Eskimo village (450) which shows the low median age, the wide-based population pyramid, and the high dependency ratio characteristic of underdeveloped communities; and studies of the Amish (137) of Holmes County, Ohio. Of course census material on a much larger scale has continued to appear, and descriptive analyses thereof, for example on the demography of Nepal (365). But the relative absence of descriptive demographic studies of biologically meaningful population units in the future may well give rise to exchanges similar to that of Birdsell (62) versus other workers on Australian aboriginal groups as to the size and structure of functional units of society. Of such studies that have appeared, some occur in a historical context. Thus Ashcroft (11) examined the sex ratio of Guyanese of African and Indian origin over the period 1946 to 1962. McArthur (439), in the new journal Human Biology in Oceania, examined the birth rate of Fijian (Indian and indigenous) populations 1956–1966, showing a dramatic change in the former but none in the latter. Alvaredo (6) showed that Hava-supai population size had been stable through some 200 years, and attributed this to cultural practices.

Demographic variables.—Most new sample and analytical studies have concerned the determinants of demography. Fertility has attracted much at-
tention. Heeren & Moors (276) investigated actual and intended reproductive behavior in Utrecht women, finding that birth control practices increased as family size increased, that only 40% of women had attained or exceeded the intended number of children after 6 years of marriage, that desired family size was greatest at the lowest and highest educational achievement, but that despite fertility differences with religion there was no difference in the use of oral contraceptives. Religious differences in fertility have been examined in Canada (398), the East Pakistan (115), in Thailand (239), in the United States (275), where the proportion of Catholics is an important cause of variation in fertility between counties, and in the first stage of the Hull (U.K.) family longitudinal study (517). Higher fertility in lower educated women has been shown in Athens (594) and in Indian women in Lucknow (297), where a preferred family size of two or three is conspicuous among the better educated. Opinions on ideal family size have been recorded in young people in the American south (254), and ideal compared with actual fertility in British couples (517), and in rural and urban Nigerian women (503). Economic activity of women in Guatemala City (225) had a depressing effect on women's fertility independent of their age, marital status, and educational attainment, partly due to the fact that a higher proportion of economically active women are never married. Long (397) examined the effect of migration and found that the fertility of migrants within both Canada and the United States is generally lower than that of nonmigrants. Migrants in Brazil (308) eventually assume the fertility patterns of urban dwellers. The importance of age at marriage has been demonstrated in several studies. In West Malaysia (508) differences in age at marriage occurred with ethnic group, with the wife's educational attainment, and with the husband's occupation, and when these were taken into account, higher fertility was correlated with early marriage, while women married more than once had lower fertility than once-married women. At the population level, of particular interest is a study of the causes of fertility differentials in the Sudan (280), where settled populations have fertility levels 50% higher than nomadic. A higher age at marriage, more childlessness, polygamy, the general severity of the nomadic way of life, and lower nutritional standard are responsible, and the medical and physiological factors appear more important than differences in the marriage patterns in producing the fertility differential. A trend to declining fertility has been demonstrated in East Pakistan 1958–1967 (656), in New Zealand from 1961 onwards among non-Maori women (34), and in Australia 1946–67 (79).

In Taiwan (313) among women not using contraception the time required to conceive increased with age and depended on the outcome of the previous pregnancy. It required an average of 17 months to conceive when the preceding pregnancy ended in a live birth and the infant survived at least a year, but when the preceding pregnancy ended in abortion the interval averaged 10 months. In the United States (273) birth spacing varied with mother's age, parity, and duration of marriage, while level in parents' educa-
tional attainment largely accounted for differences between white and non-white. The effect of lactation on birth spacing was examined (76) in Ruanda, where amongst lactating women the family spacing effect was maximal during the first 9 months, though the majority of conceptions were found to be delayed by some 15 months. There have been many studies of contraceptive methods in use. Cliquet (120) showed that substantial sections of the Belgian population are limiting their family size in a way which is inefficient. In India (339) postpartum abstention observed by women was generally what they thought custom demanded, educated women tending to observe a rather shorter period.

Factors affecting sex ratio have been examined in India (505). The distribution of sexes in families differed from the binomial expectation, and there was excess masculinity associated with first birth in both urban and rural areas. Female infanticide does not detectably affect the sex ratio (506). In Australia (531) variations in sex ratio were almost entirely attributable to parental age, with no effect of season of year, rural versus urban domicile, single or multiple births, though seasonal fluctuations were suggested in the sex ratio of live births in Canberra (405). In the United States, after taking account of the effects of race and birth order, sex ratio was shown to be correlated with socioeconomic status, particularly in the lower socioeconomic groups (671).

The improvement of death rates with social advance is widely reported, e.g. in Santal villages accompanying improved housing and medical facilities (606). Frisancho & Cossman (211) showed a greater decline in neonatal and infant mortality in the mountain states of the USA and South America than in the low altitude states. Birth weight continued to be of primary importance in neonatal survival (618).

Much attention has been given to foetal wastage. In Taiwan, Jain (312) observed that foetal wastage increased with pregnancy order and that levels of spontaneous abortion appeared to be below those in the United States and India; induced abortion is more common in younger women. A survey in Detroit (123) showed that retrospective surveys underreport foetal mortality and that prospective surveys were more efficient. The foetal mortality rate is higher where the family income is higher, in wives who regard their income as inadequate, and in wives who work by comparison with those who don't. Bresler (83) in Rhode Island showed increased foetal loss with outcrossing, as measured by an increased number of countries of birth represented in the great grandparental generation. He also showed increase with age of mother. A review (201) of Brazilian data suggests an increase in abortions at higher pregnancy orders where radiation exposure has occurred.

Genetic demography.—A third main type of demographic study concerned genetic demography. On the one hand there have been small limited studies such as that of Ghosh (229), who collected fertility and mortality and other relevant demographic information among the Kota of the Nilghiri
hills in order to examine selection intensity, giving values of $I = 1.37$, $I_m = .45$, and $I_f = .64$. These figures are higher than those calculated by Salzano & de Oliveira (598) in a fuller demographic survey of the Terena Indians of the southern Mato Grosso, Brazil (an Indian group contrasting with other Brazilian tribes in that the Terena are increasing in number), or in the Cayapo of Para (597) or the Maca of Paraguay (599). Of the several studies that have appeared on effective population size, the one based on coefficients of kinship in successive generations (468) is of particular interest. Lazo et al (380) examined consanguinity in Valparaiso over the period 1917–1966, showing a decrease in the percentage of consanguineous marriages over the period in each zone of the province, while urban zones show less consanguinity than rural ones. The frequencies of consanguineous marriages as a whole are very similar to those found elsewhere in South America and are generally lower than in Europe, especially Spain. The frequency of consanguineous marriages has been studied in Madhya Pradesh (244), in Ireland (427), and in the Balearic Islands (692) over the period of 1930–1963, and their spatial distribution and their relationship to population density shown. Among studies of mating patterns, departures from the ideal endogamous marriage patterns of two barrios in a Mayan town were shown by both demographic and gene frequency results (181). Positive assortative mating for stature and weight in Cashinahua is largely due to their common association with age (321).

Inbreeding coefficients have been calculated in northeast Brazil (737), in Wainwright Eskimo (451), and in Switzerland (469) from pedigree data. The related coefficients of kinship have been calculated for the Jicaque Indian isolate (109), for Pingelap and Mokil atolls (470), among the Swiss (469), and in Japan (302). Estimates of inbreeding from isonomy have been attempted in small cities of Japan (738), from which it appears that the amount of random inbreeding has remained constant over the last three generations while the inbreeding due to nonrandom marriage decreased. In Switzerland (299) the random component increased during the 19th century and subsequently declined. There are beginning to emerge the results of applications of bioassay of kinship, for example in Switzerland (469) and in Japan (302), where the estimates of the kinship coefficient from the ABO blood groups are in good agreement with those calculated from pedigree data. A different approach to genetic structure is that of the groups of South American Indians studied intensively by Neel and his coworkers. The influence of cultural factors on the demography and pattern of gene flow from the Makaritari to the Yanomama Indians is well demonstrated (102), and from comparison of the results of genetic distance calculated from marker loci with the analysis of migration it appears that in such small populations genetic isolation does not necessarily equate with geographic distance functions (716).

The effects of inbreeding have continued to be investigated using demographic material. Hussein (298) examined the effect of endogamy on fertility
in Egyptian Nubians, showing increased fertility but also increased child mortality amongst offspring of first cousin marriages. Morbidity and mortality have been investigated in Florence (60) and in Fukuoka, Japan (735), where as early as the third month of life there is a significant elevation of mortality of offspring of consanguineous compared with nonconsanguineous unions, while the rate of stillbirth and perinatal death is also significantly higher. By contrast, in Hirado (610), where some 15% of unions are consanguineous, and moreover some 10% also involve inbred husbands or wives or both, there was no effect of parental inbreeding on stillbirths, but both maternal inbreeding and parental consanguinity influenced the probability of childhood death. The effects of inbreeding assessed in terms of lethal equivalents have been estimated in Portugal (204), Fukuoka (735), Tristan da Cunha (570), and in Brazil (203, 364), where the high load of lethal equivalents previously reported in negroes is not supported by the new data. No effects of inbreeding have been discerned on vision or hearing (477) in Hirado, or on physical development, tapping rate, blood pressure, intelligence quotient, and school performance (478), or in Brazil on celibacy (202).

Studies of the occurrence of consanguineous unions by computer simulation procedures have continued, for example, in a Japanese situation (407), while their application to the Yaminomama (408), incorporating detailed demographic variables, demonstrated unexpectedly, the role of sibship size in determining a man's ability to obtain wives, and variability in frequency of cousin marriages through time. Tracing the founder effect in pedigrees, complete from isolate initiation and the subsequent passage of genes through generations, as in the Hutterite H-leut (425) or Tristan da Cunha (569), has proved to be a powerful method for elucidating selective forces and very instructive in showing how much gene frequency change is attributable to chance occurrence.

**GENETIC STUDIES**

Progress in genetic studies has continued with undiminished vigor. Many of the papers that have appeared are a consequence of the discovery of the new polymorphic systems, particularly the isoenzyme systems, of the last few years. Many report investigations of their modes of inheritance (e.g. 91, 179, 357, 370, 371, 409, 462, 564, 565, 602, 734). Particularly exciting is chromosome mapping. With the increasing number of single gene normal variants now known, endeavors to establish linkage groups for them and to assign them to particular chromosomes are increasing and have greater chance of success. Apart from the genes on the X chromosome, two so far can be convincingly located on an autosome: the Duffy blood group gene on chromosome 1 and the alpha locus of haptoglobin on chromosome 16 (577). Several linkage groups are now reasonably well established—e.g. of AK with ABO (719) of the loci controlling the Gc and albumin variants (81), an HLA locus is linked with that for PGM locus 3 (373), and the latter is not linked with PBM1 (369). There have, of course, been reports of no linkage, e.g. of
the human haemoglobin variants with blood group loci Rh, ABO, P, Kidd, Kell, Diego (476), and for PGM with blood group loci (372, 720). Suggestive but unproven localizations have been proposed for the MN blood group locus on chromosome 2 (716a), and of a triphosphate isomerase locus on the short arm of chromosome 5 (646). Identification of population differences in gene localization, however, is still for the future.

Similarly, cytogenetic studies of populations are still in their infancy. Contrasting results are found amongst an Eskimo isolate (99) showing an absence of numerical and structural systematized anomalies, and in a Hottentot sample with a high percentage of aneuploidy (279). No studies have yet been published of population variation in the fine structure of chromosomes, while the polymorphisms that are currently appearing, for example in the Y chromosome, also remain to be investigated.

In anthropological work, to establish the frequency distribution of the new genes in human populations is still a primary task. Many papers report frequencies of genes in single systems or in a few systems, while relatively few report comprehensive results.

Red cell enzymes.—Red cell acid phosphatase types have been examined, e.g. in Poland (734), central Italy (612, 700), Germany (85, 564), south Bohemia (596), northern Bavaria (253), Iceland (509), and Sinai (75); among the Caribs of Dominica (272), Makiritare Indians (718), Indians in Bombay (67), and Australian aborigines of the northern territory (345). It appears that the frequency of gene Pα is fairly constant over the whole of Europe, diminishes in frequency in Africa, remains much the same over Asia except in the northeast where there is some elevation among Japanese and Aleuts approaching the higher frequencies amongst northern groups of Amerindian peoples, but is absent or infrequent in the Australian region.

Red cell adenylate kinase has been examined, e.g. in Vietnam (82), northern Brazil (16), India (67, 97), Israel (668), New Guinea (636), west Malaysia (106), North Germany (85, 719), Switzerland (523), Italy (612), Iceland (509), Denmark (371), Sweden (639), Norway (45), Finland, and among the Greenland Eskimo (178), Caribs (272), and Makiritare Indians (718). The distribution of the genes governing variants of this system were summarized (677), and with the additional data that have appeared since, it seems that the AK2 gene is mainly found in European populations (up to 5%) with the exception of the Lapps, in whose unmixed representatives it is absent. The AK2 gene is virtually absent from African populations south of the Sahara. Southwest Asia resembles or is slightly lower than Europe in its frequencies; there is a rise to a high frequency of the AK2 gene of up to 15% in India, quite different from the lower or zero frequencies in eastern Asia. The gene is absent in Australian aborigines (345).

For phosphoglucomutase, of the three loci governing the enzyme variants only the gene frequencies at locus 1 are sufficiently known as yet for more
than a very few populations. Red cell PGM frequencies have been studied e.g. in Finland and Sweden, among Greenland Eskimo (179), Caribs (272), Chilcotin (4), and Makiritare Indians (718), in Indonesians and Chinese (388), Bombay Indians (67), Japanese (307), in New Guinea (636), north Germany (85, 565), Rome (457) and Aguila (612), in Lapland and Norway (460, 461). Frequencies of the PGM, gene appear to increase from south to north in Scandinavia, and are particularly high in the Lapps. They are fairly constant over most of Europe, though a north-south gradient may exist, and over Africa; then they rise slowly into India and diminish again eastward. Frequencies are low in Australian aborigines and among all American populations tested except the Greenland Eskimos. Variants at the second locus are much less common. Frequencies of variants at the third PGM locus, examined on placental material, have been reported from Czechoslovakia and Norway (283, 465).

Erythrocyte glucose-6-phosphate dehydrogenase has continued to attract attention, as regards both the frequency of the enzyme deficiency (usually in relation to the malarial hypothesis) and the types and frequencies of variant enzymes. Studies have been made in Greece (8, 649), Sinai (75), Mozambique (560), the West Indies (575), India (595), and Italy (222). The number of variants that are appearing indicate that G6PD is a "permissive" enzyme tolerating a variety of aminoacid substitutions without important loss of function. The total frequency of such variants is indeed appreciable. Frequency of deficiency varies among local populations in Vietnam (82), where there was a variable correlation with frequency of haemoglobin E but no clear correlation with endemicity of falciparum malaria. The distribution of G6PD deficiency and thalassemia in Spain (518) does not support the malarial hypothesis. In the Ferrara (222) area of Italy, G6PD deficiency frequency is not correlated with malarial incidence, due to a significant lack of deficient individuals in areas where malarial rates are high, whereas the frequency of thalassemia is correlated with G6PD deficiency frequency and malarial incidence; the highest G6PD deficiency gene frequency observed is consistent with that expected from 25–30 generations of malarial selection.

Information on 6 phosphogluconate dehydrogenase gene distribution was recently drawn together (676), and further gene frequency studies have appeared from Germany (85, 545), New Guinea (636), South Africa (241), Bombay (67), Brazil (16), among the Australian aborigines (66, 345), Caribs (272), and Makiritare Indians (718). In Europe it appears that the PGDe gene is least frequent in Ireland (1%) but elsewhere is between 2–4%. It is generally at higher frequency in Africa, and particularly in the northeast among the Amhara and Beja, and elsewhere in the continent it is approximately double the European frequency. The high zone from northeast Africa extends into southwest Asia, affecting particularly Arabs and Yemenite Jews, and otherwise the gene remains at about double the European frequency. In eastern Asia it is at low to moderate frequencies, but these rise in
the sub-Himalayan regions of Nepal and Bhutan. It is between 4–6% in Australian aboriginals, but is virtually absent from Amerind populations so far examined.

Of the other enzyme variants in red blood cells, adenosine deaminase has been investigated in English (292), Danes (163, 370), and Germans (85, 546), and unpublished material has been summarized (697). The lowest frequencies of the ADA\(^2\) gene occur in Africa, moderate frequencies (4–9%) in Europe with a suggestion of an increasing north/south gradient, higher frequencies in southern Asia, and the highest so far recorded from New Guinea (12–17%). Lactate dehydrogenase variants are very infrequent in European populations but are widely distributed at low frequencies in Indian samples, with 2–3% frequency of variant phenotypes in south India (7, 150); they are absent from Caribs (272) and Australian aboriginal populations (345). Phosphohexose isomerase frequencies have been examined in north Germany (563), and again it appears that variant phenotypes are more common in south asianic populations than in African or European. Carbonic anhydrase variants have been studied in Indonesians, two subjects with variant phenotypes occurring in 357 individuals (388). The frequency of variant forms of peptidases were examined in Indians in Bombay (67), in European and West Indian samples (387), in Babinga pygmies (601), and in Australian aborigines (65, 345), where the peptidase B variants that occurred were quite different from those observed in European samples. Catalase variants were sought in Indonesians (388) but no variants were observed. NADH diaphorase frequencies were studied in European, Indian, and Negro samples (293), the individual variants being very rare but with a collective frequency of about 1%; there is a suggestion of an elevated frequency in the Mediterranean region.

**Blood groups.**—Reports of surveys of frequencies of single blood group systems continue to make valuable contributions to knowledge of their anthropological distribution, for example the frequencies of the Duffy groups in Budapest (325), the Xg groups in Thailand (555), the Australian aborigines, and in New Guinea (624). ABO frequencies are reported from the Moscow region (248) and from the Macao Chinese (5), Orissa castes (107, 414), Lahaulis (118), and Khasi (147). ABO and Rh frequencies are reported on new samples from Gran Canaria (526) and the Madang district of New Guinea (650). Detailed compilations of ABO and Rh groups in French Canada (412) and in Britain (359) show marked gene frequency differences between regions. New blood groups continue to appear, e.g. the Stoltzfus (59), the subtype B\(_m\) (300), and the Gerbsh group, which may be useful for anthropological distinction of Melanesians in Papua and New Guinea (77). Most serological studies, however, report frequencies of a number of blood groups, e.g. in Iceland (509), Brittany (314), Lapland (463), Hungary (714), Romania (166), Sardinia (33), Mozambique Bantu (429), south Sinai (75), Iran (20), India and adjacent territories (52, 54, 55, 366, 471,
511); also among Habbanites (74), Burmese (301), Koreans (21), Ainu (455), Greenland Eskimos (180), West Virginians (326), South American Indians (236, 466), Australian aborigines (625) and inhabitants of New Guinea and adjacent islands (78, 232, 636, 651).

In several studies the blood group frequencies have been used to investigate the genetic structure of populations, by bioassay in central and south American Indians (587), or testing Malecot's theory of decrease of kinship with distance in Belgium (165). Mechanisms of differentiation of gene frequencies have been examined in New Guinea (232), Bougainville (205), and among the Makiritare Indians (226). The serological relationships of populations have been examined in Oceania and southeast Asia (236), in eastern Europe (94), and in India (228). Gene frequency variation for serological traits shows minimal differences between the Papago and the Pima, and a general correspondence of linguistic and genetic relationships among all the Uto-Aztecan tribes (483), while local gene frequency variation among the Papago is mainly attributable to isolation by distance, affecting intergroup mating frequencies and migration patterns (733). Selective differentiation of ABO blood group gene frequencies has been examined arithmetically by Thoma, comparing variances and covariances within and between groups of populations in Europe (673) and France (672), and noting a slight secular trend to increasing A and B in Greece (691). Differences between populations have been suggested in epistatic selection in rhesus and MNS groups (683). The associations of blood groups with disease continue to attract attention, e.g. positive associations have been suggested with enteric bacterial agents and with particular cancers (576, 707), but no association has been found with syphilis (616) or leprosy (709); the ABO associations with disease have been well reviewed (708). Of particular interest is the demonstration of an interaction of secretor and Lewis types with isoantibody and immunoglobulin levels (250), suggesting association with resistance to infection. The possibility of selection in a number of blood group systems has been examined through age and sex effects on frequency (622), and differences in gene frequency between parents and children (627), while evidence for the selective effect of foetal and neonatal mortality with respect to ABO and Rh continues to accumulate (121, 221, 520). The possibility of an incompatibility effect of Xga being responsible for the secondary sex ratio has been raised (310).

Blood groups have continued to provide the primary information in race mixture studies, often in conjunction with other characters (533–536).

Serum polymorphisms.—A polymorphism of human serum that appears to hold considerable interest for population studies is that of the C3, the third component of complement (β,C-globulin). Several alleles appear to be responsible for the variants so far discovered. Of the population studies of frequency, in Norway the C3a allele has a frequency of .84, the C3b of .14 (47), similar to those in Denmark (164). Slightly different frequencies ap-
peer in Swedes (87) and frequencies significantly different from both in
Lapps (88). A similar difference occurs between Norwegians and Norwegian
Lapps (669, 670).

Frequencies of genetically controlled variants of the serum enzyme pseudo-
docholinesterase continue to be investigated. There are four alleles at the \( E_1 \)
locus, while the \( C_5 \) variant is additionally controlled by the \( E_2 \) locus. The
allele \( E_1^* \) occurs at between 3–5% in European populations, but is rare or
absent in most oriental populations and in Negroes. Population studies have
continued amongst Eskimos (611), Italians (699), and in Finland (635),
where significant differences occur between frequencies in Lapps and Finns
in the frequency of the \( C_5^+ \) component, while there appear differences be-
tween subgroups of Lapps.

Population studies of serum alkaline phosphatase are few, partly because
of the influence of diet and age on the level of alkaline phosphatase in the
serum, and partly because the expression of the gene or genes responsible is
modified according to the genes of the ABO and secretor systems (e.g. 713).
Placental alkaline phosphatase does not have the same limitations in expres-
sion, and several studies have appeared, e.g. in Japan (306), Hawaii (41),
Malaysia (68), and in the United States (80), where the gene frequencies of
subjects of British and Italian origin did not differ significantly from popula-
tions in Europe, while the gene frequencies of United States Negroes were
intermediate between Nigerian Negroes and those of the white populations.

In the protease inhibitor (Pi) system, the \( \alpha-1 \) fraction shows phenotypic
variants that vary in frequency and type between populations, of which stud-
ies have continued, e.g. in Japanese (268), Norwegians (186), Lapps and
Finns (185). Of human serum albumin, eight distinguishable types have now
been reported (350, 717), and studies to determine population frequencies
have continued, notably among Eskimos (521).

Studies on the group specific component (Gc) continue to show a wide
range of frequency of genes in this polymorphism. Compared to Norwegians,
Norwegian Lapps have a high frequency of Gc\(^1\) (464), while other samples
have frequencies to be expected from their geographical position [e.g. in Bo-
logna (184), Japan (662), Venezuelan Mestizo (220); among the Makirit-
tare Indians (10), Eskimo (180), Athabascan, Algonquin and Sioux Indians
(81), Zuni and Papago Indians (89), Swat Pathans (647); and in Iceland
(509), Hungary (714), and Mozambique (429)].

Haptoglobin types have perhaps received more mention than any other
serum component, and the geographical distribution of their frequencies has
been reviewed (710, 715). In Makiritare Indians, tribal frequencies fall well
within the range of American Indians reported (10). In a very large Swedish
sample, some heterogeneity of gene frequency could be detected, and a few
larger more homogeneous regions delineated (290), e.g. northern and south-
west Sweden, whereas central and southeast Sweden, which had been rela-
tively homogeneous in blood group investigations, showed more marked vari-
ation in haptoglobin frequencies. Frequency studies have been made among
Congolese and Mozambique Bantu (291, 429); in Kenya (92); with Zuni
STUDIES OF MODERN MAN

and Papago Indians (89), Habbanites (74), west Bengalese (472), various Indian populations (39), and Slovakian gypsies (219); in West Pakistan (647), Bhutan (471), Finland (173), Hungary (561, 714), and in central Italy (703). Studies differentiating Hp1 into its fast and slow subtypes are fewer in number, e.g. in Finns (173), Eskimo (180), and in Norwegian Lapps (195) where the proportion of Hp1F to Hp1S was the same as in other Norwegians. Of particular interest has been the proposal of selection of Hp types by interaction with ABO groups (346).

Transferrin studies have accompanied a proportion of the haptoglobin investigations. In Makiritare Indians there was no variation in transferrin type (10). Transferrin variants B₁ and D₁ were observed in Congolese Bantu (291). No transferrin variation was observed in West Bengal (472). The TFD allele reaches very high frequencies (up to 28%) in some Australian aboriginal groups of northern territory (345). The geographical distribution has been reviewed (712).

The ceruloplasmin variants continue to show little population variation; e.g. the Makiritare (10) showed no variants.

The Gm polymorphism is of particular interest in anthropological studies. Not only is there great complexity at the loci responsible, with many alleles available to give the variety of combinations of antigens that are found, but also there is a strong tendency for particular combinations to occur in particular human groups. Moreover, it is known on which fragment of the molecular chains particular Gm factors are situated, and this situation varies from major race to race. For example, the Fc and Fd fragments of the γ-1 chains in Europeans contain either Gm1 and Gm17 respectively or Gm22 and Gm3. In Orientals both Gm1 and Gm22 factors can occur on the Fc fragment when Gm3 is on the Fd fragment. Characteristic linkage groups appear to differ in the two races, Gm1 and Gm3 being inherited in coupling phase in Orientals and in repulsion in Caucasians. Thus population studies tend towards establishment first of the frequency of the antigens, secondly of the patterns in which they are associated with each other, and thirdly of their assignation to a particular molecular fragment. At the simplest level frequencies of Gm1, Gm2, Gm5, and Inv1 have been examined in Kenya (282) and Gm1, 2, 5, and 6 in Yugoslavia (200). The frequency and associations of a larger number of antigens have been studied in South African peoples, differentiating Bushmen from non-Bushmen, while the Gm1, 13 distribution reveals a marked cline increasing from north to south along the eastern coast (319, 429). Frequency studies have been made in Kurdish Jews (655), Ainu (653), Danes (588), Finns (674), Chinese (609), Australian aborigines (654), and Melanesians (129), and in relation to linguistic and historic data in Bougainville Island (206). The position of Gm25 and its relationship to other Gm factors has been studied (567), and again there is different association in Negroes as compared with Europeans and Asians.

Other inherited characters of the blood.—Reports include studies of frequencies of abnormal haemoglobins in various areas e.g. in Orissa (107),
Gujarat (511), of haemoglobin E in Assam (145), Burma (301), of Hb New York in Taiwan Chinese (64), of haemoglobin J in southeast Asia (63), in Kenya (92) and the inter-Andean corridor (614). Studies of newborn in west Malaysia show haemoglobin Barts present at different frequencies in ethnic samples (399, 475) while a foetal haemoglobin variant is reported at high frequency in Maltese infants (101). The complexity of the factors balancing the haemoglobin S polymorphism remains. Pulmonary function studies suggest that SS homozygotes, despite their low haemoglobin level, have a chronically expanded capillary bed as a compensatory mechanism to maintain an appropriate carbon monoxide breath-diffusing capacity (191). In AS heterozygotes cells parasitized with plasmodium falciparum are more prone to sickling than nonparasitized, and parasitized cells once sickled are more easily removed from the circulation (404).

Though the recognition of antigenic systems on the white blood cells was at first technically difficult, and the genetic problems that soon emerged were seen to be complex, today the HLA polymorphic system is rapidly rivaling that of the Gm groups in terms of its usefulness in anthropological work. There are two closely linked allelic series, and though earlier population frequency studies are perhaps not comparable with the more recent, again each continental group appears to be characterized by its own frequencies and its own associations of antigens. For example, Greenland Eskimos have a very high frequency (81%) of HL-A9 by comparison with 21% in Danes (347). Studies have recently been carried out on African pygmies (69) who though more similar to neighboring Bantu than to Europeans, differ from them, while the differences between the three subsamples of pygmies correspond generally to their geographical relationships; an interesting development is the use of the HLA frequencies to measure the genetic distance between the major groups studied. Similar sample surveys have been carried out in Austria (435), Japan (628), and among Greenland Eskimos and Danes (347).

Of the newer discoveries for which little information on population variation is yet available, the genetic basis of at least one variant form of soluble red cell glutamic oxaloacetic transaminase is established (116). Phosphoglycerate kinase showed a low frequency variant in a New Guinea sample (117), the gene responsible being apparently X-linked. Nucleosidase phosphorylase shows low frequency variants (171) and so does alcohol dehydrogenase (640).

As regards β lipoprotein structure, where serological studies show genetically controlled differences, the frequencies of the genes of the Ag system have not yet been as fully explored as the great range indicated in the earliest studies appears to warrant. However, surveys are continuing, for example in Norway (464, 643) and in Hungary (714). In Greenland Eskimos (46) the high Ag gene frequency is characteristic of east Asian populations, while in the second system (Lp) the Lp(a+) phenotype frequency is similar to western Europe.

Miscellaneous characters.—Studies of color vision have continued in sev-
eral populations, e.g. among Miri of Assam (648) and the Jat of northern India (113). An interesting development has been the use of detailed anomalous investigation for those subjects whose results on screening tests were suspicious (1, 2), and it is of interest that a Uganda survey by this method gave an increased frequency of color blind individuals over previous estimates which apparently failed to detect anomalous trichromats.

Salivary amylase promises to reward further attention. In a sample of some 600 Swedes and Lapps, about 15% showed isoenzyme patterns different from the normal, suggesting frequency differences from Czechoslovak samples (638, 690).

The excretion patterns of beta-aminoisobutyric acid were studied in Eskimo (180), Egyptian, and Hungarian samples (256, 349). Taste sensitivity to PTC has been efficiently investigated by serial dilution methods in Formosan aboriginal groups (402), among Lapps (463), and Finns (177), showing variation between Lapp groups; the postulated relationship to endemic goiter was not clarified in a Peruvian Indian sample (510). Other studies were made amongst the Seminole Indians of Florida (288), the Jats (114), Gujarats (615), Oraons and Munda in India (686), in Assam (149), and tribes of the Indian frontier (637), in Kurds (389), in Yugoslav and Italian families (86, 218) and in a Venda sample (160). Several studies show distinct differences in the distribution of cerumen types in New World and other populations (180, 388, 522) and a relationship to climatic factors is suggested (442).

Characters of complex inheritance.—A great amount of work is in progress in dermatoglyphics, particularly in India and Russia. An increasing proportion of new samples incorporates quantitative analysis of fingers or palms. The following have been investigated: Australian aborigines (544), Peruvian Cashinahua (316), Jivaro (665), Caingang and Guarani (572), southern African groups (161), Newars (49), Miri (648), and Bengalis (604). Pattern frequencies have been analyzed in French Basques (170), Lapps (382), Finns and northwest Russians (342), Latvians (155), Slovaksians (540), Romanians (542), Turkmen and Uzbek (17, 233a), Iranians (19), Indian populations (26, 35, 37, 141, 251, 318, 605, 607), and in the New World in Araucanians and Mexicans (539, 739). Palmer quantitative analyses, particularly of the interdigital ridge counts, have been the sole subject of papers relating to Taiwanese (103), Indian groups (38, 51, 554, 688), and Tibetans (678), while others, e.g. (53), report palmar qualitative and semiquantitative variations. Plantar analyses have been reported in Punjabis (151) and a Romanian isolate (541), and a further attempt at their rationalization in a new classification has been made (519). Further attempts at understanding inheritance of particular dermatoglyphic features, e.g. of interdigital ridge count (515, 538), of palmar (396) or finger patterns (566), have in other authors' hands been less satisfactory. Of particular interest, however, is the asymmetry that presents bilaterally. Suggestions that it is inherited have been made (e.g. 252, 634), and if this is so, population comparisons may be profitable.
However, it is in their application in problems of differentiation of peoples (585, 687) that an interesting trend can be detected. For example, in Cyprus dermatoglyphic comparison of mountain and coastal peoples does not support previously described serological, haematological, and historical differences (527). Finger dermatoglyphics in Polish gypsies differ from surrounding peoples in the direction of some Indian populations (422). In Chile, pattern frequencies vary with geographic isolation (589). On a larger scale there are emerging characteristic features of the major continental groups of man (124). Yet by contrast the fact that finger characters appear to differentiate at a different level from palmar characters suggests to some (e.g. 619) that dermatoglyphic characters are not suitable criteria for the study of population differentiation. With a view to understanding their biological significance, studies have continued of the association of dermatoglyphics with disease, e.g. with childhood cirrhosis (108), mental retardation (23), and coeliac disease (152).

Collection of quantitative data on skin color by reflectance spectrophotometry continues, e.g. among Mozambique Bantu (721), in New Guinea (711), and in India (331). Study of the effect of known amounts of polychromatic ultraviolet light on skin pigmentation suggests that there is a variation of tanning capacity between individuals (335), and that the change in tanning varies with the initial pigmentation of the skin (332). The effect of age differences on arm and forehead of Baniya boys aged 10–16 was demonstrated (333), with a suggestion of loss of pigmentation in adolescence, and this was confirmed in studies of both sexes of Tibetan refugees (681). Of studies of inheritance, family studies suggested general agreement with a hypothesis involving five gene pairs (334), and an interesting contribution came from a computer examination of polygenic models (394). The application of spectrophotometry to studies of skin color, with particular reference to Latin America, was reviewed (269).

Twining continues to attract interest. The incidence of multiple births was reported from New Britain (613), India (243), and Nigeria (494). Ethnic differences in twinning rates within Nigeria were mainly due to variation in the dizygotic rate, the monozygotic rate remaining quite constant (495); the incidence of triplet and higher multiple births was lower than expected (496). Zygosity of multiple births was examined in relation to placentation (497, 498). Epidemiological studies (473, 532) show that the proportion of multiple births increases with increasing maternal age. Twin series have been the basis of study of the inheritance of a number of characters, e.g. the activity of glucose-6-phosphate dehydrogenase (42), of the medullary structure of human head hair (24), and maximal aerobic power (348).

The methods of quantitative genetics have been employed in studies of a number of continuously varying characters. The temporal change in the variability and heritability of human fertility was studied in Japan (304), where the fertility pattern showed a constant mean and variance before 1910, with a subsequent decline in both, apparently due to the spread of birth control in
recent years. No correlation in fertility was observed between father and offspring; that between mother and offspring was small, but there was a significant positive though low sib/sib correlation which showed little temporal change. In a study in Greece (434) a polygenic basis for levels of serum cholesterol was suggested, both from parent/offspring and sib/sib correlations.

**Nutrition**

The distribution of lactase deficiency provides an intriguing example of a possible cultural correlation. The milk of mammals contains lactose, the assimilation of which is dependent on an enzyme (lactase or \( \beta \)-galactosidase). Some infants have defective lactose absorption causing malnutrition in infancy, which may be due to a deficiency, or a variant form, of the enzyme lactase, and there is evidence that this is a recessive character. Some humans lose their lactase activity with age; they are thus intolerant of lactose and experience gastrointestinal discomfort, or a more severe reaction, upon drinking milk. How this adult condition is related to the infant lactase deficiency, or how it is inherited, is not clear. Since the condition is much more frequent in Negroes and American Indians than in Europeans it has been suggested that man may be polymorphic for the loss or retention of lactase activity in adulthood, and that prior to the domestication of animals the deficiency was widespread, but that with the introduction of milk into the adult diet new selective pressures were created so that the deficiency was selected against. There have been several studies of lactose intolerance in various populations [e.g. in Arabs (590) and in Thai families (194)], and the problem has been extensively surveyed in the east African situation (122) and more broadly (441) in the light of cultural practices. Elucidation of the genetic basis of the condition is still the primary requirement.

Nutritional studies have been made in a variety of populations. In south and central America, the outlook appears grim. Winton (726) indicates the scarcity of information on, but probably near adequacy of, the diet of Columbian Indian groups living by agriculture. Yet the majority of reports concern inadequacy. Household energy budgets of sisal workers in northeast Brazil (249) show a bare energy balance, so precarious that the children of the families show retarded growth. Malnourished children of a semirural community in the Columbian Andes show retarded cephalometric maturation (98). In Chile in malnourished children there is significant growth retardation and low intellectual capacity (459). Food supplementation had little effect in a Guatemalan village (490). Studies in Jamaica suggest that infection is secondary to malnutrition in children (449), and that child welfare clinics have minimal beneficial influence on child growth (157). Similarly in Costa Rica anemia appears primarily of nutritional origin and parasite infestation an aggravation of it (493). As an illustration of the possible nutritional effect of a cultural practice, in Andean Indians those who chew coca leaf show inferior nutritional status, a higher prevalence of conditions result-
ing from poor hygiene, and a diminished work performance, and it is suggested that the poorer nutrition arises from blunting of the sensation of hunger (90).

Studies of Wainwright Eskimos (608) and Lapps (500) in the Arctic indicate adequate nutrition despite a harsh environment, and similar results come from Tristan da Cunha (104, 105). The contribution of nutrition to windigo psychosis (586) and Igloolik Eskimo dental health have been considered (431). Diet in Japan appears to be improving; the general nutritional status is good, though there is still some malnutrition (305). In India in only one in ten cases is protein deficiency the result of an inadequate protein intake; instead it is the total energy intake which is inadequate so that there are insufficient calories available to utilize the protein ingested (664). From other tropical areas are noted urban/rural differences in nutrition and size of youths in Chad (136), malnutrition and vitamin deficiencies in Gilbertese children (722), and an increasing frequency of premature birth and infant mortality following periods of deterioration of nutrition in the central Flores of Indonesia (235). In New Guinea supplementation of protein in previously protein-deficient children at boarding school produced a dramatic acceleration in height and weight growth, whereas extra calories increased only weight and skinfold thickness (416). Investigations of Australian youths showed no correlation between daily energy expenditure and intake, and the results imply that calorie balance is only achieved over periods longer than a week at least (444, 445). This is also shown in army recruits (172). The longterm effects of malnutrition are still little known, especially of malnutrition at particular stages of development, though severe malnutrition with hospitalization clearly has a pronounced effect on intelligence and learning (130). The recovery rates of children following protein calorie malnutrition have demonstrated the phenomenon of compensatory “catch-up” growth (13, 491). Malnutrition during early infancy retards physical and intellectual development (492), though it is suggested that retardation in preschool children seems to be overcome in teenagers (489).

Endemic goiter in Ceylon appears to be due to environmental deficiency rather than presence of goitrogens (156). Plasma aminoacids have been compared between samples in Uganda in relation to endomyocardial fibrosis, but the findings that adherence to a limited vegetable diet may introduce a chronic stress as regards certain essential aminoacids are of wider applicability (131). Haemoglobin levels in Jamaica children suggest that pubescent girls are nutritionally marginal in their iron and protein intakes (575).

**Dentition**

There have been a number of quantitative studies of dental data, e.g. dimensions of teeth in Belgian students (84). A study of the distribution of number of teeth in a Hungarian sample shows the distribution to be non-Gaussian; the location of the mode and its rate of change with age differs in men and women (682). Multivariate techniques have been applied to age
changes in the dimensions of the dental arches, revealing periods of maximum change which correspond to the major phases of permanent tooth eruption (379). Dental asymmetry has been shown to be an attribute of low heritability which has application as an indirect measure of population fitness, for asymmetry increases as environmental conditions deteriorate (18). The effect of culture change on Eskimo dentition continues to attract attention; the precipitous increase in caries rate can be related to increasing consumption of processed food, but periodontal disease cannot, and Eskimo teeth seem clean regardless of diet (431). Torus mandibularis incidence in Eskimos is described (432); the incidence appears to decrease as the native diet is abandoned (433). Tooth eruption in Hungarian nursery-school children has been chronicled (421), and the Mongoloid complex in deciduous dentition sought among the Ainu (262).

Considering features of individual teeth, quantitative analyses have been made of the shovel-shaped character in the incisors (263) and the hypocone of the molars (264). A family study from Liverpool demonstrates the simple model of inheritance of Caribelli’s cusp to be quite inadequate (240). Agenesis of permanent maxillary lateral incisors has been studied on a population level in Hawaii to test hypotheses of the interaction between the development centers of the teeth (641). Pedigree data on this condition support a dominant autosomal mode of inheritance with reduced penetrance and variable expressivity for most cases of missing and peg-shaped maxillary lateral incisors (732). In individuals with dental agenesis and hypodontia, crown size of erupted teeth is detectably different (223).

**Growth and Development**

In the literature of human growth and development, papers analyzing one or several contributing factors in the observed variation of growth processes predominate markedly over purely descriptive reports. Eveleth & Tanner (183) have compiled a directory of growth and physique studies undertaken as IBP projects, a useful guide to the many projects which have not as yet yielded publications.

The contributions made to birth weight variation by genetic and maternal factors, parental age, heterosis, parity, and sex have been discussed (730, 731) and a worldwide compendium of birth weight data provided (446). Seasonal variation in birth weight is reported from France (43, 525); from the Middle Flores, Indonesia (234a), both seasonal and geographical variation is reported, birth weight decreasing from west to east across the island. From a world analysis, an association of birth weight with climate, and independently with race, is suggested (568). The influence of economic and social factors on birth weight is reported from Bengal (29), where these appear to contribute a larger share of variation than biological variables, and from Czechoslovakia (659), where the incidence of low birth weight in illegitimate births is twice that in legitimate births.

Growth and development during the period from birth to 3 years has
been extensively studied in eastern Europe (329), series being described from Poland (330), Slovenia (529), and Yugoslavia (482). An analysis of the rate of growth in California infants shows parent/child correlations in height which rise markedly in the first year of life and have little dependence on race (725). A strong effect of socioeconomic influences on growth in infancy is reported from Nigeria (556) and Jamaica (158).

Several studies of preadolescent growth have appeared. One interesting result of a British investigation of determinants of height at age 7–8 years is that children of mothers who smoked during pregnancy average a centimeter shorter than children of nonsmoking mothers (238). From Japan comes a factor analysis of growth of public school pupils (336). Periodicity (548) and seasonal variability (358) in growth of preadolescent children are reported from Poland. Descriptive data are available for children in Dakar (426), Prague (486), Hungary (189), and the Soviet Union (30, 663).

Regarding growth through adolescence, studies in nonindustrialized societies provide valuable background to the extensive study of growth in industrial nations. The Bundi (417) and the Asai (418) tribal groups of the New Guinea highlands show very slow growth as children, much delayed onset of maturity, and quite low adult stature which seems proportional to, and possibly determined by, the low growth rate. It is suspected that natural selection for slow growth and shorter adult height occurs in conditions of nutritional deprivation. A study of the Sara of Chad advances the possibility of adaptive changes in growth in the tropical environment (136). The work of the Arabic-Polish Anthropological Expedition included studies of growth and development of children at Fayum Oasis, Egypt (317, 430, 623), through the ages of 7–18 years. Comparisons are drawn of measurements, indices, and anthropometric traits with western Egypt and Cracow series, demonstrating among other things the linear physique of the Fayum males. Linear dimensions, girths and hand measurements, and data on the appearance of pigmented axillary hair are presented for a Punjabi male series aged 11–18 (631–633), as is growth data on a small Punjabi series of children of both sexes (31). From Hong Kong come cross-sectional statures and body weight data for over 30,000 children, aged up to 20 years, and longitudinal data on 15,000 (400, 401). Chinese children are shorter, lighter, and slimmer than British, but differences in the second decade seem to be in the timing rather than magnitude of the preadolescent growth spurt. Series of observations on child growth including the adolescent period are also available from Hungary (22), Bulgaria (Sofia) (328), Greece (217), Verona (125), and arctic Russia (374).

In addition to studies of growth at adolescence, there have been several studies of timing of onset of puberty and menarche. Large series of U.S. data on peak velocity of the adolescent growth spurt suggest that attainment of a critical weight can change a critical metabolic level to trigger the adolescent growth spurt and menarche (214, 215); this phenomenon could also account for the secular trend to earlier menarche. Similar observations on Brno schoolgirls of the relationship of the onset of menarche and development of
secondary sexual characteristics to height and weight, imply at least a partial common accelerating factor (361). Reviews of world literature indicate that the inception of menarche is earliest in the temperate zones (728), and demonstrate a climatic (temperature) effect (568). A significant association of season of birth with age at menarche, and strong positive association of ponderal index with age at menarche are demonstrated on U.S. data (286). It is reported from Macedonia (138) and Poland (376) that daughters of agricultural families show a higher mean age at first menarche than daughters from nonagricultural families. Increase in mean age of menarche with decrease in family income is reported from Singapore (15). Perhaps related is a delay in onset of menarche with increase in family size in industrial northeastern England (573). Reports appear from France (320), Hungary (174), and Czechoslovakia (132) of the statistically significant coincidence of month of menarche and month of birth. Data on sexual development of Czech (133), Russian (375, 740) and Kirghiz (139) children have also appeared. A positive association of birth weight in boys with advancement of puberty was also described (44).

Secular trends in growth and maturation have been reported from several countries. As part of a more extensive study of growth and development of Negro and white children in Cuba, a secular trend from 1919–1969 in height and weight is reported, much more marked for white than Negro boys (377). From St. Vincent comes a report of a secular trend in size over the last 20 years; height and weight standards have been prepared for West Indian children of lower socioeconomic class and predominantly African origin (12). A secular acceleration in infant size over 20 years is reported from Poland (702); from both Poland (3) and Czechoslovakia (660) come reports of a secular trend in height and weight of youths. When the schoolchildren in a Yugoslav isolate in the absence of any influx of genes, showed an increase in size over those of 30 years ago, secular increase was attributed to improved living conditions (309). Secular trends in growth and sexual maturation observed in both rural and urban Romanian boys and girls are likewise attributed to improved social conditions (134). Secular trends in age of puberty have also been sought in the Soviet Union (736) and in Orissa, India (516). However, no secular trend to earlier maturation over 30 years is detectable in Tristan da Cunha (424). It has been possible to demonstrate a secular trend toward increased adult stature in the Skolt Lapps (386), using data from long continued study of the group. In view of the persistent genetic isolation, this increase in stature is attributed to improved nutrition. Secular trends in adult body measurements have been studied for the citizens of Moscow by taking note of the year of birth of subjects (706), and for the Western Apache by measuring the sons of a series of men measured in 1940 (452). Examples of plasticity of physique under migration are provided by the Japanese-Americans of Hawaii, who over the two generations following migration show an increase in stature (216), and by Puerto Rican children resident in New York City, whose stature at age 12 increases with length of residence
there (488). Jürgens attempts to provide a historical framework for changes in adult stature, stressing the effects of living standards, social change, and internal migration in reducing geographical variation in human morphology (327).

Surprising, in view of the interest in nutrition and in secular trends in growth and development, is the rarity of studies of heterosis and growth. The skeletal maturation (hand-wrist radiographs) of male Japanese-white hybrids is seen to be in advance of Japanese standards before age 11, but then slightly behind them (343). Significant differences in height and weight are noted for male and female Wainwright Eskimo and hybrid children (315). Using village endogamy and exogamy, Czech workers find no effect upon height for a series of schoolchildren, but a significant depressing effect of village endogamy upon height in university students (192).

Skeletal maturation continues to draw attention. While analyses have been made of postnatal ossification timing variation in boys and girls as it sheds light on the selective inactivation—or lack of it—of X chromosomes (224), most workers have attempted to elucidate problems of social and adult stature variation. It has been shown (213) that the prolonged period of growth seen in Central American adolescents does not make up for a low rate of growth and may be a cause of the small stature of these people. It has also been shown that the appearance of sexual dimorphism in bone subperiosteal cortical area is retarded in children from Central America as opposed to those from the United States (212). Comparisons of the rate of skeletal maturation of Philadelphia Negro and white children, in the age range 6–13 years, show little difference (419, 420). The lack of global applicability of any one set of skeletal maturation standards is emphasized by reports from Jamaica (423), Japan (182), Hong Kong (381), and Finland (675) of disparities between series studied and British or American skeletal maturation standards. Studies have also appeared of the correlations between rates of maturation of various segments of hand and wrist bones (578–582) and of correlations between midshaft breadth and compact bone thickness among long bones of the extremities in children aged 6–16 years (323). A secular trend toward acceleration in rate of skeletal maturation, amounting to 6 or 7 months over the decade, has been demonstrated on Polish (642) data.

Finally, there is a group of papers treating growth of specific parts of the body. These include a study of the variability of the cephalic index from age 7–25 years in a Romanian village sample (481), a study of the age changes in location of anthropometric landmarks (727), and an examination of correlations of weight, height, vital capacity, and blood pressure in boys aged 10–17 years (140). A comparative study of degree of spinal curvature in Negro and European subjects at the time of the pubertal growth phase gave a result contrary to the frequently advanced opinion, Negro young people being characterized by a gentler lumbar curvature than European counterparts (428). Norms of growth in the nose and mouth region for use in the repair of facial clefts have been calculated (260), and it has been demonstrated
that presence of a facial cleft seems to have no effect on the growth rate or form of the maxillary sinus and antrum higmori.

**Anthropometry**

In anthropometric studies, head and face measurements of women in Goteborg (385) suggested differences between professional subsections of the population. In a very large sample of army conscripts in Greece, born 1927–1945, mean stature showed substantial geographic variation and an interesting secular trend, with an increase during periods of normal social and economic development and decrease during times of deterioration (691). A secular change in head shape since 1920 is suggested by head and face measurements in a Romanian isolate (480) but no secular trend in stature in 30 years was shown in Tristan da Cunha (424). Complexity of interpretation of apparent metrical differences in migrants is stressed by Hulse (296). Descriptive anthropometric data are given for samples from France (176), Hungary (255), a Romanian isolate (9), Angola (603), Wainwright Eskimos (696), Dama and Bushmen in southwest Africa (277, 353), and several Indian peoples (27, 28, 36, 143, 146, 456). Of unusual interest are the anthropometric data on a Negro population in India (Gujarat) for several centuries who are derived from slaves or sailors (56), and of people in an isolate of the Guayaki of eastern Paraguay who are notable for very short stature and arm disproportion (453, 454). Two subtribes of the Bori Ahmedis, living in different environments in southwest Iran, differ anthropometrically (207). In Surinam, Trio and Wajana Indians differ in skinfolds, as well as anthropometric measurements and indices (234). Stature and body indices are analyzed among Egyptians and Poles (237), while basic metrical data of Czech external male genitalia (190) and Assamese feet (159) are presented. Compilations have been made of stature and other characters in women (447) in samples from all over the world, and in male samples from Africa (338). Volumetric studies (390) indicate that of the total body volume, the lower limbs account for some 30%, the upper limbs some 10%, and the trunk, head, and neck some 60%. There is no association of eosinophil count with physique (95).

An interesting development is the attention given to the anthropometry of the elderly; for example, a French sample between the ages of 50 and 90 (72) shows a decrease in height, weight, and chest circumference with age. Measurable constitutional characters in old people of 60–96 in Budapest suggest differential survival (162), while in Czechs it is shown that the height and breadth of the ear change more after the age of 50 than during puberty (259). Here there is an interesting parallel with increased demographic interest in old people (e.g. 96).

This perhaps is a reflection of the wider interest in anthropometry for applied purposes. Anthropometric data for ergonomic purposes have been collected on German forest workers and Vietnamese, for the purpose of designing German tools for use in Vietnam (368). Photogrammetric data on the
length of limbs and limb segments in young British women have applications in clothing and industrial design (270). Skinfold measurements in British business men are used as indicators of obesity (562). A possible relationship is suggested between susceptibility to uterine cancer and head shape in Polish women, which may perhaps indicate some environmental or common constitutional factor (360). The peculiarities of athletic physique have attracted much attention, particularly but not exclusively in eastern Europe (487). Serious sports training at 10–14 years of age seems to have no effect on skinfold thickness (93), though another longitudinal study of the relationship between body composition and anthropometric characters of samples of boys demonstrated effects of different amounts of physical activity (512). A longitudinal study of Harvard men (1880–1916) classified by their athletic activity at college showed that major athletes tended to be fatter, more muscular, and stockier than nonathletes, but experienced reduced longevity (530).

Body composition has been determined by anthropometric procedures amongst the Cashinahua (322), and comparison has been made between Tunisian and Czech boys (514); the Tunisian boys of the poorer social classes being retarded in somatic development but advanced in functional development, e.g. in muscle strength, the step test, and respiratory function. Comparison of American and Filipino women by anthropometric and biochemical techniques gave consistently greater results for the American sample, but the two groups were remarkably similar when the chemical measurements were expressed per kilogram of body weight (484). The evaluation of body composition from anthropometric characters has been studied in Yugoslavia (504), in Britain (324, 724), in adolescents and young adults, and in children (196). Hydrostatic weighing shows that measurements at several sites are required (513) to assess the amount of body fat from skinfold thickness, but the incorporation of other variables such as height and age detract from the estimate (258). There is only a moderate relationship of percentage body fat with Heath-Carter endomorphy and no relationship of lean body weight with mesomorphy (723). Lean body mass varies with age (458). In Germany skinfold thickness in boys and adolescents shows no social gradient (73). Middle-aged sedentary men can appreciably reduce their body fat and improve their cardiovascular function by moderate walking (537).

**Morphological Studies**

Wanke’s typological classification continues to receive much attention in eastern Europe, and has been applied to samples from Belgium (169), Taiwan (168), and Romanian forestry workers (167). Other attempts at typological analysis have been applied in samples from the Chad area (245), Somali (592), west Africa (40), the Sudan (657), and variation of constitution indices with blood group in Egypt claimed (188). The relationship between body type and performance in a mountain area was assessed, and as might be expected, mesomorphs tended to do better than endomorphs (187). A review of somatotypes of athletes showed clear discrimination among some
sports (100). Factor analysis, however, does not support the reality of Kretschmer’s athletic type (354). How morphological characters vary with age in the period 20–69 years (111) among professions and territorial groups (355, 356) have been investigated, and the morphological characters of Latvian children (362) and Khasi of Assam (142) described. Of particular body regions, studies have been made of muscular variation in the arm in Angola Africans (584), of digital length in Assam and Yugoslavia (148, 705), of other hand features (70) and of rib asymmetry (684). Facial asymmetry (193) shows no tendency to increase with age in children, and face morphology was studied in Hessians (499) and a Romanian isolate (593). The conformation of the orbital region was described in adults in a Romanian isolate (547), and of the auricular lobe in Norwegians (448). The incidence of cleft chin and the type of cleft differs sufficiently in endogamous groups in Manipur as to warrant investigation of its inheritance (629). Alveolar morphology was examined in relation to tooth location in Bratislava children (474), and types of mucosa relief of the palate distinguished in a Bulgarian sample (689). A study of pilosity in Javanese (311) and Ainu (271) shows the usual patterns of variation and suggest that the latter are not exceptionally hirsute. Microscopic details of head hair structure are reported from Eskimo and Ulchias (233) and several peoples from Assam (144), and hair patterns of the scalp in white and Negro Americans classified (617). Pigmentation of hair and eyes is described from northern Egypt (242) and the Kashebe population (110), while pigmentation of the iris was reviewed (61). Tongue rolling frequencies have been examined in a Transylvanian sample (695) and further studies of the possible mode of transmission made (289). The distribution of midphalangeal hair seems now well removed from a simple Mendelian trait (644, 694) though descriptive frequency studies continue, e.g. in Bihar and west Bengal (153, 230, 693). Hypertrichosis of the ear rims is a hairy perennial particularly in Indian studies (528, 553), and attention is now being given to association with other aspects of body hair (550). Ear lobe attachment also attracts Indian workers, for example amongst the Newar of Nepal and the Onge of Little Andaman (50, 507, 620), and an age effect demonstrated (57). Occipital hair whorls have been studied in Kashmir (58) and in sibships (698), though the pattern of inheritance remains obscure. Hand clasping and arm folding have been studied in a number of Indian samples (112, 413, 630, 679, 680, 685) and in Hungarian children (197). Appreciable differences in frequencies occur. Tongue pigmentation studies from India (549, 551, 552) claim autosomal recessive mode of inheritance.

Physiology and Adaptation Studies

Work in exercise physiology and environmental adaptation has been marked by a questioning of old assumptions. This questioning had been most fruitful in study of the capacity for exercise. It has been shown (126, 127) that aerobic capacity may be described by a limited number of indices: car-
diac frequency at a specific oxygen uptake, together with either total body
potassium or both the vital capacity and width of muscle mass of the thigh.
Genetic and environmental components of these dimensions allow study of
ethnic variation. Preliminary results show the capacity for exercise and the
transfer factor to be independent of ethnic group, while lung volume and
ventilatory capacity are greater in Europeans; vegetarian diet has no effect
(128); but for unacclimatized subjects, raising the environmental tempera-
ture or lowering the ambient pressure even slightly markedly reduces per-
formance. A high correlation has also been demonstrated of lean leg volume
with maximum oxygen uptake in young female physical education students
(261).

Findings in more traditional studies of response to exercise include the
adjustment of heart rate and stroke volume during unsteady states of exercise
so that the relationship between cardiac output and oxygen uptake is similar
to that of steady state exercise (231).

There has been interest in studying work capacity in subjects other than
the standard healthy European male. Significant differences have been shown
in work capacity test performance between male and female physical educa-
tion students (411). A study of training effects on young, nonathletic females
demonstrates that as intensity of training increases, so does its effectiveness at
improving oxygen uptake and reducing heart rate (351). Cardiac output and
oxygen uptake during submaximal exercise have been studied in boys and
girls in the 6 to 14 year age range (32, 406). A study of Singapore police
cadets reports low energy costs of common activities, attributed to small body
size of the subjects (25). A review of world variation in arterial blood pres-
sure (729) analyzes biogeographic, socioeconomical, altitudinal, and possible
genetic factors relevant to the variation.

In the field of altitude adaptation the tone was set by three useful general
articles. The need for standardization of terminology and procedure in the
study of physical activity of high-altitude natives was stressed (701), as was
the point that not all alterations in cardiorespiratory characteristics seen in
high-altitude residents are necessarily adaptive—in fact, some may be patho-
logical (437). The differences between high altitude and mountain habitats
and the large numbers of persons living at high altitude were emphasized,
and the impact upon these people of environmental stresses of hypoxia, cold,
rugged terrain, sunlight, wind, and precipitation reviewed, both as they affect
the ecology of these districts and as they directly affect the human inhabitants
(119).

Growth and development of children at high altitude has received careful
study. For the Quechua of Peru (210) are reported late sexual dimorphism,
slow and prolonged growth of body size, late and poorly defined adolescent
growth spurt, and accelerated development of chest size. It is suggested this
pattern is related to hypoxic stress. Frisancho (208, 209) has reviewed An-
dean studies and concludes that the low birth weight of high-altitude popula-
tions is an adaptive response to oxygen requirements, that the small adult
stature is the result of slow prenatal and postnatal growth, and that the enlarged heart, increased lung volume, and right ventricular predominance are due to accelerated development in childhood and adolescence. He emphasizes the present impossibility of determining any genetic components in these developmental responses.

Studies continue of the details of physiological adaptation of high-altitude natives. Young adults in Leadville, Colorado, show a higher apparent diffusing capacity of the lung than their sea-level counterparts (154). An enhanced Bohr effect had been demonstrated in Peruvian Indian highlanders (467), but it has also been shown that this shift of the oxygen dissociation curve is unimportant in the acclimatization of newcomers to altitude (383). Simultaneous measurement of plasma volume and cell mass in the blood of Peruvian high-altitude residents showed an inverse relationship of plasma volume to hematocrit resembling the polycythemia of congenital heart disease (600). A lower pH of cerebrospinal fluid in Andean high-altitude natives has been demonstrated and offered as an explanation of the ability of these people to hyperventilate at high altitude in the absence of hypoxic peripheral chemoreceptor drive (645). Quechua subjects showed the ability to maintain significantly higher skin temperatures than whites in tests of extremity exposure to cold air and water (391).

In investigations of high-altitude exercise performance of male university students of highland Indian and European white ancestry, some exercise performance advantages were found which seemed to result from high-altitude residence, but there was no clear evidence of a racial difference in performance (436). An inexplicable low-fasting blood glucose level in high-altitude natives was confirmed (524). Quechua males living at high altitude were found to have urinary protein excretion rates elevated by comparison with Quechua living in Lima, another inexplicable result (557). Renal circulation and oxygenation were found to be normal in Peruvian natives of high altitude (558).

While most studies of heat adaptation focused on individual physiology, the genetic implications of a hot desert environment to populations of simple technology were also examined (571). It is suggested from ecological considerations that for these people selection may be intense, colonization of new habitats difficult, high fertility of questionable fitness value, migration between varied habitats difficult, and small population size a genetic danger, all because of the narrow physical adaptation which may be necessary for survival in hot deserts. There is a distinct need for separation of cultural, physiological, and genetic components in this adaptation.

Turning to specific studies of heat adaptation, comparison of the water economy of Europeans and Australian aborigines shows total body water and rates of water turnover in drinking, sweating, and micturition to be far higher in aborigines (410). Their sweat sodium concentration was half and urea concentration twice that of Europeans. In a study of heat tolerance in white, mestizo, and Indian desert dwellers in the southwestern United States and
Mexico, none of the groups showed sequential changes which might indicate adaptation to the arid heat exercise test (265). On a very small number of subjects, it appeared that Africans could maintain lower deep body and skin temperatures than Europeans, and that their sweating rate was higher (502).

There have been several studies of the sweating response. Active sweat gland distribution was studied in Europeans and Caingang Indians (352, 574); a circadian variation in the sweating threshold was found in Europeans (135). In a study of ethnic variation in sweat gland response to pilocarpine, the Sudanese and European male sweating rate was found to be higher than that of Indian or Bantu subjects, while Sudanese females perspired at a rate very significantly higher than European, Indian, or Bantu females (440). Variability of sweating response in Indian army subjects was discussed with application to evaluation of physical work performance in hot humid climate (48). Studies of thermoregulatory responses in Swedish men and women demonstrate sex differences, principally in the sweating mechanism, similar to those between acclimatized and unacclimatized persons (199). It was shown that very premature babies have no sweating response while in full-term babies the response is less than adults (198), and that the range and pattern of control of heat loss in neonates is essentially the same as in adults (284).

There have been investigations in Europeans of body temperature regulation after prolonged intermittent exercise (257) and in states of hyperhydration (246) and hyperthermia (591), situations worthy of study in other ethnic groups living under thermal stress.

An example of human adaptation to heat stress by adjustment of the microclimate is provided by a study of the Nasake fishermen, who pitch a tent on their boat to achieve an ambient temperature of less than 27°C when outside temperatures reach 31°C (501).

A challenge to common assumptions in cold adaptation study was made by Hanna (266), who with Peruvian Quechua subjects showed that the high variation in response to cold stress seen in adult males in the laboratory was only part of the variation seen in the field and in the total population. He questions whether laboratory responses may be used to support evolutionary interpretations. Just the sort of study called into question looked at cold response of the faces of Oriental and European subjects to currents of freezing air, and found no evidence of unusual responses in the Oriental face, often claimed to be the product of selection by cold stress (652).

Responses to hand and finger immersion in cold water have been studied in Greenland Eskimos (403), Japanese [including Ainu and female professional divers (344, 666)], in whites and Indians in Peru (393). Adult highland Indians maintained higher hand and foot skin temperatures than young ones, while all Indian subject groups could maintain higher skin temperatures than whites. This result indicates the possible operation of both developmental acclimatization and genetic adaptation in skin temperature response to cold.

Studies of metabolic effects of food ingestion during acute cold stress
showed that in white subjects the elevation of heat production obtained did not alter tolerance to a standard cold exposure (583). In highland Quechua Indians, coca chewing could be shown to have little effect on cold tolerance and alcohol consumption only a slight effect (267, 392). Peripheral rewarming after exercise was studied in Norwegian subjects (278).

Physiological studies have been undertaken and results reported for a variety of parameters in a wide range of ethnic groups. Physical fitness and respiratory function have been studied in the Wainwright Eskimo population (559), maximum oxygen uptake in Skolt Lapps (340), bone mineral content in Wainwright Eskimos (438). Circadian rhythms in body temperature and eye-hand coordination and circannual rhythms in several variables were found in Wainwright Eskimos (71); diurnal rhythms in urination were reported for European subjects on a 21-hour routine in the Arctic (626), and for Europeans born and resident on the Galapagos Islands (395). The calorigenic effect of norepinephrine in the Ama diving women of Korea was investigated and found minimal (337). High levels of erythrocyte magnesium are reported for the Merina of Madagascar (281); territorial variation of haemoglobin levels in healthy males is reported from Russia (621); and a somewhat high incidence of glucose and protein in urine is reported for a series of university students from Quebec (667).

Conclusion

Though the period covered by this review is short, some trends that fit with others over rather longer periods may be discerned. Lasker's (378) presidential address to the A.A.A.S. stresses the dichotomy between historico-descriptive studies in physical anthropology and the study of the general process of human biology by comparative and experimental methods. He compares the numbers of articles falling into these two categories in three journals over the period 1964 to 1968 with those of 1946 to 1950, and finds that there has been an increase from 31% to 47% of papers in the category "comparative," "critical," or "experimental." This is a rate of change of less than 3 percent per year, and may indeed be an overestimate since the three journals are amongst the most forward looking. But it is our impression that this trend is continuing in 1969–1971. Straightforward descriptive papers are mostly confined to the newer characters investigated, for example the more recent genetic systems, while those of longer-established interest, e.g. in growth, tend to be more analytical. Dependence on research facilities is also apparent, descriptive and simpler inquiries emanating from the less advantageously equipped centers.

A second trend Lasker notes is the realization that such general processes may be studied over the whole range of cultural variation in modern man, from the most primitive to the most urban, and indeed this is essential to test their biological truth. Certainly the publications reviewed here range over the entire field of cultural achievement, from first contact primitives in New Guinea, Australian aborigines, south American Indians to urban western Eu-
uropean, North American, and Russian. There are naturally differences in em-
phasis, since some problems are more aptly investigated at particular cultural
levels. But in 1971 physical anthropology has moved far from the mere curio
classification of the early decades of the century, and today sets up and tests
its hypotheses on a variety of population units, be their size 50 or 500,000,
their daily concern cassava or computers.

A third trend now noted is integration. Though studies devoted to say
dentition or blood group frequencies alone must necessarily continue, there is
much cross-fertilization among the topics here reviewed. And it is where the
borderlines between topics are probed that more spectacular advances come,
e.g. in the study of population structure through combination of genetics and
demography, of child development through nutrition and growth, of adapta-
tion through physiology and anthropometry. Of particular relevance here are
the intensive studies on isolates. Mention of individual topics of these has
been made above, but when they are drawn together an extra dimension is
added to their understanding. For example, a survey comprising demography,
serology, anthropometry, skin color, and other morphological features has
been made among the Melungeons of Tennessee (534). They are shown to
be primarily of European origin, and analysis of birth places, isonymy, differ-
ential gene frequencies by generation, all indicate the dissolution of the iso-
late to be occurring. Among European peoples, the Lapps (341) and the
gypsies (14, 415, 485, 661) attract much attention. The genetic isolation of
the latter from surrounding populations is emphasized and their non-Euro-
pean affinities indicated, and growth studies indicate new attention to the pro-
cesses by which the differentiation occurs. The village of Moeciu de Sus in
Romania has been intensively studied, investigations including various ge-
netic characters, sexual dimorphism of anthropometric dimensions, morpho-
logical factors, and eye and hair color (247, 479, 704). Tristan da Cunha
remains amongst the best investigated isolates from the genetic, anthropomet-
ric, physiological, dental, nutritional, and epidemiological points of view.

Another trend that emerges is the capitalization on the computer revolu-
tion. Not only has computer use produced models for testing in the biological
situation as mentioned throughout, but it greatly facilitates the interdiscipli-
ary studies that are developing and allows more sophisticated methods of
analysis to be applied to older problems, for example examining generalized
distance among populations examined metrically (285, 376, 443, 658),
groupings of populations (295), genetic structure of populations (287, 303),
and dynamics of evolutionary processes (175, 363).

A final and welcome trend is the influence of the human adaptability pro-
ject of the International Biological Programme. This at its inception promised
attack on problems of human response to heat, cold, altitude, food shortages
etc., particularly on a comparative basis taking into account differences in
gene pools, in growth and physique. It called for agreement and standardiza-
tion on a world scale so that results would be comparable, and the concept
was of far-reaching practical implication since only through enhanced under-
standing of the ecological basis of biological productivity is human welfare assured. It is rewarding to find much realization of its initial promise. There has been a concerted effort in some 42 nations supporting IBP to pursue in a coordinated manner the many objectives towards which physical anthropology has been tending over the last two decades; and an appreciable number of papers on the results of IBP activities have already appeared and activities reviewed (e.g. 227, 294, 384, 543).

Over all then, the period 1969–1971 is characterized by vigor and volume. The outlook for the future is promising.

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DATING METHODS

JOSEPH W. MICHELS

Department of Anthropology, The Pennsylvania State University
University Park, Pennsylvania

INTRODUCTION

Archaeological dating has undergone rapid development as part of a more general trend towards increasingly sophisticated scientific methodology, a trend that is being felt throughout the discipline of archaeology. We have not only seen refinement of traditional dating procedures but also the introduction of new techniques that considerably expand our ability to date past events. As in the case of other rapidly developing areas of archaeology, a substantial literature has accumulated that poses a challenge to archaeologists who merely want to keep up to date. I was asked to write this review when the editors learned I had just completed a book on dating methods (3). One observation that I can make as a result of my experience is that many of the important developments in archaeological dating represent contributions by the biological, physical, chemical, and mathematical sciences. A good portion of the relevant literature is written by specialists outside archaeology who often presume backgrounds in their respective fields that archaeologists seldom possess. As a result, the literature is not readily assimilated by the general archaeological profession. This problem has prompted recent efforts at syntheses that are intended for archaeological consumption such as the collection of essays edited by Michael & Ralph (29), those edited by Brothwell & Higgs (5) and the above-mentioned text (31). In this paper I will try to highlight some of the developments which are discussed in detail in these general works and attempt to show the magnitude of progress that has been made in the area of archaeological dating.

The fact that recent technological breakthroughs are leading to a routinization of chronological analysis has considerable significance for the development of a more rigorous anthropological orientation in archaeology. Taking one example, we might note that there is growing momentum towards the application of general systems theory to archaeological analysis. One of the highlights of a systems approach is that it facilitates a dynamic analysis of cultural process (8). In such a framework the reconstruction of an archaeological "phase" is viewed as the characterization of a complex cultural system at a given state along a multistate trajectory (4). The study of multiple phases is intended to document changes through time in the various compo-
mements of the system in order to discover the sequence of transformations that brings the culture to a new state of relative equilibrium (2). Knowledge of the sequence of transformations within the cultural system is a prerequisite to any attempt at causal explanation of cultural process. Thus, although time is obviously a continuum, it is necessary to delineate analytically significant segments so that specific events can be represented in a transformational sequence. The more precise the delineation of temporal segments the more useful they become in the context of a dynamic analysis. With the advent of a new range of dating techniques, and with the continuing refinement of earlier techniques, the archaeologist finds that he can now achieve considerably more success than ever before in establishing precise and reliable chronologies for this purpose.

One way to approach the review of new developments is to discuss them in relationship to the three major objectives of archaeological dating: (a) phasing, (b) determination of relative age, and (c) chronometric dating. All efforts at dating tend to fall within one or more of these underlying strategies.

NEW DEVELOPMENTS IN PHASING

Phasing involves the recognition of classes of archaeological units that are distinctive from a temporal standpoint, i.e. classes that represent discrete and contrasting temporal segments of the time continuum. Diagnostic artifact types and feature types are selected to serve as the defining characteristics of these classes. Traditionally these diagnostic characteristics were a limited set of variables that defined what taxonomists call "monothetic groupings" (39). There is a danger that such classifications will be arbitrary and will fail to discriminate archaeological units of a more complex structure.

Recently a number of archaeologists have been concerned with establishing classes of archaeological units that reflect not only variation in time and variation in space but also discrete behavioral sets. To accomplish this, they have made use of a variety of computer-assisted statistical procedures, including factor analysis (3) and cluster analysis (9, 21). Their investigations have demonstrated that archaeological units can be statistically discovered as well as defined through the analysis of multiple variables. Such a strategy tends to reduce the kind of arbitrariness in classification that sometimes occurred in the past and also tends to permit recognition of more complex as well as less obvious classes. Attribute-system analysis, artifact typology, archaeological-component discrimination, and archaeological-unit classification are all important aspects of the general problem of periodization or phasing. At present the most that can be said is that we now recognize fruitful avenues of research and we are beginning to pursue them. Results are still minimal and we cannot yet say that day-to-day problems of phasing are being solved along new lines. Traditional techniques of archaeological-unit classification, with their reliance upon monothetic groupings, and intuitively derived artifact typologies still prevail and will continue to prevail within the near future.
ESTIMATING AGE BY RELATIVE DATING

Relative dating techniques have changed very little during recent years. Basically, there are two aspects of relative dating: one is “sequence dating,” where the effort is to establish the relative chronological position of a series of archaeological units or phases; the second is “cross-dating.” Cross-dating presupposes that a sequence of archaeological units has already been established, and the effort here is to determine the relative position of the newly recovered component by correlation with a similar component already sequence dated. Sequence dating today, as in the past, rests primarily on stratigraphic analysis of deep site deposits and by seriation of single component sites. A favorable recent development in stratigraphic analysis has been the more rigorous training in geomorphology that many of the younger archaeologists seem to be receiving, especially those that specialize in Pleistocene and Early Holocene archaeology. This will contribute immeasurably to the value of future research on early man in the New World and on the Paleolithic of the Old World. For an excellent discussion of Pleistocene and Holocene geochronology and geomorphology, see Butzer (7).

One special development with regard to stratigraphic analysis that has come about through developments in another dating technique—obsidian hydration dating—has to do with evaluation of the stratigraphic integrity of a site deposit. Large samples of obsidian specimens can be dated cheaply so that strata within a deposit can be evaluated one by one to determine the incidence of intrusive materials (30).

SERIATION

Sequence dating through seriation has a long history in archaeology (37), and although opportunities for application of the technique are somewhat limited, a rather substantial literature was accumulated during the 1960s (11). An important breakthrough has been the successful programming of the Brainerd-Robinson seriation technique for computers so that a large series of components can be seriated conveniently. Computer programming for seriation analysis has undergone refinements, with at least six different programs reported on in the literature (for the most recent see reference 19). Besides the attempts to make seriation more convenient through use of computers, there is a continuing interest in the more conventional graphing techniques developed by Ford (16) and also Meighan (27).

As Dunnell (11) correctly points out, not all successful seriations are chronologically valid. A seriation is merely an exercise in unilinear scaling unless and until the archaeologist is able to establish that the single dimension being measured is time. This is accomplished by considering the extent to which the groups being ordered are (a) of comparable duration, (b) belong to the same cultural tradition, and (c) come from the same local area.
Cross-Dating

As mentioned earlier, cross-dating presupposes the existence of a sequence of classes of archaeological units, and consists of attempting to assign a newly discovered component to one or another of the ordered classes. Once assignment can be made successfully, it is possible to infer by correlation the age of the newly discovered component is the same as that of the class to which it is assigned. As in the case of sequence dating, cross-dating is an old and well-established technique for relative dating that has been conducted very successfully in both Paleolithic and Holocene archaeology.

In Paleolithic archaeology there is a heavy reliance on paleontological and geomorphological correlations and less of a reliance on correlation of material culture. Consequently, archaeology continues to rely on specialists in Pleistocene geomorphology and paleontology to insure maximum precision in identifying the geochronological framework of archaeological units. In this context it might be noted that one recent development is the attention currently being given to the skeletal remains of very small mammals (micro-mammalia) found in Paleolithic sites in Europe (23). Such small mammals underwent relatively rapid evolution during the Pleistocene, so there is adequate taxonomic variability to draw upon for purposes of faunal-complex definition. Furthermore, they can often be recovered in sufficient quantity to permit a relatively precise faunal-complex assignment.

Fossil pollen analysis of Palynology is another specialty within paleontological cross-dating. Although it serves principally as a technique for paleoecology and only nominally as a technique for dating, it has on occasion offered a quite useful framework for the relative temporal placement of archaeological sites within a single region (25).

Cross-Dating by Correlation of Culture Traits

Although the geochronological methods reviewed above are of prime importance in Paleolithic studies, by far the most common approach to site unit cross-dating for younger sites is that of culture trait comparison and correlation. Perhaps because of the immense success archaeology has achieved in traditional methods of culture trait cross-dating, there has been very little modification in traditional strategy. Yet, although the basic strategy of culture trait cross-dating has not changed, there have been innovations that have increased the precision and reliability of the correlations themselves. These are the same innovations that were mentioned earlier in connection with phasing and with frequency seriation—namely, the adoption of multivariate statistical procedures and the utilization of computer technology. The Kaminaljuyu project offers a current example (38).

Chronometric Dating

The term "chronometric dating" refers to quantitative measures of time with respect to a given scale. It is synonymous with the more traditional term
"absolute dating" but is gaining favor among dating specialists who regard it as a more appropriate term. The scale or standard of measurement in chronometric dating is our calendar year. Measurements of time intervals between events or between an event and the present are expressed in the number of years that have elapsed between the two temporal points. Chronometric dating, therefore, allows us to do two different but related things. We can quantify time, and we can date events by reference to the Christian calendar.

All chronometric dating techniques are based on natural phenomena that undergo progressive change at uniform rates through time. By knowing the rate of change and the amount of change, the number of years that have elapsed since the process of change began can be computed. There are many complications associated with correlating measured changes in natural phenomena with our calendar system. Advances in one or another technique, therefore, often mean that one of these complications has been resolved. With the exception of radiocarbon dating, the methods available are not widely in use. Many of them are less than 12 years old and have been slow to be adopted. All, however, are well enough established that their promise for contributing significantly to the future of dating looks very bright.

The methods are evolving in an interdependent manner. For example, radiocarbon dating is calibrated by reference to dendrochronological scales; archaeomagnetic dating depends on both dendrochronology and radiocarbon dating; fission-track dating and potassium-argon dating techniques serve to scale each other. As one method becomes more precise, its utility as a scale for critically evaluating another method increases. With better scales, error can be more accurately detected and more effectively corrected, in turn making the latter method more reliable. It is not uncommon nowadays for an archaeologist to deliberately seek out deposits that permit him to recover samples for several different dating methods. At Kaminaljuyu, for example, the author and his colleagues were able to recover obsidian, fired clay, charcoal, and diagnostic pottery all in association at a number of different locations. Such data gives the archaeologist new flexibility and a new perspective in the interpretation of chronology, and at the same time aids those researchers working to improve the dating techniques represented.

Dendrochronology

Dendrochronology has been most extensively applied in the southwestern United States. It is, therefore, not surprising that leadership in research and development in dendrochronology is provided primarily by the Laboratory of Tree-Ring Research at the University of Arizona. The laboratory provides continuing development of new techniques for both the recovery of tree-ring data, its analysis, and ultimately its interpretation. Periodically, results of this work are made available through the publication of the Tree-Ring Bulletin.

In a recent review of the chronological aspects of tree-ring dating, Ferguson (14) provides a glimpse of the new techniques that have been developed in recent years. These would include the development of new core extraction
techniques and equipment, and mechanical and statistical aids to assist in the establishment of master tree-ring series. Furthermore, there is a better understanding of the way in which tree rings develop and the significance of their variation (18) which have led to the development of a dendroclimatic model by which the data used in chronology building can be understood. Many of these innovations are illustrated in the dendrochronological program of the Weatherhill Mesa project. This project had one of the most intensive dendrochronological analyses ever undertaken in Southwestern archaeology. More than 500 individual dates were obtained from over 1900 specimens of wood and charcoal collected during the excavation of 11 ruins (24).

Although the primary focus of tree-ring dating in North America has been in the southwestern United States, there have been sporadic applications of the technique in the Great Plains, the Southeast, the Northeast, the Mississippi Valley, Mexico, and Alaska. Tree-ring dating has also been established in many parts of Europe, ranging from Great Britain to as far north as Finland and the Soviet Union. Turkey and Egypt are reported to have tree-ring dating programs as well. For additional information and specific references please refer to the appropriate chapters in (5, 29, 31). As in the past, much of the impetus for the development of dendrochronology is interest in both archaeological dating and the acquisition of paleoclimatic data.

Archaeomagnetic Dating

Archaeomagnetic dating is based on the principle that the direction and intensity of the earth's magnetic field vary through time (6). Clay soils contain magnetic minerals, and when the soil is heated to a certain temperature the ferromagnetic particles assume the direction and proportional intensity of the earth's magnetic field. They will retain this direction and intensity after they are cooled. By measuring these quantities, the age of the sample can be determined if the changes in the earth's magnetic field are known. The changes in direction and intensity of the magnetic field are referred to collectively as the "secular variation curve." Secular variation curves have been established for varying time segments in only a handful of regions of the world. These include Europe, Japan, the Soviet Union, and parts of North America. Calculation of the secular variation curve of a given region requires the presence of an independent chronometric scale with which to measure a temporally staggered series of archaeomagnetic determinations.

Dunbois [whose work is described by Weaver (41)] has established a secular variation curve for the American Southwest based on clay samples collected from firepits in 16 pre-Columbian Indian villages. By determining the magnetic alignment of his samples, whose ages were already known from carbon-14 dating or tree-ring analysis of timbers found in the same ruins, Dunbois worked out a polar calendar extending back almost 2000 years. The geomagnetic pole seen from the vantage point of the American Southwest has meandered 8800 miles during that period of time. He reports that he can achieve accuracy to within 50 years. The establishment of a secular variation
curve for Mesoamerica is the subject of a doctoral dissertation currently under preparation by Daniel Wolfman at the University of Oklahoma.

**Radiocarbon Dating**

Radiocarbon dating is by far the most widely used method of chronometric dating available. It gained acceptance among archaeologists during the late 1950s and is now undertaken in such volume that commercial dating laboratories have sprung up to compete for the business. Research laboratories at universities across this country and elsewhere have refined the technique and have extended the range of materials to which the method can be usefully applied. Nowadays the archaeologist can consider carbon-14 dating of charcoal, wood, seeds, nutshell, grasses, twigs, paper, cloth, bone and hide, organic matter mixed with earth, peat, ivory, terrestrial and marine shells, carbonate fractions of sediments from deep sea cores and lake beds, and, under certain conditions, both pottery and iron. Along with offering this great flexibility in potential materials for dating, carbon-14 specialists have also made sample collection less hazardous by providing a range of routine decontaminating procedures that can generally solve most of the problems of contamination that might be encountered. In the past several years there has been some loss of faith in radiocarbon dating due to the discovery of significant discrepancies between dendro-years and radiocarbon years established through joint research of the Laboratory of Tree-Ring Dating at the University of Arizona and radiocarbon dating laboratories at the University of Pennsylvania and the University of California. These discrepancies appear to be the result of fluctuations in the production of radiocarbon at varying times in the past (14, 35).

A conference was called to discuss the discrepancies in radiocarbon dating and to identify possible solutions. The conference was held under the auspices of the Twelfth Nobel Symposium in Sweden, August 1969 (34). At the conference it became clear that the assumption of a uniform biospheric inventory of carbon 14 during the past 50,000 years was not entirely correct. Such variables as the rate of atmospheric mixing, the constancy of cosmic ray intensity, the role of variation of the magnetic field, sun spot activities and cycles, and variations in equilibrium conditions for the atmosphere, the continents, and the oceans were examined. Firm answers to many of the questions touching upon the true decay rate of carbon 14 and the causes for apparent variations in the amount of carbon 14 in the atmosphere are still to be found. Yet the precise calibration in a scale of calendar years that is provided by dendrochronology offers radiocarbon analysts an opportunity to define what radiocarbon years mean in terms of our calendar sequence. MASCA has published a table that indicates the amount of deviation between carbon-14 dates and dendro-dates for each sequential 250-year period (35).

The laboratory procedures for radiocarbon dating that are currently most widely used were formulated in the middle 1950s (the proportional gas counting technique). Refinements in the technique have been steadily developed
ever since then, but basically the procedures are not radically different from what they were 15 years ago. Innovation has been aimed primarily at increasing the purity of the sample before counting. Currently a few laboratories are experimenting with various liquid scintillation techniques. Significant advances have been made in the production of benzine and the prognosis for this new development looks very favorable (35). Liquid scintillation counting equipment has the advantage of being readily available commercially and is usually semiautomated; thus manpower requirements for counting are lower than with proportional gas counting.

**Potassium-Argon Dating**

Potassium-argon dating (10, 20) covers nearly the whole range of the time scale, with published dates extending from 4.5 billion years ago to 2500 years ago (12). This impressive range is due in part to the extremely long half-life (1.3 billion years) of the radioactive isotope of potassium, potassium 40, and in part to the availability of suitable procedures by which its decayed product, argon 40, can be detected.

Since the potassium-argon dating method can only be used in situations where new rock has been formed, the primary application of this technique has occurred in geology, and only rarely has it been applied to archaeological problems. The one important exception to this is the field of Early Hominid research. Already there is substantial agreement among human paleontologists about the accuracy of the potassium-argon determinations as applied to the older Olduvai Gorge sequence of hominid fossils (12). High confidence in these dates rests upon the large number of determinations made, their compatibility with stratigraphic studies, and the independant verification provided by the new fission-track dating technique when applied to the same stratum.

One area of technical development in potassium-argon dating has to do with argon extraction and with its mass spectrometry. A critical problem in dating young samples is the need to reduce the amount of atmospheric argon 40 in the extraction system and in the sample itself. If not controlled, a situation may arise where the volume of atmospheric argon 40 becomes so great as to prevent accurate determination of radiogenic argon 40 in very young samples. John Miller (33), in a recent article, reviews some techniques that are being experimented with to solve these problems. The techniques range from the more efficient pretreatment of the furnace and extraction systems and the use of advanced Omegatron-type mass spectrometers to the $^{40}\text{AR}/^{38}\text{AR}$ calculation technique.

**Fission-Track Dating**

Fission-track dating is another new technique of geochronology that promises to have important archaeological applications. So far the method has been tested and shown to be applicable for dating a wide variety of mineralogical materials over a time range that can extend from historic times to
a billion years and more into the past. From the standpoint of archaeological applications, however, the effective limits for typical samples are 100,000 years to 1,000,000 years (40).

Like potassium-argon dating, fission-track dating is applied to newly formed rock. Generally, therefore, archaeological application of this technique is contingent upon having a site that underwent inundation by mineralogical byproducts of a geological event just before, during, or shortly after human occupation. The critical factor that determines if a given sample can be dated is the uranium content. A concentration of one part in a million gives a fission-track density of 0.3 square centimeters per 100,000 years. It is difficult to work with samples having too few tracks per square centimeter, and for this reason samples of typical uranium concentrations must fall within the above-mentioned time range. For man-made objects in which high concentrations of uranium are present, such as some kinds of artificial glass, the technique can be used to date historic artifacts. One such object dated by R. H. Brill of the Corning Museum of Glass is a 19th century candlestick (4). One of the most famous applications of fission-track dating to archaeology is the dating of volcanic obsidian from Bed One of Olduvai Gorge, Tanzania (40). The result was a value of 2.03 ± 0.28 million years. When the Standard Deviation is considered, this date agrees to a considerable extent with the dates obtained by means of potassium-argon dating.

There is reason to be optimistic about the versatility of this technique for archaeological dating. For example, it has recently been reported that fission tracks disappear during annealing at high temperatures. Thus stones that have a high uranium content and which were exposed to fire would lose the fission tracks that had been accumulating since the rock was originally formed and exhibit only those fission tracks that had been produced during the time that had elapsed since the rock was exposed to fire [e.g. fire hearth stones (13)]. A second recent example (42) has to do with the fact that in a number of instances pottery contains highly radioactive inclusions. These inclusions would also have lost their original fission tracks due to the annealing caused by the firing of the pottery vessel, and thus the tracks that accumulate since firing reflect the amount of time that had elapsed between the firing of the pot and the time that the analysis took place. In this case, however, the tracks counted are not the normal fission tracks but rather the tracks produced by alpha-particle recoil events. These alpha recoil events produce small etch pits on the order of 100 angstrom units in length, and they occur some 3000 times more frequently than normal fission tracks (22). Thus, an acceptable degree of precision in counting the number of tracks per unit area can be achieved even on small surfaces such as would be provided by mineral inclusions in pottery pastes.

**Thermoluminescence**

The dating of ancient pottery by thermoluminescence measurements was discovered as early as 1953, but it has been primarily during the past 10
years that this dating technique has undergone serious research and development (26). Research has been conducted at many centers, including UCLA, University of Pennsylvania, Oxford University, Kiel University, University of Bern, and the University of Birmingham. By 1968 a consensus among researchers had been reached suggesting that the technique was on the verge of becoming fully operational as a chronometric dating method, with an accuracy of plus or minus 10 percent (1). As yet no laboratories operate on a routine or commercial basis, although archaeologists can probably look forward to such a service within the near future.

A considerable body of literature on thermoluminescence has appeared in recent years. In 1968 a representative collection of papers was published as a single volume under the editorship of McDougall (26), and a continuous stream of technical articles on thermoluminescence dating relating to refinements in technique as well as to specific instances of archaeological application can be found in various issues of *Archaeometry*, a journal published by the Research Laboratory for Archaeology and the History of Art at Oxford University. The technique has been well publicized in the popular press lately in connection with a series of investigations by the Oxford University staff on the authenticity of various ancient ceramic and terra cotta art works (15).

One of the critical technical problems that used to plague thermoluminescent dating was the nonuniformity of the natural dosage of radioactivity that pottery sherds received. The solution hit upon to cope with this problem was the separation of the fine-grained clay within the potsherd from the mineral inclusions. Thermoluminescent dating could then be applied to either one or the other or both. Inclusion dating and fine-grain dating represent, therefore, two separate strategies that have received considerable refinement in recent years. A third alternative strategy has recently been reported (42), involving the detection and isolation of highly radioactive mineral inclusions for purposes of dating. Using only the high radioactivity grains, it is much simpler to determine the natural dose rate. Although this new strategy will not increase the precision of the date, it is argued that it will make the date more reliable.

**OBSIDIAN HYDRATION DATING**

New developments in both relative dating and chronometric dating application of obsidian hydration measurements (32), have stimulated considerable interest among archaeologists in North and South America, the Near East, and Japan. Other areas that could make use of the technique, such as East Africa and Eastern Europe, have yet to recognize its potential. Currently, there are several large-scale obsidian dating projects under way: in Central Mexico, the Guatemala Highlands, and in the Great Plains of the United States and Canada. In each case, approximately 2000 hydration dates are being processed. Other important applications of obsidian dating, involving 300 to 500 dates, have occurred in Alaska, California, West Mexico, and Ecuador. The two obsidian dating laboratories which have been in operation continuously since the middle 1960s are at UCLA and The Pennsylvania
State University. Most dates have been processed at either one or the other of these two laboratories.

The basic laboratory procedures for obsidian dating were designed by Friedman and Smith in 1959, and underwent some revisions and refinements between 1964 and 1966 (32). Recent technical developments in obsidian dating have focused primarily upon the establishment of hydration rates for specific archaeological regions. Several methods for calculating hydration rates have become available. They involve the use of either an independent chronometric scale, such as radiocarbon dating, or the use of archaeological estimates of age. In the context of attempting to calculate hydration rates some controversy has developed as to the validity of the hydration equation that, according to Friedman, applies universally to obsidian hydration. Meighan (28) has argued on grounds of archaeological evidence that the equation provided by Friedman is incorrect, at least in certain situations. However, the archaeological evidence recovered by most researchers tends to unequivocally support the Friedman equation.

The controversy points to a very salient deficiency in the development of obsidian hydration dating: basic research on the physical chemistry of the hydration process. Important work has been done (17) but the nature of the controversy clearly suggests that further work is needed.

The advantages of obsidian dating are many. They include the fact that dating can be accomplished relatively rapidly, at small expense, and the object being dated is the artifact itself. Due to low cost and the ease with which dating can be accomplished, large numbers of dates can be processed for any given assemblage or site, thereby enabling the archaeologist to avoid the kind of sampling errors that are often associated with other chronometric techniques. In addition, the increasing availability of radiocarbon dates makes it more and more convenient to attempt to establish provisional hydration rates for specific geographical areas. And even in situations where no rate is yet established, the fact that hydration values can be assigned to specific artifacts permits precise artifact seriation.

CONCLUDING REMARKS

It is a paradox that each new dating method makes chronology less important and less interesting. How pleasant it will be—perhaps in 10 years time—when we shall have a number of well-calibrated fixed points on the chart, and can use the artifacts, the basic material of archaeology, for purposes other than dating! How satisfactory if radiocarbon and thermoluminescence and fission-track dating would render obsolete the seriations, the typologies, and the cross-datings (36).

This wishful comment by Colin Renfrew was inspired by a survey of the implications for Old World archaeology caused by the dendrochronological calibration of radiocarbon dates. He observed how erroneous much recent chronological thinking had been and how sterile much of the chronological discussion. The same exasperation, I am sure, could be voiced by any one of
us who practice in archaeological areas that have witnessed the long-term
development of traditional chronologies.

I cannot escape the conclusion based on my reading of the literature that
Renfrew's wishful comment will be a wish come true in the foreseeable future.
The prognosis for the chronometric techniques herein reviewed is very good.
There is no question but that the errors associated with their application will
soon be controlled even more rigorously, and that the chronologies that they
provide will be reliable and reasonably precise. In a word, archaeological dat-
ing will become routine, and we will be able to focus our attention on the
behavioral aspects of our data. There is, moreover, one way to hasten the
arrival of such a day, and that is for all archaeologists to make maximum use
of these new experimental techniques. We must provide space in our research
budgets for funds to underwrite such dating so that adequate field data can
be generated with which to evaluate the techniques, and so that all of us can
become more familiar with the dating options available.
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INTRODUCTION

This paper reviews published material concerned with the analysis of pattern in the physical remains of human settlement. Although my major concern is with prehistoric settlement, I have also tried to deal with studies which focus on physical traces of occupation, whatever their status with respect to historical documentation. I have made no attempt to discuss the literature on settlement of industrialized societies at the nation-state level, nor have I tried to incorporate the abundant literature from locational geography, plant ecology, or animal ecology where techniques and methods have been developed which have a definite, although largely untapped, potential for new analytical approaches to archaeological settlement. Because I have focused on studies whose explicit purpose has been the description and analysis of settlement patterns, I have made no effort to discuss the vast literature—ethnographical, archaeological, and historical—which contains data of potential utility to settlement pattern studies.

In this review I trace the historical development of the settlement pattern concept in archaeology; define the principal concerns and issues which investigators have considered to be important; discuss something of the substantive results of settlement pattern study through a sampling of representative published materials; and provide an assessment of the major accomplishments and future needs of settlement pattern analysis in archaeology.

DEVELOPMENT OF CONCEPT AND METHODOLOGY

From the English-language literature it is apparent that the study of ancient settlement has developed through two largely independent traditions: one in America and the other in England. The American tradition finds its most immediate roots in Morgan's last published work (67), which originally appeared in 1881. Here Morgan posed the critical question of how the remains of aboriginal residential architecture in North America reflected the social organization of the prehistoric peoples who occupied them. The inade-

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quacies of Morgan's interpretative scheme are well known. But, whatever the criticisms we now find it so easy to level at his work, the fact remains that Morgan made a real effort to grapple with a series of complex questions which had seldom been asked before, and which remained unasked and unanswered for nearly 40 years after 1900. These same questions are still at the core of modern settlement pattern study.

While Morgan is the best known of the late 19th century American anthropologists who interested themselves in the sociology of architectural remains, there is at least one other who should be mentioned. During the 1890s Mindeleff (66) conducted a series of investigations in the American Southwest. On the basis of ethnographic analogy he offered an interpretation of settlement accretion and growth, and presented a method for reconstructing occupational chronology and settlement composition from archaeological remains.

The provocative problems and hypotheses posed by Morgan and Mindeleff lay dormant until the late 1930s, when Steward published two important studies on aboriginal social organization in the North American Southwest (84, 85), in which prehistoric regional and community settlement patterns were used to infer general developmental processes. Steward's work, unlike that of Morgan and Mindeleff, stimulated a series of productive and enduring innovations in archaeological research. The 1940s saw the initiation of two major field programs concerned with locating and mapping archaeological sites on a regional scale with the express purpose of inferring sociological processes from changes in site patterning through time: the lower Mississippi Valley survey undertaken by Phillips, Ford & Griffin (70), and the famous Viru Valley survey carried out by Willey (97). The latter explicitly acknowledged an intellectual debt to Steward. An even earlier survey in the Plain of Antioch, Syria (Braidwood 8), apparently independent of any influence from Steward, may well be the earliest systematic settlement pattern survey carried out by an American archaeologist.

Although the Mississippi Valley survey was largely designed for the purpose of analyzing ceramic stylistic variability over space and time, it also attempted a regional random sampling of site types and a classification of settlements on the basis of architectural manifestations and surface area. Changing configurations of site types through time provided a basis for a limited set of inferences on social organization within the large survey area. It was in the Viru Valley, however, that the first archaeological study aimed explicitly at inferring cultural processes from the regional patterning of settlement was carried out. This study is also significant in that it marked the first formal statement of the scope of prehistoric settlement pattern studies and their potential utility in archaeology:

The term 'settlement patterns' is defined here as the way in which man disposed himself over the landscape on which he lived. It refers to dwellings, to their arrangement, and to the nature and disposition of other buildings pertaining to community life. These settlements reflect the natural environment, the level of
technology on which the builders operated, and various institutions of social interaction and control which the culture maintained. Because settlement patterns are, to a large extent, directly shaped by widely held cultural needs, they offer a strategic starting point for the functional interpretation of archaeological cultures. (Willey 97, p. 1).

The Viru Valley program was innovative in methodology as well as in its overall objectives. For the first time in the western hemisphere vertical air-photos were employed in site location and mapping. Equally impressive was the focus on intensive sampling within a relatively small area as a means of delineating processes operative within a considerably larger system. An effort was made to classify each site in terms of considerations of location, architecture, refuse accumulation, and surface area.

The mid-1950s marked the beginning of a proliferation of archaeological settlement pattern studies in America. This included not only descriptive works, but an increasing number of theoretical and methodological contributions as well. Perhaps the first major effort to integrate the concept of settlement pattern within a general developmental classification of culture was at a series of seminars held by the Society for American Archaeology in 1955 (Beardsley et al 4). Here an attempt was made to define seven major sociocultural stages, each characterized by a "community pattern" in which the principal variables were subsistence base and settlement configuration. Population expansion produced by increasing productivity in subsistence base was viewed as the major causal factor in the evolutionary progression. The subsistence base itself was considered as the main determinant of settlement permanency and arrangement. The major significance of this study for archaeological settlement pattern analysis was that for the first time an explicit effort was made to predict what the archaeological manifestations of each community pattern would be. Because of the imprecision and vagueness of its definitions and processes, this scheme has found only limited utility in structuring archaeological fieldwork and interpretation. However, it has had a significant impact on a number of subsequent theoretical works (17, 35, 101).

The publication in 1956 of *Prehistoric Settlement Patterns in the New World* (Willey 99) marked the point at which large numbers of archaeologists in the United States first became fully aware of the potential significance of settlement pattern studies. This work comprised a collection of papers by numerous writers, each of whom organized data from his area of specialization within the framework of his conception of settlement pattern. Quite expectably, nearly all contributors were forced to use material which had been collected for other purposes. Equally expectably, the diverse orientation and concerns of individual papers reflected the lack of common agreement on what were the proper scope and objectives of settlement pattern study in archaeology. Three papers from that collection may be singled out for their contributions to the developing concept of settlement patterns.

Willey (98) expanded upon his earlier discussion and emphasized that "settlements are a more direct reflection of social and economic activities
than are most other aspects of material culture available to the archaeologist. Because of this, settlement investigations offer a strategic meeting ground for archaeology and ethology . . .” (98, p. 1). Concerning the status of settlement pattern study in archaeology, Willey argued that “there is no settlement pattern approach to archaeology. An awareness of settlement data simply extends the net of archaeological interest to take in a larger and legitimate part of the record.”

Speaking as an ethnologist, Vogt (93) stressed the significance of settlement pattern investigation as a point where archaeologists, ethnologists, and geographers could meet to consider the contribution of their respective expertise to common problems. He envisioned the scope of settlement pattern study as including

a description of (1) the nature of the individual domestic housetype or types; (2) the spatial arrangement of these domestic house types with respect to one another within the village or community unit; (3) the relationship of domestic house types to other special architectural features . . .; (4) the overall village or community plan; and (5) the spatial relationships of the villages or communities to one another over as large an area as is feasible (93, pp. 174–75).

From this descriptive level inferences could be made on the basis of ethnographic analogy with related living populations, and deductions from universally valid laws derived from cross-cultural data. Vogt saw three kinds of interrelated interpretation as most appropriate:

one which explores the relationships of living arrangements to geographical features, such as topography, soils, vegetation types, or rainfall zones; a second which focuses upon the social structural inferences that can be made about sociopolitical and ceremonial organization; and a third which concentrates upon the study of change through time with a view to providing materials for generalizing about cultural processes.

Sanders’ (73) study provided some useful definitions of scope and terminology. He introduced the concept of symbiotic region into the archaeological literature, and was mainly interested in analyzing the distribution of human settlement in the context of agricultural systems, local specialization, and interregional exchange. He distinguished between community settlement patterns and zonal settlement patterns. The former comprises “the individual units of population, including such data as types of communities, organization of public buildings, street and population distribution and form, density of community population, and character of the resident population. It would also include house types and solar organization” (Sanders 73, p. 116). Zonal settlement patterns, on the other hand, “are concerned with the distribution of community sizes, distances between communities, density of population, and the symbiotic interrelationship between communities . . .”

The immediate impact of Prehistoric Settlement Patterns in the New World was particularly noticeable in works dealing with the American Southwest (e.g. Bluhm 7, Dittert et al 30, Herold 41, Martin et al 58, 59). Studies from other areas were less numerous, but significant papers dealing with
Mesoamerica (R. Adams 1, Bullard 12, Sanders 74) and the eastern United States (McIntire 60, Ritchie 71, Sears 77) also appeared soon after 1956. Many of these were attempts to draw sociological inferences using distributional data which had previously been obtained with other problems in mind and where a real sampling and chronological control were sometimes inadequate for meaningful settlement pattern analysis. The major exceptions were Sanders', Bullard's, and Adams' work in Mesoamerica, where extensive regional surveys were carried out. Sanders also attempted to supplement surface observations with systematic test trenching within site areas. This innovative technique permitted him to appraise occupational density in areas where no structures were apparent on the surface.

The period between the mid 1950s and early 1960s witnessed the initiation by American archaeologists of the first major field programs whose scope and objectives revolved very largely around regional settlement surveys on the Viru Valley model—e.g. R. Adams' project in the Diyala region of Iraq during 1957 and 1958; Sanders' work in the Teotihuacan Valley, Mexico, beginning in 1960; and Willey's Belize Valley, British Honduras, project between 1954 and 1956.

The late 1950s also saw a growing awareness among American archaeologists of the utility of ethnographic analogy in settlement pattern interpretation. This had actually been initiated by Wauchope's pioneering study of contemporary Maya architecture in the 1930s (94). Coe (22) pointed out the potential parallels between Khmer settlement patterning in the tropical rainforests of southeast Asia and that of the ancient Lowland Maya. Willey (100) stressed the significance for prehistory of Hogbin and Wedgewood's ethnographic study of community types in Melanesia. Miles (61, 62) suggested the extended boundary town, a pattern characteristic of Classic Greece, as a model for the prehistoric Maya. And in 1958 Chang (16) published the first serious attempt to generate and test hypotheses concerning the relationships between social organization and settlement patterns in simple "neolithic" societies (nonstratified groups practicing subsistence agriculture). Chang reiterated and elaborated some of these same problems in a later paper (17) in which his main concern was the formulation of a typology of settlement and community patterning for aboriginal Arctic groups. An important contribution of this latter study was the concept of "annual subsistence region": the territory over which the members of a community moved during an annual cycle, and in which several different kinds of settlement sites may be occupied in the course of the seasonal round of subsistence activities.

The appearance of Prehistoric Settlement Patterns in the New World was also soon followed by the first significant awareness among American ethnologists that their expertise could contribute to the study of prehistoric settlement patterns. This is not to say that ethnologists had never expressed interest in contemporary settlement. James (48) and Lévi-Strauss (55), for example, had long before been concerned with the relationship between social organization
and the physical form of a human community. After 1956, however, for the first time ethnologists found they were being asked questions concerning causal and functional aspects of settlement patterning for which they already had a few answers. Carneiro (14), for example, argued convincingly that the requirements of swidden agriculture were not a factor in the small size and instability of settlements among traditional horticulturalists in the Amazon Basin. A somewhat different kind of contribution has been that of Naroll (68), who utilized a sample of 18 ethnographically known societies to establish a quantified relationship between floor area within a settlement and the population of the community. Cook & Heizer (24, 25) have subsequently pursued the quantification of the settlement size-population relationship in somewhat more detail.

Beginning in the mid-1960s American archaeologists were becoming increasingly aware of some of the methodological and analytical limitations of definitions and concepts utilized in settlement pattern studies during the previous decade. As a consequence the past 6 or 8 years have seen some important breakthroughs in this field, especially as regards methodological sophistication. One of the more significant contributions has been the development of the "settlement system" concept, a refinement and clarification of Chang's "annual subsistence region." Although the actual concept and its significance for archaeology had been expressed many years before by Thompson (89), its utility for American archaeology had been quite limited prior to the 1960s. Winters (104), writing in the early 1960s on prehistoric settlement in the Wabash Valley, may have been the first American archaeologist to formally use the term. Somewhat later (105) he made an explicit distinction between settlement pattern and settlement system. By settlement pattern he "meant the geographic and physiographic relationships of a contemporaneous group of sites within a single culture" (105, p. 110). Settlement system "refers to the functional relationships among the sites contained within the settlement pattern . . . the functional relationship among a contemporaneous group of sites within a single culture." The limits of a single "culture" were defined by the distribution of distinctive stylistic traits. Definition of the settlement system itself depended upon ability to infer the time of year during which different kinds of sites were occupied and the activities which were performed at these sites. The necessity to know seasonality and function for numerous sites within a local region required systematic collection and analysis of a wide variety of data, some of which had seldom been regarded as significant in archaeological research: faunal and floral remains; behavior and availability of important plant and animal species; climatological considerations; organizational aspects of various subsistence techniques; architectural features; precise knowledge of differential abundance and distribution of artifact types, especially those for which function was known or could be readily inferred. Another innovation was Winter's use of a "Systematic Index" as a quantified measure of functional differences between sites. The sum of fabricating, processing, and domestic tools from excavation units divided by the number of tools which can be regarded as weapons for hunting gave a
ratio which Winters regarded as a reasonable indicator of functional variability between sites within a settlement system. Even as rudimentary a level of quantification as this required new sampling procedures to insure intersite comparability.

Even prior to Winters' pioneering use of the settlement system concept, Vescelius (92) and Binford (5) had urged greater rigor in sampling procedures, both within individual site areas and over large regions, in order to permit valid quantitative manipulation of data in reconstructing settlement patterns and settlement systems. Binford & Binford (6) have utilized factor analysis in an impressive effort to reconstruct Paleolithic settlement systems through the detection of patterned artifact variability in Mousterian tool assemblages from France and the Levant. Using a refined tool typology, Wilsen (103) has demonstrated the significant differences between artifact assemblages of several Paleo Indian sites in the United States. The suggestion is that a variety of different activities were being performed at different sites. Because of limited sampling in local areas, no settlement systems analysis was possible, but the indication of a relatively complex settlement system is clear and the lines of future research are apparent. Two of the earliest applications of quantitative techniques to artifactual material were the investigations at the Carter Ranch site, Arizona, Here Longacre (56) and Brown & Freeman (10) analyzed intrasite stylistic variability in ceramic material. Their work suggested the presence and location of matrilocl residential groups within a Puebloan community. The differential distribution of ceramic types within the site was also deemed suggestive of the functions performed in specific localities, and a general pattern of multifunctional rooms was indicated. Hill's (43-45) work at Broken K Pueblo, Arizona, Whallon's (96) stylistic analysis of Iroquoian pottery, and Deetz's (28, 29) analysis of Arikara ceramic stylistic attributes are further examples of the potential value of sophisticated analytical techniques and rigorous sampling procedures in reconstructing functional and societal aspects of archaeological remains. Cowgill (26, 27) has also indicated the utility of factor analysis and computer manipulations in the analysis of artifact material from the ancient urban center of Teotihuacan (Millon 63-65). Cowgill's efforts are additionally significant as they represent the first serious application of sophisticated quantitative methodology to a complex society where social stratification, urbanization, and state formation create additional complications for settlement systems analysis which have not posed problems for workers investigating the egalitarian and simple ranked systems where the most impressive methodological advances have been made.

Most American workers involved in settlement pattern studies of complex societies in Mesoamerica and Mesopotamia have operated more in the tradition initiated by Willey (e.g. R. Adams 2, Parsons 69, Sanders 75). Here the emphasis has been upon systematic extensive regional surface survey, within regions of several hundred to several thousand square miles, in order to define the extent of the system, delineate broad problems, and formulate hypotheses regarding site function, demography, land use, and polity, which can
be tested and refined through subsequent intensified investigation. Here inferences have been drawn from the gross outlines of settlement configuration, from surface indications of differential architectural complexity within and between sites, from site locations with respect to topographical and resource features, and from the changes in these variables through time. Due to the complexity of the systems involved and to the fairly recent inception of work by relatively few individuals, few investigations of complex societies have progressed to the second stage in which preliminary hypotheses are subjected to more rigorous testing. I know of only a single instance in which research of this latter type has been carried out and published: Wright's (107) investigation of a small Early Dynastic urban system in southern Mesopotamia. Here data from regional survey and selective excavation were combined with textual and ethnographic information to test hypotheses relating to administrative complexity and relationships between inhabitants of different settlement types within the settlement system.

In the mid-1960s some aspects of the American tradition of settlement pattern archaeology were transplanted to Oceania. This was particularly marked in the case of eastern Polynesia, where archaeologists from universities in New Zealand and Hawaii have begun a vigorous subtradition of their own (e.g. Buist 11, Green 35–37, Groube 39, Kennedy 53, Leach 54). Green's 1963 paper (35) may be said to mark the beginning of systematic settlement pattern archaeology in Oceania. Faced with extremely fragmented data and a virtual absence of a sound chronology, Green constructed a trial model of occupation and settlement into which known sites and features were fitted on the basis of the limited inferences which could be made about their character and function. A series of developmental stages were defined [in which certain considerations from the Beardsley et al (4) formulation were incorporated with early historical and semilegendary material from an initial occupation about 800 years ago through successive periods of expansion and subsistence intensification to the "Classic Maori" era just prior to European contact in the late 18th century. Sites were ordered by how they appeared to fit the predictions of the proposed model. The attempt provoked considerable controversy and directly stimulated a number of field programs aimed at testing and evaluating some of Green's propositions. Special emphasis has been placed upon the sophisticated use of historical materials as a supplement to archaeological research in creating a firmly established contact-period baseline from which to work back into prehistoric times. The short time span of human occupation in eastern Polynesia makes this an ideal research strategy.

As yet, few of the above-mentioned investigations carried out during the 1960s have been incorporated into projects aimed at reconstructing full-scale settlement systems. However, we are clearly at or near the point where it is analytically and methodologically feasible to begin to do this. We can conclude this section with reference to what is probably the most comprehensive recent statement concerning the goals and methods of settlement pattern archaeology. In three related papers, Struwer (86–88) provided a rationale and ideal research strategy for describing and explaining the rapid cultural
changes which occurred in the Illinois Valley between terminal Early Woodland and Middle Woodland times. Previous work had indicated the major outlines of increasing cultural complexity over this period. From this observation Struever proposed a general hypothesis which argues that a series of selective pressures were operative during the last two centuries B.C., such that significant changes in subsistence and social organization were adaptive responses. In testing this hypothesis it becomes Struever’s task to describe the subsistence and organizational basis of the earlier and later systems, to delineate the selective pressures brought to bear on the earlier system, and to show how the changes which transformed the earlier system were responses to these pressures. The key to his research strategy is the reconstruction of settlement systems which succeeded each other in time.

A reading of Struever’s discussion of what needs to be done in order to properly achieve these objectives is a truly sobering experience for any archaeologist who has contemplated a program with similar goals. Ideally the research should be initiated with a reconstruction of paleoenvironment delineating the significant microenvironmental zones for the period in question. Each microenvironment so defined is then to be systematically sampled by surface survey in order to locate a representative number of sites within each zone. This latter procedure assumes that the loci of activities related to the extraction of different resources will correlate with the natural distribution of these resources. Careful surface sampling indicates the location of sites of the appropriate time period. Analysis of surface collections is the basis for definition of preliminary settlement types on the basis of differences in type and relative abundance of various artifact types. At least two examples of each provisional settlement type should then be excavated in such a way that the complete range of artifact variability and features can be detected. This involves sampling over the entire extent of the site’s surface. The excavation program should be initiated with test pits randomly placed within a grid over the site. This should permit an evaluation of initial impressions from surface survey. Larger-scale excavations should then be carried out on the basis of where activity areas have been defined by the random test excavations. By this means different artifact types and features can be clearly associated. Finally, a whole section of the site should be exposed, where a stable ancient land surface can be identified, in order to provide a large enough sample of artifact variability and feature association so that sampling error will be kept at a minimum.

Once these procedures have been carried out and analysis completed, the two settlement systems in question may be said to be properly described. This is not to say, however, that the transformation of one system into the other has been explained. To do this, Struever argues that it will be necessary to subject adjacent regions to similar investigations in order to understand the selective pressure impinging on the Illinois Valley system.

Needless to say, a research program of the scope outlined by Struever has never been carried out. At the moment its major utility lies in reminding us what our present programs lack and cannot yet achieve.
THE ENGLISH TRADITION

In England during the late 19th and early 20th centuries there had been considerable interest in what might be termed the “ethnic” identity of some archaeological remains—e.g. “Celtic” fields and farmsteads. Settlement types, identified from surface remains and excavation, were commonly identified with specific ethnic groups, and their distribution associated with the spread and movement of these groups. However, there was no real concern with or concept of settlement patterning until the early 1930s when Fox’s pioneering study (34) marked the beginning of a new era. Fox’s work was quickly followed by several additional papers (e.g. Childe 19, Grimes 38, Hogg 47, Woolridge & Linton 106). These early investigations focused principally on establishing correlations between the known distribution of ancient remains (tombs, artifacts, earthworks, residences) and environmental features. Their main concern was to draw inferences on subsistence and economy from the location of remains at any given period and the changes in distribution through time relative to such factors as soil type, forest cover, drainage, natural defensive situation, waterways, etc.

Even at present, English archaeologists concerned with the archaeological manifestations of regional population distribution (e.g. Fleming 33, Simpson 78, Stevens 83) depend very heavily on surveys and excavations carried out by numerous investigators for many different purposes (among which settlement pattern analysis is seldom included). With the very major exceptions of Clark’s excavations at Star Carr (21) and the extensive regional surveys carried out in Italy by the British School at Rome (Duncan 31, Kahane et al 52), I know of no substantial fieldwork program initiated and carried out expressly to define and analyze settlement patterning by archaeologists working within the English tradition. The most productive methodological advances here have probably been in the realm of the expanded and increasingly sophisticated utilization of archival and other historical materials in conjunction with settlement pattern investigation (e.g. Jones 49–51, Smith 81). I think it is also fair to say that the development and conceptualization of settlement pattern study in England has been considerably less self-conscious than in the United States, and that its theoretical underpinnings have remained largely implicit and undeveloped. I have been able to locate only one substantial reflective statement by an English archaeologist (Clark 20) on the role of settlement patterns in archaeological interpretation:

The house . . . exists to provide shelter for a family and its plan must to some extent be determined by the structure of a family. In the same way the manner in which houses are aggregated in settlements, or alternatively occupied as isolated units, must be connected with the larger organization of social communities. More than this, the character of early settlements and their sites is more or less strongly influenced by the nature of relationships existing between different communities: above all there are questions of security and these involve not merely general political considerations but also the actual methods of warfare in use among neighboring peoples (20, pp. 131–32).
Clark's study, as he himself states, has little further to say regarding the application of these sociological considerations to actual European archaeological settlement data.

SELECTED CASE STUDIES

Trigger (91) has argued that archaeological settlement patterns can be analyzed on three levels: the individual structure, the local settlement, and the distribution of settlements within a region. While true settlement systems analysis demands an integration of all three levels, each can be meaningfully investigated in relative isolation. The second major section of this review will discuss substantive results of selected examples of settlement pattern research in archaeology at the three levels indicated by Trigger. Space limitations will force me to be highly selective, but I hope to at least be able to indicate some major accomplishments and problem areas.

THE INDIVIDUAL STRUCTURE

Studies focused exclusively on a single structure are very scarce. The best example I have found is Robbins' (72). His concern is with generating hypotheses regarding the correlation between house shape and relative residential permanence, and testing them statistically with cross-cultural ethnographic data. As such, this work does not represent the investigation of any specific structure, nor does it deal with internal partitioning or range of household activities. Nevertheless, Robbins does offer a useful analysis of one attribute, and his methodology is worthy of emulation. A general review of ethnographic literature suggested a correlation between circular houses and impermanent settlement, and between rectangular houses and sedentary occupation. Furthermore, the archaeological record indicated a shift from round to rectangular dwellings accompanying the transition to more fully agricultural subsistence in different parts of the aboriginal United States. Cross-cultural data from HRAF files were then subjected to statistical tests which showed a significant correlation between sedentary, agricultural populations and rectangular house plans, and between relatively mobile, essentially non-agricultural groups and circular house plans. Robbins also attempted to factor out the effects of diffusion within his sample of societies, and argued that his observed correlations probably represented a "true functional relationship."

THE SETTLEMENT

This level of analysis is sometimes referred to as community settlement pattern. The assumption is often made, sometimes implicitly, that the archaeologically definable settlement is equivalent to a significant social unit to which the term "community" is applicable. Studies are fairly numerous. Examples would include W. Adams (3), Brose (9), Clark (21), Haviland (40), Hill (43-45), Longacre (56, 57), Millon (63-65), Sanders & Michels (76), and West (95). Ho (46) represents a case where a historian is interested in similar objectives using documentary data. Of these, Hill's work at
Broken K Pueblo in eastern Arizona is probably the most complete. It has the additional advantage of careful explication of assumptions, methodology, and limitations which seldom finds such cogent expression. For these reasons Hill's investigation becomes a convenient point around which to structure a discussion of the accomplishments and problems of community settlement patterns.

Hill has two major objectives: (a) to describe social structure and community organization at Broken K Pueblo so that (b) this can serve as a base from which to generate hypotheses dealing with the marked changes in the general organization of Pueblo society which occurred between 1150 and 1300 A.D. His research design is structured on the assumption that patterned behavior is reflected archaeologically by patterned artifact distribution. To detect these patterned distributions of artifacts and to infer the sociological behavioral referents that produced them are thus Hill's immediate goals.

The first task becomes one of defining a settlement or "community" on the ground. This is seldom a straightforward procedure in archaeology where seasonality, long-term and multicomponent occupation, a dispersed settlement configuration, and noncontemporaneity of features can pose serious problems even for the description of a settlement at any given point in time. For Hill this is somewhat simplified as he is working in a nonalluviated area where there is considerable cultural continuity from prehistoric times into the present, at a site whose occupation probably did not exceed 150 years (ca. 1150–1300 A.D.). At Broken K Pueblo it was possible to clearly delineate the full extent of a compact pueblo settlement together with most of its individual rooms and other architectural units. The site extends over a total surface area of roughly 300 square meters, and contains about 95 rooms grouped about a large central plaza. While the absolute contemporaneity of some features remains uncertain, Broken K offered a nearly ideal situation in that Hill could define a uniformly well-preserved archaeological community with reasonable probability that he was dealing with a synchronic occupation, he could structure a meaningful sampling design by using visible architectural features as sampling units, he could confidently employ ethnographic analogy as a powerful heuristic tool, and he could deal with this entire modest community within a reasonably short period.

Hill's research posed two major questions which lie at the heart of community settlement pattern analysis: (a) what kinds of activities were being carried out in each room? and (b) what kinds of residential groups occupied the site?

A 50% random excavation sample of rooms was deemed desirable and adequate. Random sampling was used so that all kinds of rooms would have an equal chance of being represented. Each room so selected was completely excavated in natural levels, and material on floor surfaces was separately screened. This latter procedure was felt to be particularly critical in providing objects of definite room association for inferring function. Pollen samples were also taken from each room, and often provided key information. Room types were defined on the basis of associational tests using 12 different attrib-
utes: floor area, firepits, mealng bins, ventilators, artifact types, lithic waste, animal bone, seeds, pollen types, sherd.s, pottery-type factors, and ceramic design-element factors. On this basis Hill was able to specify the probable "functional characteristics" of the standard habitation, storage, and ceremonial room types. The importance of this analysis was that these probable "functional characteristics" were systematically demonstrated rather than merely assumed on the basis of analogy with modern Puebloan architecture. It is interesting to note that pollen provided the only firm evidence for assigning the category "storage" to units which would have traditionally been designated as storage rooms on the basis of their size and lack of food-preparing equipment. The key to Hill's whole functional analysis was assignment of a "functional meaning" to specific attributes and attribute clusters. This was done mainly on the basis of general ethnographic analogy.

Hill's analysis of social organization is cautious but highly provocative. Factor analysis of pottery types and ceramic design elements is the primary basis for inference. This indicated two major units, each of which shared a substantial number of stylistic elements which were absent or infrequent in the other. Five smaller units were also delineated on the basis of factor distributions within each of the two main units. Objects probably associated with female activities tended to cluster in this manner. Following Longacre, who observed that among modern Pueblos ceramic design styles are passed from mother to daughter within localized matrilineages, Hill infers that his localized stylistic clusters represent "uxorilocal residence units." Although he regards their existence as tentative, Hill notes that each of the two major "uxorilocal residence units" at Broken K are about the same size (about 20 rooms) as the entire settlement at Carter Ranch where occupation dates somewhat earlier. This suggests that the Carter Ranch community was sociologically equivalent to one of the major uxorilocal units at Broken K, and that community expansion may have proceeded through the addition of such units through time. Hill also suggests that his two major residence units at Broken K may equate with the phratry or clan units of modern Pueblos, while the five smaller units he distinguished may represent the clan or lineage units of modern Pueblos. He emphasizes that no demonstration of this latter hypothesis is now possible, but suggests that it may prove useful to compare the archaeologically derived residence units for different prehistoric settlements in the Southwest once an adequate sample becomes available.

While Hill's investigation provides a model of sound conception and methodological rigor for the study of community settlement patterns, a direct emulation of his analysis to achieve similar objectives at many other sites would obviously be out of the question. Any attempt to do this at a community like Teotihuacan, Kaminaljuyu, Tikal, or Chan Chan would involve inordinate outlays of time and money. Deeply stratified occupations, with little if any recognizable architecture, likewise will demand radically different research strategies. Small settlements with brief time depth, otherwise ideal for the kind of analysis Hill has performed, may be deeply buried with few surface traces, and thus require extensive stripping merely to define their areal
extent. Differential destruction in historic times can pose almost insurmountable obstacles to a systematic sampling program. Thus, it is fair to say that few have had Hill’s success because few have had his advantages. It is also possible to argue that few archaeologists interested in community settlement patterns have designed their research so well. Of course, this is largely nobody’s fault. It has been predominantly a case of new interests evolving ahead of methodology. Thus, most investigators who embarked in the early 1960s on projects dealing with aspects of community patterning found themselves trying to answer new questions when available techniques for data acquisition and processing were much more appropriate for traditional problems of the 1940s and 1950s.

A program like Hill’s at Broken K represents a successful application of some new techniques to provide some answers (or at least some testable hypotheses) to some of the new questions of community settlement pattern study in archaeology. As such, it now behooves others to make use of those aspects of Hill’s research which are appropriate to their own investigations, to reject or modify that which is unsuitable or inadequate, and to attempt to devise new approaches to deal with the special problems which every site presents. It seems clear that adequate investigations of community settlement patterns in the future will increasingly demand some measure of rigor in problem definition, sampling, data collecting, and analysis in order to provide answers to specific questions of function and social organization.

The Region

In theory, for settlement pattern analysis it would seem that a region could be treated much as if it were a site. That is, it can be delineated on the ground, separated into units (stratified) on the basis of different attributes and combinations of attributes, and systematically sampled. In practice, of course, a region is so much larger and more complex a unit than the individual settlement that it must be treated somewhat differently if for no other reasons than limitations of time and money. We have just noted that the success of Hill’s sampling program at Broken K Pueblo was dependent upon the facility with which the architectural components and limits of the settlement could be ascertained. To do this for a region is obviously a major undertaking. What Hill could accomplish at Broken K in a few weeks by means of surface mapping and small-scale trenching requires several full field seasons at the regional level. Thus, while zonal studies are the most abundant in the published literature on settlement patterns, practically all are tentative and descriptive in character—i.e. they function mainly to delineate relevant attributes, define problems, and generate preliminary hypotheses. This is certainly not to say that such studies have been without value for interpretative work. Far from it, for they have provided much of the key demographic and distributional data from which a wide variety of inferences have been made. However, such inferences remains much more subject to subsequent rejection and revision than if they were the products of adequately demonstrated relationships.
A few examples of published regional settlement pattern studies include the work of Kahane et al (52), Fleming (33), Childe (19), Fox (34), and Jones (49–51) in Europe; R. Adams (1), Willey et al (102), Sanders (74, 75), Parsons (69), Bullard (12), Charlton (18), Coe & Flannery (23), and Spores (82) in Mesoamerica; Willey (97) in Peru; R. Adams (2) and Wright (107) in Mesopotamia; Hester & Hobler (42), and Trigger (90) in northeast Africa; Winters (104, 105) and Fitting (32) in the United States; Green (35), and Kennedy (53) in eastern Polynesia. These are varied in orientation and methodology. From this diverse collection I will use Sanders' (75) and my own work (69) in the Valley of Mexico as a focal point for discussion. The reader should be aware that my own work has grown directly out of Sanders'. Sanders in turn found a very significant stimulus in the earlier work of Willey.

At the regional level the initial task becomes one of defining the survey area. This has sometimes been a rather arbitrary and ad hoc process, but proved fairly straightforward in the Valley of Mexico where an area of about 3000 square miles is sharply demarcated on three sides by massive mountain ranges. Ethnohistoric and archaeological data accumulated over the preceding century have clearly indicated that the Valley of Mexico was a key nuclear area in Mesoamerica during much of the prehispanic era. The success of the Viru Valley settlement pattern survey in illuminating some major developmental processes within the broad Central Andean region provided Sanders with a model for a similar project in the Valley of Mexico. The area selected was the immediate hinterland of the ancient urban center Teotihuacan, a well-defined subvalley of about 600 square kilometers in the northeastern Valley of Mexico. Here Sanders introduced the method of field-by-field surface survey using large-scale vertical airphotos for direct field plotting of archaeological features in this area where alluviation and vegetation cover did not generally pose problems for detecting the remains of ancient occupation. Over a period of several field seasons, which also included some refinement of the known ceramic sequence and considerable experimentation with survey techniques, Sanders was able to plot the distribution of prehispanic occupation for eight or nine phases over large continuous tracts with some confidence that all sites had been located. These distributional patterns served as the basis for inferences regarding changing patterns of land use, population expansion, sociopolitical evolution, and economic integration. A major weak point was that there was no good control over site function or the range of activities which had been performed at various localities through time. In essence, Sanders' 4-year program accomplished for the Teotihuacan Valley what Hill's initial mapping was able to do for the Broken K Pueblo site in a few weeks: to establish a base from which an adequate sampling program could be designed. It is difficult to see how it could have been otherwise.

Several additional points can be illustrated with reference to the expansion of the Teotihuacan Valley regional settlement pattern survey into the adjacent Texcoco Region to the south (another area of about 600 square ki-
lometers bounded by distinct natural features. Its methodology and objectives were similar, and we viewed the Texcoco Region survey as a second step in a long-term effort to do adequate settlement systems analysis in the Valley of Mexico. Four interrelated objectives emerged as particularly critical: (1) a classification of sites which was methodologically sound and analytically meaningful (this involved some inferences about site function); (2) an estimation of relative population levels for different time periods; (3) a chronological framework within which settlement configurations could be structured; and (4) some understanding of the productive potential of the survey area for each period in question. Key attributes which our survey technique permitted us to measure fairly adequately were settlement surface area (distribution of surface pottery and rock rubble); occupational intensity (density of sherd debris and architectural remains); architectural complexity; relative abundance of key ceramic assemblages whose chronological placement was known; placement of settlement relative to a variety of topographical features and natural resources; placement of sites with respect to other contemporary sites of various types; and changes in these distributional patterns through time. A number of implicit assumptions are immediately apparent, few of which have been formally or adequately tested, but which presently appear intuitively valid or reasonable to us.

Somewhat less apparent to the general reader will be several serious weaknesses which should be spelled out since they are potentially applicable to any regional settlement pattern study of comparable scope. First, there is no systematic control over site function. Some functional aspects have been tentatively inferred at a very general level from considerations of size, location, architectural characteristics, and quantity of occupational debris. The major problem produced for this stage of analysis is in population estimation. Having no proper understanding of site function, we consequently are unable to describe a settlement system. This inability in turn means that we cannot be sure whether or not some sites or parts of some sites are temporary or seasonal residences (or perhaps not even residences at all) of people who live elsewhere during other portions of an annual cycle. Even worse, as it is very probable that settlement systems were changing rather radically in this region over time, we are often unable to tell whether a substantial change in number of sites from one period to another implies a significant change in population size, or a major change in the settlement system, or both.

Second, there is a very real element of subjectivity in the acquisition of data bearing upon demography. Owing mainly to considerations of time, we have estimated surface sherd density solely on the basis of simple visual appraisal in the field, and have made no effort at an objective, quantitatively derived index. This has proved effective and fairly reliable for our own survey parties, but makes it difficult for others to adequately evaluate or reanalyze our data on occupational density. This is particularly critical as our estimates of relative population have been based directly upon our subjective estimates of sherd density.

Third is the matter of sampling. While we have attempted to walk over 100% of our survey area, the procedures for surface collecting of artifacts
have been less systematic, mainly owing once again to time limitations. Since
the sole purpose of surface collecting at this point is to provide chronological
data to refine our visual impressions, there is very little problem when dealing
with single-component sites. Very substantial difficulties arise however, in the
very common situation where there has been multicomponent occupation. In
some very complex cases we will remain uncertain about site borders until the
advent of much more rigorous surface pickup.

Fourth, and perhaps most serious of all at the present stage, is the gross-
ness of our chronological control. We are still unable to distinguish periods of
less than about 200–300 years. Our settlement pattern for each period is thus
a composite of all occupational activity over a period of six to ten human
generations. It then becomes very difficult, or impossible, to adequately deal
with such crucial problems as site contemporaneity, the budding off of
daughter communities from parent settlements, or the changes in size and
community patterning within single communities and individual households
from generation to generation. A fifth deficiency is our ignorance concerning
paleoenvironment and even the details of modern land use and agricultural
productivity. Except in a few isolated localities, we are unable to delineate
the configuration of different environmental zones at a scale which would be
useful in sampling design. Our 100% coverage partially reflects our inability
to properly stratify the environment for any time period.

These substantial limitations notwithstanding, it is still fair to say that
these regional surveys in the Valley of Mexico and elsewhere have been use-
ful and necessary. This is particularly true if one views them in proper per-
pective as the initial groundwork being carried out so that sampling pro-
grams can be designed in which specific sites are selected for systematic sur-
face pickup, test excavation, and large-scale excavation within the framework
of settlement systems analysis elaborated by Struever (86–88). Our present
results indicate an initial inhabitation by ceramic-using peoples who were
presumably agriculturalists early in the first millennium B.C. A pioneer phase
in the Early and Middle Formative saw a broadly dispersed settlement pat-
tern in which a substantial majority of occupation was clustered at those few
localities where lacustrine resources and well-drained land with high water
table are juxtaposed. The succeeding 500 years through the end of the first
millennium B.C. featured a continuous expansion of sites into previously unoc-
cupied areas, increasing size and density of occupational loci, and an in-
creasing diversification of settlement types (as defined by architectural fea-
tures, surface area, and density of occupational debris). During this same
period there was a marked north-south differentiation with a relatively flores-
cent occupation in the wetter Texcoco Region in contrast to a relatively im-
poverished occupation of the drier Teotihuacan Valley to the north during
the period prior to about 200 B.C. This dichotomy was reversed after about
200 B.C. when population expansion and rapid urbanization in the Teotihuac-
Can Valley led directly into the full florescence of the Classic period after 100
A.D., when Teotihuacan became a major power center with pan-Mesoameri-
can influence. All indications now point to the evolution of a primary state
system here between about 200 B.C. and 100 A.D. This process correlates
directly with uniformly high population density throughout the survey area, expansion of irrigation technology in the Teotihuacan Valley, and intensification of regional symbiosis, although cause and effect relationships between these and other variables remain nebulous. Our surveys make it clear that rapid urbanization at Teotihuacan in the first century A.D. was accompanied by a very marked depression of population in the Texcoco region to the south. This suggests purposeful population concentration by the Teotihuacan state. The highly structured arrangement of Classic rural occupation in the Teotihuacan Valley reinforces the impression of state planning in settlement configuration.

The collapse of the Teotihuacan center by about 700 A.D. correlates with another marked dislocation of occupation in both survey areas as the old urban community shrinks to a fraction of its former size and large blocks of population disperse over areas where Teotihuacan state policy had presumably maintained very limited occupation. A broad empty zone in the central Texcoco Region suggests a frontier situation between new state centers at Tula and Cholula. The final Postclassic florescence, for which we have some vague details from ethnohistoric sources, is expressed on the ground by rapid population expansion and multicentered urbanization after about 1200 A.D. The character of markedly distinct Formative, Classic, and Postclassic settlement systems is becoming increasingly better defined. It remains to improve our descriptions of these systems and to explain the evolutionary processes which transformed them.

SUMMARY AND CONCLUSIONS

The English-language literature indicates two separate traditions of archaeological settlement pattern studies: an American and an English. The American tradition developed out of the remarkable perceptivity of late 19th century anthropologists such as Morgan and Mindeleff, but its full potential did not begin to be realized until the stimulating work of Steward in the 1930s. Following three pioneering regional surveys in the late 1930s and mid-1940s, descriptive and theoretical studies have proliferated since the mid-1950s. Since the time of Morgan, workers in the American tradition have always been much concerned with the sociological implications of settlement pattern data. This has resulted in a very conscious effort at explicating assumptions and conceptualizing the problems and potential of field research strategies and analytical methodology. Considerable work carried out during the 1960s has been expressly directed at settlement pattern analysis in both simple and complex societies, and has included both intensive investigation of individual sites and systematic regional surveys.

There has been no uniform agreement among American archaeologists either upon the proper scope of settlement pattern studies or upon their potential role in prehistory. Nevertheless, the past 15 years have seen a rather impressive achievement from the situation when Prehistoric Settlement Patterns in the New World was published, when useful data for drawing sound sociological inferences was almost absent, to the point where a considerable body
of material can now be drawn upon to help provide at least a tentative reconstruction of key cultural processes in some areas. Perhaps even more significantly, with the introduction of the settlement system concept and its increasing familiarity, archaeologists now have a sound conceptual framework in which to structure settlement data and to provide a basis for fieldwork strategies aimed at providing specific kinds of information to illuminate specific questions and problems. Thanks to Struver, they also have a much better idea of what their past and current research programs have generally failed to do and what they might one day be capable of accomplishing.

The English tradition derived obliquely out of the interests of late 19th century and early 20th century historians and cultural geographers in the ethnic association of ancient field systems and settlement types. The inception of true settlement pattern study in archaeology began with the work of Fox in the early 1930s. Since that time there has been a major concern with drawing inferences from correlations between environmental variables and the distribution of ancient monuments and artifacts. There has always been a close association of settlement pattern analysis with historical source material. Theory and underlying assumptions have remained largely implicit. Fieldwork directed specifically at settlement pattern problems has been limited.

A limited discussion of selected work at three levels of investigation (the individual structure, the settlement, and region) has indicated something of the accomplishments and the shortcomings of archaeological settlement pattern research. While detailed studies of individual structures are few, the literature now contains a respectable number of investigations at the settlement and regional levels. Few have attempted to describe settlement systems or to explain the transformations of settlement systems through time. However, much of what has been done consists of preliminary work essential to the eventual success of settlement systems analysis. At the settlement (community) level the most complete results have been achieved where rigorous sampling and sophisticated quantitative analysis have been used at small nucleated sites of short occupation where the limits of settlement and architectural units can be readily delineated prior to intensive excavation. In such cases there has been some encouraging success in inferring room function and suggesting the locale and composition of residence units. More complex sites have required geometrically greater outlays of time and money to accomplish even initial mapping and sampling. To this point, regional studies have remained largely at the preliminary stage. With a few exceptions, their main concern has been to describe regional settlement configuration, to define problem areas, and to generate hypotheses toward which subsequent research can be directed.

Perhaps the most appropriate note on which to end this review is with an estimation of some of the major needs and key problems which now face settlement pattern archaeology. These may be grouped under two main categories: methodological and conceptual. The former is relatively straightforward and comprises the related problems of sampling, refined chronology, functional interpretation, and paleoenvironmental construction. Sampling has
been inadequate for many settlement pattern studies. It is absolutely crucial
that structures, settlements, and regions be properly described and subdivided
for programs of surface survey, surface pickup, test excavation, and large-
scale excavation. Only then can inferences based on correlations and associa-
tions between variables be made with the confidence that they are representa-
tive. Adequate sampling is dependent on the ability to measure the full range
of regional variation in a number of key variables, including both archaeolog-
ical and environmental features. Among other things this means that the dis-
tribution, availability, and productivity of key resources must be recon-
structed for various periods in the past, with the present situation often offer-
ing a baseline from which to project backward. Very seldom is there ade-
quate information concerning even modern conditions.

The limitations of our present ability to specify the chronological place-
ment of archaeological features are obvious. Only through a sound demon-
stration of contemporaneity between objects, rooms, houses, and communi-
ties can a settlement system even be adequately described. Only through a
separation of generations can many key demographic processes be properly
analyzed. The ability to infer the function of artifacts and artifact classes of
all kinds (e.g. tools, structures, sites) is perhaps the most fundamental prob-
lem within settlement pattern archaeology. Upon this rests the whole utility
of the settlement system concept. Recent advances in the methodology of
functional inference are among the most significant contributions of the past
decade.

The conceptual requirements of settlement pattern archaeology stem
from the general failure of anthropologists to develop adequate models from
historical and ethnographic data which can be used to help structure the
known archaeological record, to help formulate new questions and new prob-
lems, and to help design new research programs aimed at providing some
new insights into these questions and problems. Willey (97) and Vogt (93)
long ago stressed that settlement pattern study in prehistory necessitated close
relationships between archaeologists and ethnologists. Despite some very sub-
stantial contributions, this common meeting ground has been generally
avoided since Chang's (16, 17) initial efforts. Such recent work as Skinner's
(79, 80) study of market hierarchies and settlement distribution in tradi-
tional China, and Chagnon's (15) delineation of relationship between Yano-
mamo warfare patterns and settlement size and distribution are examples of
the kind of studies which archaeologists need to incorporate more systematic-
ally into their settlement pattern research. Campbell's (13) ethnographic
reconstruction of a traditional Eskimo settlement system proved highly illu-
minating in detecting lacunae in the archaeological record of settlement loca-
tion in the same area. This study has the great advantage of dealing with a
simple system so close in time to the present that living persons are useful as
informants. Most other situations will pose far greater problems, but Camp-
bell's work provides a good illustration of one kind of integration of ethno-
graphic and archaeological data which offers great promise in settlement pat-
tern archaeology.
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DEMOGRAPHIC STUDIES IN ANTHROPOLOGY

PAUL T. BAKER
AND
WILLIAM T. SANDERS
Department of Anthropology
The Pennsylvania State University
University Park, Pennsylvania

INTRODUCTION

Demography and Biological Anthropology

A common definition of the field of biological anthropology is that it concerns the description, causes, and consequences of variability in human biological characteristics. As such it is somewhat surprising that so little attention has been devoted to such an important biological parameter as human demography.

In the early development of anthropology there was a brief interest in demography as attempts were made to discover whether some men had found a way to increase longevity. These myths persist with reports of great age in the USSR and the South American Andean region. Anthropologists were also curious about the longevity of our ancestors so that both historical and skeletal studies of mortality were in vogue. At the opposite end of the life cycle considerable effort was expended on the study of sex ratios at birth and the nature of seasonality in birth. Despite these early interests the field of demography was not pursued with any depth until recently.

At the moment interest in demographic data is very much on the rise and shows no sign of being near its peak. The reasons for the increased interest can be catalogued into three discrete developments of the 1960s. First, the development of extensive research programs on the nonhuman primate in his natural environment has produced a new body of descriptive data. Second, the development of human population genetics from the general synthetic theory of evolution has indicated the need for new types of demographic data on nonwestern populations. Finally, the strong interest in a subject area best called human ecology has suggested new ways in which we can understand the immediate causes and effects of population differences in demographic characteristics. In this paper we will attempt to highlight the current status of research in these topics even though much of the subject matter has been

151
generated by individuals who would not consider themselves as anthropologists even of the biological stripe.

**Demography and Cultural Anthropology**

Although the literature in cultural anthropology has always included demographic data, cultural anthropologists as a whole have shown a peculiar disinterest in demographic studies. This situation, although improved greatly over the past 10 years, still leaves much to be desired, and one cannot yet speak of a focus of research on demography in cultural anthropology comparable, say, to that in sociology.

Basically, studies in human populations include two kinds of data: (a) population size, density, and geographic distribution; and (b) the demographic structure of a given population, that is, the breakdown by the biological (sex, age) or social categories (social class, occupation, rural versus urban, kin group membership, etc). In either case the approach may be diachronic, in which case data on mortality, natality, and migration are of major interest, or it may be synchronic. Ethnographic monographs generally are rather barren when it comes to demographic data of any kind, and much of the recent stimuli to the recording of demographic data and study of demographic processes have come from outside the field, particularly from sociology, geography, and economics. The major exception to this statement is the theoretical line of anthropology that can be included under the rubric of multilinear evolution and cultural ecology where there has been a strong emphasis on demography.

In reviewing the past and recent past literature on demography in cultural anthropology, the studies seem to group into the following types: 1. Theoretical papers on the functional relationships between population size, density, and geographic distribution on the one hand and the evolution of stages of political and social evolution on the other. Some of these studies are addressed to broad schemes of relationships such as that between population size and pressure and the evolution of states. Others have a much more specific purpose, for example, studies of the relationship between population density and land tenure. 2. Descriptive attempts to reconstruct the size, density, and distribution of native populations at the moment of European contact, and, in the case of archaeologically based studies, at some point in the past. In the case of the latter, the studies have often focused upon changes in demographic characteristics. 3. Descriptive studies of the demographic structure of a selected population. Although most of these studies have involved contemporary populations, archaeologists have also attempted to analyze ancient populations as well, using skeletons from cemeteries as their sample. These latter studies have been limited primarily to identification of sex, age, and cause of death. Studies of this type are rare, and coverage, in terms of geographic distribution and cultural typologies, are completely inadequate.
DEVELOPMENTS IN DESCRIPTIVE DEMOGRAPHY

NONHUMAN PRIMATES

In 1955 it was probably no exaggeration to state that the vast majority of anthropologists would have accepted the following propositions: (a) primate population sizes are controlled by food resources; (b) the social organization of primate groups (excluding man) is set by their species specific genes; (c) the development of continuous sexual receptivity in the female primate formed the basis for human social organization. These propositions were based on a continued acceptance of Malthusian precepts and the observations of primates in captive conditions.

The detailed field studies which have been undertaken since the 1950s show that these propositions were at best vast oversimplifications. Of course, primates, even in the nonhuman categories, cannot be spoken of as unified categories. The demographic and social characteristics of groups vary enormously. Lemurs are so much different from apes that the chimpanzee is much more like a man than he is like a lemur. This means that almost any generalization probably has a very limited application.

The relationship between food supply and population size is a particularly baffling problem. It seems obvious that the food supply should be the limiting factor and yet none of the field studies shows a direct link. Indeed, primatologists have failed to find a single instance of a nonhuman primate population in its natural habitat which was suffering from food shortage. Perhaps more detailed and lengthy studies are required, or it may be that the link between food supply and population control is always indirect. Some evidence of indirect links between food supply and population size is provided by the studies of aggression. In baboons (36, 67), African vervet (47), and Indian langur groups (127, 155) aggression has been found more pronounced in groups living in food-poor environments than in food-rich environments. Since a high level of aggression in many primate species causes a significant increase in mortality, the link between food, aggression, and population size seems possible. This simple chain is, however, made less credible by evidence from studies of the Indian rhesus which demonstrate a decline in aggression during real food deprivation (128). A possibly stronger explanation is that social spacing mechanisms of all forms led to primate populations which generally stayed well below the food-carrying capacity of their ecological niche. Unless future field work changes our knowledge, even this explanation seems inadequate for many primate species. For example, the mountain gorilla shows low levels of aggression, does not demonstrate strong territoriality in any form, and never seems to suffer predation or food shortage (120). Obviously, the relationship between food supply and the population sizes of primate groups is not going to be amenable to any simple explanation. As we will discuss later, man is almost certainly no exception to this generalization.
As has been stated, all of the simplified propositions made about the primates in the 1950s now seem totally inadequate. Thus, we might treat together both the idea that social organization was genetically determined and the idea that the evolutionary changes which led to man’s social structure were determined by changes in sexual behavior.

In the early 1930s Zuckerman (157) compiled the information then available on the behavior of nonhuman primates. He was impressed by the apparently enormous amount of time spent by nonhuman primates in sexual activity and by the long period of child dependency found in many groups. From these observations he speculated that a sequence in primate evolution might involve first, a bond of offspring to mother due to long dependence, and second, a bonding of male to female caused by the development of continuous sexual receptivity on the part of the female. Viewing what was then known about hunters, he suggested that the earliest human social unit was the family derived from prolonged child dependency and sexual bonding.

To some extent his findings have been supported. When lemurs are contrasted with apes the prolonged dependency of the ape offspring is obvious. Seasonality and the specificity of mating for reproduction has also been modified in the so-called higher primates. Even so, the relationship is not simple, and among the various monkey species mating behavior ranges from sharp seasonality to almost no seasonality at all (63, 145). Some species such as baboons mate very frequently, while red-tailed monkeys apparently mate very infrequently. Even closely related primates such as the chimpanzee and the gorilla show an enormous difference in sexual behavior. With close observation in the field very little sexual activity was seen among gorillas, while chimpanzees, even in their natural environment, show considerable sexual activity (142). In spite of this difference, both groups mature between 8 and 11 years of age, both produce offspring with about 3 year spacing, and both have a similar life span, to more than 30 years. In defense of Zuckerman’s hypothesis, it can be noted that neither chimps nor gorillas are as monogamous as man. But this means little since seasonally mating primates maintain their social cohesiveness as well if not better than primates with continuous sexual bonding.

Reproductive behavior probably offers as good an example as any to refute the old promise that social organization is genetically set in nonhuman primates. Thus, the marked seasonality in the reproductive behavior of the rhesus in his native habitat almost completely disappears when he is kept in indoor captivity. Indeed almost all higher primates increase their sexual activity in captivity (145).

While studies of primates in their natural environments have provided descriptive information which refutes many of the previous generalizations, they have not yet provided sufficient information to develop new demographic generalizations. Anyone who has attempted to collect demographic information in nonliterate human societies will appreciate the difficulties of data collection on long-lived animals.
From the limited data available to us, one interesting observation is the apparent similarity between the basic demographic features of chimpanzees, gorillas, and man the hunter. All three groups have females with similar reproductive spans (about 20 to 25 years), all average about 3 years between single offspring, and curiously, all 3 may have similar mortality curves. Humans, of course, live somewhat longer than the apes, but this is primarily accounted for by a longer period before reproductive maturity. Starting with the age of sexual maturity, at least one chimpanzee has been found to have a reproductive span of 33 years. Just how long they can live is not known, but even 33 years added to the age of sexual maturity would account for 99% of the population among a human hunting and gathering group.

The mortality curve among human hunters will be discussed in greater detail later, but it can be suggested that both they and the great apes may have a rather linear mortality from birth onward, which contrasts sharply with the various curves we are accustomed to finding in both peasant and industrial populations.

**Living Human Populations**

A purely descriptive demography of living human populations has never been popular among cultural anthropologists, and the same can be said for the biological branch of the field. Nevertheless, in recent years anthropologists and geneticists have published some descriptive demography on groups of hunters (35), isolated tribal and peasant groups (29, 61, 66, 95, 99, 107), and island populations (74, 102, 138). These supplement a slightly more comprehensive literature on peasant groups which has been published by demographers and economists interested in the impact of technological change on these groups. The collection of nonpeasant demographies seems in no case to have been stimulated by an interest in demography alone. Instead the purposes were almost always the application of the data to specific evolutionary or social problems.

While an interest in evolutionary and social problems has spawned a new interest in the descriptive demography of nonwestern populations, it has also stimulated the development of sophisticated populations models (76). For some purposes the creation of simulated populations provides a better test of a hypothesis than real groups. In a sense, simulated populations are not a new concept since the creation of population models dates back in its simplified form to the founders of population genetics and demography.

The new aspect arose when computers with sufficient speed and capacity were developed so that models with multiple population parameters could be simulated. The uses of simulated populations have included investigations of mating structures, consanguinity, fertility, population growth, vital rates, selection, gene flow, and genetic drift. Since MacCluer's summary of simulation applications to anthropology and human genetics contains over 100 references, there seems little utility in repeating these references, but in relation only to the demographic description of human populations it should be noted
that the techniques provide a powerful tool for improving the demographic data on small populations (41, 77, 78).

Although the number of demographic studies of small groups has increased significantly over the past 10 years, the literature is still rather scanty. In Lee & DeVore’s book, *Man the Hunter* (71), which summarizes the recent developments in studies of hunting and gathering groups, for example, only 4 papers of the 35 papers presented utilized demographic data, although some demographic material is included in a number of other papers. It is clear from the papers and the succeeding discussions that few explicitly demographic studies have been conducted among hunting and gathering societies.

Three of the papers—by Yengoyan (154), Meggitt (79), and Birdsell (10)—deal primarily with population size and density of local groups and dialectic tribes in Australia, and the relationship of these two factors to marriage patterns and resource utilization. Birdsell’s paper also focuses on population controls found among hunters and gatherers and how they operate to produce a relatively narrow range of family, band, and tribe size. Although the Australian data is really ethnohistoric rather than contemporary and hence suffers from problems of accuracy of reporting, the data does suggest a coherent relationship between rainfall, abundance of food, local group size, and the size and density of tribes.

The density of population relates closely to rainfall, and the latter to richness of food resources. The local group size remains relatively constant as does its relationship to territory. What is highly fluid (and this is a fundamental area of disagreement between Birdsell on the one hand and Meggitt and Yengoyan on the other), however, is individual or family band residence. Apparently there are considerable shifts in residence on the part of such individuals from year to year, and band membership therefore is not constant.

As the rainfall and density fall to lower values, the number of marriage classes rises. The ecological value of this process seems obvious. It forces a local band to seek mates from a wider geographic orbit, thus providing a means of economic security by spreading its kin relationships over as large a territory as feasible. The maximum number of marriage classes therefore occurs in the desert areas, whereas simple moiety systems occur in the richest portions of the continent. An interesting second effect of this process is that the tribal size varies inversely in relationship to population density, and the larger tribes are found in the drier areas and the smaller ones in the richest environments. Since tribes are not organized political entities, but in fact are the product of social interaction (primarily through the marriage relationship), this is a predictable pattern since local groups would have to range more widely in search of mates in drier areas and therefore spread their social contacts among a greater number of people and over a greater geographic space. Apparently the process works in such a way that the decrease in density in the drier areas is compensated for by a total increase in the number of bands with whom an individual interacts, thus creating a larger
tribe size. The range is considerable (100–2100) and this is where Birdsell would disagree with the other two writers.

The paper by Dunn (39) is primarily a survey of the general health, disease, nutrition, and cause of mortality generally among hunting and gathering groups, based on data from the South African bushmen, the Australian aborigine, the Eskimo, the African pygmy, and the Semang. The general picture that emerges from these studies is that we have grossly underrated the ability of hunting and gathering groups to obtain food from their landscape. It seems that starvation or severe food shortages are extremely rare among such groups. Where they do occur, they are usually among those groups that depend primarily upon hunting rather than gathering, such as the arctic and subarctic groups. With respect to nutritional balance, most hunting and gathering groups apparently have a better diet than most farming peoples.

Causes of mortality that vary considerably, according to the particular hunting and gathering group, are infectious and epidemic diseases and hunting accidents. Generally speaking, the mortality among hunters and gatherers who live in nontropical environments from epidemic or infectious diseases is rather low, and hunting accidents of course become a major factor only amongst those groups that depend primarily upon hunting as a source of food as opposed to gathering. Tropical hunters and gatherers, on the other hand, suffer from infectious types of diseases about as frequently as do farmers in those same areas.

There is an unfortunate tendency in some of this recent literature perhaps to underrated the insecurities of life among hunters and gatherers. We make this point because the age and sex distribution of hunting populations is not strikingly different from a typical peasant profile. Furthermore, if the hunting and gathering life way is so secure, then one is hard pressed to explain the emergence of food-producing economies. At any rate, the major causes of mortality among hunters and gatherers seem to be social, particularly the practice of infanticide.

Reconstruction of Prehistoric and Ethnohistoric Populations

A considerable literature exists on the size and distribution of the aboriginal population of the Americas at the time of European contact, much of it dating back to the very beginning of American anthropology. The first major synthesis was Kroeber's monumental Cultural and Natural Areas of Native North America published in 1947 (64). Interestingly, it was part of Kroeber's only substantial foray into what we might call cultural ecology. Most of Kroeber's North American (north of Mexico) data was derived from an earlier study by Mooney (1928). Kroeber also made an effort to estimate the population of Mesoamerica based on "nothing more than my personal opinion as based on impressions, somewhat molded by comparisons with the population size and density north of the Río Grande."

Following Kroeber, Cook, with Borah and Simpson, none of whom are anthropologists (12, 25, 26, 28), published a great number of papers and
monographs on the population of Mesoamerica. For South America, Rowe in 1947 (104, for the Inca empire) and Steward & Faron in 1959 (132) estimated the population for that continent. More recently, Borah has been surveying the central Andean data, and C. T. Smith (124) has published a study of the population of the same area. Other recent and older attempts to reconstruct aboriginal American populations include Sauer on northern Mexico (115, 116) and the Caribbean (117) and Cook & Borah (27) on the latter area.

The most explicit statement of methodology is found in the work of Cook, Borah, and Simpson in connection with their Mesoamerican studies. During the period from 1565–1580 the Spanish government and church conducted detailed censuses, many of which can be cross-checked, and the authors used this tax data to reconstruct the population during that period. These censuses provide a reliable base for estimating minimal population size at the time of Spanish contact since all evidence indicates that the population was considerably larger at that time and was subsequently reduced by epidemics. The major problem then is estimating the magnitude of population loss. To establish this the authors have used estimates of the population of towns and sizes of Indian armies in 1519–1521 by the Spanish conquerors and a few presumed reliable post-Conquest references to population during the 1530s and 1540s. On the basis of this they have plotted curves of decline.

A major problem in using this earlier data is the strong probability that it was manipulated for political and economic reasons. Another problem is that the 1530 and 1540 censuses exclude the tax-exempt portion of the population and this class was of considerable size in some areas. If we exclude completely this earlier data and simply utilize the tax figures for the late 16th century as a minimal population for 1519, there is still the problem that some areas of Mesoamerica suffered a catastrophic decline in population and the 1565–1580 base line is therefore useless. In fact the evidence shows a striking correlation in Mexico between the degree of population loss and elevation, with the heavier losses occurring at the lower elevations. The tropical lowlands probably suffered a 90% population decline between 1519 and 1580, for example, whereas the area above 2000 m suffered a loss of perhaps only 60%. All of the area was exposed to such epidemics as measles, smallpox, chickenpox, and whooping cough, but the lowland populations suffered the added burden of malaria and yellow fever.

Dobyns (37) has recently utilized the Cook-Borah calculations of curves of population decline for central Mexico and applied them to the population of the New World as a whole. As a result, his estimates of population for the hemisphere are approximately eight times those of Kroeber and Steward & Faron. In a recent paper Sanders (111) has criticized the Cook and Borah methodology as applied to the Central Mexican data. This criticism was primarily leveled at the pre-1560 data, although he agrees with the authors in their assertion that the contact period population was considerably larger than Kroeber's estimate.
DEMographic STUDIES IN ANTHropology

It is also a very dubious proposition that all areas of the New World would have suffered the same rates of decline considering variations in the contact period population density and degree and nature of contact with Europeans.

The other major area of the world where considerable effort has been devoted to the reconstruction of native population size is Polynesia. The literature unfortunately is extremely fragmentary and consists of a large number of attempts to reconstruct the population size and density for particular islands. There have been no researchers in Polynesia comparable to Cook and Borah with their highly structured and sustained effort. In 1958, Sahlin published his *Social Stratification in Polynesia* (105), in which the extensive demographic literature was collated and played a major role in his theoretical reconstruction of Polynesian chiefdoms.

The archaeological literature on demography is enormous, and much of it has been summarized by Cook (24) in his recently published *Prehistoric Demography*. Only a brief summary will be presented here. Interest in demographic data in archaeology spans the history of archaeology. The older archaeological literature was focused primarily on the individual site or local community as a unit of study. It included intensive analyses of skeletal remains from cemeteries to obtain data on age, sex, and mortality on the one hand, and estimates of the size and changes in size of the population of the community on the other hand. Other methods of estimating the latter were excavations and counts of houses or rooms and calculations based on rates of refuse accumulation. These interests are still being followed today, and the methodology differs from the older literature only in the use of more sophisticated chronological controls and statistical techniques of analysis.

Efforts to reconstruct the demographic structure of a population by a detailed analysis of the skeletal remains is not new (65), and some of the classic efforts date back to the 1920s (135). Essentially the same problems slow this effort today as in the past. These problems are threefold. First, there are very few areas of the world where skeletal remains are preserved in such a form and quantity as to assure proper population sampling. Second, the amount of expense and time required to obtain and process a sufficient sample is very great. Third, the process of determining the age and sex of individual skeletons remains an inexact art (3).

There has been only slight progress in alleviating the first two problems, which explains in part the slow growth of information on the topic. The best hope is that as the methods for aging and sexing skeletal material improve, more fragmented samples may become useful, and that simplified techniques for these determinations can be developed so that processing time can be significantly reduced. There has been progress in the development of more accurate sex and age criteria but not at a level likely to speed processing time. A number of quantitative methods have been developed for determining the sex of an adult skeleton (49, 94, 139). These reduce the training required for individuals making skeletal sex determinations but does not seem to increase
accuracy greatly. New techniques were also developed for aging adult skeletons, and these have improved the estimates in the 20 to 50-year age span (62, 75). Of considerable importance to future efforts are attempts to determine the number of children born to a woman from the condition of pelvic joints (133) and new efforts to identify specific disease pathologies from skeletal traits (5, 15).

The recently published demographic studies based on skeletal remains do not contain any particular surprises, but Angel’s long-term study of the mesolithic to historic transition in the Mediterranean basin reaffirms the feeling that the fertility, sex ratio, and longevity of a population is related more to the group’s particular culture and ecology than its stage of cultural evolution as such (4).

The major new development has been the derivation of demographic information for larger geographic units. Major objectives have been to estimate population size, density, and distribution through the various time phases. The methodology for this development was designed by Gordon Willey in his Viru Valley study in central Peru (147). The strategy includes large-scale surveys in which sites are mapped, located, classified into types, and estimates made of the extent and the density of occupational remains for each site. In cases where surface architecture is abundant this can be ascertained by room counts or house counts; where such remains are buried, subjective impressions of refuse density are used. Chronological control is provided by surface sampling. Surveys are frequently combined with excavation of selected structures to provide control on structure function. A major problem has been the conversion of room or house counts or estimates of density of such structures to population figures. The usual technique has been to use an assumed average family size and multiply this figure by the number of houses (or number of rooms assumed to represent the residence of a family, in cases of conjoined room settlements). In a few areas where obvious continuities exist between the settlement types of contemporary populations and the prehistoric, the average family size or average settlement density has been derived from these sources. Naroll (88) assessed a sample of ethnographic cases of egalitarian, agricultural societies and demonstrated a relatively narrow range of variation in the relationship between family size and roofed-over spaces. Several archaeologists have applied this formula. What is needed are formulae that can be applied to hunting and gathering and agriculturally based, class structured societies to broaden the applicability of this approach.

In the past 10 years, this zonal settlement pattern approach has been increasingly used in New World archaeology as a whole and in the Old World in parts of Western Europe and the Middle East. Specialists in Mesoamerican archaeology have been particularly active in this respect. In the Maya lowlands these studies include Willey et al in the Belize Valley (148), Sanders in Quintana Roo (109), the University of Pennsylvania Tikal Project under the direction of William Coe (22), and Andrews at Dzibilchaltun (2). In the central Mexican area Sanders’ Teotihuacan Valley Project (110), several
projects by Parsons in the Basin of Mexico (90–92), Snow's work in Tlaxcala (126), and Tschol (141) in the Puebla Plain are examples of this type of research. Spores (129) has applied this type of survey to the Nochixtlan Valley, and Bernal (7) and Flannery et al (44) have surveyed the Valley of Oaxaca in the state of Oaxaca. Other surveys of this type have been conducted by Sanders & Michels (112) in the Valley of Guatemala, Michael Coe in the south Gulf coast (unpublished); the New World Archaeological Foundation, under the direction of Gareth Lowe, in the central valley of Chiapas (73), and Michael Coe, Kent Flannery (21), and Edwin Shook (123) on the Pacific coast of Guatemala.

Most of the studies have included at least some consideration of relative and absolute distributions and changes in population, and several of them have focused heavily on such data. Mesoamerican specialists have also been active in applying the same methodology to large single sites such as Millon's work at Teotihuacan (81, 82), Blanton at Monte Alban (unpublished), Litvak-King at Xochicalco (unpublished), Gorenstein at Tepepi el Viejo (unpublished), Sanders & Michels (112) at Kaminaljuyu, Willey & Smith at the lowland Maya sites of Altar de Sacrificio (149), and Seibal (unpublished) and Pollock et al (96) at Mayapan.

Curiously, the area where this methodology was initiated, the central Andes, has seen little subsequent research of this type. Examples are Lumbreras (unpublished) and later MacNeish's studies (unpublished) in the Ayachuchi Valley, surveys by Lanning and Patterson (68, 93) in several central coast valleys (Chillon, Lurin Valley), Thompson's survey of the Casma Valley (140), Proulx (97) in the Nepeña Valley, Schaedel (118, 119), and more recently, Moseley (84), on the north coast.

West (146) and Moseley (unpublished) have also applied this methodology in an intensive survey of a single large site, Chan Chan and Lumbreras (unpublished), to the site of Wari.

The Southwest is another New World area where the research strategy of zonal settlement pattern surveys have been extensively carried out, and these studies have generally included attempts to reconstruct the demography. An enormous amount of research has been published on the Southwest, including a great number of what we are referring to as zonal settlement surveys. In fact, one could argue that the approach was developed first in this area since Colton's surveys around the Flagstaff area in Arizona were published in 1949 (23). The more important recent ones in terms of demographic analysis have been summarized by Cook in his paper on prehistoric demography (24). Included are Danson's (33) paper in which he summarizes the demographic histories of three subareas within two areas (the upper Little Colorado Basin and the Tularosa-Apache Creek); Bluhm's study (11) of the Pine Lawn Valley in New Mexico; Longacre's (72) study of the upper Little Colorado River Basin; and Zubrow's (156) study of the Hay Hollow Valley in eastern Arizona.

In the Middle East comparable surveys that include attempts to recon-
struct population history include those of Robert Adams (1) and Henry Wright (151–153) in the Mesopotamian heartland, Smith & Young (125) and Hole, Flannery & Neely (57) in upland valleys in Iran.

Most zonal settlement pattern studies have had two major objectives, the definition of ecological processes and the reconstruction of changes in institutions through time. They vary in the way in which the researcher views the interaction of these two processes. The derived demographic data has been used to measure the success of the population in adapting to their environment and as a measure of social evolution. The implication in this latter approach is that there is a functional relationship between the sizes of communities and organized societies and social structure and hence represent an attempt to apply the evolutionary models of Steward and his successors (see discussion below). Most of the researchers have furthermore been governed by the principle that increasingly successful ecological adaptation is a factor in social evolution while recognizing the great importance of feedback relationships. A primary assumption in these studies, essentially derived from Bos-erup's thesis (see discussion below), is that unregulated or only partially successful regulation of population growth has been the normal human situation and that such growth has been a major factor in producing changes in the subsistence strategy and institutional structure of human population.

With respect to results of a less theoretical nature, the demographic profiles for the various local areas are highly variable in rates of growth and occurrences of peaks, depressions, and stabilization phases of population as compared to each other. The factors responsible for these variations are obviously complex and include local and extra-local ecological and political events and processes.

Many published graphs of the population history of agricultural peoples, however, are amazingly similar in the appearance of the curve of population history. Typically, such curves are relatively flat during the early phase of the history of colonization but rise very steeply in the later phases. A common rate of growth in the colonization phase is an approximate doubling of population per century. A sustained growth rate of this type will apparently produce a curve of the type noted, and this may be considered as a normal curve of human population growth during colonization of new areas. This is far below Birdsell's (8) theoretical optimum fertility potential of humans in which he postulates a doubling per generation. At any rate, the steepness of the later phases of growth is apparently a function of the size of the base population, and this pattern suggests a possible explanation for a problem that has plagued archaeologists for some time—the rapid evolution of complex political systems. Typically, the profile of political evolution reflected in settlement pattern surveys is one of a relatively long initial period of stable conditions followed by a rapid development with evidence of a very rapid shift to rank and stratified societies and centralized political systems within a total period of only a century or two.

If we agree that a major stimulus in the evolution of large-scale political
systems is population growth, then the explanation for their evolution within such a short period may lie in the nature of population growth. For societies organized on a tribal level, a tribal size of up to 5000–6000 people is conceivable, but a doubling of population to 10,000–12,000 would require a chiefdom type organization in order to maintain it as a stable society. This means that chiefdoms would evolve from tribes within a single century. Although chiefdoms of 24,000 (doubling of the 12,000) can exist, their stability is extremely low, and when the size doubles to 48,000 they can usually exist only for a generation or two. This means that the state level of political organization can evolve within a period of only over two centuries from the chiefdom level, and the process of political evolution from a tribe to a state could occur during a period of two or three centuries. Considering the fact that many archaeological chronologies involve blocks of time of this length, it is not surprising that the archaeologist is perplexed by the sudden shift from a relatively simple to a highly complex political system.

A major theoretical point here is the question of why in some cases population increase is followed by political fragmentation and stabilization at lower levels of political organization, whereas in other cases the changes in structure occur which then enable this large society to function.

CULTURAL EVOLUTION AND DEMOGRAPHY

With respect to the interaction between demography and cultural evolution, the major stimulus has come from two sources: Julian Steward, the anthropologist, and Ester Boserup (13), the economist. In Steward’s case, his interest in demography was closely interwoven with his pioneering efforts in cultural ecology, in which population size and density were seen as measures of the adaptive success of a given population to its food resources, and secondly, as closely related to levels of sociopolitical integration. Succeeding anthropologists have generally conceived the problems in those terms. This orientation is clearly evident even in Steward’s early studies of the Basin Plateau area, becoming fully developed during the period of his editorial work on the Handbook of South American Indians.

Recent writers who have addressed themselves to the broad processes of cultural evolution include Carneiro (16, 17), Dumond (38), Service (122), Stevenson (134), and Harner (52), using primarily ethnographic data, and Sanders & Price (113), using archaeological data. Basically the theoretical argument is as follows. As a society increases in size, new means of integration must be achieved, since kin-based institutions that function well to regulate disputes or organize exploitation of resources cannot do so when the number of people reaches certain levels. As the society gets bigger, the major changes are from informal to formal legal sanctions, dispute arbitration, and leadership statuses; from relatively self-sufficient economies to those of increasing symbiosis through specialization and trade; and from relatively equal access to basic goods to marked inequities. The three processes are closely interrelated in that more formal political organization is needed as the society
becomes more economically heterogeneous and inequalities of wealth provide the basis of political power. Carneiro has attempted to apply a rigorous statistical method to show the relationship between community population size and these processes, although even he does not suggest absolute values. All of these writers furthermore recognize that per capita productivity, in the Leslie White sense, must be considered in any rigorous application of the principle since an increase in per capita efficiency may act like an increase in the numbers of people. The inadequacy of White’s original formulation of unilineal evolution was specifically in this area since he grouped all preindustrial agriculturally based cultures together as a single stage of energy utilization and yet the ethnographic sample would include groups with an enormous range in levels of political integration. Of course, part of this variation may in fact be related to variation in per capita efficiency of production among agriculturalists, but total numbers of people also appear to be a critical factor in this formulation.

Most anthropologists would accept the previously stated principle that a combination of increase of societal size and increase in per capita efficiency are two major processes stimulating changes in social structure. Where debate has been sharper is over the question of the direct relationship between changes in levels of population density and the indirect and direct effects of these changes on the size of the society. The essential question is why do large societies form. One explanation is that increases in population density produce pressures on basic resources, resulting in competition and conflict. The ultimate results of these stimuli are overall political integration, more authoritarian power systems, and economic specialization. The aboriginal Californian example also suggests that food resources, in terms of quantity and seasonality, must be controlled; that is, one must have food production rather than food collection as the basic subsistence strategy. There are a few notable exceptions, of course, such as the Northwest Coast Indians with their organization into small chiefdoms, but even they remained extremely small in size and showed little capacity for expansion in size and complexity of organization. Often added to this model are the influences of local environmental variation which lead to uneven population distribution. The argument here is that areas of greater population potential will be more densely settled than areas of less potential, and the former will emerge as the centers of the large emergent society.

Most writers have seen the evolution of large societies as the product of conquest and have seen their emergence as essentially a coercive process. Netting (89), in a paper to be published this year on the emergence of chiefdoms in Africa, argues that the process need not necessarily be one of conquest. What can happen is that a rise in population density results in a high frequency of contact between communities, raising the possibilities of conflicts, not only over basic resources such as land but over a great variety of situations. Groups caught in this situation may voluntarily surrender some of their autonomy to resolve these frequent conflicts. In the African case, the
authority that is deferred to is one already in existence, the shaman-priest. His new role as judicial arbiter, through the medium of gifts from the litigants and their use in redistribution, provides him with a new basis of power, economic, combined with his older sacred power. There is a real question here, however, as to whether the higher levels of political integration of the type that we call the state could emerge under this kind of a process.

In looking over the model it is clear that it is not population density per se that is producing the changes but competition over resources and density is only a crude measure of such stresses. The problem is the measurement of ecological pressures with more definitive criteria than crude population density figures since the relationship between absolute density population numbers and resource pressure will vary from environment to environment. Harner (52) suggests using the percentage of the food supply that derives from hunting and gathering as contrasted to agriculture (in agriculturally based societies) as a measure, since hunting and gathering resources would clearly decline as more and more land is brought into agricultural production.

One other element of major significance has been added to the theoretical model by Carneiro (17), the distinction between a circumscribed and an open environment. Although the use of these terms suggests a dual typology of environments, in fact what exists is a continuum between two poles. This concept is very closely related to Boserup's (13) theory that extensive cultivation is less labor-demanding in relationship to production than is intensive cultivation in most environments. This means that people tend to colonize new areas before they intensify the use of the land already in cultivation. The result is that for a period of time population density does not increase but the population simply spreads out and occupies larger areas. If geographic barriers such as large bodies of water, high mountain ranges, or sudden shifts in environment (such as those found in tropical mountain regions) divide large areas into a great number of smaller, relatively isolated zones, then intensification of agriculture and hence a rise in population density will occur in a much shorter period of time than in large regions of relatively homogeneous agricultural potential. This point is critical since it helps to explain why political evolution occurs at a faster rate in some areas of the world as opposed to others.

Boserup's thesis is undoubtedly applicable as a broad principle, but for anthropological purposes it should be somewhat restated, or rather, reduced to her basic premise, i.e. that the per capita return of yield measured against labor input is the major determinant of economic behavior. This has been frequently referred to as the "law of minimal effort." It seems generally true that cultivators with steel or stone tools in a forested area would find extensive cultivation more productive than intensive, particularly if intensification simply means a reduction of the fallow cycle and an increase in the period of cultivation. If major innovations of techniques are involved, however, then intensive agriculture may, even in terms of per capita return, be actually
more productive. We here refer specifically to the set of techniques usually referred to as wet rice agriculture where, under certain soil conditions, it may actually provide a higher per capita return than extensive cultivation in the same area, even considering the extra labor input.

Furthermore, with neolithic tools the extra labor involved in clearing may in fact reduce the per capita return to such a degree that intensification may occur relatively early in the colonization process, in which case the colonization process of neolithic farmers might be strikingly different from that described by Boserup. In arid environments the level of crop security and the yield under extensive practices is probably so low when compared to intensive systems of farming such as irrigation that the added input of labor is more than compensated for by increased yield and security in production. Intensification under these conditions would seem to be a much more rapid process—possibly an initial one in the colonization of such areas.

If we use the principle of minimal effort rather than a simple extensive versus intensive agricultural principle, then we probably do have a powerful tool for predicting the behavior of human populations. The key to success in this endeavor is clearly the accumulation of more data on energy flow systems for groups with different technological levels living in different environments and with different patterns of land use. Vayda (143) has recently suggested that clearing primary forest, even with iron tools, is so much more laborious than clearing long-fallow secondary bush so that warfare may occur over the possession of such secondary bush. This of course would again slow up the process of colonization and possibly act as a population control.

Some of these studies, as we noted, have a much more limited objective. Goodenough’s (51) paper on the relationship between land tenure—population pressure—residence and the social structure of Malayo-Polynesian communities is a case in point, and Rappaport’s (98) study of fusion and fission of agnicline groups among the Tsembaga in the New Guinea highlands are examples. Summarizing very briefly what these studies show is a strong functional relationship between unilineal descent, unilocal residence and unilineally based social structure with low population density and land pressure. As the population pressure rises and land shortages occur, such groups tend to break down, and land inheritance and residence tends to be based on both consanguineal and affinal relationships. The result of these processes is a shift away from patrilineal or matrilineal systems of descent to those of the bilateral type.

Of great interest in all of the above formulations is the question of the effectiveness or ineffectiveness of conscious population control mechanisms among human populations, that is, to what degree have human populations successfully stabilized their population size and limited their growth, and finally, what are the factors that have stimulated them to do so. There is virtually no literature that explains this process and yet if population growth is conceived of as a major stimulus to cultural change, then it is obviously of critical importance to know why populations stabilize at various levels.

Recent studies of the demography of the Bushmen by Lee & DeVore
(71) have pointed out the complexity of the problem. Most evolutionists who have used the demographic argument have thought of populations as being controlled primarily by the abundance of the food supply. In the case of the Bushmen, however, they control their population size (primarily by infanticide) not in relationship to the abundance of food but because of the problems of mobility in their annual seasonal movements. A great number of recent writers have designed ideal subsistence models for hunting and gathering groups based on a careful assessment of food resources. Even allowing for seasonal shortages and for year-to-year variations in the food supply, all of these studies indicate that hunters and gatherers stabilize their population density far below the theoretical maximum permitted by the food supply, a process that in still only imperfectly understood.

DEMOGRAPHY, POPULATION GENETICS, AND EVOLUTION

While the descriptive study of demography was kept very much alive in the past decade, the great expansion of demographic interest is quite directly related to the development of population genetics. When Fisher, Wright, and others laid the foundations of a comprehensive genetic theory of evolution their analysis made imperative studies which included demography. As they developed models they were necessarily forced to base these models on certain assumptions about mating patterns, fertility, and mortality (43). Not surprisingly, these assumptions were questioned and there began a continuing effort to refine evolutionary theory based on observed rather than assumed demographic structures.

This effort had progressed to a sound base by the late 1950s. Thus, in contributing a chapter to an anthology on demography, Spuhler (130) clearly indicated that the physical anthropologist's major interest in demography was its potential contribution to population genetics. Even so, Sutter in 1963 (136) still found it necessary to warn the population geneticists that they were not paying sufficient attention to the demographic structures of human populations.

In order to understand the interest of population geneticists and biological anthropologists in this aspect of demographic data, at least the fundamental theory must be appreciated. At the base is the Hardy-Weinberg theorem, which simply demonstrates that under ideal conditions, including large population size, with random mating, gene frequencies will not change from generation to generation. Since evolution is frequently defined as changes in allelic frequencies, what can cause changes? New genes can be introduced by the occurrence of genetic novelty whether that novelty is a chromosomal aberration or a point mutation. However, this would have very little effect on the frequency of genes in a population unless something led to the subsequent rapid generational increase of this genetic change. One possible cause of change could be gene flow. If new genes were introduced into a population by mating with individuals from a different gene pool, frequencies would change.

In a different vein, gene frequencies can be altered from generation to
generation due to chance processes, often described as sampling error. Since each generation is only a sample of the genes present in the parent group, a number of random processes effect gene transmission. Some of these processes have been labeled. If an allele is carried by very few individuals in a population, chance can theoretically become a major factor in what happens to that particular gene. Genetic drift, as this phenomena is generally called, is best described statistically, but it can be understood by thinking of a group of 100 individuals in which only 10 possess or lack a particular gene. In this situation many things could happen to the 10 which accidentally would lead to the gene disappearing in the next generation or radically changing its frequency. Such an example is, however, a poor substitute for the multiple implications of this statistical concept. Another process, often termed the founder principle, refers to unusual gene frequency distributions found in populations which are established by a small group taken from a larger parent population. A third of the many sampling effects occurs in a parent population when the migrants out of a parent group are not a random sample of the genes in the general population.

The intricacies of human mating patterns can cause many other departures from the Hardy-Weinberg equilibrium. Any arrangement which leads people with similar phenotypes to mate more frequently will change the distribution of genotypes within a population. This phenomenon or its reverse is called assortative mating and has some special consequence when inbreeding is the basis of preferred mating.

Despite the possible importance of all the previously listed factors, the major cause, over the total history of man, for changes in gene frequency must be selection. Chiefly by processes which have selectively increased and decreased the frequencies of specific genes we can explain the organization of the biological world including man and his variability.

Having briefly reviewed population genetics theory, let us examine in more detail some of the recent contributions of demographic study to the theoretical components.

**Gene Flow**

Quite clearly, gene flow has been relatively high in almost all human populations. If this were not the case we would almost certainly not be a single species. How high gene flow has been and the effects of cultural change upon it are being investigated by demographic studies on contemporary populations of differing cultural patterns. What might have been the situation during man's long history as a hunter and gatherer has been investigated by the study of recent and existing groups. While much more detailed studies are needed, the data available on groups such as the Australian aborigine (9, 154), Bushman (70), and Eskimo (80) suggest a moderately high rate of gene flow encouraged by social mechanisms which acted to stimulate outbreeding. Even such cultural universals as the incest taboo are effective in this regard.
Rather more detailed studies have been undertaken on a tribal level with Amerindian groups in South America. These studies by geneticists from the University of Michigan working with various South American groups show gene flows to be rather high but to have different social causes than those suggested for hunters or demonstrated for peasants (20, 87, 144). Using demographic and other data, these studies suggest that organized warfare causes gene flow of a different geographic nature from that found among hunters. Among peasants new factors such as social class become important to the exact nature of gene flow.

Two major gene flow studies on peasants were accomplished by use of church records in Italy (19) and England (14, 56). In both instances it was shown that simple physical distance could be used in a mathematical model to describe the rates of gene flow. The data from England had sufficient time span to permit the analysis of how the technological changes of the 1800s drastically increased mating distance. This drastic rise in the rate of gene flow affected much of Europe and holds for many developing countries (137). The rates of flow are now so high in countries such as the United States that geographical genetic differentiation is being rapidly reduced (131, 150). As with descriptive demography, research in the topic of gene flow is being extended by the use of computer simulation models (48, 76). Such models have been used to evaluate observed data (59) and to check on earlier mathematical models (103).

**Genetic Drift and the Founder Principle**

The importance of genetic drift factors in human evolution has been the subject of lively debate during the last decade. Since man is often a migrant animal and a consistent outbreeder, the rate of gene flow has been rather high for most human populations. Thus, it seems that the prerequisite small breeding isolate required for significant drift effects must have been rare and mostly confined to microevolutionary effects. However, many recent migrations such as the peopling of the Western Hemisphere and the Pacific Islands and the forceful movement of slaves allowed many possibilities for the operation of the founder principle.

The study of drift can involve several kinds of techniques; of these, demographic data provides one of the easiest and most definitive. Demographic attempts to show the extent of drift effects in peasant populations have indicated that for most such groups gene flow is high enough so that drift effects are probably of little consequence. In Peru and Chile, peasant communities and market towns both seem to have gene flow rates much too high to allow significant drift effects (32, 69). Cavalli-Sforza, from his study of historic Italian peasants, indicated that because of the small village size and the compounding effect of inbreeding it was conceivable that the genetic differences between the villages could have been the result of drift effects. Nevertheless he notes that there is no proof that the differences were actually produced by drift (18).
At the tribal level of organization gene flow rates are reported high in the Amazon basin (108), but somewhat lower rates appear to occur among the Melanesians in the Pacific (46). Social behavior which may increase drift effects has been reported by Neel (86), who notes that village chiefs among the Yanomama contribute a very substantial proportion of all genes to subsequent generations. Thus, family size variance is high and this increases the magnitude of potential drift effects. However, if the acquisition of chiefdomship is mediated by specific genes, this could serve as an example of extreme selection.

Studies of hunters have so far failed to show by demographic means any strong case for intergroup differentiation as a consequence of drift or the founder principle. This may simply be a product of data paucity, but at present it seems to us that the best instances where genetic drift has been demonstrated to have a major effect on the genetics of a group is among small island populations and religious isolates. A good example is provided by Roberts' study of the peopling of Tristan da Cunha (100). This small and inhospitable island probably represents an extreme case of geographic isolation, but because the settlement occurred in the 1800s it probably had as much contact with outside peoples as less isolated islands which were settled by earlier populations. In this instance Roberts not only demonstrated the founder principle but also showed how factors unrelated to ordinary definitions of natural selection modified the genetic structure of the group (101).

**Assortative Mating and Inbreeding**

Assortative mating is potentially a subject of major importance to population genetics. All societies have mating rules, and whether we discuss the so-called free choice system of Western society or the highly prescribed mating rules of the hunter, all imply the strong possibility that the practice will segregate gene pools within populations. It has been claimed that assortative mating is a major evolutionary feature in modern society and that civilization depended in part on the assortative matings which began with the start of social class (50). Despite these claims, the evolutionary importance of assortative mating has been difficult to demonstrate.

Using demographic data it can be shown that within the United States today our free choice system leads to mating based on social race, social class, years of education, residential propinquity, religious affiliation, etc (42). Insofar as these identifications are related to genetic differences we should be in the process of forming distinct gene pools. In fact the gene flow across these barriers is substantial and the criteria for mate choice changes with each generation.

In the more stable cultures of the past, assortative mating patterns were often more stringent and durable. The Jewish ghetto of the Middle Ages left a small residue effect (39, 40), and the caste structure of India has a demonstrable effect on the gene frequencies of its constituents (114). In both instances, the mating practices may have only perpetuated previous difference
rather than creating segregate gene pools. In one recent study it has been claimed that the long-term preference of the Japanese upper class for light skin color produced some class segregation in this trait (58).

As one views the studies of peasant, tribal (34, 106), and hunting societies (53), the possibilities for assortative mating effects seem to decline. This occurs in part because whatever the marriage rule the declining number of free sexual partners of appropriate age decreases the possibility of choice. It is certainly premature to reach a definitive conclusion, but it may be hypothesized that assortative mating was relatively inconsequential until man reached the denser populations aided by agriculture. Natural populations may not be sufficient to test such a hypothesis but again simulated populations may help test this proposal.

While the effects of assortative mating have not been rigorously explored, the special case of human inbreeding has been exhaustively explored and probably passed its peak. Again, demographic techniques are not the only ones available, but they form a major portion of the literature. It would be unduly lengthy to review all of the studies on inbreeding, and so it may be appropriate to cite only some of the more complete ones (121).

The temporary conclusion which may be drawn is that inbreeding was common to many peasant groups but the biological consequence which is of social importance is usually small. When small groups containing particular recessive genetic defects inbreed, the frequency of the particular defect not surprisingly increases in phenotypic manifestation, but for most groups, inbreeding leads to only faintly detectable changes in phenotypic fitness (45).

**Genetic Selection**

Since selection may be the major source of human population difference in gene frequencies, it might reasonably be assumed that demographic data is of major importance to the discussion of selection, but this is not directly the case for man. In nonhuman animals, population numbers and demographic responses to the environment are prime indicators of the population’s response to selective pressure. Man’s adaptive success can also be measured by his numbers, but the effects of culture so override the other causative factor that demography tells little about selective processes (6). Furthermore, Morton recently pointed out that if selection was operating at a low level, then the number of individuals required to demonstrate selection in operation would be so large that all the present-day tribal and hunting groups would yield numbers too low to be of much value (83). In a less pessimistic manner we will discuss in the next section some ways in which genes, physical environment, and culture may be studied in reference to demography, but here we will limit ourselves to the direct uses of demographic data in natural selection.

Some years ago Crow tried to derive an index which could predict from demographic data what the possibilities of selection were in a given population (30). His model, called the total selection index, has now been calcu-
lated for a number of groups based on their known fertility and mortality pattern (32, 60). Quite clearly, populations which have high fertility differentials within them and have high mortalities before adulthood could be selected more rapidly than populations which have low, uniform fertility and low preadult mortality. Whether such populations are actually being selected differentially, however, is another question. The total selection index is thus no more than a statement of the rates by which populations could change and does not tell whether two groups are in fact being selected at different rates.

All this is not to say that demography is not important to the theory of selection. It is in fact basic, and geneticists can be quite dogmatic in stating from demographic data that the nature of selection has changed with industrialized civilization from a selection based primarily on differential mortality to a lower selective pressure based on differential fertility (31, 85). Finally, it is clear that population geneticists are not going to be completely satisfied with any claim that selection has occurred unless the proof is in the nature of demonstrated gene frequency changes traced through demographic models.

HUMAN ECOLOGY AND DEMOGRAPHIC VARIABILITY

The confirmed evolutionist ultimately believes all biological variability, including the demographic patterns of man, to be explicable in terms of evolutionary processes. Yet as shown in the previous section the population geneticist is currently using the variability in demographic parameters as causative or independent variables. These parameters have not been generally studied by biologists as dependent variables. Population differences in birth sex ratios and mortality patterns were considered at least partially dependent on genetic factors and disease, but the causes for the differences in population fertility were almost entirely the province of the social scientist.

The social scientist did not believe that population differences were caused entirely by social aggregate variables, but by training and choice his studies concentrated on the relationship between such independent variables as attitudes, social organization, economic forms, and demographic behavior (55).

Perhaps the factor which was most significant in changing these disciplinary traditions has been an increasing amount of data on non-Western nonmodern populations. Demographic studies by anthropologists and biologists on these older societal forms have revealed patterns which cannot be adequately explained by the traditional approaches.

The dilemma and its possible solution is well illustrated in a new book reporting on a Wenner-Gren conference held in 1970 (54). At this conference cultural anthropologists, demographers, geographers, population geneticists, and human ecologists were brought together to exchange their views on how populations could be studied and what kinds of explanations or predictions could be obtained from the various approaches. Various strategies for understanding emerge from the papers presented at the conference. It is suggested that the sociocultural level is an important variable. Thus, hunters are different from peasants, who are different from modern industrial societies,
and as one group begins merging into the other transitional patterns emerge. Within any one cultural or technological level the economic resources of an area relate to the demography, and finally within societies social aggregate variables relate to the demographic behavior of subgroups. Unfortunately, these strategies are not enough to explain all of the variability so other biological variables must be introduced. The disease and particular environmental stresses of a region are often significant to the demography of that area, and there exists at least suggestive evidence that genetic differences between populations may in some cases affect such variables as the birth sex ratio, overall fertility, and certainly particular mortality.

The participants, after reviewing the bewildering array of strategies and contributing variables, reached the following general conclusions:

1. Descriptive demographic studies are still worthwhile.
2. If the broad array of contributing factors is understood and controlled for, studies of the relationship between particular independent factors and demographic variables can be profitably explored.
3. Yet if we hope to reach accurate levels of understanding and prediction about the demography of particular populations, we must mount very broad-based ecological studies.

They unhappily noted that this will be difficult because of the traditional diversity of disciplines concerned with population problems and the practice of scientific funding which follows the lines of academic disciplines.

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ENVIRONMENT, SUBSISTENCE, AND SOCIETY: THE CHANGING ARCHAEOLOGICAL PERSPECTIVE 9506

EZRA B. W. ZUBROW

Department of Anthropology, Stanford University
Stanford, California

INTRODUCTION

It is a cliche to begin a review article by acknowledging that the subject is too large to be examined in any comprehensive manner. Environment, subsistence, and society are each the domain of at least one discipline. Environment is considered to be the proper subject for ecologists, biologists, geologists, and geographers; subsistence is the domain of economists, nutritionists, and various agricultural specialists; society is studied by anthropologists, sociologists, and psychologists, to name only a few. Yet some men have attempted to relate the three subjects into an analytical framework. Few men have been as concise and yet been able to maintain as consistent a world view on this subject as John Aubrey (1626–97), the first person to consider Stonehenge and Avebury in archaeological perspective. He stated:

Let us imagine then what kind of countrie this was in the time of the Ancient Britons. By the nature of the soil, which is a sour woodsere land, very natural for the production of oakes especially, one may conclude that this North Division was a shady dismal wood; and the inhabitants almost as savage as the Beasts whose skins were their only rayment (Daniel 9).

One of the few advantages of trying to research the current status of such a broad subject is that one often gets an impression of the directions in which the scientific domain is headed. Thomas Kuhn (21) has suggested that there is an evolutionary progression in the development of sciences. Essentially he suggests that a developing science passes through the following stages:

1. First, there is a pre-paradigm stage in which there are many subjects and schools of thought competing for prominence. Since there is no common body of belief, each researcher is forced to build his field from its foundations.
2. Second, out of this disparate body of contention, there emerges either a single theory or a set of new theories which not only reorganizes the

1 This paper has benefited from the suggestions and help of Albert Ammerman, Betsy and Hugh Blackmer, Caroline Bledsoe, Bernard Siegel, Janet Simpson, and Marcia Zubrow. Illustrations are by Katherine Nigh.
data, but provides a set of grounded theories (Glazer & Strauss 14) which most practitioners accept.
3. Third, there is a period of normal science in which the basic grounded theories or paradigms are logically explored, tested, and fleshed out.
4. Finally, the results of the normal science show sufficient anomalies that attacks are made on the assumptions of the paradigm or its theories and a new paradigm is discovered.

It is the contention of this paper that archaeology is in the pre-paradigm stage, but there are indications that the first explicit paradigm is already in the process of being developed. This explains why there seems to be coherent direction in particular trends of archaeological research involving environment, subsistence, and society, while in other areas the archaeological research seems to be directed towards idiosyncratic goals and is a haphazard quilt of patches.

**HISTORY OF THE PRE-PARADIGM**

Historically, archaeologists have not been intellectually isolated from either their anthropological colleagues or from the other social and physical sciences. Thus the development of the competing theoretical formulations has been influenced by the changes taking place in anthropological theory and in the social sciences in general. There has been considerable importing and exporting of ideas and data. If one examines the intellectual “balance of payments,” archaeologists have, on the whole, imported their theoretical formulations and exported data and other types of substantiating evidence.

Trigger (28) in an excellent article has surveyed the developing interrelationship between archaeology and ecology. He clearly details pre-paradigmatic competitive theories in four sections: a historical survey of the European tradition, the American systemic approach, deterministic ecology, and open system ecology.

Figure 1 is my diagrammatic version of the chronological, spatial, and theoretical relationships between the competing theories which anthropologists and archaeologists have used to explain environment, subsistence, and society. It is partially based upon Trigger (28) and is somewhat oversimplistic and incomplete. For example, major schools of anthropological theory dealing with subsistence and society have been omitted. The diagram shows major theoretical schools of anthropology and a few representative cultural anthropologists (on the left) and archaeologists (on the right) who contributed to them. I did not include the French sociologists and structuralists, the English structural functionalists, or any of the “psychological anthropologists.” I am not aware of a clear correspondence between their theoretical work and archaeological theory or archaeologists. I am sure that the reader will see what he thinks are incorrect evaluations in the diagram. Probably no two anthropological or archaeological theorists would see this highly involved mass of theories, complex claims, and counter claims in exactly the
same perspective. For example, I might wish to emphasize the similarity between White's (29a) and Steward's (26) concepts of population pressure as a determinant of social organization. Both see one function of social organization as minimizing the law of diminishing returns. The reader may see an important relationship between Durkheim's concept of a division of labor and Krzywicki's (20) feedback mechanism. The former is a mechanism for promoting social solidarity through diminishing population pressure and competition. The latter promotes social solidarity within groups by the increased social variability between groups. This is the result of institutional variation, increasing social isolation, and social isolation increasing institutional variation through time.
THE NEW PARADIGM

As Hammond (16) has stated, a new generation of archaeologists has adopted a more explicit model toward developing a social science. The explicit model is based upon four interacting components: a change in goals, the use of formal explanation, a methodology for comparison and testing of hypotheses, and a systematic definition of cultures and their environment.

A CHANGE IN GOALS

Martin (23) has suggested that the change in archaeological goals was the result of dissatisfaction of many archaeologists with traditional goals. These traditional goals were determining the origins of a particular cultural trait such as sedentism or domestication, the reconstruction of prehistoric cultures, and time-space systematics (the location of a cultural phenomenon in both time and space). A new set of problems became visible as the cultural chronologies and content became defined through years of work in time-space systematics. As the questions of what, when, and where were answered, the question of why there was cultural variation became partially soluble. It was not soluble, however, by continuing to obtain more information about what, when, and where. Instead, an examination of cultural processes which might cause this variation, their antecedent conditions, and attempts to explain why these processes operate became appropriate. This change in archaeological goals has been reflected by a change in topical interest. Thus Hammond sees the trend of archaeological research to be in the direction of "the increasing study of economic, sociological, and behavioral evidence." As one would expect, this has resulted in an increased interest in formal hypotheses about environment, subsistence, and society. It will be shown later in this paper that the shift was not a simple process.

THE USE OF FORMAL EXPLANATION

The philosophical bases of explanations of cultural process are derived from models developed by Hempel and Oppenheim and are parsimoniously presented in their archaeological context by Fritz & Plog (12), and somewhat more completely by Watson, LeBlanc & Redman (29) in Explanation in Archaeology. As Winter has noted, an emphasis on hypothesis testing provides (a) a framework for structuring research (Winter 30, Fritz & Plog 12, Hill 18); (b) a means of gaining knowledge (Winter 30, Binford & Binford 5); and (c) a method for building explanations (Winter 30, Hill 19, Fritz & Plog 12). Since this subject has been adequately covered in the literature, I will refer the reader to the cited studies for more detailed information.

COMPARISON AND TESTING HYPOTHESES

Methodologically, two problems arise. First, how does one test a hypothesis? This problem has caused a change in emphasis from descriptive techniques to verification techniques. Describing data for the sake of description
is no longer sufficient (Zubrow 34). Instead, the data must be organized to indicate the empirical validity or nonvalidity of a hypothesis. Hill has presented the case for the generation of hypotheses derived from multiple sources such as ethnography, ecology, etc; the development of alternative hypotheses to explain a set of phenomena; and the use of test implications for verifying hypotheses on independent data. Perhaps most critical to the testing of hypotheses is the generation of “test implications.” The test implications are derived from the hypothesis and “amount to a listing of all of the evidence one would expect to find if in fact the proposition is correct (and, conversely, a listing of the kinds of evidence which, if found, would demonstrate it to be false)” (Hill 18). Examples of relatively complete sets of test implications for a series of hypotheses are developed by Hill in his Prehistoric Social Organization in the American Southwest (18). Space does not allow a full quotation of one of his sets of test implications. However, one might note the following abstracted table as being exemplary.

<table>
<thead>
<tr>
<th>Resident pattern</th>
<th>Distribution of stylistic items related to female activities across site</th>
<th>Distribution of stylistic items related to male activities across site</th>
</tr>
</thead>
<tbody>
<tr>
<td>Uxorilocal</td>
<td>Nonrandom</td>
<td>Random</td>
</tr>
<tr>
<td>Virilocal</td>
<td>Random</td>
<td>Nonrandom</td>
</tr>
<tr>
<td>Neolocal</td>
<td>Random</td>
<td>Random</td>
</tr>
<tr>
<td>Bilocal</td>
<td>Random</td>
<td>Random</td>
</tr>
<tr>
<td>Duolocal</td>
<td>Nonrandom</td>
<td>Nonrandom</td>
</tr>
</tbody>
</table>

The second problem is how to gauge the relative merits of one or more hypotheses, theories, or models which purport to explain the variation in a set of data. Adequacy criteria, such as predictability, are used to determine whether or not an explanation, a theory, or a hypothesis is not only sufficient, but whether it is better than another. Usually there are minimum standards of predictability that must be met before a hypothesis is confirmed. When comparing theories, models, or hypotheses, one is often tempted to assume that the preferable theory, model, or hypothesis is that which is most predictive. However, this is an oversimplification. Zubrow (33) has shown that it is necessary to use multiple criteria of adequacy simultaneously. These include predictability, parsimony, symmetry, productivity, applicability, and generality. Otherwise any of the latter five may be sacrificed for minimal gains in predictability.

**Systemic Definitions**

The fourth component is a systemic definition of culture. It is operationally useful for several reasons:
1. Systems theory is a level of model building between the highly generalized construction of pure mathematics and specific theories of specialized disciplines (Boulding 7). Thus it acts as a bridge for the numerous theories that many disciplines have contributed to the problem of the interrelationship between environment, subsistence, and society.

2. Systems theory in both its inductive (Bertalanffy 2) and deductive (Ashby 1) derivations provides an answer to the complexity of variation in both the physical and social spheres. Through cybernetics (the study of homeostatic mechanisms), information theory (which allows information to be quantified as negative entropy), and game and decision theory (which allows competition and choice to be expressed quantitatively), systemic analysis allows one to operationalize concepts which are applicable to organized wholes—including interaction, centralization, competition—and finally form a general definition of systems as a complex of interacting components.

3. Systems theory allows one to transcend the boundary between the biological, physical, and social sciences by having a similar theory for open and closed systems. Closed systems are open systems with a zero value for input.

The actual definition of a general system is too complex to discuss here. It will suffice to state that at an informal level, a system is a set of inputs, outputs, components, and interrelationships. A more sophisticated definition is given by Hall & Fagan (15), and for the connoisseur, a mathematical definition will be found in Wymore (31).

At present there are two systemic definitions of culture being used by archaeologists. Clarke (8) presents a model which exhibits the following features:

1. Culture is a system.

2. The system changes through time through a succession of states.

3. The sequence of states, the trajectory, depends on the internal structure of the system, its history, and the interrelationships of the system through input and output channels which with other systems make up the cultural and social environment.

4. Regulatory mechanisms act as homeostatic determinants attempting to keep the system in equilibrium states while deviation-amplifying processes push the system toward disequilibrium states. The system thus contains both positive and negative feedback.

5. Information theory may be used to define particular relationships as well as particular feedback and deviation-amplifying processes through variety, equivocation, and redundancy.

6. The system is only partially observable (Doran 10).

Binford has defined culture as "an extra somatic adaptive system that is employed in the integration of a society with its environment and with other sociocultural systems" (3). The system is subdivided into at least three subsystems—an ideological subsystem, a social subsystem, and an economic subsystem. Changes in one subsystem cause changes in the other subsystem. Since the economic subsystem has a tendency to produce larger quantities of
material artifacts, it is over-represented in archaeological deposits and must be monitored in order to infer changes in the ideological and sociological subsystems. Archaeologists are not, however, limited to studying material culture, for the "non-material aspects of culture are accessible in the direct measure with the propositions being advanced about them" (Binford 3). In fact, if archaeologists limit themselves to only material culture and historical explanation, they are probably guilty of mixing variables (Binford 5).

There is a difference between Clarke's and Binford's systemic definitions of culture. Clarke emphasizes (a) that the homeostatic abilities of a cultural system in a fluctuating environment are dependent on "information variety," which is dependent in turn on the amount of information transmitted and the number of paths by which it can be transmitted; (b) that an equilibrium state for a culture is to be regarded as one in which destruction of "information variety" is minimized; (c) that redundancy is the critical variable determining the consistency and stability of the system (Doran 10).

Binford's definition is a specific case of the general type of system which Hall and Fagan call "compatible systems," those systems which reflect the best adaptation to the environment whether it is a mathematical, physical, or any other kind of environment. Flannery (11), the Binfords (4), and their collaborators have operationalized compatible systems for domestication and other economically functional activities. Two points are important. First, these systems are ecologically based, and the important unit which flows through the system is energy calculated on the basis of population and resources. Second, the equilibrium states and disequilibrium states are determined by the relationships between the environment and the cultural system. A shift from an equilibrium state to a disequilibrium state can be caused by changes in either environment of the cultural system.

The two definitions are different in that information flows are calculated in the Clarke system, while energy or economic flows are used in the Binford system. Thus the equilibrium states are different. For Clarke, equilibrium is defined analytically within the system by minimizing destruction of variety. For Binford, equilibrium is defined as inputs or outputs of the system being equal and constant system structure. The critical relationships are between the environment and the culture.

Although the two concepts of culture are not mutually exclusive, combinations of information flows and energy flows (e.g. Shannon-Weaver model) open a series of problems and areas of potential confusion. This makes operationalization difficult. For example, how does one even interpret the possible combinations of the two types of equilibrium states?

**Criticisms of the "New Paradigm"**

The change in theoretical orientation in the development of this potential paradigm has not been made without criticism. Kushner (22), acting as "amicus curiae," has pointed out several weaknesses which he sees in processual archaeology, such as a tendency toward environmental determinism, an in-
sufficient allowance for an individual culture's particular Weltanschauung, potential areas of confusion with regard to adaptation, a tendency to consider cultures as isolates, and problems in the systemic concept of culture.

Informally, considerable criticism of processual archaeological theory has been the result of the overstatement of bipolar viewpoints. What has often been overlooked in the heat of theoretical polemics is that the various schools are interdependent for all their difference of goals and methods, explanations, etc.

CONTRIBUTIONS

In the process of researching this article I began to collect bibliographic materials on archaeological studies which consider aspects of environment, subsistence, and society. I soon came to three realizations. First, the professional research falls into only a limited set of general categories. Second, recent contributions are part of an ongoing process towards the solution of a particular set of problems. In other words, there are trends in research. Third, there have been few if any attempts to summarize the status of archaeological research in such broad topics as environment, subsistence, and society.

I decided I would try to delimit some general categories of research and show how the relative importance of the categories changed through time by examining a reasonable sample of the literature. I planned for this categorical examination of the literature to provide two functions. First, if a new paradigm had been developed, I hoped the categorical distribution would demonstrate the shift in topical emphasis. Second, the categorical examination was to show descriptively whatever changes had taken place in this scientific domain.

A major problem is simply to decide which literature should be included. If a summary study is to have meaning, the literary contributions should be in some sense representative of the work which has been done or is in process. Preferably, I would have gathered all the archaeological literature which had been written concerning environment, subsistence, and society. Then I could have used the complete data, a simple random, or a stratified sampling design to assure the representativeness of the literary sample. Not only was this too large a task to be completed in one year, but there are also considerable boundary problems. For example, when does one include a geological or biological article which is relevant to, clarifies, or makes use of archaeological work. Archaeology does not have the clear-cut boundaries that Derek Price (24) was able to use so effectively in the development of citation matrices in his 1965 article, Networks of Scientific Papers.

In order to gather my sample of literature, I chose 80 well-known schol-

*Because of space limitations, a detailed 1700-reference bibliography could not be published in this chapter. However, copies of the large substantive bibliography are available upon request from the author.
ars who had articles or books on the archaeological aspects of environment, subsistence, and society published since 1969. I collected all the citations or references they had used in their contributions to develop a bibliographic sample of 2092 references by over 800 authors. (The sample was limited to contributions in English.) I hasten to point out that this sample is not statistically representative of all the literature in the field. However, it is representative of what is considered relevant in English by major contemporary scholars.

Scientific articles may be considered in a manner roughly analogous to the radioactive isotopes with which archaeologists are so familiar in C\textsuperscript{14} or K-Ar dating. Articles have differing half lives. Some have very short half lives and after publication are seldom cited, or for that matter even read. Others have long half lives and are continually being cited. In fact, an operational definition of a classic is a contribution which is heavily cited. On the other hand, a review article such as this may be defined as a heavily citing contribution. It is an unfortunately true generalization that review articles are seldom classics and they have at best only limited half lives. A short time period brings new literature and innovation to any discipline, and any particular review article becomes obsolete and must be replaced.

Price's citation matrix is simply a matrix with binary cells which had along its axes the papers in chronological order. The columns represent a citing article and a row a cited article. A strongly represented column is an article with many citations or a review article, while a strongly represented row is an article which has been cited numerous by others and is a classic.

Blackmer (6) summarized the results of two citation matrix studies by noting for N-ray studies:

1. Review articles are regular occurrences and occur approximately every 40 articles, and cite nonclassics.
2. Classics are irregular occurrences.
3. With the exception of review articles, articles cite only recent articles and classics.
4. A recency boundary of the previous 50 articles exists and represents a potential research frontier.

Noting for "dissonance" literature:

1. Review articles are irregular and cite classics.
2. Classics are more frequent, more variable, and harder to distinguish.
3. The recency boundary does not exist.

In cases where the construction of a meaningful citation matrix is impossible due to considerations of size, the temporal inequality of the samples of citing and cited articles, or problems of definition of field boundaries, it is possible to use the Price and Blackmer conceptions of the literature of a scientific field and use descriptive statistics to ask similar questions.
With this background what would one expect to find in a categorical distribution of literature if a new paradigm has been developed? One would expect:

1. A well-defined classic contribution or set of classic contributions that set forth the basis for understanding the domain of research within the framework of the new paradigm.
2. At least one well-defined recency boundary that would correspond to the development of the new paradigm and probably begin with the paradigm’s classics. (As the research frontier moves on, this recency boundary may be supplanted by others.)
3. Major differences in categorical emphasis between cited contributions published before and after the new paradigm.

In a pre-paradigmatic stage contributions should be published within multiple categories reflecting the decentralizing influence of the multiple competitive theories. This in turn should be reflected by the choice of the contributions which the citing authors use. After the development of the new paradigm, there should be a shift from an approximately equal distribution of categorical interest to one in which certain categories are more strongly represented. These categories are deemed more relevant than others by the increasing number of participants working within the framework dictated by the new paradigm.

**Classics**

Of the 2092 references cited which make up my sample of the archaeological environment, subsistence, and society literature, 1776 (85%) are individual, unique titles. The remaining 316 citations (15%) are repetitions of citations. They represent a centrality within the literature which makes up the core of material that is most frequently used. However, no single contribution was cited more than 7 times. In the dissonance literature classics were cited anywhere from 15 to 60 times by any 80 authors being considered. Surprisingly, Zeuner’s *A History of Domesticated Animals* (32), published in 1958, may be considered the most classic work since it was the single contribution cited most frequently (7 times). But one could not consider it either the basis or the precursor of the new paradigm. In reasonably close succession one finds works by Binford, Flannery, Hole, Helbaek, Braidwood, and several others. It is clear that there are not real classics in this field in the same sense that classics exist in the dissonance literature. No one is playing the role of a Keynes to economics or a Chomsky to linguistics.

On the other hand, the considerable degree of centrality in the literature appears to be the result of the necessity of archaeologists to be familiar with each other’s data. Only recently have archaeologists moved away from studying the unique aspects of each other’s data and begun looking at replicable processes which are temporally and/or spatially independent.

Perhaps one should be cautious in examining the evidence for all three
expectations before drawing any definitive conclusions. However, the above indicates that alternatively: a new paradigm exists without clearly defined classic contributions; or a new paradigm does not exist; or the new paradigm has so few participants that the classic contributions cannot be significantly defined.

**Recency**

It is clear that, similar to the N-ray literature, a recency boundary exists. Table 2 (Summary Statistics) shows the number of studies cited by date of their publication. Of these studies 51% have been published since 1961 and 70% since 1956. Even if one examines the distribution of the papers by types this recency boundary holds reasonably constant. The 1776 studies discuss aspects of 30 topics 5202 times or approximately 3 topics per contribution. If one calculates what percentage of the total number of topics were discussed between 1961–1970 and between 1956–1970, one obtains values .52 and .71. This shows a high degree of correspondence to the percentages for studies. There is a certain degree of variation by categorical topic or by time period. Between 1961–1970 the percentage varied from .23 (morphology) to .76 (palynology) with a standard deviation of 11.9; between 1956–1970 it varied from .50 (South America) to .86 (dating) with a standard deviation of 8.7. It is possible to conclude that there is a clear recency boundary (i.e. most authors use a majority of “recent” contributions, defined as those between 1956 or 1961 and 1970). However, the degree of actual closure of the recency boundary varies from topic to topic but is more consistent in the longer of the two periods.

It might be tempting to conclude, on the basis of the decreasing number of contributions (506 to 407) and of topical discussions (1509 to 1202) between 1961–65 and 1966–70, that the literary domain is no longer growing as quickly as in the early sixties.

However, it is necessary to be cautious. Remembering that the 80 citing authors published during and after 1969, the sample size for 1966–70 is skewed downwards. First, since there is a publication lag, the citing authors are publishing one or two years after preparation. Thus one would expect more of the recent contributions to be available to the authors than was actually the case. Second, there is a saturation lag (i.e. the lag between date of publication and the date when it is sufficiently well known to be cited or has widespread popularity in the field). Even if one compensates for the downward skewing, however, there is a decrease in the growth of the materials being used during the last half decade under study. The number of studies after compensation for the smaller sample size is 507 (instead of the actual 407), while the expected number of contributions on the basis of the growth rate for the early 1960s is 772.

Given the publication lag, the saturation lag, and the decreasing growth rate, is it possible to see how closely the recency boundary corresponds to the beginning of the potential new paradigm? This question is difficult to answer. In 1948, Taylor (27) published his revolutionary *A Study of Archaeology* in
which the goals and methodology were taken to task. This study had remarkably little effect until the early 1960s. In the early 1960s at Chicago, Binford, Martin, Longacre, Flannery, Hole, Watson, et al, and at Boston Deetz and Dethlefsen, as well as others, began the development of what might be considered the potential new paradigm. Their major contributions began to appear in the latter part of the first half of the sixties and the earlier part of the second half. Many of them have multiple contributions in the central core of multiple cited contributions. It is clear that their work is well within the recency boundary. Of the multiple citations between 1961–1970, 12% refer to contributions by the above authors. It probably would be stretching the data to state that there is a complete correspondence between the recency boundary and the proposed new paradigm. However, there does seem to be sufficient data to suggest that the recency boundary does not contradict and in fact supports the possibility of a new paradigm.

**Changing Categorical Emphasis**

I wished to examine the topical composition of the environment, subsistence, and society literature and how various topics have had changing relative importance, as seen through the temporal filter. Thus I asked a series of eight compositional questions and defined 30 topical categories in order to try to answer the questions and to measure the changes. These correspond to the 8 parameters and 30 topics in Table 2. The questions were:

1. What was the data orientation of the study? (i.e. site reports or regional and cultural syntheses).
2. With what geographic area was the study concerned? (i.e. North America, South America, Europe, Middle East, Africa, Australia and New Zealand, Asia, and the Pacific Islands).
3. With what broad cultural or temporal period was the study concerned? (i.e. Pleistocene, Paleolithic, Mesolithic, or Neolithic and later).
4. What were the environmental parameters that the study discussed? (i.e. general environment; geological, geographic or typographic factors; climate; and palynology).
5. What were the economic parameters that the study discussed? (i.e. generalized subsistence, subsistence patterns based upon plants, and subsistence patterns based upon animals).
6. What were the societal parameters that the study discussed? (i.e. generalized social factors, use of major ethnographic analogy, and demographic factors such as migration).
7. What were the biological parameters that the study discussed? (i.e. evolution, skeletal studies, morphology, and genetics).
8. What were the methodological studies that allow archaeologists to make inferences about environment, subsistence, and society through both space and time? (i.e. methodological techniques and dating).

The reader will undoubtedly note that the topical categories are not nec-
essarily refined, complete, or mutually exclusive. He may ask why these topical categories were used rather than others. The categories were based upon two criteria. First, a small sample of the literature was examined, and it was found that the studies could be more easily grouped into these categories than others (60 potential categories were under consideration). Second, the author must admit to a choice of categories based partially on his own preferences.

If one considers the eight parameters across time, several interesting conclusions may be drawn. Figure 2 is the graphical representation of the subtotals in Table 2. I have decided to use graphs for the presentation of data in this part of the paper rather than tables because it is easier to visually separate out the relationships. However, all the data may be found in Table 2. In addition to a major expansion in literature cited during the post World War II period, there was also a minor expansion during 1936–40. These two expansions will be seen in many of the parameter discussions and will be used to compare the change in topical emphasis. It is difficult to tell whether there was just one period of growth which the war interrupted or whether there were two periods of growth. If one considers the literature cited about economic parameters and compares it to the geographic literature cited, one sees that the area interest should not be considered quite as consistently important as it traditionally has been. In fact, the cited economic literature between 1936–40 is larger than the geographic literature, but then it submerges again. Between 1956 and 1960 the economic literature equals the geographic literature and surpasses it between 1961–65. Comparing the environmental to the temporal category, one is struck by the rapid increase in the environmental category in the 1950s. Remembering the downward skewing of the 1965–70 sample, it is fair to state that the biological, the methodological, and the environmental parameters are the categories which appear to be maintaining their expansion. This expansion has a clear temporal ordering beginning with an increase in geographical area literature in 1946–50, followed by economic parameter, the temporal parameter, the environmental parameter, scope of study, society, and finally biology and methodology which began expanding in 1951–60. This temporal ordering roughly corresponds to eventual growth. The parameters which represent the majority of the studies have generally a larger, earlier growth rate. The post World War II expansion does not clearly show a relative change in categorical emphasis. For example, the geographical area, the temporal, the environmental, and the economic parameter represent .70 of the categories between 1931–35 and .71 between 1936–40. They increase to .77 between 1961–65 and .76 between 1966–70. There is an obvious demarcation between these four categories and the others. If one considers a single category such as the economic parameter, the percentages are .18, .26, .25, and .19 for the same time periods. It seems clear that there are few major changes in parameter emphasis brought about by the hypothesized development of the new paradigm. However, it is possible that the broad parameters mask changes that are taking place at a more specific, topical level.
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Figure 2. The distribution of cited literature: The parameters.
Figure 3 shows the relative data orientation of the literature cited. It compares highly localized data collections (i.e. site reports) to regional and cultural syntheses. In general, I would have expected the site reports (.39 of the total) to hold up better in comparison to regional and cultural syntheses (.61 of the total) than they did. One argument would be that there should be more site reports than syntheses because the syntheses are dependent on the site reports for data. Secondarily, data are not usually as easily invalidated by changing interpretations at the theoretical level as are syntheses and regional interpretations. However, it is clear that the citing contributors have relied reasonably heavily on syntheses. In addition, it is interesting to note that prior to the postwar expansion there is a clear cycle between syntheses and site reports. Between 1916–25 syntheses cited outnumber site reports; between 1926–40 site reports are more numerous. After the war, syntheses become dominant and the cycle is broken. Even with the general downward skewing the maintenance and even the growth of syntheses and regional studies in comparison to the site report decline during 1966–70 seem to reinforce the development of a new paradigm. The study of cultural processes makes the site reports, as a nonproblem-oriented collection of data, less relevant than data collected and presented in other forms.

It is possible to divide the areal category into more specific topical categories which are North America, South America, Africa, Asia, Australia and New Zealand, Middle East, and the Pacific Islands (see Figure 4). The postwar expansion is as clearly demarcated in this parameter as in the previous ones. The war had the least effect upon citation for North American contributions. This graph points out several trends. There is a clear predominance of cited literature after 1936–40 from North America. This is not surprising in that
Figure 4. The distribution of cited literature: Spatial parameter.
the sample is limited to English. What is more surprising is the overall distribution through time. North America predominates with .32 of total areal topical discussions. It is followed by Europe (.19) and Africa (.19) and then by the Middle East (.15). I must admit that I expected the Middle East, a traditional archaeological research area, to have a larger percentage of the total areal sample. There is a clear redistribution of the topical emphasis. The data in the postwar period clearly fall into two groups. One is composed of North America, Africa, Europe, and the Middle East; the other areas fall into the second group. There is a clear time lag which separates the postwar expansion of the two groups. The first group has a much larger growth rate between 1946 and 1965 while the second group reaches a rough postwar plateau by 1951–55. Group one comprised .85 of the published areal discussions cited during 1936–40. By 1961–65, this percentage increased to .90. However, if one examines North America alone, the percentage increases from .22 to .35 while the Middle East decreases .22 to .15. (Europe maintains a percentage of .22 while Africa varies between .19 and .18 for the same periods.)

During the last half decade under study, Africa, Asia, Australia, and South America demonstrate growth. Decreasing numbers are clearly affecting most of the others (even when the downward skewing is being compensated and compared to real or extrapolated growth rates). Although there are definite shifts in topical emphasis, such as the postwar decline of the Middle East and the growth of research in the less developed areas, they could be ascribed either to the development of the new paradigm or to the desire to fill out areas which were relatively unknown chronologically or culturally. The necessity for comparative data is a prerequisite for all theoretical formulations whether paradigmatic or competitive.

The temporal dimension has been divided into four categories. I noted that the cited contributions either emphasized major cultural stages, which also correspond to temporal units such as the Paleolithic, or geological time units such as the Pleistocene. Almost all of the contributions which concerned this temporal dimension fall into the Paleolithic, the Mesolithic, and the Neolithic, which was combined with the post-Neolithic. Cited contributions which emphasized the geological time scale almost always dealt with the Pleistocene. Attempting to add the Holocene or the post-Neolithic as separate categories was considered and rejected. Primarily, I rejected the added categories because the number of contributions in the categories were small enough that the extra refinement would have had dubious validity due to sampling error. Figure 5 shows once again the massive post World War II growth of the literature cited. The Neolithic and the Pleistocene, although separated by a 5-year time lag, are clearly being influenced by a different set of processes than the Mesolithic and Paleolithic. The shift in topical emphasis is clear cut. If one compares the relative percentages of the four categories during 1936–40 and 1961–66, the Pleistocene gains (.09 to .39) at the expense of the Paleolithic (.17 to .11), Mesolithic (.09 to .05), and even the
Neolithic (.65 to .45). The Paleolithic appears to be maintaining its growth within the last half decade better than any of the other categories. The Mesolithic decrease in the last decade is serious in that it cannot be ascribed solely to downward skewing and may very well reflect its transitional status not only as a temporal or cultural unit but also as a theoretical construct. There appears to be a shift in topical emphasis away from cultural stages which would be expected if the paradigm's process orientation became prevalent since these stages reflect historical reconstruction classifications. However, the shift may also be interpreted similarly to the geographic areas, for the largest loss is in the best-known stage, the Neolithic.

The environmental parameter (Figure 6) was divided into four categories: studies of the generalized environment; studies which are concerned with geology, geography, and topography; studies of climate; and studies which use palynology primarily to reconstruct the environment. The geological, geographic, and topographic studies were combined into one category because in attempting to reconstruct the prehistoric land surfaces and their relationships to cultural phenomena all three are often considered. The inclusion of the palynological variable in this broad category reflects partially my personal preference. The problem which I faced was that palynological studies usually have many facets. For example, palynological studies are used to reconstruct particular aspects of the environment, to analyze prehistoric economic systems,
and to date sites. Many palynological contributions not only include all three of
the above but simultaneously include methodological innovations. I have at-
ttempted to differentiate these aspects of palynological studies and categorize
the studies accordingly. Under palynology in this graph are included studies
which primarily attempt to deal with environmental parameters. Several
trends are apparent from the above graph. Perhaps the continual growth rate
of the studies of generalized environmental studies after 1946 is the most
surprising. It is not even affected by the downward skewing. Although the
growth of cited subsistence studies is larger (see Figure 7), the generalized
environmental studies are continuing to grow at a much faster rate if the
1966–70 data are indicative. The generalized environmental category and the
combined geological, geographic, and topographic category precedes the cli-
matic category in major growth by a decade.

It is clear also from the temporal ordering that palynology is a recent
addition to environmental studies. Its major effect has been felt in the late
1950s and early 1960s. Continuing the comparison between 1936–40 and
1961–66, the shift in categorical emphasis within environmental studies cited
may be easily seen. The generalized environmental topic shifted from (.59 to
.33) while geology, topography, and geography gained from (.24 to .35), as
did palynology (.00 to .12) and climate (.18 to .20). It would be fair to

![Figure 6. The distribution of cited literature: Environmental parameter.](image-url)
characterize this relative change in topical emphasis by stating that as the numbers of environmental studies increase, there is a shift in emphasis from generalized environmental studies to more specific studies. However, numerically generalized environmental studies are continuing to grow and predominate. This change in emphasis corresponds to what would be expected as a new energy-systemic paradigm develops. In order to define the inputs within the system, such as carrying capacity, a greater knowledge of detailed environmental parameters is necessary. This detailed environmental data also makes possible the definition of the structure of the environmental cultural interface if one is actually going to build a systemic model which simulates for a prehistoric site or a region what Forrester's (13) model attempts for a city.
The economic parameter is divided into three topical categories. They are general subsistence studies, studies of subsistence systems based primarily on plants, and studies of systems based primarily on animals. The postwar expansion for generalized subsistence is greater than for any other topic. The economic parameter shows two clear periods of growth. One culminated in 1936–40 and the other in 1961–65. There is a temporal ordering which reflects changing emphasis. After 1936 the number of cited studies for plants is greater than the number for animals until 1961–65. This clear botanical preference reflects an interest in the origin and the development of agriculture. This interest continues as at least one very profitable research line today even if it would appear that there has been a diminishing number of studies during 1966–70. For example, two very recent studies by Solheim (25) and Harris (17) which were published in April 1972 argue that agriculture did not only develop independently in the Middle East and the New World but also in Southeast Asia. Solheim suggests that there is evidence that the southeastern Asian horticulture is at least 2000 years earlier than the first domestication of plants in the Middle East.

It is not until 1951–1955 that cited studies of generalized subsistence surpass cited studies concentrating on the plant subsistence systems. This change is corroborated by the relative values of topical emphasis between 1936–40 and 1961–66. Generalized subsistence studies (.37 to .42) and studies of animal subsistence (.24 to .28) increase at the expense of studies of plant subsistence (.39 to .29). The large drop in the number of cited plant and animal subsistence studies in comparison to the generalized subsistence studies between 1966–70 would indicate that the latter actually are continuing to grow even if one does not compensate for smaller sample size. If one argues similarly to the previous discussion, that the potential new paradigm requires more detailed information in order to design and operate systemic models, then the evidence from above partially contradicts the environmental evidence. Although there is increasing specificity with regard to the greater emphasis of animal subsistence studies, there is also greater emphasis on generalized subsistence studies. This corresponds to the decreasing importance of autoecology as synecology became more relevant. On the other hand, if one wished to argue that the new paradigm requires a broader and more generalized set of information in order to understand the general relations between the various subsystems, then the environmental data partially contradict the potential existence of the new paradigm.

Within the societal parameters (Figure 8) I differentiated general societal studies, ethnographic analogy, and demography. General societal studies include attempts at societal reconstructions, analyses of social organization such as Deetz’s, Longacre’s, or Hill’s, and studies of status, political structure, etc. Ethnographic analogy delineates those cited contributions which either are ethnographic or which use ethnographic analogy as a primary mechanism for inferring social or cultural phenomena. Demography is a variable similar to palynology which could be examined under several of the parameters. What
is being tabulated in this discussion are cited studies which consider the social
determinants and consequences of the growth, composition, size, and struc-
ture of human populations. This diagram reveals the continuation of the bi-
modal growth of the archaeological literature with peaks in 1936–40 and
1961–65. Generalized social studies do not begin to predominate the litera-
ture until after World War II. Although there is growth in all three topical
categories immediately after the war, ethnographic analogy and demographic
studies reach a rough plateau between 1951–55 which delays their growth by
a half decade. Even with delayed growth they reach their smaller maxima 5
years earlier than the generalized society contributions. It appears that none
of the three topical categories has been able to maintain its growth. Both
ethnographic analogy and demography show declines between 1961–70.
Thus one may not ascribe their seemingly diminishing importance to the
downward skewing of the sample. During the last half decade it is clear that
generalized society is having the most difficulty. This seeming variation is not
completely corroborated by the relative shift in emphasis among the descripti-
tive categories. For example, there is a clear gain in generalized society (.64
to .71) and demography (.04 to .09) at the expense of ethnographic analogy
(.32 to .20) when one compares 1936–40 to 1961–65. Not only does the
generality-specificity argument hold, but if a new open-systems paradigm has
developed, one would expect, as do Binford and Longacre, that the whole
societal domain would increase positively. All the societal topics should in-
crease if there is an attempt to interconnect the technological or economic
subsystem with the social and ideological subsystems.

The biological parameter (Figure 9) of environment, subsistence, and so-
ciety is the aspect least developed in the cited literature. I subdivided the parameter into evolutionary, osteological, nonskeletal morphology, and genetic studies. Since the numbers are so small, there is a reasonable probability of sampling error which could invalidate the results. The postwar expansion took three forms. Evolutionary studies quickly grew and then reached a plateau. Skeletal or osteological studies have a consistent period of growth, while morphological and genetics studies grow and then decline. There are clear changes in emphasis. Comparing the two periods which usually show maximum expansion (1936–40 and 1961–66), there is a relative increase in the topical emphasis of evolution (.11 to .33), skeletal studies (.33 to .35), and genetics (.11 to .24) at the expense of morphology (.44 to .08). It would be nice to be able to ascribe this partial shift toward specificity to the new paradigm. However, the situation is not simple. The rapid evolutionary growth corresponds to the growth of neo-evolutionary theory in the late 1950s. Similarly, genetic studies of all sorts received a large boost in the 1950s and 1960s as a result of Watson, Crick & Wilkin’s DNA model and code. Thus there appear to be multiple alternative causes for the change, and one should not prematurely ascribe the results to a new paradigm.

Archaeological methodology (Figure 10) is divided into two topics: non-dating techniques and dating. It could easily be argued that these categories are at different levels of contrast within the classification hierarchy. There are several reasons why nondating and dating techniques are put into contrast. First, whether one separates the nondating techniques or combines them they only represent .39 of the methodology parameter. Since all of the nondating techniques never surpass 30 contributions in a 5-year period, one could have serious problems with sampling error if he subdivided even further.

Part of the postwar expansion in dating reflects the development of new dating techniques. During the late 40s and early 50s, radio carbon dating was derived from Libby’s Nobel prize work. Other techniques which came into wider use in the 50s and 60s include obsidian dating, archaeomagnetism, potassium-argon, as well as many others. The graph shows through time a reversal of emphasis between dating and nondating techniques—for example,

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**Figure 9.** The distribution of cited literature: Biological parameter.
.43 to .70 and .57 to .30 between 1936–40 and 1961–66. The reversal corresponds to the increased development and sophistication of the dating techniques. However, it does appear that this reversal was the result of new sophistications in hardware which were taken advantage of by archaeologists rather than the result of a new theoretical paradigm which demanded a more accurate dating. A brief examination in fact shows that the major growth in dating studies precedes the development of the potential paradigm.

This part of the paper served two functions: (a) to show if a new paradigm had been developed and was being used; (b) to show the changes in what was presently considered relevant. If a new paradigm had been developed, I suggested that a set of classics would be defined, that a well-defined recency boundary would correspond to the classics, and that there would be a shift of topical emphasis towards greater specificity. Our analysis of 2092 citations showed:

1. No clear set of classics.
2. The most cited reference is not a theoretical statement of the new potential paradigm.
3. There is a 15% centrality of multiply cited literature.
4. There is a recency boundary which does correspond to the new paradigm.
5. At the parameter level there does not appear to be major change in relative emphasis towards specificity.
6. There is an increase in regional and synthetic studies which reinforces the new paradigm since nonproblem oriented collections of data are becoming less relevant. (It reflects a decreasing specificity of data but an increasing specificity of problem orientation.)
7. Geographically there is a definite shift in topical emphasis with a recently increasing emphasis on areas which are less well known cultur-
ally. However, this shift cannot be ascribed solely to the development of a new paradigm, for increased comparative data are valued by all competitive theories.

8. There is an increasing temporal interest in the Pleistocene.

9. There is a shift within the environmental parameter towards more specificity which supports the paradigm.

10. Generalized subsistence studies and animal studies have relatively increased at the expense of plant studies. This particular change, it has been argued, supports the development of the new paradigm.

11. Within the societal parameter there appears to be an increasing tendency toward generality rather than specificity and an inability to maintain growth, so that one may conclude that the evidence from this parameter does not support the existence of a new paradigm.

12. Although there is a partial shift toward specificity within the biological parameter, it is difficult to use it to support the new paradigm since the shift predates the development of the paradigm.

13. Methodologically, there has been a major shift from nondating techniques to dating techniques.

SUMMARY AND CONCLUSIONS

This review article has attempted to define the changing perspective of the archaeological discipline with regards to environment, subsistence, and society. It has suggested that there has been a series of competitive theories defining the types of work which have been published. It suggests as a hypothesis that there has been a theoretical change in archaeology which has been called “a new paradigm,” and it has briefly discussed and summarized some theoretical problems. Finally, it has tried to test for expected changes in the archaeological literature that would confirm the existence of a new paradigm. The evidence neither clearly supports nor invalidates the hypothesis. If a new paradigm exists (which I personally believe), one may only conclude that it has not permeated the discipline sufficiently to have a major literary influence.

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ENVIRONMENT, SUBSISTENCE, AND SOCIETY

KARL G. HEIDER
Berkeley, California

INTRODUCTION

The relationship between environment, subsistence, and society has been one of the constant and chief concerns of anthropology. Here we shall examine recent developments which relate to this area. An annual review might be expected to cover only books and articles with a publication date of the previous year. But since this is the first annual review, I am arbitrarily widening the scope of this survey to include the last few years, roughly, the 1968–1971 period. This dateline artificially excludes the major influential works of such anthropologists as Julian Steward, Harold Conklin, Charles Frake, and Clifford Geertz. However, there are so many excellent recent summaries of the development of this field that there is no point in adding another. The interested reader can turn to Vayda & Rappaport (56), Netting (41), Damas (11), Anderson (1), and Harris (26).

Here we shall try to focus on the cutting edge of the field. What is the state of the theory? What new methods and techniques are emerging? And what recent substantive knowledge has been brought forth?

To anticipate the conclusions, we will find that the theory has been hampered by a simplistic polemic; that important refinements in methodology are now emerging, perhaps more from cultural geography than from anthropology; and that the actual substantive results, both in terms of satisfactory descriptions of individual societies and of determination of regular principles of relationships, are still too often trivial or overgeneralized.

The subject can be phrased as the interrelationship of environment, subsistence, and society, or it may be more succinctly coded as ecology or cultural ecology. This can be misleading, for “ecology” now means so many things to so many scientists, parascientists, and antiscientists that any use of it will be challenged. I think that it is enough to say here that most anthropologists and geographers who use the term “cultural ecology” are concerned with the interrelationships of environment, subsistence, and society.

It is also well to note here that although we are most interested in studies which emphasize the relationships between environment, subsistence, and society, we must pay close attention to some studies which although they focus primarily on subsistence activities, utilize methodological innovations which promise to aid the more integrated studies.
The notion of change is central to this entire subject. The study of inter-
relationship depends on the notion of adaptation, which is, of course, a pro-
cess: the process of change. A study may focus on change over time, or it 
may look only at the result of change. A common criticism of much anthro-
pology is that the conceptual models cannot handle change, or that they ig-
nore change. But in the area of ecology, change is a necessary, even if implicit, 

factor.

Nutrition and demography are two important topics which I shall only 
skirt here. They are important to the matter at hand, but each covers such a 
specialized approach and literature that it seems best to leave them for sepa-
rate treatment in a later Review. Also, I am here neglecting the relationships 
between ritual and ecology (e.g. Rappaport 43) and among values, personal-
ity, and ecology (e.g. Campbell 7, D. R. Lee 37, Edgerton 14).

THE HOLISTIC APPROACH TO A COMPLEX SYSTEM

The basic position states that there is an interrelationship between envi-
ronment, subsistence, and society; that this interrelationship is systematic; 
and that research strategy should be directed towards describing specific sys-
tems and towards discovering the regularities or general principles of these 
systems.

In other words, the research strategy is a strategy of holism. Although the 
word “holism” is somewhat out of fashion these days, its practice is more 
rigorous than ever. Holism is simply the search for systematic interrelation-
ships. As I have pointed out elsewhere (30), it is a descriptive technique and 
not a theory. From a holistic account of a system one can move towards 
defining the relative importance of the various relationships in a system, and 
even move towards an understanding of causality. At the risk of being seri-
ously misunderstood, I will state here that a major fault of most ecological 
studies is that they are not based on the sorts of meticulous data which are 
necessary to develop a holistic description of a society. Rather, they have 
chosen their sides (materialism vs idealism) on the causal problem and pro-
ceed from there. But beneath the almost obligatory polemic, the best studies 
have taken what I am calling a holistic approach. It would be fruitless to 
develop the case for holism in abstract, but in the following pages holism will 
readily be seen as the major principle of these studies of the relationship be-
tween environment, subsistence, and society.

Now it is possible to treat the environment, and the subsistence activities, 
and the social aspects of a society between the covers of a book and not 
achieve systematic analysis. There are recent ethnographies of this sort which 
even include the word “ecology” in the title or the subtitle. But it takes more 
than a rainfall chart in an ethnography to make it “ecological.” If the word is 
to have any meaning at all, it must refer to a system. Of course, it is not 
necessary to condemn such nonecological efforts, nor the many other quite 
legitimate sorts of anthropological inquiry which do not address themselves 
to systematic relationships.
HARRIS AND "DETERMINISM"

The most thoroughgoing recent consideration of the theoretical issues was made by Marvin Harris in his massive history (26) and in his subsequent textbook (28). He developed the case for the principle of "techno-environmental and techno-economic determinism," which is investigated by the "strategy of cultural materialism":

This principle holds that similar technologies applied to similar environments tend to produce similar arrangements of labor in production and distribution, and that these in turn call forth similar kinds of social groupings, which justify and coordinate their activities by means of similar systems of values and beliefs (26).

But "determinism" is a frightening word for most anthropologists, and Harris' intentionally cumbersome hyphenated qualifiers have not been as clarifying as he intended. He argues strongly for the search for causal relations as explanation against the nonprocessual ethnography; and he argues that the search for cause, or the independent variable, must be among materialistic factors, not idealistic factors.

Throughout his book Harris argues that the cultural materialistic strategy can better explain cultural events than can alternative materialistic explanations. He points out that accounts of warfare, or potlatching, or trade are incomplete and often erroneous when they do not take into consideration economic and ecological factors.

In his key example, he draws from his own research to show "what is involved in a cultural materialistic explanation of the difference between race relations in Brazil and the US" (26). He demonstrates that the cultural materialistic explanation must incorporate a wide range of "economic, demographic, ecological, political, military, and cognitive data" (26).

So he is far from advocating a narrow, monistic determinism. Although he retains the word "determinism," he stresses the necessity to be broadly holistic. And although he has stressed the materialistic factors, he has not hesitated to consider nonmaterialistic factors.

But 40 pages after presenting his own model for explanation, Harris blasts "eclecticism" as: "often little more than a euphemism for confusion, the muddled acceptance of contradictory theories, the bankruptcy of creative thought, and the cloak of mediocrity" (26).

It is hard to disagree that muddled bankrupt contradictions are bad and also, at the other extreme, that simplistic idealistic explanations are bad. But by this point one begins to suspect that if anything is bankrupt, it is the meanings of words like "determinism," "materialism," "eclecticism," and even "holism."

"ENVIRONMENTAL DETERMINISM"

"Environmental determinism" is certainly the most recurrent nontheory in ecological studies. It has a long and dishonorable status as straw man in
lower level courses in anthropology and geography, and even in professional writings. The position, as described by its attackers, demands a causality so direct, efficient, and exclusive that it is easy to refute. It is so easy to refute that one might legitimately wonder how it could ever be defended. Indeed, as Harris (26) and the geographer Lewthwaite (40) convincingly showed, even the most notorious “determinists” qualified their positions considerably. But polemic prevails. Harris, after writing an entire book on the complexity of “techno-economic determinism,” was attacked by Service (48) as a monistic determinist! Harris replied (27), hoping to clarify in 8 pages what apparently could not be understood in 700. Unfortunately for his position and for the clarity of intellectual discourse, Harris perversely persists in keeping to the aggravating term “determinism” even though his holistic position has taken him far beyond what most people understand by determinism.

However, Lewthwaite developed the idea that a “tactical determinism” was thriving in modern geography and, by implication, in anthropology. By this he meant the methodological approach based on “the assumption that there are processes which are normally law-abiding and thus allow prediction and retrodiction (40).”

Further, the search for regularities (determinism) is guided by the assumption that some kinds of events are more likely to be the independent variables than are others. This is a position of “probabilism” (cf also M. Harris 27 on this point).

The argument has been too much concerned with connotative semantics. I think it will be more fruitful to turn to specific approaches.

THE LOGICS OF SYSTEMATIC EXPLANATIONS

The search for systematic explanation must involve explicit cross-cultural comparison. The logic of a single example is likely to be misleading. For instance, one can ingenuously develop the case that foragers in an arid land will have minimal cosmological and kinship elaboration (e.g. the Kalahari Bushmen); or one can equally ingenuously show why foragers in an arid land will have highly elaborated cosmologies and kinship systems (e.g. Australian Aborigines), but one cannot maintain both arguments.

Or one can turn the argument around and show, by treating the environments of the Australian Aborigines and the Kalahari Bushmen as “identical,” that kinship and cosmology are quite independent of techno-environmental factors. [Indeed, this is like Führer-Haimendorf’s strategy in comparing societies in Assam and India (22).] But that strategy also is faulty. If one is dealing with a system of multiple variables, one must pick societies as much alike as possible in order to control for as many variables as possible. For example, Damas did this in his comparison of three contiguous Eskimo groups (12).

Alternatively, one can use a sample large enough to override the uncontrolled variables.
THE SEARCH FOR REGULARITIES

The search for general principles poses particular problems. For example, Boserup (5) elaborated the thesis that agricultural intensification and innovation are the results, not the causes, of population growth. Harner (23) broadened the notion, suggesting that food scarcity, measured by population pressure, is a "major determinant" of social evolution. He suggests that among agricultural societies, those with greater population pressure are more likely to have developed greater political complexity and integration, more class stratification, and to have shifted from unilineal to cognatic descent organization. As the criterion for population pressure, he uses the degree to which a society with at least some agriculture still practices subsistence hunting and gathering. Using large samples (from 242 to 848 societies) he tested the four social features against the demographic indicator and found not only significant correlations, but correlations which indicate that an increase in population pressure precedes change in social factors.

His findings are provocative, but not conclusive. Since direct data on population pressure are not available, he chose to represent the demographic variable by the more easily available subsistence measure, and has at least been able to show a regular general relationship between subsistence pattern and social features.

Now if Boserup is right that increased population causes intensification of agriculture, Harner's logical chain becomes: population growth causes food scarcity which means population pressure which results in intensification of agriculture and other social adaptations for better management of scarce resources.

In the constant interplay between the general and the specific, we now need good ethnographic data on this system. Studies of the Kalahari Bushmen carried out by R. B. Lee and his colleagues in anthropology, demography, and nutrition have begun to appear in print and promise to give us a unique body of quantitative data bearing on this and many related problems. Lee's work shows an unusual concern for testing systematic regularities with specific data. Lee was able to observe the !Kung Bushmen over a long period when they were shifting from pure hunting and gathering to mixed foraging and agriculture. He is able to show (39) that most of the foraging activity was carried out by women, and that the necessity for the foraging Bushman woman to carry her infants during their first 3 years of life formed a powerful incentive for women to space their births widely enough so that they would not be carrying two infants at the same time. The obvious disadvantages of closer spacing of births is lessened when Bushman women come to practice some horticulture and thus are more sedentary.

The implications of the Bushman data are complex. With the disadvantages of closely spaced birth removed, will the population in fact increase? Is sedentariness a cause or an effect of a change in birth spacing? That is, does a woman have more babies when she becomes sedentary? Or do those women
who for whatever reason have more surviving babies take up some horticulture and become more sedentary? It rather appears as if the possibility of partial sedentariness will cause a rise in the birth rate. This, of course, goes against the suggestions of Booserup and Harner, noted above. But it may be true of only the first step into farming. Certainly horticulture is a more flexible subsistence base than foraging. So while change in subsistence may be causal at the first moment of transition to horticulture, it may be that subsequently the population growth is the causal factor, or independent variable, in relation to subsistence intensification and ultimately other social factors.

**LOGIC OF THE UNIQUE EVENT**

The studies by Harner and Lee mentioned above represent the extreme applications of the holistic ecological strategy. The first used data from hundreds of societies to establish general principles, and the second used detailed data from a single society to test a principle. Most anthropological studies are based on only one society. Where they try to explain an ecological pattern, they base the explanation on the logic of the unique event.

**TWO ESKIMO REINTERPRETATIONS**

It would seem that the most enthusiastic use of ecological explanations has been in Eskimo studies. Perhaps this is because explorers and anthropologists who visit the Arctic from temperate zones have been particularly impressed by the harshness of the natural environment. Eskimo wife-trading and female infanticide have both been explained as adaptations to the severe climate. Female infanticide was supposed to result from a necessity to keep the population small and to keep a sex ratio balance under circumstances where men often died in hunting and fishing accidents. Eskimo wife-trading was supposed to be a necessity for the survival of men who made visits to distant places without their wives, but who needed a woman to prepare their hunting clothes.

However, in a reanalysis Hennigh (31) has suggested that Alaskan Eskimo wife-trading is not so much an economic expedient as a strategy for building a wide social network beyond actual kin ties. This interpretation is consistent with Balikci’s observations on the Netsilik Eskimo of Canada (3).

A more complex reinterpretation of Netsilik female infanticide was presented by Freeman (21). The behavior must be treated on both the motivational and the adaptive level. It is clear that the Eskimo did explicitly state that girls were less desired than boys. However, female infanticide intensified a sexual imbalance of adults. Freeman suggested that the immediate cause of Eskimo female infanticide was the need for the father to assert (or reassert) his dominance in a family when that dominance has been threatened by the birth of a girl (but apparently not by the birth of a boy). The motivational argument is ingenious, but it seems not totally conclusive. First, the reports of infanticide seem to date from two or three generations ago. It is not at all clear from Freeman or even from Balikci (2) that modern ethnographers have studied Eskimo societies which actually practiced it, and it would seem
that data of this sort is particularly unreliable. Second, Freeman noted that
the Pelly Bay Netsilik Eskimos are one of the few Eskimo groups which sys-
tematically practiced infanticide. Did other Eskimo groups have less mas-
culine insecurity, or perhaps a functional equivalent to female infanticide?

Freeman also analyzed the effects of female infanticide. These do not
seem to be recognized by the Eskimos themselves. First, already mentioned,
is the imbalanced sex ratio, even among adults, which is at least partially
rectified by personnel movements to or from neighboring groups. The sec-
ond effect, which Freeman stressed, is that the population pyramid is kept
narrow, with a greater proportion of older people. In ecological terms, this
has several adaptive advantages: “it increases the diversity of the population,
it improves the ratio of biomass to energy-consuming activities thus increas-
ing the inertia of the system, and it increases the capital store of information
in the system (21).”

This explanation is appealing, in part because the effect can be proved
without regard to Eskimo knowledge or motivation, and perhaps in part be-
cause it introduces some relatively novel ideas into anthropological discourse.

But once again the lack of closely controlled comparative data raises tan-
talizing questions. What of the groups which neighbor the Netsilik and who
do not practice systematic female infanticide? Do they have squat, disadvan-
tageous maladjusted population pyramids? If they do, does it matter, or are
Freeman’s concepts trivial? If they don’t, how do they manage at all?

In short, while such unique logical single-society explanations are sugges-
tive and do bring us closer to explanation, they stop short of satisfying our
legitimate questions.

CONTRASTIVE COMPARATIVE EXPLANATIONS

With reasonable data and a logical argument, anthropologists can usually
describe and account for a cultural system. But it has long been recognized
that convincing explanations must be based on cross-cultural comparison.
This has rightly been called the laboratory of anthropology, for by carefully
selecting the societies to be compared, some variables can be controlled and
the workings of the system better seen. This is an ideal, of course. There are
some major drawbacks. One is that the holistic approach assumes a complex
cause-and-effect relationship between many variables—in short, a system
whose elements are not amenable to experimental isolation.

Second, the similarities which pass for controlled variables are often ex-
aggerated (as in my earlier example of Bushmen and Aborigines). Third,
when it can be shown that two societies are identical (similar) except for two
variables, then it is easy to assume but difficult to actually prove that one is
the cause and the other the effect.

ATTEMPTS TO MAKE ENVIRONMENT THE INDEPENDENT VARIABLE

One model for contrastive comparison uses closely related, culturally sim-
ilar groups which live in slightly different environments to show that the
slight environmental differences cause differences (usually slight ones) in
their social patterns. The assumption is that formerly the two groups were similar (or even were one group) and that any present differences can be accounted for by differential adaptation to the environments.

_Eskimos again._—Damas' treatment (12) of three contiguous Eskimo groups is an exceptionally good example of this approach. Following Steward and Helm, he began by laying out differences between the "cultural core" or the "exploitative pattern." The major variable involved subsistence activities: the Copper Eskimo and the Netsilik had a yearly cycle alternating sharply between sea and land fishing and hunting; the Inglulik spent much more of the year exploiting sea resources, especially sea mammals.

Some patterns were common to all three groups and could be explained in terms of common subsistence patterns. For example, the large winter camps of 100 or more people were adaptive to the winter breathing-hole hunting of seal, in which a large number of hunters is more advantageous than a few hunters. But although he considered the particular hunting technique "largely deterministic" of the large winter aggregations, Damas is careful to point out the many social and ritual motives for the winter gregariousness. Summer hunting and fishing activities for the most part demanded smaller groups. But the larger groups which then gathered at sewing places in the autumn seem to be motivated purely by social consideratons, for "economic activities were virtually at a standstill in that period" (12).

One of the social differences between the two ecological areas concerns the limits of prohibited marriage. The Copper Eskimo and the Netsilik permitted marriage with relatives while the Inglulik did not. Therefore, while the Inglulik could maintain broader incest rules and still afford men a reasonable choice to marry, the Copper and Netsilik, with an already decreased pool of women, had to draw narrower incest lines. And finally, in order to tie female infanticide into the greater ecological harshness of the Copper and Netsilik environment, Damas followed the same earlier authorities whom Freeman later (21) brought into question (see above).

Damas' original intent to document the effect of ecology on social structure is not very successful, as the weakness of the second example shows. However, there is not room here to reproduce all the intricacies of his discussion. His study stands as a masterfully well-considered holistic consideration of the interrelationship of environment, subsistence, and society, and it has been strengthened considerably by the simultaneous consideration of three similar societies.

_Cree and Ojibway._—Rogers (45) examined the ecological systems of Cree and Ojibway hunting groups in the eastern sub-Arctic of Canada. From the Cree in the east to the Ojibway in the west, the population density shows a regular decline. This seems to be a reflection of the fact that the western part of the Laurentian upland of Quebec and the Hudson's Bay lowland, the region of the east, have considerably less food and resources than the Laurentian upland of Ontario, to the south and west.
But despite the variation in overall population density, the sizes of actual social units (the hunting and gathering bands) remained fairly constant across the entire region. Rogers suggested that while the group size in the east may have been limited by the scanty resources, the richer resources of the west could allow larger bands among the Ojibway. He suggests further that the environmental variable is overridden by a cultural variable, witchcraft. Although the Cree knew witchcraft, it was of little importance to them. However, the Ojibway showed a high level of intragroup conflict of all sorts, and especially witchcraft. In a further comparison, those Ojibway who have moved from the boreal forest out onto the plains to the west seem to have lost the intense witchcraft pattern. To generalize then, it is suggested that among the Ojibway, where larger groups were advantageous in subsistence exploitation (the plains), intragroup hostilities, especially witchcraft, were less; where smaller groups were at least not disadvantageous (the boreal forest), "atomism," supported by strong witchcraft patterns, prevailed. Further, within the boreal forest, where resources were scarce, groups were kept small by direct environmental pressure; where resources were more plentiful, the cultural factor of witchcraft which ultimately was itself a reflection of environmental factors, inhibited the formation of larger groups.

In short, although Rogers was still fighting the bogey of "environmental determinism" in saying that "no strictly environmental explanation" could account for the variation in social organization in the eastern sub-Arctic, it turns out that his cultural factor, witchcraft, seems to reflect environmental features. It is not a matter of choosing either environment or culture as a causal agent, however, and his paper demonstrated well the complexities of the system. It also allows one to suggest profitable future research. Is it really true that Ojibway groups moving from boreal forest to plains would "lose" witchcraft? Is witchcraft peculiarly Ojibway, or would Cree moving to the richer areas of the West intensify their witchcraft? (In other words, is witchcraft a potential which is realized only under favorable ecological conditions?)

*Mbuti pygmies.*—Although the general trend of research indicates that subsistence activities and social structure reflect varying adaptations to varying environments, it is not surprising to find quite significant social and subsistence variations within the same environments. Turnbull's analysis (52) of the Mbuti pygmies of the Congo rain forest presented a dramatic example of this.

Although the Mbuti all lived in a remarkably uniform tropical rain forest environment, half of them were net hunters and the other half archers. The respective social groups reflected adaptation to the differing demands of the hunting technologies: larger groups (6 to 30 nuclear families) were needed to provide enough nets, net handlers, and people to drive game into the nets; on the other hand, for hunting with bow and arrow, the optimal size of the hunting group was three men. But most remarkable was the behavior of the two kinds of hunting groups during the honey season. The net hunters con-
sidered the honey season as a time when game was plentiful, and since therefore large net groups were not necessary, they split into temporary smaller groups.

The archers considered the honey season as a time when game was poor and when they must form larger cooperative hunting groups. Turnbull suggested that for each the honey season provided an opportunity for brief social adjustments, but adjustments which fulfilled opposite needs. For the net hunters, whose technology dictated that they live in larger groups for most of the year, the honey season was a time when social pressures and interpersonal antagonisms could be eased by separation; for the archers, whose technology made them live in small isolated groups most of the year, the honey season was a time to refurbish the broader social network, and to redefine territorial boundaries. Thus again we see the interrelationship of the ecological system. For most of the year, Mbuti live in groups which reflect their subsistence adaptation, with the honey season functioning as a time of adjusting or easing problems inherent in the different social patterns.

Turnbull, like Rogers (above), set his debate into a framework which took a stand against environmental determinism. It is clear that this is unnecessary. Turnbull's own data shows that one must view the different Mbuti groups in terms of the complex systematic interrelationship in which the environment plays a role, although of course not the sole causal role.

_African pastoralists._—In Great Britain the holistic ecological approach has not flourished. One can point to a few important landmarks—the early textbook _Habitat, Economy and Society_ (subtitled _A Geographical Introduction to Ethnology_) by Forde (18), whose background was in geography before he turned to anthropology; and Evans-Pritchard's first book on _The Nuer_ (15). But these are exceptions in the last 40 years of British social anthropology. Recently Forde, in his 1970 Huxley Memorial Lecture, presented an extensive programmatic statement on ecological theory and coupled it with a specific comparative analysis. He emphasized the necessity to include "exogenous factors" in an analysis of social structure, and in particular "the body of known and available resources and means for providing and utilizing material goods of all kinds" (19). He developed at some length what we have been referring to as the ecological or cultural materialistic approach, concerned with Steward's cultural core or Helm's exploitative technology. The only real surprise is that he ignores Steward, Helm, Harris, Geertz—indeed, all American contributors to this line of thought.

The substantive part of Forde's paper is an attempt to demonstrate that among some pastoral peoples of East Africa, the extended agnic system is a response to different environmental factors. For example, as the Nuer moved eastward into an area which demanded longer yearly transhumance between wet season villages and dry season pasturage, more extended agnic ties were more adaptive in uniting people against external enemies and in preventing internal fission. On the other hand, the Karimajong tribes, living in small,
dispersed settlements because of the harshness of the environment, have relatively little emphasis on lineage depth, and resolve internal conflicts through nonlineal institutions of age and generation sets.

Forde specifically framed his ecological argument as an alternative to Sahlins' earlier (46) treatment of the same problem. But it seems quite unnecessary to insist that external social conflict (Sahlins) or environmental factors (Forde) provide alternative explanations. From a holistic viewpoint, both factors are logically part of the complex Nuer system. Even more important, the issues raised by Forde are based on empirical considerations which demand more precise data. The Culture and Ecology in East Africa Project (cf Edgerton 14) promises solid comparative data on pastoral and farming societies in the area.

ADVANCES IN DESCRIPTIVE TECHNIQUES

Some of the most important work done in the last few years has been focused on the understanding of specific systems. Of course, standard ethnographic replication studies continue, each one adding a bit more to our knowledge of the world's societies, each one filling in a piece of the global ethnographic map or a row of Murdock's ethnographic atlas. But essentially more significant are a few methodological or substantive advances. These are seminal, or perhaps one should say fecund, for they may and should influence future research.

DEMOGRAPHIC MEASURES

An example of this is new thinking about the interrelated notions of population density, carrying capacity, and population pressure. As ethnographic measurements they are trivial data, but to have meaning for comparative studies, they must have a greater degree of refinement than has been common.

Population.—Population density is superficially a simple measurement: it is the number of people per square unit (acre/hectare/square mile/square kilometer) of land. Counting people is fairly straightforward, even though the difficulties of making a accurate census, both in tribal societies and in nation states, are well known (cf van de Kaa 53). But most comparative uses of population figures have some concern with social density or energy use. And for these purposes each individual human being is not equal to each other human being. Conflict potential, energy consumption, and energy production all vary drastically across the age range of a population. The ratio of dependent, nonproductive youth to independent, fully productive adults may be a crucial variable. The nature of the age distribution of a population must be taken into account. Sahlins, for example, in his recent fine-scale analysis of household economy used adjusted figures for consumption such that an adult male counted as 1.0 units, preadolescent children as 0.5 c.u., and women as 0.8 c.u. (47).
However, it may be desirable for some analyses to go a step farther. Rogers, in the study mentioned above, based his argument on different compositions of population pyramids on the ecological grounds that smaller units (children) are less efficient than larger units (adults), and be concluded that a lower ratio of children to adults was more efficient (adaptive) than a higher ratio.

Area.—The second part of the population density calculation, land area, is likewise more complex than it first appears. Any population uses different sorts of land in different ways with differing intensity. In the New Guinea Highlands, for example, some land is under crops, while other land is between crops in some sort of fallow, and still other land is in virgin forest. Each type of land is essential. But how much should be counted in a calculation of population density? A Pacific coastal community may use the resources of farm, bush, lagoon, and sea. What is to be counted? Are the few acres under coconut palm to be lumped in with the hundreds of square miles of fish-rich ocean?

The somewhat analogous problem is well understood in nutritional studies. The balanced range of a diet is considered, and no nutritionist would be satisfied with a dietary description which was reduced to the figure of gross weight of daily food consumption. Likewise, in calculating population density in New Guinea, the extensively exploited forest acreage cannot be simply added to the intensely exploited farm acreage, nor can it be omitted.

The measure of population density is not necessarily a spurious statistic, but it is certainly too complex to be expressed by a single number. Rather it can be represented by the relationship between population size (developed from a weighted population pyramid) and land area (developed from an analogous weighted and differentiated land use “pyramid”).

Carrying capacity.—When population density has been utilized in analysis, it is usually done in connection with a judgment about “population pressure,” or “carrying capacity.” John Street, a geographer, has challenged the validity of these concepts (51). If carrying capacity implies anything, it suggests that at some point the population will reach a size where the resource base will start to deteriorate. Most frequently this means soil degradation resulting from overly intensive agriculture. But Street argued that anthropologists have not paid sufficient attention to the factors which constitute soil degradation, that they assume some ideal balance, which usually reflects observed land use patterns, and they assume that more intensive use will result in degradation. It is worthwhile quoting Street at some length:

Determination of that fallow period which allows maximum population density consistent with maintenance of soil resource is difficult if not impossible. Deterioration of the land is a cumulative process and short term changes may be so slight as to be exceeded by errors in measurement (51). Much research is still needed to indicate the effect of the shifting cultivator on his environment:
measurement of erosion, measurement of leaching, measurement of yields under diverse systems of land management and in various environments. To date anthropologists and geographers have confined themselves to field studies with relatively short periods of direct observation, while experimental studies of tropical subsistence agriculture have been few and often ill-structured. To achieve progress it is necessary to conduct experiments on replicated plots, to control variables other than those under investigation, to continue observations over a long span of time, to describe the experiments in sufficient detail so that other workers in the field may properly evaluate them (51).

There is no question that populations fluctuate, and that this fluctuation results in varying degrees of population pressure, which may be more or less apparent to the people involved. There is general agreement that change in population size will have some behavioral results. Not surprisingly, geographers tend to focus on technological responses, while anthropologists perceive social responses.

Vermeer (57) described the Tiv of Nigeria, whose population varied from 91 people per square mile to 600 per square mile. He observed that in those regions where the population density reached 400 per square mile, the traditional crop rotation system yielded to a new system. Yams, which were important as a foodstuff and played a major role in ceremonial contexts, were replaced as a first crop by sweet potatoes and peanuts. This is course is consistent with Boserup's thesis, discussed above. But the next question, which is obvious to an anthropologist at least, concerns the effect of this agricultural (and presumably dietary) change on Tiv ceremonial and social behavior.

Among anthropologists, Vayda had emphasized the function of tribal warfare as a response to population pressure, both through homicide and through redistribution of population (54), and Kelly (35) summarized and reexamined the arguments concerning "demographic pressure and descent group structure in the New Guinea Highlands." Kelly, like Vayda, emphasized the various social strategies for land reallocation.

In a more recent reconsideration of warfare in the New Guinea Highlands, Vayda takes a considerable step towards ecological holism, and outlines the complex variety of data that will be necessary to handle the basic question, "what are the systematic consequences of population pressure on horticultural societies?" (55). The monograph by Clarke (8) and one forthcoming by Waddell (58) are good examples of the next step, which will begin to produce the sorts of data needed.

Spencer's landmark work on shifting cultivation (50) falls outside our time period, but much recent work has centered on this form of agriculture. In fact, it seems likely that for a number of reasons the most successful ecological studies will be made on societies engaged in shifting agriculture rather than other subsistence techniques. This is no general rule, of course (cf the work by Lee and others on the Bushmen). But shifting cultivation is still widely practiced throughout the tropical world by small-scale societies in gar-
dens which are small and easily measurable. Fishing technology has been least studied, perhaps for the reason that it is so difficult to examine the fishing grounds themselves.

THE IMPLICATIONS OF DIVERSITY

Botanically.—The kinds and varieties of crops have profound implications. It is now well recognized that monocultural fields are far inferior to polycultural fields in most respects. In polycultural fields the different crops form a more stable system in which the soil is less vulnerable to erosion and leaching. Igbozurike (32) and D. Harris (25) both pointed out that in many tropical regions the lure of cash crops has induced a change to the more unstable single-crop systems.

D. R. Harris (24, 25) has examined differences between the two dominant Latin American patterns of shifting agriculture, milpa (based on maize, beans, and squash) and conuco (based on root crops). Both are polycultural. Root crops are more starchy, with less protein, but take less fertility from the soil and cause less erosion. But the milpa pattern is more apt to exhaust the soil rapidly and necessitate higher mobility. In his study in the Upper Orinoco region of Venezuela, he found that major differences in burning over for the two crops resulted in differences in the necessary organic carbon in the soil. Before planting maize, the garden fires were so intense that they consumed carbon already present; manioc gardens did not demand such complete clearing, and there was an increase in organic carbon in the soil after burning. Also, the process of harvesting the manioc tubers churned more organic carbon into the soil.

But this is only part of the story. Because of the low protein content of root crops, groups dependent on them must usually supplement their diet with animal or fish protein. Denevan (13) studied different Campa groups in eastern Peru who grew mainly manioc. Their protein came primarily from game, and the rapid depletion of game in an area kept them seminomadic. In other words, the lack of a stable source of protein from domestic animals or fishing or rich game overrode the ecological stability of root cropping.

Culturally.—The discussion of polycultural fields usually centers on the mix of different species, but the more important species ("the staples") are typically represented by a large number of varieties. Yen (59) pointed out that mixed varieties of sweet potato result in staggered harvest and give a better ground cover. He has also found that different varieties of sweet potato have different reactions to fungus disease, and although this "does not indicate a selection for resistance . . . the mixed variety (and species) stands may actually help in reducing, while not suppressing, disease incidence" (59).

Johannessen (34) made an intensive study of Musa (bananas and plantains) in Central Africa, interviewing over 100 persons in 6 different cultural entities. He found that farmers often have as many as 8 varieties of banana and 4 to 13 varieties of plantain, and that there was a conscious attempt to
grow as many varieties as possible for protection against "disease, pests, or meteorological catastrophe."

Johannessen found that varieties flowed along the kinship networks: each farmer obtained more varieties from his father than any other source; next in donor frequency were wives' relatives; and then a variety of close cognates. There was a strong tendency for free exchange of varieties: of 708 transactions, only 13 (2%) involved any barter or payment.

_Cognitively._—But the great proliferation of varieties raises questions about cognition which are rarely mentioned in the literature. Heider pointed out (29) that descriptions often state that there are dozens of varieties of a food plant, without showing how the people distinguish such a great number of subcategories, or even whether the total number may not be a composite of many different individuals' knowledge. Heider showed that the Grand Valley Dani of West Irian "have" more than 70 names for sweet potatoes, but that no individual knows that many, and that there was great disagreement in the application of varietal names. Pardee (42) demonstrated a similar variation in the use of names for wild yams by the Bellona Islanders (Solomon Islands), although Pardee's data may be in part explained by the fact that wild yams had not been harvested for food for 30 years.

**SPATIAL STUDIES**

An important quantitative methodological refinement which will immeasurably aid in understanding agricultural systems is locational analysis. First developed by von Thünen in 1826, it is well known in cultural geography [cf Brookfield's summary of its implications and applications (6)]. Basically, locational analysis is concerned with spatial relationships between resource production areas (gardens) and consumption areas (villages, markets) and the effect which this has on work and value of product. Clarke, in his study of a New Guinea Highlands group (8), sketched a locational analysis with some examples of individual men's garden holdings. He suggested some social and economic implications of the wide distribution of gardens, mainly having to do with various sorts of information which a man gathered in the course of walking to his gardens.

Blakie's study (4) of agricultural decision making in four north Indian villages was an exhaustive comparative application of von Thünen's basic concept, although he found that he had to consider many factors other than location alone in order to account for the systems in the various villages.

Blakie's work suggests that location analysis might be a powerful tool for the descriptions of an agricultural system. But the implications of the approach for anthropology have not been realized, except for the related central place analysis of market networks (Skinner 49; Jackson 33).

**ENERGY STUDIES**

Studies of energy use, including nutritional input and work output pose especially difficult technical problems. To monitor the eating and work pat-
terns of even a small group is a monumental task. Sampling techniques are required, but the samples are small and the effect of error is apt to be serious. A very few energy studies have been attempted so far, but unfortunately there has not been enough explicit discussion of precisely how the data were obtained and what degree of confidence can be placed in them. This is a criticism which can rightly be leveled against much anthropological data. But because of a certain aura of numbers themselves, quantitative data seem especially compelling.

To be specific: I know from my own work among the Dani of West Irian (Heider 30) how difficult it would have been to obtain satisfactory records of work input and tuber output for a single sweet potato garden. Planting and weeding are done by women in time segments ranging from 10 minutes to an hour or two, and often when the women are casually stopping off at one garden on their way elsewhere. Harvesting takes place equally casually: sweet potatoes are taken from any one garden area every day or two over a period of several weeks. I could conceive of obtaining accurate data only by spending an immense amount of time personally watching individual gardens. The Dani may well be more indifferent, uncooperative, or greedy than other Highland Irianese and New Guineans. But when I read of quantitative reports of sweet potato harvest in other parts of the Highlands (e.g. Clarke 8; Rappaport 43, 44), my admiration is tempered by a desire to know more about the data gathering procedure.

Lee, especially in his study (39) of the effects of birth spacing on a Bushman women's load, has been more explicit about the source of data than most. Other important recent studies of energy input and output have been done on Eskimo subsistence activities (Freeman 20 and Kemp 36).

Foote and his colleagues in the Environmental Systems Group of the McGill University Geography Department have studied four Eskimo groups in an attempt to develop a systems analysis of Eskimo use of "space and energy sources" (Foote 16; Foote & Greer-Wooten 17). The project was broadly interdisciplinary and gathered extensive data on physical, biological, and social-cultural subsystems. Preliminary reports told how data were coded and utilized in a massive "systems simulation program":

For example, variables recorded on summer seal hunts have included wind speed, wave height, spotting range, seal behavior, hunting equipment, travel speeds and shooting success. These have been combined to provide a quantified measure of probable hunter success. In this instance, data from Alaska and Arctic Canada show such remarkable statistical similarities that one can predict the probable hits an Eskimo hunter will make given a reasonable number of attempts by a skilled hunter under any one of several environmental situations (16).

Another aspect of the simulation program has been to simulate decision models in hunting.

Sahlins (47) has examined energy use not as a relationship between environment and subsistence activities, but as a relationship between subsistence
and society. In an important exploratory essay he examined the implications of Chayanov’s findings to “primitive” horticultural societies. Chayanov, in research on peasant household economies, had found that the intensity of an individual’s work effort varied inversely with the number of people for whom the individual was providing. The greater the ratio of workers in a household, the less any worker would produce and the further that household would be from fulfilling its production potential. Sahlins pointed out that intuitively this is consistent with the folk wisdom of “imperialist ideology,” that the natives are lazy and won’t work to better themselves. But a perfectly “rational,” nonsurplus economy is nonexistent. Sahlins used quantitative data from a few tribal societies to investigate the “social inflection of the Chayanov slope”—that is, the way in which political, social, and ritual factors or motivations caused different households to raise or lower their production. Sahlins’ results were highly suggestive and he concluded with optimistic warning: “The difficulties of this work are only exceeded by the promise of a new economics, distinctly anthropological, and more indebted to the structures of the societies with which it deals than to the categories of the one from which it comes” (47).

CONCLUSION

This review has focused primarily on the nature of a systematic, or holistic, approach to the description of societies. This approach considers societies in terms of the interrelationships of their physical environment, their subsistence technology, and their social organization. A variety of studies have been discussed which attempt to synthesize different systems, and which introduce refined techniques for describing ecological factors. The next years should see (a) more holistic descriptions of societies, based on better data; (b) the full publication of a number of major interdisciplinary studies carried out during the 1960s on, among others, the Bushmen, Eskimos, East Africans, and New Guinea Highlanders; and (c) more substantive findings concerning the regularities in the relationships between environment, subsistence, and society.
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ETHNOHISTORY: A REVIEW OF ITS DEVELOPMENT, DEFINITIONS, METHODS, AND AIMS

ROBERT M. CARMACK
Department of Anthropology
State University of New York, Albany

HISTORY AND ANTHROPOLOGY

The relationship between history and anthropology has too often been viewed in terms of the misleading dichotomy between the ideographic, the specific and unique, and the nomothetic, the abstract, and general (see, for example, Boas 13; Harris 40; Kroeber 46; White 102). Nagel (67), who traces the dichotomy back to Aristotle, has convincingly demonstrated the use of generalizations and specific cases in both history and science, and Sahlins and Service have done a similar thing through their analysis of specific and general evolution (75).

The early evolutionists were not historical in the ideographic sense but they were in the diachronic and documentarian senses. The founding fathers of anthropology—Morgan, Tylor, Maine, Marx, and others—sought to find the origins and antecedents of sociocultural systems and to trace their evolution through time. They based their studies on documentary accounts about native cultures written by travelers, missionaries, etc. As Kroeber repeatedly claimed, the historical approach was precociously applied in the social sciences, compared to the biological and physical sciences (47). The delayed development of a historical approach in the other sciences, Kroeber argued (48), was the requirement of a prior development of systematic, synchronic generalizations.

This suggests one explanation for the patently ahistorical phase which followed the evolutionary beginnings of anthropology—viz. the need for detailed, synchronic studies. The aversion of Radcliffe-Brown and Malinowski to historical studies is well known. The structure-functionalism of British social anthropology was ahistorical on all counts. In the study of the structures of whole societies it was nomothetic, in the almost total reliance on ethnographic observations it was antdocumentarian, and in failing to study social change it was synchronic.

In spite of Harris’ recent attempt to portray the American culture historians’ work as exclusively ideographic (41), there is good reason to view it as ahistorical in certain fundamental ways. Certainly the culture historians cannot be accused of failing to make diachronic studies, as under Boas’ guidance
culture traits were studied in terms of their diffusion through time between adjacent cultures. But Kroeber insisted that (46) by studying process, diffusion process, they were missing the essence of history, which is “descriptive integration.” It must be admitted that a view which conceived of culture traits diffusing mechanically within culture areas along fixed vectors (age area) does seem far more nomothetic than ideographic.

Kroeber also noted the culture historians’ (46) general failure to use documentary sources. Voegelin (99) reminds us that Sapir devoted only 5 of 86 pages in his famous essay on Time Perspective to the direct historical method. Yet we can speak only of tendencies, for, as Fenton (33) and others have pointed out, among the American culture historians there were pioneers in the use of documents (e.g. Swanton, Speck, Dixon, and Keesing). Still, as with their British counterparts, ethnographic fieldwork was the sine qua non of anthropological research. In part this was a reaction to the rapid disappearance of native cultures, but it must also be seen as a distrust of and disinterest in historical sources and problems.

Historical trend.—There has been a pronounced shift of interest in anthropology toward history since the end of World War II. Mention has already been made of Kroeber’s early views on the subject, and in a series of essays in the late 1950s he continued to discuss the topic (47). He argued for the basic similarity between history and anthropology since both develop only fluid and weak generalizations with few causal statements, both study civilization as a tradition of historically accumulated patterns, and both are natural rather than experimental in their basic approach (49).

In 1945 White argued that the historical approach, the study of the origin and diffusion in time and space of cultural forms, was itself a kind of “sciencing,” but to be contrasted with the functionalist approach, also scientific, which focuses on how cultural systems work as a result of the varying contributions made by its different elements. A third approach, evolution, he believed to be the most fruitful of scientific theories. This did not mean, however, that we should ignore history. In fact, it was his argument that evolution was a process by which “an organization of functionally interrelated elements is temporally transformed” (103), and hence its explication depends on the combined approaches of history and functionalism.

Eggan (28) argued a similar thing, without the evolutionary connotations of White. He called for a synthesis of the structure-functionalism of British social anthropology, and the culture historians’ interest in temporal “process.” This could be done through what he called “controlled comparisons,” where structure-functional systems are placed in limited geographical, cultural, and historical frameworks. Eggan was only stating a position already largely adopted in ethnology, as may be seen from Lewis’ analysis of comparative studies in anthropology between 1950 and 1954. Lewis (57) discovered that comparisons between historically related societies—i.e. those found within a single community or in the same culture area or nation—
made up about 60% of the cases, versus only 40% of cases where this control was not used.

A simultaneous historical trend was taking place among the British social anthropologists. Evans-Pritchard (30), influenced mainly by the work of social historians like Maitland, Vinogradoff, Bloch, and Weber, warned that the very concepts of structure and function could be demonstrated as valid only through historical studies. What is needed, he said, is an enlightened combination of specificity and generality, so that analysis becomes neither so general as to be valueless (as he claims has happened with such concepts as taboo and lineage), nor so specific as to be trivial (which is too often the case with the writings of historians).

Evans-Pritchard, like Eggan, was reflecting a view held by many of his colleagues. The appropriate field of study for most British social anthropologists had become a structure-functional unit over time (Gluckman 38; Smith 81; Southall 82), and numerous studies of structural systems were being analyzed diachronically (e.g. Barnes 4; Fallers 32; Smith 80).

Herskovitz (42) has called our attention to the link between acculturation studies and renewed historical interests. By turning to the relationship between the dominant and subordinate cultures of colonial societies, both American and British anthropologists were forced to study cultural dynamics, "the mechanisms that had brought about the observed results in the institutions and beliefs of peoples who had been in contact." Fortunately, functionalist concepts were available to provide the framework for such refined studies, but they required intricate detail of relationship which the old methods of "inferential reconstruction" through trait analysis could not provide (Leacock 50; Voegelin 99). There was no other recourse but to turn to the documents in order to obtain such detail. In the phrase of Herskovitz, "historical reconstruction has given way to ethnohistory" (42).

Definitions.—The historical interest in anthropology has stimulated both anthropologists and historians to clarify the similarities between the two disciplines. In the application of both rules of enquiry and interpretation, the historian follows the canons of the scientific method. As noted by Swartz in an important essay (90), not only does history have much science in it, but so do the descriptive integrations of culture historians like Kroeber. Avoiding causal statements only prevents them from empirically testing their generalizations and unstated assumptions about "patterns."

Anthropologists as divergent as Lévi-Strauss (54), Evans-Pritchard (30), and Kroeber (47) have insisted that history differs from anthropology mostly in orientation rather than in aim. Indeed, the two fields overlap in so many ways that their differences are often expressed in terms of style rather than method and purpose. Historians study written documents, it is stated, anthropologists study living peoples. The extensive use of documents by anthropologists, however, betrays this supposed difference. Historians seek literary quality in their writings, while anthropologists are primarily concerned with sci-
ence in their statements. This “difference” is denied by the observation of recent students of anthropological method and theory who state that there is a pronounced humanist, literary aspect in the writings of anthropologists (Manners & Kaplan 61). Historians make ethical judgments about the subject matter they study, while anthropologists only analyze cultures. Recent discussions in anthropology (see volume 9 of Current Anthropology) make it abundantly clear that this has not been the case for anthropologists.

The view, stated well by Taylor (91), that history does not involve the comparison and generalization of cultures and societies, is probably unacceptable to most historians today. A more acceptable thesis today would be that the explanatory generalizations of history are more particularistic and implicit than those of anthropology and other social sciences: “In history, the stock of explanatory generalizations is given as primitive concepts; in science, the search for increasingly broad explanations is the characteristic preoccupation” (Spaulding 85). Perhaps we should agree with Mills (63), that more important in the long run than whether or not historians are social scientists is the point that the social sciences are “historical disciplines.”

**Defining Ethnohistory**

A field of study which might be termed “ethnohistory” has yet to be defined. In fact, the derivation of the term itself is not known for certain, and its meaning varies widely from one context to another. In a series of essays discussing the concept, published as volumes 8 and 9 of the journal *Ethnohistory* (1961–62), most of the authors emphasized that ethnohistory is a method or technique, not a discipline (Fenton 34; Leacock 50; Washburn 101). The essayists proposed that ethnohistory might serve as a means for combining the generalizing aspects of ethnology with the careful evaluation of sources and interest in time sequence of history (see also Lewis 56; Nicholson 70).

Sturtevant (89) suggests that there are three basic “dimensions” which probably would be widely accepted as generally characterizing ethnohistory: its focus on the past condition of cultures; its use of traditions, either oral or written, as the primary data source; and its emphasis on change over time in the cultures studied (diachronic dimension). It is noteworthy that except for the second dimension (use of traditions), these are precisely the characteristics used by Binford (8) to define the field of archaeology. In fact, the problem of defining ethnohistory as a field or subfield of anthropology is similar to that with which archaeologists have wrestled. They too recognize the historical side of their work, for they study the past, have a dominant interest in chronology, and produce mostly particularistic constructions (Spaulding 84). Yet their ultimate aim is generalization and explanation of cultural development (Binford & Binford 9; Taylor 91), and for that reason archaeology remains a subdivision of cultural anthropology. Most of them, therefore, conclude that archaeology “is just a special method and set of techniques” for recovering and analyzing a certain kind of data, i.e. artifacts.
Theory.—It seems likewise clear that there is no ethnohistoric theory independent of other theories in cultural anthropology. Further, the use of historical methods and sources for theoretical or explanatory purposes on both logical and practical grounds would seem best not subsumed under any subfield called ethnohistory. For example, the diachronic test of theory is of such broad significance as to take us beyond the limited designs of most ethnohistoric studies. Increasingly it appears to us as a fundamental part of any theoretical approach, and hence a component of such basic epistemological procedures as classification, comparison, generalization, and model building. For this reason, we find functionalists arguing that they must trace equilibrium-maintaining systems over time if they are to validate the claimed functional explanations of institutions (Cancian 19; Evans-Pritchard 30). White (102) and other evolutionists have made it clear that their explanations depend on demonstrating that systems of culture change over time, specifically in ways which are adaptive to the ceaselessly changing energy relationships between man and his environment. Harris (40) has correctly defined the Marxian conflict theory as involving an interrelationship between techno-economic, sociopolitical organization, and ideology which changes through time as the result of class conflicts. As an anthropologist interested in psychology, Spiro (86) sees changes in cultural systems over time as both being caused by the need to express psychologically repressed forces and as causing changes in the personality structures of individuals. The pioneer structuralist, Lévi-Strauss, long ago stressed (54) that students of the unconscious structures of cultures must have recourse to history in order to reveal the constancy of these structures “throughout a succession of events.”

It would be unwise to label the theoretical use of history by such a wide variety of anthropologists as ethnohistory. Their inclusion of a historical dimension is basically a recognition of the dynamic nature of social living and the need to build change into their explanatory models. This diachronic trend will undoubtedly continue, and perhaps “social change” will eventually cease to be isolated as a subject for specific analysis as is now done. Possibly the ethnohistoric studies to be discussed below will increasingly provide the historical methods, data, and interpretation of data for use in more dynamic theories of the future.

Evans-Pritchard’s suggestion (30) as to why the historical method has been uniquely important in theoretical work is still a valid one: it allows us to both detail specific changing variables (because of the time depth) and to control our comparisons of variables (because the same cultural tradition is under study). While anthropologists have achieved useful results by comparing roughly contemporaneous cultures aligned in developmental sequences presumed to be equivalent to historical sequences (Fried 35; Murdock 65; Service 79), the over-reliance on material factors, and the extreme generality of the findings suggest that there are important advantages to using the historical approach.

The use of history as a mode of explanation, the so-called “genetic” ex-
planation, is related to theory in another way. According to Brown (18), genetic explanations in the social sciences are distinguished by their particularity. They consist of "explaining" social institutions by specifying the purpo- sive acts of individuals at certain times and places which bring into existence the behavior or institution under investigation. In the definition of historical explanation given by White (102), institutions need not be linked to individuals and their purposes, but as disembodied patterns they are historically explained when their origin and subsequent diffusion in time and space are described.

That the clarification of some act or institution in this way should constitute an explanation is probably stretching the meaning of that term beyond epistemological usefulness. To know the origin and subsequent diffusion of an institution can be important, but not because the latter is explained in that way. So-called genetic explanations are much like Merton's "manifest" functions of institutions: they are not explanations, but are themselves components of the structure of these institutions. Just as we feel the Hopi snake dance is not explained by the statement that it functions to bring rain (Merton 62), so too we feel the stock market crash is unexplained when it is re- cited to us that certain stockholders met at a certain date and decided to withdraw their money from the exchange (cf Brown 18). We are left with larger questions unanswered: What general and even unconscious factors cause the Hopi to dance for rain, or cause brokers to withdraw their money on certain occasions? It is precisely the process of going beyond the narrative and reportorial approach in providing answers to important social problems which requires the "sociological imagination" that should characterize the social sciences (Mills 63).

We will conclude with Nagel (67) that history uses rather than generates "general laws." "Rather than explaining something as a 'persistence from the past,' we ought to ask 'why has it persisted?" (Mills 64).

Methodology.—We are left to conclude that, like archaeology, any exclusive definition of ethnohistory depends primarily on methodological considerations. Ethnohistory is a special set of techniques and methods for studying culture through the use of written and oral traditions. As methodology it is complementary not only to archaeology, but also to historical linguistics, ethnography, and paleobiology.

Unlike other subfields such as archaeology, however, ethnohistory can make no claim to unique or special techniques independent of history. In using the documentary sources, our methods differ in no way from the techniques of source preparation and criticism employed by historians. In fact, as far as the methods of source criticism are concerned, it seems painfully obvious that anthropologists using documents have much more to learn from historians than vice versa. Nicholson (71), Evans-Pritchard (30), and Sturtevant (89) have generally castigated anthropologists for uncritical use of documentary sources. As noted by Nicholson (71) for Mesoamerican documents:
the usual questions must be asked of each source: who wrote it, why, when, and where—and it must also be carefully compared with all other relevant sources. Although this undoubtedly sounds commonplace, particularly to any trained historian, anyone familiar with the pre-Hispanic branch of Mesoamerican ethnohistory is well aware how rarely it has actually been accomplished in practice.

Evans-Pritchard sees our deficient use of source criticism as carrying over into other aspects of our research. Ethnographic monographs are documents too, he reminds us, and we have trusted them far too uncritically (30). Many ethnographies fail to provide us with an evaluation of the sources of information from which the descriptions were taken. It is not necessary that we know the names of informants, but we should at least be given information about their sociological positions, their relations to power, and personality factors which might uniquely influence their reports. The same information about the ethnographers recording the data is needed.

Often it is argued that ethnohistoric methodology is distinguished by its eclecticism: documentary analysis is combined with archaeological, ethnographic, linguistic, and other kinds of data-gathering methods. Hence, its cultural reconstructions of the past are broad when compared with those of history. While it is true that anthropologists are especially prone to combine archaeological and ethnographic data with their use of documents, historians also have a long tradition of such "multi-disciplinary" approaches to the study of the past. Bloch, who has written perceptively on all of these questions (10), emphasized the importance of studying "tracks" left by peoples of the past—artifacts, old customs, archaic words, etc. Further, he gave it as a basic maxim that only through understanding social life of the present can we hope "to derive the elements which help us restore the past" (11). He advocated viewing history "backwards," when possible, by starting with the present and working back in time.

As might be expected, anthropologists have insisted that the historian who has also done ethnographic fieldwork with the people being studied has a crucial advantage. It is claimed that he is in a strategic position to evaluate the reliability of the historical sources, for his work with informants from that culture and his knowledge of similar cultures enable him to understand their biases and distortion (Lurie 59; Sturtevant 89).

Surprisingly, anthropologists with ethnographic expertise have often applied their knowledge to the study of written documents only, to the neglect of oral traditions. Posnansky (72) relates that he was discouraged from initiating historical studies of pre-Colonial African cultures because most tradition from there was in oral form. He found that social anthropologists carrying out field studies in Africa tended to use only those oral traditions which related to their theoretical models (of functionalism). They generally lacked training in historical methodology, and not only made little attempt to answer historical questions, but often ignored the historically most significant traditions.
It is significant that it was a historian and not an anthropologist who finally provided us with a serious discussion of the kind of source criticism which must be applied to oral traditions (Vansina 96). Vansina convincingly argues that if oral traditions are studied in their functional contexts, source criticized, classified as to historical type (commentaries, tales, lists, poetry, formulae), and checked against data from archaeology, linguistics, ethnology, and physical anthropology, they can be made to yield probability interpretations of the same general approximation as all historical "truths."

The integration of oral and written historical sources with archaeological data in order to generate research problems has become known in anthropology as the "direct historical method." It is a procedure so extensively and successfully employed in Near Eastern studies that history and archaeology there are often subsumed under the same term (as either "history" or "archaeology"). Those same studies, however, reveal some of the problems associated with their combined use which have made anthropologists somewhat hesitant to employ the method extensively. For example, many specific interpretations of the archaeological record made by Near Eastern specialists seem speculative and susceptible to acrimonious debate (e.g. was there really an Abraham or Homer, and if so, at what particular times, and in what places did they live?)

While remaining skeptical, anthropologists have nevertheless engaged in considerable direct historical research, especially in areas where the native populations had strong historical traditions (Mesoamerica, Islamic Africa, West and Interlacustrine Africa, parts of Polynesia). Even for North America, Steward could report in the early 1940s that considerable direct historical work had been done on aboriginal cultures in the Southwest, Northeast, Great Plains, and southeastern United States (88).

Since Steward wrote, it is my impression that the direct historical method has been poorly exploited by anthropologists. Recent theoretical works on archaeology almost completely ignore the topic (Binford & Binford 9; Taylor 91; Willey & Phillips 105), and I know of no good recent general statement on the problems, aims, and implications of it by either ethnohistorians or archaeologists. Hopefully, this lag only represents a transitional period during which archaeologists and ethnohistorians are busily gathering, classifying, and analyzing their data in preparation for new syntheses which will soon follow. This at least is what happened in the case of Near Eastern studies (Albright 2). Yet some of the lag is undoubtedly the result of the failure of ethnohistorians and archaeologists to realize the increased capacity for asking new questions and getting answers to them which can come from effectively (not superficially) combining the two approaches.

To return to the problem of defining ethnohistory, we have seen that it involves a set of techniques for gathering, preparing, and analyzing oral and written traditions. The aims for which these methods are employed are those of cultural anthropology in general, and have to do with theories of culture. Therefore, ethnohistory itself cannot be considered an independent discipline.
In that ethnohistoric methods do not differ from those of history except for a somewhat greater emphasis on combining these methods with those of archaeology, linguistics, etc, ethnohistory is less easily defined as a subdiscipline than archaeology or historical linguistics. Yet many anthropologists who consider themselves to be ethnohistorians feel that it has a subject matter, however vaguely it might be defined, and that this, combined with its historical methodology, qualifies it as a subfield of anthropology.

A careful reading of the literature with this question in mind leads to the conclusion that the following subjects are the ones most often studied by ethnohistorians: specific history, historical ethnography, and folk history. These topics all involve a concern with Sturtevant's cultural reconstruction and diachronic dimensions (89), which along with the methodological requisite of using sources were said to characterize ethnohistory.

In the summary to follow of ethnohistoric research related to these three topics, I shall briefly discuss each topic in terms of the development of methods, concepts, and conclusions from ethnohistoric research on native cultures in North America, Africa, and Mesoamerica. No comprehensive summary of the ethnohistory of these three areas is attempted here, but rather, I have selected a few noteworthy and illustrative cases. Good bibliographic reviews of Mesoamerican ethnohistory are available, and the interested student should consult them (see the bibliographies in the Handbook of Middle American Indians, volumes 2, 3, 4, 10, 11; Bernal 7; Nicholson's annotated bibliographies in the Handbook of Latin American Studies from 1960 to 1970). Students of North American Indian tribes have produced a great amount of ethnohistoric research, especially since the historical impetus resulting from the Indian Claims Act of 1946. The journal, Ethnohistory, is primarily devoted to ethnohistoric studies of North American Indians, and can profitably be consulted for that purpose. For African ethnohistory, the best survey articles are by Vansina (95), Lewis (56), and Adams (1). In addition, the volumes of the Journal of African History provide a compendium of much important ethnohistoric work on African cultures for recent years.

There is a growing interest in ethnohistoric research on cultures of other areas besides these three. For example, recent ethnohistoric research on cultures of the Pacific islands can be followed in the Journal of Pacific History, while summaries of work now being undertaken in East and Southeast Asia have been prepared by Benda (6) and Cohn (25). Murra (66) has summarized the rather extensive ethnohistoric research on the native cultures of Andean America.

The Journal of Comparative Society and History, broadly representative in its areal coverage, though strongly sociological in its orientation, has many articles which fit our definition of ethnohistory. The student will find there considerable discussion of areas of traditional interest to historians, such as early Western Europe, classic Greece, Rome, China, and India. These cultures in the past have received scant attention by anthropologists though
some ethnohistoric work has been initiated on a limited scale (Cohn 25; Kluckhohn 45). The historical topic which has received perhaps more scholarly research than any other in the West, the Judeo-Christian cultural development, has been virtually ignored by anthropologists. We might anticipate that a closer integration of Biblical and classical research with anthropological studies might some day provide a strong impact on the direction and sophistication of ethnohistory.

Specific histories.—By specific history I will mean the writing of histories of specific societies in terms of their past events or culture traits as manifested in time, space, and concrete act. For anthropologists, this has generally implied the construction of past events for tribal, peasant, and other societies neglected by Western historians.

As might be expected, such histories have tended to be more social and cultural than narrative, though the latter type history has been written in a few cases. Mostly this kind of work has been labeled “culture history” since, rather than reconstruct specific names, dates, or activities, the origin and diffusion of culture traits have been traced within the context of general areas and relative times. In practice, narrative history and culture history are usually combined, the culture historian using exact dates, places, and events when they are available, and the historian trying to work out patterns of migration and relations with surrounding cultures as these are indirectly suggested by a variety of informational sources (Greenberg 39).

Until recently, specific history dominated ethnohistoric research on North American Indian tribes. This type of approach was applied by Boas, and is well illustrated by his reconstruction of the diffusion and transformation in form of folktales among the Northwest coast tribes (12). Very early Sapir abstracted the general principles and controls needed to effectively use the method and evaluate its results (76). According to a modern summary of ethnological research on the North American Indian (Fenton 33), culture history continues to be one of the main tasks of ethnohistory.

Recently, American ethnohistorians have begun to argue for a “history in the round,” in which a more narrative history of the American Indians would be written in order to counterbalance the Anglo-oriented accounts which dominate our American histories (Leacock 51; Washburn 100). A limited amount of general North American Indian history has already begun to appear (Leacock & Lurie 52).

In African studies there has been a shift away from the history of European expansion to the investigation of aboriginal, local history (Vansina 95). The Africans themselves have insisted upon the development of a history comparable to that produced by Western historians, and not merely the broad patterns of culture history (Vansina, Mauny & Thomas 98). This view harmonizes well with the social anthropologists’ general skepticism of ethnological studies, and the result has been a predominance of histories shallow in time depth and narrow in cultural scope.
The extensive use of oral traditions by African peoples has provided cases where culture historical methods can be checked against results obtained from documentary studies. One conclusion reached by African ethnohistorians is that while tribal tradition, such as contained in genealogies, can be historically misleading (Bohannan 15; Cunnison 26), it usually contains some historical reliability (Lewis 55; Vansina 96). Different segments of genealogies may have different historical validity, just as different types of societies are found to express different levels of historicity in their genealogies and other oral traditions. Of these social types, state societies are the most reliable (e.g. Southwold 83). The first steps in working out regional histories of aboriginal African states have been taken, by focusing in chronology and dynastic list comparison (see volume 11 of The Journal of African History).

What has most impressed and pleased the social anthropologists working in Africa is that specific histories have proved useful in improving functional analysis. Accurate historical reconstruction of such matters as lineage origins, dynastic successions, changes in brideprices, etc, provide a basis for understanding the relationship between social function and political manipulation (Lewis 56).

Specific history in the form of culture history has long dominated Mesoamerican ethnohistoric studies. There is both an extensive corpus of native and Spanish documents and a rich archaeology for the area, so that in no other part of the world would the application of the direct historical method seem more favorable—with the exception of the ancient Near East. As a result, Mesoamerican specific histories often manifest an enlightening blend of specific detail and general statement which tends to be lacking in similar work from other parts of the world.

Considerable narrative history has also been produced by Mesoamericanists, on such topics as the origin and development of the Aztecs, the decline and fall of the Toltecs, the formation of Maya political states, etc. Biographical history forms part of this literature, and includes accounts of prehispanic luminaries (such as Moctezuma, Nezahualcoyotl), and the Spanish conquistadors.

Most Mesoamerican culture historic studies have focused on those civilizations which reached the highest levels of cultural development—viz. cultures from the Valley of Mexico, Valley of Oaxaca, Yucatan, and highland Guatemala (Nicholson 71). The literature on these kinds of studies is too extensive to summarize briefly, though the interested student might gain some of the flavor of this work by consulting the most ambitious attempt at culture historical reconstruction yet attempted, Jiménez-Moreno's summary of the history of the major cultures of central Mexico (43).

Surprisingly little specific history has been written on the posthispanic cultures of Middle America, and the use of oral tradition for historical purposes lags behind other research. Redfield, who pioneered community studies in the area, virtually ignored oral tradition and local histories, and most of his successors have perpetuated that unfortunate trend. However, recent pub-
lications, plus conversations with younger anthropologists now working in Middle America, suggest that much more serious attention is now being given to local histories.

Historical ethnographies.—By historical ethnography I will mean the process of reconstructing past societies and cultures, whether as institutional parts or cultural wholes. This is the work which usually passes as ethnohistory in anthropology, and its popularity is directly related to the discipline’s goal of finding as wide a variety of cultures as possible for purposes of comparison. It should be remembered that historians are also actively engaged in this kind of historical research, so that there is a particularly large overlapping of the two fields in this regard.

A long series of useful historical ethnographies have been prepared on the North American Indian tribes, and many of them now enjoy classic status in anthropological literature (see the references in Fenton 33). Most of these reconstructions involved the use of “upstreaming,” where the anthropologist queries old informants who remember the time when their culture was in a more pristine state (“memory ethnography”), and combines his own observations of surviving cultural patterns to project these data to the precontact period.

A younger generation of scholars studying North American Indian cultures has turned more to the documents, partly because of the doubtful assumptions about cultural stability inherent in the upstreaming technique, and partly because of the radical transformation which modern Indian societies have suffered under the impact of industrialization. Along with this shift in method has come a shift in topical interests—away from kinship and religion toward economic and political problems. The literature is too vast to summarize here, though the direction of research can be grasped by recalling the interest generated in anthropology by the ethnohistorically raised questions about land tenure among the Algonquins, potlatch distribution on the Northwest Coast, the shift from a horticultural to a hunting technology in the Great Plains, the development of politico-religious “revitalization” movements. These same political and economic interests can be seen in a recent collection of North American tribal histories, a volume which also reveals that ethnographic reconstruction rather than specific history is now the overriding interest (Leacock & Lurie 52).

Social anthropologists working in Africa have increasingly turned their hand to reconstructing the past social life of African societies. Generally, their reconstructions have been placed in a temporal framework to facilitate comparisons between historical phases.

Though methods similar to upstreaming have been widely employed by Africanists (Vansina 96), there appears to be a growing tendency to use more documentary sources (which go back to the 17th to 19th centuries for some areas). This trend may be illustrated by recent attempts to reinterpret the functions of Nuer prophets and Zulu cyclic rebellions through references
in early missionary and traveler's accounts (Beidelman 5; Braroe 17).

The pioneer ethnohistoric reconstruction for African societies is Evans-Pritchard's account of Cyrenaica Sanusi society (29). By using both documentary and ethnographic sources, he was able to reconstruct the development of Sanusi tribal society into a more centralized political form. Since that time, many reconstructions of great sophistication have been produced (e.g. Balandier 3; Fallers 32; Smith 80).

The reconstruction of prehispanic Mesoamerican cultures through the use of documentary sources is often used to define ethnohistory for that area (Nicholson 71). We now have useful reconstructions for several of the major cultures which flourished there at contact: e.g. Tarascan, Otomi, Totonac, Mixtec, Pokoman, Yucatec, Nicarao-Mangue (for citations, see Carmack 20). Remarkably, there is still no adequate general synthesis of Aztec culture, though in several essays many aspects of that culture have been reconstructed (see citations in Nicholson 71). Further, there are many Mesoamerican cultures for which we have extensive documentary information but lack full reconstructions: e.g. Tlaxcala, Huastec, Zapotec, Quichean, Pipil.

A major weakness of Mesoamerican historical reconstruction is that many of the ethnographies now available are superficial, topical, and largely descriptive (especially when compared with their African equivalents). As a consequence, comparison within the culture area is rare, and more complete and functionally related studies will have to be made if we are to generate useful theories from the Mesoamerican data.

Historical ethnographies for the posthispanic period in Middle America are scarce, except for cases where reconstructed prehispanic cultures are traced into their colonial manifestations (Gibson 37; Scholes & Roys 77; Spores 87).

_Folk history._—The ethnohistorian studying folk history examines the view a society has of its past. This view, of course, is an integral part of culture, so that its study is actually a special aspect of ethnographic reconstruction. The study of folk history would be a literal meaning of the word ethnohistory, the history of ethnic groups or cultures, and used in this fashion it would be similar to such terms as ethnomedicine, ethnobotany, ethnomusicology, and the like.

Historians have long been interested in "historiography," in the sense of how the past is expressed in the written histories of different civilizations. Especially have they attempted to describe historiographic trends in the West, though other historical traditions associated with universalistic religions are receiving an increasing amount of historiographic attention (e.g. Rosenthal 74; Wright 106).

An aspect of folk history which has received some special attention by anthropologists is the cultural attitude a people have with regard to the passage of time itself. Whorf long ago argued (104) that there was cultural relativity on this point, and he attempted to compare the Hopi view of time and
its expression in language with our own patterns. More recently, attempts have been made to generalize about cultural ideas of time and to assign them to different levels of cultural evolution (Kirsch & Peacock 44). The timeless view of the past, characteristic of “primitive” cultures, is contrasted with the cyclic concept of archaic and historical societies, which in turn gives way to a more linear, open view as modern society is ushered in.

The study of folk history among tribal peoples raises the question of myth, legend, and their relations to history, a somewhat neglected topic in anthropology according to Evans-Pritchard (30). The old view that folk history can be classified as myths, involving nonhuman characters, legends, where supernatural persons are thought to represent distorted historical facts (Clarke & Clarke 23), and history per se is inadequate for anthropologists studying societies whose folk histories do not parallel those of the West. A reading of papers from a symposium on the study of myth held in 1955 (Sebeok 78) makes it clear that folklorists and anthropologists have generally turned away from the historical implications of myth and other folktales. Topics now being emphasized are the semantic structure of myth, its relations to ritual, its basis in psychological states, its social functions, and its use in culture historical reconstruction (see the essays in the Sebeok volume by Binney, Lévi-Strauss, Raglan, and Thompson). Still, in working out the structure of meanings which folklore holds for different peoples, some clarification of their folk history is usually made, however implicitly. Further, there are folklorists who insist on the historic element in folklore, and that it must be taken into account if we are to correctly understand the material (Dorson 27).

In general, students of North American Indian culture have paid little attention to folk history per se. Though Boas (14) had ideas about the way folklore developed out of the use of imagination in the context of everyday experiences, his primary concern was with tracing their origin and diffusion as culture traits. Lowie’s approach (58) was similar, though he was even more emphatic on the lack of a “historical sense” in tribal Indian traditions. He argued that our job is simply to accurately record their view as part of culture. Folk traditions were to be studied not as views of history but as subject matter for reconstructing history through “the objective methods of comparative ethnology.”

The continuation of a culture historical approach to folk history can be traced through Thompson’s studies of North American Indian tales (93, 94). His approach is largely one of breaking down myths into traits or component elements, and analyzing their distribution in space. The shift of interest to structural and other approaches to myth and folklore was noted above (Sebeok 78). Even more recently, attention is being given to the role of historical traditions in the American Indian’s adaptation to an ever-encroaching U.S. society (Lurie 60; Sturtevant 89).

Under the influence of social anthropology, students of African societies have devoted considerable attention to the functions of folk history. An often
stated caveat is that these functions have to be taken into account in working on historical problems. For example, Boston (16) states that "each people's oral traditions have their own kind of historical perspective and the historian's first task is to understand this perspective before trying to fit the traditions into a unilinear time scale."

When sociopolitical function and context are taken into account, different types of historical tradition can be distinguished. Thus, Vansina (97) is able to distinguish between the more complete history of a centralized state (Ruanda) compared to that of a segmentary state (Burundi), and he discusses other types corresponding to town and village organization. Similarly, many students have argued that truly historical traditions are maintained only by societies organized on a state-level (e.g. Cohen 24; Vansina 96).

On the other end of the scale, folk histories of tribal society have been shown widely to take the form of lineage genealogies, which seem to function more as supportive charters for present structural conditions than as factual accounts of the past (Bohannan 15: Cunnison 26). Often included in the accounts of tribal folk history is some description of their concepts of time. This interest owes its relative popularity to Evans-Pritchard's (31) unsurpassed descriptions of Nuer time reckoning, which, he demonstrated, was based on ecological and structural factors (lineage alliances, age grading). The situation is complex, however, and cases of tribal societies with distinct, historical traditions have been described (Lewis 55).

Studies of prehispanic Mesoamerican folk history can be referred to as historiography, since most of the focus has been on the view of the past recorded in written documents. Even though the purpose of much of this work has been simply to translate or interpret the glyphs, or, in the case of the chronicles written in latin characters, to reconstruct the history or culture of a people, the question of the societies' view of history has necessarily arisen, and has been discussed many times.

The best understood historiography is that of the Aztecs and closely related peoples from the valley of Mexico. Nicholson (68) has called our attention to the extent to which a genuine historical "consciousness" in central Mexico was dependent upon complex political developments and well-developed writing and calendric systems. Garibay (36) and León Portilla (53) have pointed to the important role played by oral tradition and memorization, even for cases of chronologized history. All seem to agree that Mexican chronicle history can be clearly distinguished from myth, though saga-like, epical tradition is common (and widespread in Mesoamerica), and is very difficult to interpret historically (as an example, see Nicholson's study of the legendary Quetzalcoatl, 69).

The Oaxacan and Yucatecan codices have been the object of much historiographic concern, though most attention has focused on the translation of the sources rather than specific history per se. Caso (21), working with the Mixtec codices, has shown that they are mainly historical, the earliest containing a date of A.D. 692. The content of these histories is primarily politi-
cal in nature: dynastic genealogies of leading communities in the Mixtec-Zapotec area (for a summary of his work, see Caso 22).

Interpretations of Maya historiography have long been hampered by our inability to read any but the calendric hieroglyphs on the three surviving codices and archaeological remains. Nevertheless, evidence from the chronicles and books of the Chilams, thought to be originally derived from codices, had led us to believe that the Maya did not have a strong historical sense. They were said to view history in a ritual, cyclic way, with events recurring every 13 katun (about 256 years), and so history and prophesy were merged into one (Thompson 92). The demonstration by Proskouriakoff (73) that the hieroglyphs on the classic Maya stelae contain historical information has revolutionized our ideas about Maya historiography, and heightened interest in cracking the glyphic code.

As the laborious task of locating, transcribing, translating, and analyzing the historical content of the huge Mesoamerican documentary corpus continues, we can anticipate more studies on the historiographic side of native cultures in that area.
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SOCIAL STRATEGIES AND SOCIAL RELATIONSHIPS

NORMAN E. WHITTEN, JR.
Department of Anthropology
University of Illinois, Urbana-Champaign

AND DOROTHEA S. WHITTEN
Center for Latin American Studies
University of Illinois, Urbana-Champaign

INTRODUCTION

A shifting emphasis from concern with social or cultural rules to an interest in adaptive strategies of human aggregates has become increasingly evident in anthropology during the past 20 years. The general theoretical basis for such work seems to lie in multilinear evolutionary theory, especially as set forth by Steward (135, 136), and discussed as “specific evolution” by Sahliins & Service (123). The fundamental problem to which such theory pertains is simply how to conceive of continuity and change within a unified frame of reference. In social anthropology there are several milestones, such as the conceptual frameworks of Firth (47) and Lévi-Strauss (92) and the ethnographies of Leach (86) and Barth (11). Alland (2) has made a major effort to consider evolutionary processes and ongoing adaptation of human aggregates within a unified scheme, which unfortunately omits most of the productive social anthropological studies. Among other things, this review seeks to redress such omission.

The analysis of social strategies is emerging as the core of adaptational...

1The actual review was undertaken under the auspices of National Science Foundation Grant No. GS-2999 as part of the conceptual refinement necessary to understand differential ethnicity as adaptive strategies in the face of national development policies in “third world” nations. The authors are convinced that a comprehensive review of this subject could encompass virtually all of the social and behavioral sciences. Where possible we have referred the reader to existing reviews of relevant literature of many crucial approaches touched on here, such as “adaptational theory,” “game theory,” “conflict theory,” “exchange theory,” “social change.” We wish to thank Paul Ballanoff for his aid in framing the review and for contributing a number of productive ideas. All faults in commission, omission, and judgment are, of course, our own.
theory. Social strategies are conceived of as dynamic individual, group, or aggregate plans of action carried out over a specified time period. They are abstractions derived from observations of social interaction and may be conscious or unconscious, explicit or implicit. Social strategies are seen as adaptive when applied to aggregate action plans in relation to external or internal exigencies or constraints (33). Productive concepts for such study have emerged from research ranging from micro-observations in small groups (142) to macro-generalizations covering many generations of community life (149) and even to epochs of human evolution (129). Contributors have used qualitative and quantitative procedures. Some employ a concept of “minimax” drawn from game theory as pioneered in anthropology by Moore (105), Barth (12), and Davenport (40), and recently given considerable review by Buchler & Nutini (29).

Such approaches imply a trend away from summary statements about societies, cultures, entities, domains, institutions, and group boundaries—all of which suggest “invariant reference points” (82, 108), homeostasis (120), and equilibrium (61)—toward a concern with variance generated by forces which lead to unending adaptation and adaptability of human aggregates in specified environments (16, 17). But we are convinced that the study of strategy does not negate a study of structure. Strategy and structural approaches are complementary, and tendencies toward the former must ultimately contribute to the latter if progress is to be made in anthropological theory. Buchler & Nutini (29, p. 6) put it this way: “Social scientists . . . fail to realize that in describing and explaining social phenomena, both the rules and the deviations from the rules are inextricably interconnected, and that one of these

We use the term “adaptational theory” to mean essentially what Steward (136) meant by “multilinear evolution.” Approaches falling within the sphere of adaptational theory are set out in the 106 essays reprinted in Y. Cohen (35). The biological models for adaptational theory are reviewed by Alland (2), who also includes discussion of literature on social and cultural adaptation. Recent work using this approach in contemporary, complex societies can be found in the monographs and essays included in Steward et al (137) and Steward (138).

In fact, one political scientist actually stated an underlying strategy as a limitation on the potential for organizational adjustment of a South Italian community, thereby removing strategy analysis from adaptational theory, and making a “strategy” synonymous with “ground rule”—i.e. a cultural constraint. “A very simple hypothesis will make intelligible all of the behavior [of the villagers] . . . and will enable an observer to predict how [they] . . . will act in concrete circumstances. The hypothesis is that [they] . . . act as if they were following this rule: Maximize the material, short-run advantage of the nuclear family; assume that all others will do likewise” (8, p. 85).

This approach was also used by Foster (55) in his “image of limited good” paper. Banfield and Foster state supposed “ground rules” as though they were “strategy rules.” In spite of the terminology used, approaches such as those illustrated here by Banfield and Foster do not fall into the sort of thinking exemplified by the various authors and theorists discussed in this review.
conceptual systems without the other will give only a partial account of the situation."

Over two decades have passed since Firth (47) established this very point. Perhaps Firth's lesson was ill-heeded, or misunderstood; or perhaps we have only recently gained the perspective necessary to return to his position. He wrote (47, pp. 39-40):

There are structural elements running through the whole of social behaviour, and they provide what has metaphorically been termed the social anatomy, the form of society. But what is this form? It consists really in the persistence or repetition of behaviour; it is the element of continuity in social life. The social anthropologist is faced by a constant problem, an apparent dilemma—to account for this continuity, and at the same time to account for social change. Continuity is expressed in the social structure, the sets of relations which make for firmness of expectation, for validation of past experience in terms of similar experience in the future . . . at the same time there must be room for variance and for the explanation of variance.

This is found in the social organization, the systemic ordering of social relations by acts of choice and decision. Here is room for variation from what has happened in apparently similar circumstances in the past. Time enters here. The situation before the exercise of choice is different from that afterwards . . . Time enters also as a factor in the development of the implications of decision and consequent action. Structural forms set a precedent and provide a limitation to the range of . . . alternative that makes for variability. A person chooses, consciously or unconsciously, which course he will follow. And his decision will affect the future structural alignment (emphasis added).

Many of the concepts found in the current literature on social strategy and social interaction are suggested by Firth's statement and lead us to modes of analysis such as those set out in generative models of social change (16), game theory (29), and network analysis (103, 153).

In rethinking relationships between social continuity and social change, many anthropologists began to confront the very notion of "interaction" as an observational basis from which second order generalizations (e.g. structure, strategy) could be built. Game theorists, conflict theorists, network analysts, exchange theorists, and those building generative models of social change began to substitute a concept of "transaction," for the long-accepted "interaction." The transaction concept takes cognizance of strategy in interaction by elaborating on the concept of prestation already well established in French structural anthropology since its original development by Mauss (98). It is perhaps unfortunate that a reanalysis of works by Mauss, Durkheim, Radcliffe-Brown, and Malinowski in terms of more dynamic concepts of the 1960s was not undertaken by strategy theorists [Singh Uberoi's work (130) is a notable exception]. Rather, literature from game theory, as produced by such authors as von Neumann & Morgenstern (111), and Luce & Raiffa (94), began to provide a basis from which many anthropologists could generate a model of social strategy applicable to an understanding of inter-
personal interaction which leads to change and social continuity of recognizable form.

This review is divided into two major subsequent sections. The first deals with social strategy as a plan of action related to an aggregate of people. The second section deals with concepts of reciprocity, dyadic ties, networks, and transactions which form a dynamic basis for the evolution of adaptive strategies. This section also includes a summary review of game theory and related concepts of exchange theory. We then focus directly on the idea of "interaction strategy," summarizing it briefly in the concluding section.

Adaptive Strategies

Webster’s New World Dictionary defines strategy as follows: “1. the science of planning and directing large-scale military operations, specifically (as distinguished from tactics), of maneuvering forces into the most advantageous position prior to actual engagement with the enemy; 2. a plan of action based on this; 3. skill in managing or planning, especially by use of stratagem.” Stratagem refers to “device or act of a general: 1. a trick, scheme, or plan for deceiving an enemy in war; 2. any trick or deception.” For most anthropologists, it is the concept of individual and group maneuver within cultural, ecological, and social structural constraints, and the coalescing of multiple maneuvers into strategies as plans of action, which has an appeal. Game theorist Davis (41, p. 10) defines strategy as “...a complete description of how one will behave under every possible circumstance; it has no connotation of cleverness.”

It is unfortunate for a science of man that the literary meaning of “strategy” relates it to “large scale military operations,” and that “stratagem” as synonymous with “trick, scheme” is related to “skill in managing and planning.” With the notable exception of Bailey’s recent Stratagems and Spoils (6), social anthropology has, more often than not, eschewed the notion of stratagem. However, some attention has been given to warfare strategy. Turney-High (143) turned to such concerns, and more recently Otterbein (116, 117) productively analyzed Iroquois and Zulu sociopolitical evolution by reference to changing warfare tactics. Wolf (154) clearly moves toward peasant warfare as an adaptive strategy in the face of radical changes in managerial-resource power domains. Also, Harner (74) analyzes Jivaro warfare strategies and recruitment tactics as culture maintenance mechanisms. Other recent analysts of war (57) do not follow a strategy approach, however. Balandier (7) goes further than most in relating power, kinship, ritual, and strategy into a dynamic unified theory.

Even the anthropology of law has some ties to strategy analysis, as Lea (85) so carefully documented over a century ago in his book Superstition.

*Serious studies of play are beyond the scope of this review. For a few illustrative analyses of play, we refer the reader Csikszentmihalyi & Bennett (38), a collection of articles by Norbeck and others in Natural History (113), Huizinga (79), Goffman (64), Berne (22), and Devereux (44).
the concept of law as "wager" and its evolution as a series of escalating com-
petitive tactics leading to higher forms of social structure has not received the
attention we might expect. Nor have theories such as Schelling's (128) which
relates power, conflict, strategy, and games into a unified system, been given
adequate attention. Even in the area of political anthropology (1, 140, 141)
it seems that social anthropologists have carefully worked their way around
the key concept of strategy.

It is in the area of cultural adaptation that the most productive work has
been done, and we turn now to some representative cases. Two pioneering
studies deserve particular attention—Davenport's (40) work with Jamaican
fishermen, and Barth's (11, 12) contributions on the Swat Pathans. We begin
with Davenport's work, which has already been reanalyzed at least three
times (29, pp. 9–12; 84, 121) from the standpoint of game theory.

Davenport (40) chose as his unit of analysis a single Jamaican village in
which every male gained his livelihood from fishing. Men worked in organ-
ized crews, each with a captain, and exploited two zones of the marine envi-
ronment: inside banks and outside banks.

With the technology, skill, and capital of the fishermen at the time of
Davenport's study, the outer banks gave a higher yield, but were less predict-
able due to heavy currents which ran 25 percent of the time. He stated his
problem as follows:

... differences between the current-ridden outside banks and the safe inside banks
constitute a choice situation for the captains as to where they will fish. These are
not mutually exclusive alternatives, so the choices are: (1) all pots set inside,
(2) all pots set outside, (3) some pots set inside and some set outside. These
three alternatives, or strategies, will be called inside, outside, and in-out (40; cita-
tion from 78, p. 309, emphasis in original).

It is important to note that the fishermen themselves perceived only two alter-
natives: inside and in-out. At this point Davenport leaves the construction of
an adaptive strategy based on ethnographic information and turns to game
theory. He assumes that he can analyze the Jamaican fishing village as if he
were dealing with two players—the village as one player, the sea as oppo-
nent. The teams are seen as maneuvering through time in such a way as to
maximize gain and minimize risk—the day-to-day choices and the villagers' ways
of fishing reflecting an overall minimax strategy.

It is now clear (84, 121) that Davenport misused the theory of games by
considering the environment as opponent. Among other things, the sort of
game theory to which he turned—a two-person zero-sum game theory—assumes
rationality by both players (111). The outside sea currents were ran-
don in terms of the probable knowledge (45, p. 31) of the Jamaican fisher-
men, and hence could not be interpreted in terms of minimax behavior.

In short, Davenport's contribution lies in his demonstration of how to
analyze fishing behavior, including group organization, economic opportu-
nity, marketing, bargaining, social hierarchy, and ranking in terms of a com-
munity's overall fishing strategy. In a reanalysis of Davenport's article by two mathematicians (121) we learn that, contrary to Davenport's calculations, the outside strategy would be the most profitable, if fishing captains and their crews could afford the risk.

The general point agreed upon is that the behavior of fishermen in interaction with one another can be understood as an adaptive strategy within a particular environment. But to understand more about the analysis of adaptive strategies we must go beyond material which simply pits village against the sea.

Fredrik Barth (12) also relied on analysis of strategy to understand the dynamics of one of social anthropology's favorite 'structures'—the unilineal descent group. Barth built his second level generalizations out of the day-to-day patterned interaction of Yusufzai Pathans of the Northwest Frontier Province, Pakistan. Here strife over control of land between powerful individuals led men to align against one another, and to change alliances according to agreed-upon rules of agnatic descent and privilege of land tenancy. Barth endeavored to understand Pathan social structure, in which landholding Pathan chiefs (Pakthuns) aggressively engaged in brinksmanship politics while Saints mustered support to mediate between potentially warring factions. Instead of building corporate units as a model of structure out of his observations, Barth sought to understand the recruitment strategy by which powerful landowners exploiting a stable environment through highly developed irrigation agriculture try to extend their control over an area to make greater profit for themselves. Decision as to what recruitment, mediation, or aggressive strategy to play runs through the entire ethnography. Consider, for example, this statement (11, p. 69):

The problem that faces the Pakhtun—... on suddenly finding himself face to face with a couple of thousand unknown persons, his potential allies or enemies for the next ten years—is how to make himself indispensable to the villages, how to tie as many of them as possible to himself.

By solving such problems again and again, a structure has been evolving in Swat since the 16th century. What Barth gives us is a series of second level generalizations about adaptive strategies which allow us to more clearly understand the social structure of Swat, and another way of understanding the structure of unilineal descent systems:

The genealogical charter [of the Swat Pathans] ... defines rivals and allies in a system of two opposed political blocs. Closely related descent units are consistent rivals; each establishes a net of political alliances with the rivals of allies of their own rivals. In this fashion a pervasive factional split into two grand alliances of descent segments emerges, with close collateral segments consistently in opposite moieties (12, p. 5).

Among other things, Barth's focus on strategy itself, expressed by Pathans through the idiom of descent, played out in a predictable ecosystem, gets us neatly around debates over the relative merits of "descent theory" (51, 60,
71, 95, 132) versus “alliance theory” (88, 91, 99, 110). It also lets us perceive actors making choices and then coping with the consequence of their choice. The end result is an asexual lineage system where the lineages themselves do not constitute corporate units, though they provide the continuity of form necessary for the formation of a dual structure of two opposed, but asexual, blocs in each region.

... the corporate groups in the political system are formed by the strategic choices on the part of the participants, and do not emerge by virtue of a mechanical solidarity deriving from likeness. Such a political system may be analysed in terms of the bases on which the strategic choices are made (12, p. 7, emphasis added).

Clearly, the analysis of strategy does not negate knowledge of structure. Analysis of strategy and choice-making for alterations in the fabric of society complements that of consistent outcomes which, through time, contribute to continuity of social form.

Taking the continuities of ecology, brinkmanship, and social structure as the “rules,” Barth analyzes Pathan structure according to game theory, specifically in terms of a zero-sum majority game with three players, each of whom may choose a partner—i.e. form coalitions against the third party. Because he is dealing with social structure (the rules) through time, and manipulating, competing actors (strategists) at a given time, Barth’s use of game theory seems particularly appropriate to the establishment of a social model which can be used to generate actual Pathan behavior in specifiable contexts. In his own analysis Barth generated the strategies and risks, and then asked whether or not the Pathans perceived things in a comparable fashion, and they did (12, pp. 18–19).

In later publications Barth (14, 15) goes on to indicate three kinds of models which anthropologists have used in thinking about society and culture, and to place what we have called adaptational theory in the third model. The first (14, pp. 32–33) is a model of structure based on jural rules, the second a model of cognitive categories, and the third (the adaptational) is what Barth calls “an interactional system”:

a model that depicts the constraints on individual behavior that arises from the behavior of others in a social system. Such models assume some form of ‘economizing’ or competition or strategy on the part of actors whereby they mutually modify their behavior in respect to the objects that are valued in their culture (14, p. 33).

He notes that such a model also provides for “change by means of simple cumulative feed-back mechanisms in such models” (14, p. 33). This approach has been independently elaborated by Bateson (19), Alland (2, p. 312), and Sahlin (124, 125, p. 136), all writing from very different standpoints, yet demonstrating remarkable convergence in anthropological thought. Most of the essays in Afro-American Anthropology (152) take this approach. We will return to Barth’s analysis and contributions later in this section and also in the section following.

Sahlins (124) also turned to the concept of “lineage” to develop a model
of adaptive strategies, although he did not use this terminology. He showed how segmentary lineage systems could evolve at a tribal level of political evolution as the most efficient way of expanding territorial advantage at the expense of other kinds of organization. This approach (spelled out more completely in 127) argues that the maximal efficiency in the economic system is dominated by the most advanced polity, and that such dominance leads to convergence in the adaptive strategies chosen by aggregates competing over finite resources. Increase in efficiency in the economy inevitably leads to societal transformation and qualitative change in the relationships between extant social systems ("tribes"). Gerlach & Hine (58, 59) recently applied an analysis of segmentation and transformation to the Black Power movements and religious sects in the United States, and Wolf (154) analyzes a number of "peasant" revolts and national revolutions in this general manner.

An advantage of Sahlin's approach is that it allows us to understand more fully how opposing strategies are played out vis-à-vis one another. The expansion of "tribal" predatory segmentary lineage systems such as those of Tiv and Nuer inevitably generate ethnic boundaries as markers distinguishing the expanding aggregate from those becoming subordinate in the new polity. By focusing on adaptive strategies and boundary markers as part of such strategy we can expand Sahlin's (24) idea of the "intertribal environment" to include all interethnic relations (10, 18).

Despres (43) effectively illustrates the value of this approach in his analysis of a contemporary nation, Guyana, in terms of adaptive strategies played out in various environments and niches. (See also 13, 42, for illustrative comparable work.) By focusing on Africans and East Indians in a plural nation and considering historical, ecological, and cultural features, Despres attempted to synthesize approaches in anthropology as diverse as the multilinear evolutionism of Steward (136, 137) with the plural society model established by M. Smith (131). He also critically appraises the conflict perspective of pluralism established by M. Smith (133) and van den Berghe (144). A summary of his analysis is clarifying in terms of the approach:

... contrary to widely held opinion, the *selective advantage of the plural society is the reduction of competition between culturally distinctive groups*. If plantations, villages and cities are considered separately, there is abundant evidence of competition between Africans and East Indians. However, when these environments are viewed as functional parameters of an overall ecosystem, it is equally evident that the process of differential adaptation, both within and across environments, has resulted in the reduction of subsequent competition. Within each environment Africans and East Indians fill different niches and, thus, they tend not to compete with one another on a day-to-day basis for the same resources. ... the same process which serves to reduce competition also serves to maintain cultural differentiation (43, p. 285, emphasis in original).

Bennett (20) uses this approach to good advantage in the "Jasper" region of Saskatchewan in his analysis of the differential adaptation of Cree Indian "marginals," Euro-American ranchers, Euro-American farmers, and the
Hutterian Brethren. This is quite similar to the “Rimrock Studies” of Harvard University (cf 146), except that the latter sought “invariant reference points” in values, where Bennett seeks the cumulative effects of variance and ethnic boundary markers. Drawing explicitly from economics, human ecology, cultural geography, and evolutionary theory, as well as from social anthropology, Bennett (20, pp. 14–19) gives explicit attention to the conceptualization of adaptive strategies:

... adaptive strategies ... [refer to] the patterns formed by the many separate adjustments that people devise in order to obtain and use resources and to solve the immediate problems confronting them ... adaptive processes ... [refer to] the changes introduced over relatively long periods of time by the repeated use of such strategies or the making of many adjustments (Bennett 20, p. 14) ... the many separate adjustments that have become patterned as strategies can also enter into culture ... as repetitive patterns of action they can be viewed by the people as traditions. (20, p. 16).

Bennett’s definitions fall in the area of organization and change, in Firth’s (47) terms. The next step is to consider structure—continuity in culture—within the same frame of reference. This is where the concept of time becomes crucial.

British social anthropologists such as Evans-Pritchard (46), Fortes (48–50), Leach (86, 88, pp. 124–36), Gluckman (61), among many others, have given considerable attention to the notion of structural, or social, time—a temporal period serving as an adequate social analog to the concept of ecological time. Tides ebb and flow, seasons “change,” and we can describe the structure of a particular natural cycle by reference to the time it takes the entire series of alterations to work itself out and begin again. An important question in the analysis of society and culture is whether we can handle social time.

By focusing on sequences of successful mobility strategies in west Ecuador, Whitten (149) described community structure in processual terms. He suggested a strategy-based model of social life adaptable to a “boom-bust” money economy. Within this model he could then subsume material relating to kin selection, struggles for political power, conflict and factionalism (147) in relation to four mobility strategies, each related directly to relative success or failure of a preceding strategy (149, p. 238). He then applied this model to areas undergoing demographic shifts to understand the strengthening of racial boundary markers (150).

“Developmental cycle” approaches to structural time have been worked out in household and family studies (52, 70, 134), in studies of kindred organization (69, 147), and in a study of family business (80). Foster (56) recently discussed ritual coparent selection in a Mexican village by reference to family developmental cycle phases, and Balandier (7, pp. 68–70) discusses Fang political enterprise in the Cameroons in sequential terms. Gluckman (61, p. 277) suggests that political process be conceived of as sequences of developmental cycles within structural time. He draws analogies from British
parliament and from African rebellions, the latter of which he uses to make a critical point:

In an African state [if we are] to analyse... day to day politics we must work out the cycle of rebellions. These did not occur anything like every day: *But the possibility of rebellion influenced the motives and actions of everyone involved in politics* (61, p. 277, emphasis added).

In the Pacific Lowlands of Colombia and Ecuador there is always the possibility of an economic boom and the possibility of total economic depression. By assessing these possibilities, not just the actual situation at a moment in time, a dynamic picture of social maneuver, its coalescence into social strategies, and the repetition of strategies through structural time becomes possible.

Barth (16) summarizes the perspective employed thus far in this review as relates social strategy to social form:

... it is unfruitful to explain a social form, a pattern, directly by hypothesizing a purpose for it. Individual actors and individual management units have purposes and make allocations accordingly: but a social form, in the sense of an overall pattern of statistical behavior, is the aggregate pattern produced by the process of social life through which ecologic and strategic constraints channel, defeat, and reward various activities on the part of such management units (16, p. 663).

At first glance this statement by Barth seems totally compatible with Leach’s (87) position, reiterated by Murdock (107, p. 9): “... jural rules are best regarded as the outcome of a statistical trend in individual choice.” But Barth (15) separates his “transactional” approach to model construction from that used by those interested in “jural rules.” Buchler & Nutini (29, pp. 6-12) even more explicitly deny convergence in thought between game theory (to which Barth turns) and the Murdock-Leach position on jural rules. They begin by separating *ground rules*—“mechanical (deterministic) models or ideal paradigms of what people should do” (29, p. 6)—from *strategy rules*—“statistical (stochastic) models of what people actually do” (29, p. 6, emphasis in original). They argue that ground rules are ideological and involve processes of concensus, and that strategy rules, by contrast, are the results of individual and group tactics and decisions. If we understand this position correctly, it states that although the ground rules and strategy rules operate at different inseparable levels, they nonetheless represent *separate models* of social reality. One does not explain one model by reference to the other. Rather, the search is for an understanding of the genesis of both models, and the understanding of how the models may then be used to generate ground rules and strategy rules at the two levels. Their edited book, *Game Theory in the Behavioral Sciences* (29), reviews the status of such thought in the social and behavioral sciences. We shall say more about this in the next section.

Analysis of strategies in natural social systems has been used in a variety of ways in anthropology and cognate fields to come to grips with the dynamics of social life. We have not yet agreed on what the appropriate model(s)
of social life may be—though remarkable convergence in thought has occurred in the past 20 years between many subfields in anthropology and allied disciplines. We turn now to the underpinnings of adaptive strategies—to reciprocity, interaction, contexts of interaction, transaction, and the concepts necessary to understand the dynamics of human social intercourse.

**Reciprocity and Interaction Strategy**

In his book, *Structural Anthropology*, Claude Lévi-Strauss (93) again and again contributes to the conceptualization of human social relationships. He states:

... as soon as the various aspects of social life—economic, linguistic, etc.—are expressed as relationships, anthropology will become a general theory of relationships. Then it will be possible to analyze societies in terms of differential features characteristic of the systems of relationships which define them (93, pp. 95–96).

We are in hearty agreement with this statement; the immediate problem which we share with the structuralists is how to understand interpersonal linkage as a first order generalization. The core concept is *reciprocity*. Human beings may reciprocate anything, from material goods to insults. The exchange itself may be culturally patterned by selection from known texts, as when “playing the dozens” (83), or “signifying” (104). Exchanges may be subject to rules generated by the very nature of a bargaining situation, as when an armed homeowner surprises an armed thief; neither wishes to shoot the other, because of mutually recognized negative consequences, but neither knows how to end the pattern of emergent exchanges, real, and anticipated (128, pp. 207–29).

In anthropology Foster (53) endeavored to understand the social structure of Tzintzuntzan, a Mexican village, by reference to “symmetrical dyadic contracts,” which refer to perpetuating exchanges of comparable goods between individuals, and “asymmetrical dyadic contracts,” which refer to perpetuating exchanges of different things between persons of different socio-economic status. He elaborated the latter concept so as to understand “patron-client” relationships in a later article (54). Radcliffe-Brown (119, pp. 133–137) had already suggested that a universal aspect of reciprocity was a tendency to give a bit more than received, thereby maintaining a set of mutual obligations. Mauss (98), of course, elaborated the entire concept of *prestation*, or gift with implied contingency as a core feature of social process.

In a review of the literature on reciprocity from Mauss (98) and Malinowski (96) to the mid-1960s, Sahlin (126, p. 139) clearly establishes that one cannot separate the social contract, or series of reciprocal social exchanges, from the flow of materials—the “goods.” Heider (75) demonstrates that “exchange” of materials is implicit in a concept of “social networks.” Whitten (148) demonstrated that, in the absence of “goods” in a money economy, lower class participants in Afro-American culture of western Ecuador and Colombia exchange verbal or other symbolic tokens, such as dance partners. Goffman (67) discusses ways by which *individuals* can serve as to-
kens, and how individuals-as-tokens and individuals-as-pawns could be analytically separated in social interaction analysis. Whitten also demonstrated, in terms of asymmetrical reciprocity between persons of different economic and ethnic standing, that a prestation by a higher status may be "closed" by the person of lower status if the latter verbally thanks, praises, and compliments the "giver" (147, 148). Reciprocity is not synonymous with dyadic contract; reciprocal exchanges can be used to forge or break, to open or close, relationships between actors.

Taking note of the dynamic use of reciprocity in social, economic, and symbolic domains has led many investigators to other concepts such as "transaction" (15), "power" (25), and "network" (153). The latter state that a network concept allows an investigator of a natural social system: "... to get at a relevant series of linkages existing between individuals which may form the basis for the mobilization of people for specific purposes, under specific conditions (153). Barth (15, pp. 3-4) defined transaction as the flow of relationships within a natural system, and also brought concepts of "value" into the quality of interpersonal relationships. Greatly shortening and simplifying Barth's contribution, we come to a clear understanding that Barth and many "network analysts" are thinking in the same way:

In any social relationship we are involved in a flow and counterflow of prestations, of appropriate and valued goods and services. Our own and our counterpart's ideas of appropriateness and value affect our relationship... one may call transactions those sequences of interaction which are systematically governed by reciprocity (15, pp. 3-4).

In the view taken here, no sequence of interaction can exist without reciprocity, provided that we properly understand that any exchange of goods or messages is by definition reciprocal. Even if actor A does not respond, say, to actor B's verbal prestation (as described in Whitten 147, 148), he nonetheless communicates something back to A. (See also 25, 77.) Sahlin (126) tries to define the full range of reciprocity from "generalized" where an actor "gives" to someone, and expects some return from someone, some time, to "negative exchange" (72) where an actor endeavors to "take" without giving.

'Generalized reciprocity' refers to transactions that are putatively altruistic, transactions on the line of assistance given and, if possible and necessary, assistance returned. The ideal type is Malinowski's 'pure gift' (126, p. 147). ... 'balanced reciprocity' refers to direct exchange (126, pp. 147-48) ... and 'negative reciprocity' is the attempt to get something for nothing with impunity, the several forms of appropriation, transactions opened and conducted toward net advantage. Indicative ethnographic terms include 'haggling' or 'barter,' 'gambling,' 'theft,' and other varieties of seizure (126, p. 148).

Sociologists and social psychologists such as Homans (77), Blau (25), and Thibault & Kelley (142) have made substantial contributions to "exchange theory" because of their focus on actual reciprocal maneuver in social situations. Blau is of particular value to anthropology because his observations
and theorizing stem from an earlier endeavor (24) to understand bureaucracy as a dynamic system of competitive social manipulators, rather than in terms of its formal plan. His concern with exchange ramifications as they contribute to power and maneuver in social life is full of propositions which can be tested and expanded in actual analysis of field data.

Anthropologists Bailey (5, 6), van Velsen (145), Boissevain (27), Nicholas (112), Mayer (100), and Whitten (151) have tried to deal with factional dispute, chains of event, and contexts of interaction. A. Cohen (34) calls such approaches “action theory.” All of these concepts are now being employed in network analysis (102, 103, 148, 153). Network analysis itself does not always stem from exchange or action theory. Some writers such as Barnes (9) and Wolfe (155) consider “network theory” to be an emergent set of related propositions in its own right, while others such as Bott (28), A. Mayer (100), Mitchell (102, 103), and Boissevain (27) see the basis for network theory in “role theory.” Among the many reviews of network analysis, the recent one by Whitten & Wolfe (153) demonstrates that concerns of structuralists and nonstructuralists both contribute to this area. The writers of this review are of the decided opinion that network analysis is best regarded as a research method designed to get deeply into naturally occurring systems of interaction which are formed as actors maneuver within cultural constraints and normative boundaries (i.e. boundaries generated by the cumulation of previous interactions).

Reciprocity and interaction strategy inevitably get us into the problem of what actors want, and what they will accept in specified circumstances. This places the anthropologist in the heart of economic theorizing, one of the crucial questions of which deals with the question of “maximizing.” Burling (31) presented an argument that continues to generate debate:

The view of society as a system of exchange, and the view that men act so as to attempt to maximize satisfactions, are fundamentally economic ones and are close to the way in which economists look upon their subject matter. However, unlike anthropologists, economists have not ordinarily been interested in finding out whether people economize intelligently, but only in figuring out how they can economize more intelligently. The difference in objectives creates an almost unbridgeable gap between economics and anthropology, because an anthropologist is always interested in the actual behavior of men in concrete situations (31, p. 819).

Burling was writing in specific opposition to the view of Polanyi (118) where concepts of “reciprocity,” “redistribution,” and “exchange” are seen as different sorts of economic behavior depending upon the level of socioeconomic evolution (“primitive,” “archaic,” and “market,” respectively). In the same year Berliner (21) also reviewed anthropology’s relationship to economics from his standpoint as an economist, but contributed nothing to our understanding of strategy, maneuver, exchange, or human goal-oriented behavior in the face of complementary and oppositional behavior.

Burling was concerned with the allocation of finite resources among alter-
native ends. "Economizing" referred to matters of choice in systems of exchange. LeClair (89) focused on this same problem, endeavoring to clarify the concept of "scarcity." Turning to marginal economic theory he also moved beyond the area of goods and production to the nature of man himself: ". . . men everywhere are confronted with the fact that their aspirations exceed their capabilities. This being the case, they must everywhere economize their capabilities in the interest of meeting their aspirations to the fullest extent possible" (89, quoted from reprint in 90, pp. 193–94).

This discussion about economizing and maximizing is currently imbedded in a great deal of debate over "substantive economics" and "formal economics," and it is not appropriate to digress into this debate here. It is the area of formal economics which deals with the issues of this review. LeClaire & Schneider (90) review such discussion and include a number of papers which do the same. Dalton (39) reviews the literature on macro-development and micro-development in "modernization" studies, many of which suggest the sort of model dealt with under the rubric "adaptive strategies," but without the same underlying basis in exchange or maximization perspectives. Erasmus (45) tried to see people at all levels of development as constantly making "frequency interpretations" . . . "predicative interpretation based on the observation of repeated events, the dominant cognitive aspect of human action. Experience or observation is the raw material from which frequency interpretations are inductively derived" (45, p. 23).

Man, according to this perspective, forever relies upon such empirical interpretations of ongoing social life to increase his "probable knowledge," a process of continuing reinterpretation and change of premises based upon past experience and day-to-day observations (45, pp. 31ff). He endeavored to place such an analysis of cognitive exchange and social mobility (through motivational processes of emulation) in an evolutionary framework. In many ways Erasmus' work allows us to move from the observational facets of interacting humans in a moment of ethnographic time to the adaptive strategies making for both continuity and transformation of a social order.

By employing a concept of "opportunity costs"—"the value of a particular resource in its best alternative use" (20, p. 15)—as it is supposedly used in economics, Bennett also attempts to understand social exchange in economic terms while maintaining a framework that allows him to understand not only societal growth, but also the differential roles or niches of ethnic and occupational aggregates in a changing society.

Sociologists have long been concerned with theories that can handle rationality of actors' choices and decisions, patterns of competition and conflict [cf Caplow (32) for an overall review]. March & Simon (97) focus on bureaucratic organizations for the development of their analysis of decision making. They argue that problem solving in industries, hospitals, and the like requires not only the selection of solutions to problems but also some program or formula for the selection process itself. Choice of a problem-solving activity naturally depends on the available alternatives. Selection of an opti-
mal solution involves comparison of all alternatives and choice of the most preferred one. By contrast, selection of a satisfactory solution involves finding an alternative that meets minimum standards. Two different sets of criteria are used and the decision-making process itself is quite different in each case. March & Simon (97, p. 169) state that “...because of the limits of human intellective capacities in comparison with the complexities of the problems that individuals and organizations face, rational behavior calls for simplified models that capture the main features of a problem without capturing all its complexities.”

March & Simon assume that people generally choose a satisficing response; they select a program of action which will meet some minimum criterion of satisfaction rather than seek the best of all possible solutions. Influence from this field of sociology has not been detected in anthropology, possibly because of the considerable amount of professional terminology employed by organizational theorists, but probably because the difficulty in handling the concept “satisficing” itself in a natural system has not been reviewed as productive by those seeking a more dynamic perspective on social relationships.

Concepts suggested by the theory of games which allow for differential power and a distinction of ground rules from strategy rules seem to be quite productive as a basis from which to view manipulation and goal seeking within a natural system with its emergent structures and strategies. In spite of this, the Biennial Review of Anthropology 1969 (115, pp. 165–166) listed only three studies on “Decisions and Games” (3, 81, 101). Three recent publications (29, 41, 106) allow anthropologists to derive concepts and techniques for the analysis of their data. Morgenstern (106) documents the proliferation of contributions to game theory since its early presentation as a unified theory by von Neumann & Morgenstern (111). The following brief summary must suffice in the review space available here.

The simplest class of games and the most easily mathematized is the one-person game, where ego makes a choice and the outcome is a direct result of this choice. Davis (41) greatly aids our understanding of the more complex two-person games by looking at them as lying on a continuum with zero-sum games at one extreme and non-zero-sum games at the other. The finite, two-person, zero-sum game is the simplest, the most restricted; it is a game of perfect information in which the players are pitted against one another with rules known and agreed upon. Rewards are set for the particular situation; what one player wins the other loses, and so rewards and losses are always in balance. Players’ strategies are governed by von Neumann’s “minimax theorem,” which assumes that each player, acting to his own best advantage, will “minimize his maximum loss” (106, p. 64). Strategy may be pure (non-random) or mixed, in which case a random device selects the strategy.

The two-person, zero-sum game assumes opposition of the players’ interests; hence, it represents the most competitive or conflict-oriented type of game. Chess is an excellent example, for here strategies can be developed which theoretically predict all moves for both sides. However, as Davis (41,
pp. 13–14) points out: "... despite the theory, game-theorists lose chess games ... because games that can be solved only in principle are treated as though they could be solved in fact."

At the other end of the continuum is the two-person, non-zero-sum game. Here the players do not win from one another but obtain their gains from an outside source. Their interests in payoffs are shared; losses are also mutual, so their cooperation must be coordinated to avoid joint losses as well as to win agreed-on payoffs. In non-zero-sum games there are more alternatives, more variables, and more players can be added, all of which add to the complexity. Davis calls attention to some of the factors we must take into consideration; among them is "utility function," or players' preferences for objects, goals, and for other players (41, pp. 49–64). He also states that the bulk of everyday problems with which we deal fall between the two extremes; they are games of great complexity for which there are no simple strategies or predictable outcomes and which combine competition and cooperation. The recent work of Ofshe & Ofshe (114) is primarily concerned with non-zero-sum games of coalition; the authors deal with a number of variables in complex decision-making models in laboratory games. While game theorists address themselves to classic issues of conceptualization in social anthropology such as exchange, alliance, cooperation, and conflict (36, 37, 109), it is the complex, multivariable level of game theory that remains to be applied in actual field settings.

Goldschmidt (68, pp. 61–64) addresses himself to the strategy rule model of game theory by reference to four elements: "persons, rules, strategies, and goals. ... What anthropologists make the primary subject of their normal discourse, game theory calls the rules of the game." "Person" may refer to an individual or to any corporate group or corporation from Nuer lineage to SONY International. Goldschmidt argues that the first three concepts are culture free, and hence applicable to cross cultural analysis as well as to specific ethnographic settings. But "goal" is not culture free; an investigator must determine in a specific ethnographic context what it is that persons are playing for within a set of rules (which they, of course, may break). He very effectively points out that applications of game theory beyond specific settings must deal with the "economic," and that assumptions about the universality of economic man must be made if game theory is to serve in anything other than specific ethnographic settings. The fact that both Davenport (40) and Barth (12) predicted what they already knew—that is, found the game theory instrument valuable only after they knew what the outcome was because of ethnographic work—may lead many to find its use limited. It is clear that the concepts of economizing man imbedded in exchange and alliance over valued commodities (whatever they may be) need clarification if game theory is to gain acceptance in anthropology.

We return now to the concepts of interaction, strategy, human maneuver, and power in social life. Over the past decade the work of Erving Goffman (62–67) illuminates the intracacies and depths of human social behavior so
often taken for granted by other investigators. By combining the sensitivity of
the symbolic interaction approach⁴ with game theory and strategy analysis,
Goffman (67) recently set out his theory of “strategic interaction.” Strategic
interaction is a style of analysis which:

... appears to advance the symbolic interactionist approach in two ways: first,
the strategic approach, by insisting on full interdependence of outcomes, on
mutual awareness of this fact, and on the capacity to use this knowledge, provides
a natural means for excluding from consideration merely any kind of interde-
pendence. ... Secondly, following the crucial work of Schelling [128], strategic
interaction addresses itself directly to the dynamics of interdependence involving
mutual awareness; it seeks out the basic moves and inquires into natural stopping
points in the potentially infinite cycle of two players taking into consideration
their consideration of each other’s consideration, and so forth (67, pp. 136–37).

We are led through the echo chamber of face-to-face interactions to examine
the contingencies and repercussions of decisions and moves (chosen courses
of actions—67, p. 190), of actors, on to the objective consequences such
moves have for the situation of the participants and hence for their larger
social arena.

To illustrate the application of strategic interaction analysis, Goffman re-
lies heavily on studies of espionage, on gambling games, and on the strategic
moves of his fictional laboratory partner, Harry. Harry is visualized in nu-
merous real-life situations having game-like qualities. Harry is caught in a
forest fire, pilots a faulty airplane, simultaneously faces a hungry lion and a
hostile spearman, and even plays poker and converses with other people. The
use of Harry focuses the reader on the very situations described by Schelling
(128). Goffman (67, p. 137) states that “... no limit is placed on its [strategic
interaction] application, including the type of payoff involved, as long as
the participants are locked in what they perceive as mutual fatefulness and
are obliged to take some one of the available, highly structured courses of
action.” He is clear that strategic interaction analysis will not give a full pic-
ture of social relationships and social gatherings, nor is it in any way a study
of communication systems (67, pp. 137–40).

⁴ Blumer states:
The term “symbolic interaction” refers ... to the peculiar and distinctive char-
acter of interaction as it takes place between human beings. The peculiarity con-
sists in the fact that human beings interpret or “define” each other’s actions in-
stead of merely reacting to each other’s actions. Their “response” is not made
directly to the actions of one another but instead is based on the meaning which
they attach to such actions. Thus, human interaction is mediated by the use of
symbols, by interpretation, or by ascertaining the meaning of one another’s ac-
tions. This mediation is equivalent to inserting a process of interpretation between

[For a thoroughgoing treatment of symbolic interaction theory the reader is referred
to Rose (122) and Swanson (139).]
Three of Goffman's earlier works (64–66) will aid the anthropologist in understanding the strategic interaction approach. All provide valuable sensitizing perspectives for the examination of social interaction in everyday situations. In Alienation from Interaction, Goffman (65) highlights properties of verbal interaction (conversation) by discussing its opposite, alienation or lack of involvement in interaction. He deals in depth with the concept of fatefulness or consequentiality of activity in another work, Where the Action Is (66). The relationship between the dynamics and structure of interaction is analyzed in Fun in Games (64).

By focusing on the strategic qualities of interactive situations, on the courses of action that have real consequences for the actors and their situations, Goffman directs our attention away from pure conflict toward a consideration of the dynamics of any type of interaction in any sort of setting. In other words, he is able to handle both disruptive and adaptive aspects of conflict or cooperation within the same framework, thereby circumventing conflict versus functional arguments. The strategic interaction approach is a means of spanning the gap between process and structure; it allows us to consider seemingly mundane interactions with reference to both strategy rules and ground rules.

How does one apply strategic interaction analysis? A multidisciplinary application to various situations, primarily from the viewpoint of game theory, may be found in Strategic Interaction and Conflict (4). More relevant to the types of problems mentioned throughout this review is the approach of Thibault & Kelley (142) in The Social Psychology of Groups. In this they present a theory of interpersonal relations and group functioning worthy of more attention by anthropologists. They begin with basic assumptions about dyadic interaction (two-person or two-party) and add further assumptions about the patterns of reciprocity that develop; they then examine the variations that occur when a third participant joins the interaction. Drawing concepts from social psychology, sociology, economics, and game theory into an integrated analysis of social interaction, they give empirical substance to their theory with diverse data ranging from social psychological laboratory experiments to studies of groups in naturally occurring situations such as industry, military forces, and families.

Their basic premise is "that most socially significant behavior will not be repeated unless it is reinforced, rewarded in some way" (142, p. 5). Thibault & Kelley use a matrix to describe objective outcomes of interaction sequences, as does game theory analysis, but they depart from the game theory assumptions of Luce & Raiffa (94) in two ways. They do not fix rewards, but rather allow flexibility for a range of reward values to be determined by empirical research. Furthermore, they do not assume complete knowledge of all possible behavior and outcomes on the part of the interacting persons; exploratory or trial-and-error interactions can thus be taken into account. This approach is therefore more applicable to anthropological concerns where rewards are unknown to the investigator or are variable for the participants, or both.
Objective consequences or outcomes are stated in terms of rewards received and costs incurred. Through continued interactions, participants in dyadic exchanges sample the behavioral repertoires available to them, tending to repeat jointly rewarding interactions until they reach the condition of interdependence, defined as dependence of the rewards of each upon the other's behavior (142, p. 22). Factors of satiation (decrease in rewards) and fatigue (increase in costs) effect this exploratory stage of interaction. Two criteria by which participants evaluate the acceptability of outcomes are posited. One, termed the “comparison level,” is the standard of attractiveness by which a participant determines if the relationship is satisfying to him. The second, called “comparison level for alternatives,” deals with dependency on the relationship. This refers to the lowest level of outcomes a participant will accept from the present relationship in the light of available alternatives.

Their analytical scheme moves from actual social processes of interaction into structural properties of norms and roles that result from interaction. While many aspects of their theory should be useful to anthropological research, two strike us as having particular appeal. One is their treatment of power (the ability of participants in a dyad or triad to affect the quality of the other’s outcomes) which enables one to trace out a dynamic picture of power maneuvers under various circumstances, such as in nonvoluntary relationships, or among persons of unequal status. Another is their conceptualization of the relationship between a person and an impersonal task (fishing or making a mask) which allows for analysis of a person's power with regard to a task.

Summary

Studies of social strategies and social relationships in anthropology have, for the most part, taken place at two levels—the aggregate and the interpersonal—which we have reviewed as “adaptive strategy” and “reciprocity and interaction strategy.” Many authors suggest relationships between the two levels and offer concepts applicable to each. The study of social strategies and social relationships, even in the most dynamic of social situations, must take cognizance of social and cultural form. Structure and dynamics in social life are seen as complementary processes at all levels of social intercourse.

Our stance is quite close to the transactional point of view presented in Toward a Unified Theory of Human Behavior (73, 76). This transactional perspective... concerns the interface between the subsystems and whole systems and between whole systems themselves. Each one of these sub-theories... is dynamic, since they are all concerned with behaviors or actions rather than with the essence or the naming of parts in a metaphorical sense” (73, p. x).

The use of transactionalism in social anthropology brings us back to the concepts of adaptive strategies in the second section of this paper. We consid-

*This transactional approach is, of course, derived from general system theory. For a presentation of such theory see the work of its founder, von Bertalanffy (23), and Buckley (30).
ered studies of strategic action and choice, such as those presented by Davenport and Barth, where analysis rested on game theory models. We also saw some of the pitfalls involved when game theory is applied without full knowledge of the human stakes involved in interaction, and still other pitfalls that develop when theoretical restrictions are imposed on data from naturally occurring systems of interaction. We have tried to indicate the merit of building knowledge of patterned interaction from day-to-day transactions among a few persons, as Goffman and Thibault & Kelley do, and also the merits of understanding the cumulation of such transactions through specified time periods.

Integration of the two levels of analysis should allow us to make first-level generalizations about day-to-day social interaction among individuals and second-level generalizations about adaptive strategies of collectivities within a unified frame of reference. The basic notion remains that of reciprocity, as Mauss, Lévi-Strauss, and many others so aptly demonstrate. We must come to grips with commonalities of reciprocity at many levels of exchange—intersubjective, interpersonal, intergroup, intercultural; and we must do this without assuming a principle of homeostasis, or balance, or equilibrium.

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ETHNOSCIENCE 1972

Oswald Werner
Department of Anthropology
Northwestern University, Evanston, Illinois

INTRODUCTION

After the first flurry of successes at discovering folk classifications by relatively rigorous eliciting techniques, few major advances have been made in the direction of a better understanding of lexical/semantic fields. Casagrande & Hale (6) provided a nearly exhaustive list of the major semantic relations, but little work followed in spite of increasing evidence that lexical/semantic relations are more readily recognizable as language universals than are semantic components (Werner 69). A partial explanation is that work in this area is extremely difficult and sociolinguistic approaches (ethnography of communication) seem to offer easier solutions. I think this is an unrealistic estimation since considerations of context, though certainly crucial, are also certainly more complex than so-called “context free” semantics.

Advances have been few because experimentation with large lexical fields exceeds the limitations of procedures by hand. It is simply too difficult to keep track of 1000 or more lexical items and their ramifications. The systematization of lexicography—and ethnosience can be viewed as an attempt at systematic and nonlinear (nonalphabetic) lexicography—has always been burdened by the bulk of data it had to process. The rather unsystematic variations of definitions in existing dictionaries are a living monument to this problem.

Progress in ethnosience is slow because the purpose of the exercise was never made entirely clear. Theorizing in terms of “synthetic informants” or in terms of question-answering systems promises to fulfill for the first time Goodenough’s dictum that an ethnography should allow one to behave like a native of that culture—at least for the general area of cultural verbal behavior based on cultural knowledge. It cannot be stressed sufficiently that

\[ ^{1}\text{This research was in part supported by a grant from NIMH MH 10940. I am indebted to the following people for their comments: Roy D’Andrade, Martha Evans, John Farella, Terry Strauss, and especially for the detailed analysis of Paul Friedrich. Any shortcomings that remain are my own.} \]

\[ ^{2}\text{I simply fail to understand Hymes’ (31) optimism or see any evidence for its justification. Compare, for example, Hymes to Roger Keesing’s recent (38) opening sentence, “Whatever happened to Ethnosience?”} \]

271
cultural knowledge is so vast that work in this area is unrealistic without machine aid. Since anthropologists cannot get inside the informant's head, psychological reality is an empty concept. Mind-like mechanical verbal behavior seems best suited for the validation of our assumptions.

The clarity of purpose of question-answering systems allows the investigator to formulate prerequisites in greater precision and detail than by any other method. The requirement that all of the investigator's ideas be representable as part of a computer program forces him into detailed conceptualization and planning exceeding in rigor by several orders of magnitude any similar requirements in anthropology today.

In order to avoid the fate of machine translation, the emphasis should be not on quick payoffs or early practical applications. If the machine is enlisted in the solution of interesting theoretical problems, applications will inevitably follow.

Answering questions by using a large data base creatively (deductively) is not easy, but no one has yet suggested a better method than attempting it by machine. In this paper I first show that indeed four relatively independent areas of intellectual endeavor (ethnoscientific, generative semantics, computerized semantic information processing, and sociolinguistics) do converge. Subsequently I try to enumerate some of the contributions made in each of these fields. Finally, I select a few topics in which I have clarified some points sufficiently for myself to share these insights.

Readers interested in more comprehensive coverage of ethnoscientific and its history are referred to the excellent reviews by Hymes (30), Sturtevant (66), Colby (8), Durbin (16), Hymes (31), Friedrich (21), Werner (75) and to Tyler's (67) compendium. With the exception of Werner (75), these papers and most of Tyler's book represent a point of view different from the one I assume in this paper.

Convergences

The convergence of ethnoscientific, generative semantics, computer simulation of semantic information processing, and some aspects of sociolinguistics (the ethnography of speaking) can be summarized roughly by the following diagram (Figure 1):

```
INPUT I  -->  OUTPUT O

QUESTION  -->  ANSWER
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Figure 1. Question-answer schema.

The specific inputs and outputs in each of the fields mentioned can be characterized as follows:

*Ethnoscientific.—If a system of this kind is proposed, for example, for a
componential analysis of a kinship terminology, the diagram can be
conceived as a device which answers the following questions: "If one person is
related to another in a specified manner, what does that person call the
other?" or "What is the second person called by the first person?" If the anal-
ysis is a taxonomy, the question may be "What 'things' (in this culture, using
its language) are considered animals?" or if some processes were part of
the analysis, "How do you make a canoe?" or "How does one conduct a fun-
eral?" The questions can be assumed to ask about general topics (e.g.
"What are all the things that have been created?" or specific (e.g. "How do
you thank the host at the end of a meal?") It seems that a question-and-an-
swer approach is particularly fitting for ethnoscienc: if the inputs are cultur-
ally appropriate and relevant questions, the outputs will be culturally ap-
propriate and relevant answers.

Generative semantics.—One has to distinguish between two cases:
1. The more extreme view where the semantic component of a language
is truly generative in the technical sense. By this I mean that the output is a
"random generation" of abstract phrase markers. A device of this sort is
hardly comparable to any human-like behavior. I shall therefore exclude it
from further consideration (see also p. 278).
2. If, however, the generating device is conceived more loosely as one
triggered by some thought it is easy to surmise that such thought could have
been evoked by a question. Since there seems to be no restrictions on ques-
tions that can evoke thought the device is comparable to a general question
answerer, analogous to the ethnoscienc examples above.

Semantic information processing.—In computer experiments of semantic
behavior, one possible aim is the simulation of human question-answering
behavior. Thus the input is clearly a question and the output is an appropri-
ate answer. If the device is general, that is, able to answer questions poten-
tially in any area of a culture, the device is then analogous to the earlier
examples from ethnoscienc.

Sociolinguistics.—Many problems in the social uses of language can be
conceived as answers to questions: e.g. "What does one do when a stranger
approaches camp?" "How does one behave in the presence of the president?"
or "When is it proper for a young man to speak?" These are questions ap-
propriate to an ethnography of speaking but also clearly questions that have pos-
sible answers. They are formulated in the native's language to reduce ethno-
centric translation bias and, equally importantly, to avoid imposing one's own
categories. Quite likely not all socially significant contexts are explicitly open
to description by native speakers. They do misjudge situations on occasion or
are unable to state the precise contextual rule. However, since verbalizable
contexts are by no means rare or small in numbers there is no reason to ex-
clude them from semantics (following Tyler 67). Contextual attributes can
be listed with other attributes. Implicit contexts are considerably more diffi-
cult. Since no minimally adequate metalanguage has been proposed for their description, I exclude them from further consideration, although this exclusion should not be construed as a lack of my appreciation of their importance.

**Typology**

If the discussion thus far is minimally successful in indicating the convergence of these four fields, then why are they separate fields, how do they differ in method and theory, and what constitutes their major emphases? In order to help the reader clarify the destination of ethnoscience, I present a typology which characterizes each field. This typology is also a programmatic attempt at integration of the four fields.8

_Ethnoscience._—Two general approaches in the field require discussion. First is the componential approach. The extension of this method of analysis to the entire vocabulary of a language has been attempted only in spirit. Katz & Postal (33) have sharply criticized anthropologists for this timidity. Nevertheless there are no empirical investigations extant that attempt a componential analysis of all the lexical resources of a language. It has been most successfully applied to small lexical sets, especially kinship terminologies.4 A subbranch of componential analysis are analyses which can be characterized either as some aspect of decision making (e.g. Gladwins 25) and/or the im-

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8Howard Maclay (44, p. 180) quotes Victor Yngve's glee in predicting the demise of linguistics as an independent discipline. Although I can empathize with Yngve's joy, I prefer to interpret the demise of "autonomous syntax" [at least in the view of some generative semanticists (e.g. as described by Lakoff 41)] optimistically. It is a positive, promising sign of the integration that Paul Friedrich (personal communication) and I believe to be the best thing that could happen to the fields concerned. I further agree with Friedrich that the most significant advancement of the 1970s will be such a unification.

4I have attempted in previous publications (Werner 73, 75) to extend the notion of components to the entire lexicon by the introduction of the 'circle star' (Θ) operation. This operation—if all the components of a language are known (a considerable, or even impossible task)—"automatically" establishes all hierarchies (taxonomic levels) with paradigms on each level of the taxonomy, and enables the investigator to add easily empirical and logical constraints on the occurrence of incompatible component combinations. To the best of my knowledge no one (including myself) has ever seriously tried to apply this model to anything more than my reanalysis of Berlin and co-workers' (1) taxonomy of squashes in Tzeltal. The primary value of my model lies in its illustration of the form a whole language componential analysis might take; especially that there is a requirement for a very large number of empirical and logical constraints that are necessary in order to exclude items not in the speaker's environment, as well as items that are conceptually impossible. The latter idea, not pursued beyond the suggestion that such a logical restriction may be possible, is sometimes raised by generative semanticists.
position of some kind of a temporal order on a decision or recognition process (e.g. Geoghegan 24). It is perhaps too early to tell if such models, whose major overt characteristics are the use of flow charts, are enough of a departure to require separate treatment. Furthermore, it is not clear if the temporal order of the flow chart is an artifact of the analysis, or a reflection of the capabilities of real human beings. Most flow charts can be represented as decision tables which provide a more synchronous impression of the decision processes. More specifically, I was able to reduce the Gladwins' (25) flow chart to two decision tables, one following the other. Although I was unable to reduce it to one table, I am equally unable to decide if the supposed temporal order of the two decision tables has some reality in real time greater or lesser than the stepwise flow charts.

Second is the lexical/semantic relations, semantic field, or lattice approach. It was first introduced in anthropology in the work of Frake (20), although any earlier attempt at drawing folk taxonomies in the strict or extended sense qualifies as well. The early origins of Field Theory are found in the works of Trier, Portzig, and Weisgerber.

By taxonomy I mean: taxonomy in the strict sense which applies only to the test frame "_____ is a (kind of) ____". By extended sense I mean the more usual interpretation in anthropology which comprises any scheme of classification. For theoretical purposes, as I hope to show and have shown, especially in Werner & Fenton (74), strict taxonomies and other transitive relations (i.e. those that form hierarchies) must be separated. Thus, for example, a taxonomy of most human ethnoanatomies in the extended sense turns out to be an alternation of strict taxonomies ("The thumb is a kind of finger") and the part-to-whole relation ("The finger is part of the hand").

The notion was extended from the beginning to the entire lexicon. The list of available lexical/semantic relations constructed by Casagrande & Hale is amazingly exhaustive (6). The relatively low level of acceptance of this model thus far is apparently due to the unfortunate selection of block diagrams for the representation of taxonomies:

<table>
<thead>
<tr>
<th>A</th>
</tr>
</thead>
<tbody>
<tr>
<td>B</td>
</tr>
<tr>
<td>E</td>
</tr>
</tbody>
</table>

**Figure 2.** Block diagram of a taxonomy. **Figure 3.** Directed graph of a taxonomy.

While Figures 2 and 3 are for all purposes isomorphic, Figure 2 fails to show clearly that for an interpretation of the formal graph the edges (lines) of the graph need to be labeled (by the relation, here T for strict taxonomy) and directed (by arrows) to imply the assymmetry of the relation, i.e. that
ATB \neq BTA. Almost no one seems to doubt—in sharp contrast to componental analysis—that extension of the relational approach to the entire vocabulary of a language is valid. Among numerous field techniques for the collection of data that have been suggested, the elicitation frame is perhaps foremost. [See compendia of field techniques in Perchonock & Werner (52), Werner & Fenton (74), and Werner et al (75).] It is relevant because a well-chosen question will elicit an explicit statement about some of the field relations that may hold between two or more terms. The ultimate test for the existence of a particular kind of linkage is, with few exceptions, the existence of a linking sentence in the informant's language. Componental analysis can be interpreted as an analysis subsequent to a taxonomic analysis, that is, componental paradigms are further structures imposed on a particular level of a particular taxonomy.

Anyone doubting the foregoing statement should consider that the isolation of the kinship vocabulary which precedes a componental analysis of kinship addresses itself to the question, "Which terms in this language are kin-terms?" or "Is ______ a kind of kinsman?" A major shortcoming of many componental analyses is that they do not take into account intermediate taxonomic organization. In the Yankee terminology, terms like "parent," "grandparent," "child," "grandchild," "ancestor," "descendant," or even "blood relative" and "relative by marriage" are surely part of the reckoning. By imposing taxonomic organization first, the paradigms to be analyzed become considerably smaller than those usually found in the literature. Furthermore, the taxonomic structure imposes some degree of order on the components that reduces the number of possible analyses [see Burling's (5) apprehensions]. Furthermore, areas with ambiguities or multiple analyses are often cases where the indeterminancy carries some cultural significance. Surely the fact that all alternate analyses of the Yankee system contrast along one and only one of the dimensions of the analysis (namely the dimension of collateral distance) implies that such a distance measure is at best vague and highly variable, or even idiosyncratic, in view of situations in which American families find themselves today.

Thus paradigmatic structuring is not alien to a lexical/semantic field type description. However, the existence of universal semantic components is debatable (see Werner 69, 76). The unifying principle in both approaches is a common concern with the lexical resources of the languages under investigation. It is therefore possible to characterize this field as lexicographic ethnosience.

Summary.—The scope of the field of ethnosience vis-à-vis the general phenomenon of language is relatively narrow: the primary focus is the lexicon. Anthropologists have investigated the vocabulary of weddings, curers, illness, religion, anatomy, firewood, how to do things (naming the sequences of behavior), or similarly what the steps are that take one through a day—all in a variety of cultures. Historically most anthropologists did not exclude the
lexicon from language as proponents of autonomous linguistics did nor did they exclude the vocabulary from culture. Almost all statements of linguistic relativity or the Sapir/Whorf hypothesis are statements emphasizing the lexical resources of language.

I have argued (Werner 71) that proponents of an "autonomous linguistics" subscribe to a bias which I called "grammaticalist." In this view everything outside of phonology and grammar—supposedly the only rule-governed parts of language—are outside of linguistics. Perhaps Voegelin and Harris espoused this view most consistently in a series of articles well known to anthropologists of the early 1950s. Most anthropologists (especially Opier as the spokesman of the other side of the Voegelin/Harris position) can be characterized as representing the "lexicologist" bias. To these anthropologists a vocabulary that was not part of language (more precisely linguistics) made no sense (and rightfully, I believe). The lexicographic interests of ethnoscience have their roots in this bias.

In addition, anthropologists were concerned about how lexical items are related to each other. Folk taxonomies emerged early as an important principle of lexical organization. Although implicitly present in many entries of standard monolingual, English dictionaries, for example, they have not yet been systematized to any extent. The list of lexical/semantic relations of Casagrande & Hale, based on some 800 Papago folk definitions, is the first to be almost exhaustive. The authors postulate a relationship of constituency (part-to-whole) but find no examples. Neither do they find the Conklin/Frake relation of "_____ is a stage of _____," probably because they are dealing with people less essentially agriculturalists than the people whom Conklin and Frake studied.

It is important to mention here the work of Romney & D'Andrade (58) and D'Andrade (12, 13, and especially 14). All of these papers use a statistical approach for the validation of lexical/semantic structure. By this I mean following the typology of Campbell and Fiske, which calls for validation by maximally different procedures and replication by maximally similar procedures.

Eventually it may become possible to simulate the statistical validations as part of the question-answering routines. Similar frequencies for men and for machines will surely be highly significant.

More importantly, D'Andrade's latest paper (14) shows that statistical methods can be helpful in finding some strong lexical/semantic relations between pairs of terms. Some of these relations may not be elicitable with ease by any other technique. It is interesting that D'Andrade's work also seems to imply that significant success lies in the application of modal logic to deductive systems (see p. 275).

Other significant contributions to the notion of lexical/semantic fields in anthropology are Berlin et al (1), Kay (34, 37), Sanday (60), and Werner (75).

The stage is now set for a sophisticated view of the lexicon. In this view
the lexicon is a complex structure, probably some sort of lattice, where large numbers of terms are intimately interrelated. The picture that emerges is close to Bierman’s stars. Bierman (2), a logician, envisioned the organization of the vocabulary as a system of relations. Each distinct lexical item occupies a node in a large plane. Lexical items that are related are linked by colored strings. The color represents the type of relations. Since these relations are binary, every ray belongs to two stars, the one from which it emanates and the one that it reaches. Presumably sentences of a language are retrievals from, or paths in, this huge network. Any sequence of nodes, if properly constrained, is a possible sentence. The problem is, however, that anthropologists have found no explicit constraints which restrict in some justifiable manner the choice of possible paths through the lattice.

The unrestricted lattice idea is obviously the major weakness of ethnoscience. In general, a most rudimentary view of syntax prevails. Sentence frames are used to discriminate vocabulary items from each other or help in the assignment of vocabulary items to specific domains. How human beings in any culture, with any language, in spite of (so to speak) huge lexical/semantic fields are able to speak or answer questions has not been broached. It is fair to say that language, or the cultural manifestation of language, is seen from the vantage point of a sophisticated lexicographer. The informant’s universe is seen as being predominantly his lexicon.

*Generative semantics.*—Following approximately Postal’s (53, p. 261) and Maclay’s (44, p. 177) representation GT-3b, generative semantics can be characterized as diagrammed in Figure 4. I am simplifying in order to serve better the purposes of this paper; for a more complex view, see Lakoff (41).

![Figure 4. Schema of generative semantics.](chart)
Roughly, Figure 4 can be explained as follows: SEMANTICS is considered first as truly generative, that is, randomly generating phrase markers P(0) or tree graphs which serve as structural descriptions of the arrangement of semantic units. I will not utilize this view because I do not see how it could contribute to ethnosience. I will use the second view throughout—any output produced by the stimulus provided by a question. This view is perhaps closer to the view represented by Binnick (3).

A second interpretation, which assumes that the character of the "device" called SEMANTICS is better known, produces one phrase marker P(0) at a time. The structure within SEMANTICS is conceived as somehow linked to a question that elicited it. That is, a P(0) may be produced in response to a question. The structure P(0) is then taken through a series of TRANSFORMATIONS which (a) insert phonological representations of semantic units and (b) change the structure P(0) into P(n)—the surface structure—which is an acceptable input to the PHONOLOGY, which in turn provides the sentence with directions for pronunciation or phonetic representations.

If, according to generative semanticists, there is a boundary left between semantics and syntax, it is the boundary between SEMANTICS and the "transformations." However, what I identify here as "transformations" must be interpreted more broadly than the transformations in the Aspects (Chomsky 7) sense (see Lakoff 42). Furthermore, as Postal (53) points out, some of the transformations must apply before the boundary that separates SEMANTICS from syntax. This is due to the fact that lexical entries themselves possess an internal structure. This structure also has the form of dependency trees. Otherwise it would be difficult to account for the underlying structure of many verbs similar to "kill." The semantic primitive of this verb is assumed to be "to cause to die." The lexical representation (for greater detail of the notational convention see Figure 5) of the verb "to kill" is then depicted approximately as in Figure 5.

![Figure 5.](image-url)
In the view of generative semanticists (especially Lakoff 41, McCawley
48, and other publications), P(0) markers do not stand alone. They are ac-
accompanied by presuppositions. Lakoff (41, p. 235) gives the following ex-
ample: A sentence such as "Pedro regretted being Norwegian" presupposes that
"Pedro is a Norwegian." A semantic representation SF of a P(0) is con-
ceived as

\[ SR = (P(0), PR, Top, F, \ldots) \]

"... where PR is a conjunction of presuppositions, Top is an indication of
the 'topic' of the sentence, and F is the indication of the focus of the sen-
tence. We leave open the question of whether there are other elements of
semantic representations that need to be accounted for" (Lakoff 41, pp.
234–35). The presuppositions contain the speaker's and the listener's knowl-
edge of the world. They are indistinguishable from standing sentences, that is,
sentences with a truth value independent of time. In the above example it is
presupposed not only that "Pedro is Norwegian," but also that "Pedro was a
Norwegian" and that "Pedro will always be a (native) Norwegian." That the
knowledge of all the world is involved emerges from the fact that not only do
we know that "Pedro is a Norwegian," but also that "Pedro is a certain man"
whom the speaker knows, but the hearer may not. That "Pedro is a man's
name," that "Pedro is human, animate and a physical object," that "Pedro is
an unusual name for a Norwegian," that "One would expect Pedro to be a
Spanish name," that "Einar, Olaf, Knut ... are more usual Norwegian first
names, "... etc, etc. All of these presuppositions may come into play at one
time or another in a discourse about Pedro and his regrets.

When Lakoff says "Pedro regretted being Norwegian." I am tempted to
ask "Who the hell is Pedro?" To which Lakoff would probably reply by pro-
viding part of his presuppositions about Pedro, e.g. "He is that nut I told you
about yesterday who likes Southern California but thinks that he can't immi-
grate because he doesn't have a valid birth certificate." Lakoff's reminder will
serve to refresh my own memory of presuppositions and/or provide new ones
as well.

If a sentence contains embedded sentences, each embedded sentence re-
quires its own presuppositions. Lakoff diagrams this as follows:

\[ S(0) \]

\[ \triangle \]

\[ S(1) \]

\[ \rightarrow \]

\[ S(2) \]

\[ \triangle \]

\[ \triangle \]

**Figure 6.** Embedded sentence presuppositions.
where the long arrow stands for "sentence S(1) presupposes sentence S(2)."

McCawley (48) further assumes (which is at least implied by Lakoff's diagram) that semantic representations are in the form of phrase markers. Rather than resembling natural language, they resemble the predicate calculus of symbolic logic. Thus Sapir's sentence "The farmer killed the duckling" may be represented as follows:

```
  S
 /|
/  |
|  |
Proposition NP:x(1) NP:x(2)

killed(x(1),x(2)) by the farmer the duckling
```

**Figure 7.**

x(1) and x(2) are the variable arguments of the predicate "killed."

The independence of the noun phrases NP is argued by McCawley on the basis of examples like "John says that Nancy wants to marry a Norwegian."\(^5\)

The sentence is ambiguous among the three senses (i) there is a person who John says Nancy wants to marry and who the speaker identifies as a Norwegian, (ii) John says that Nancy wants to marry a certain person whom John identifies as a Norwegian, and (iii) John says that Nancy wants her future husband (whoever he might be) to be a Norwegian." [McCawley continues] It is difficult to see how these senses could be assigned different 'deep structures' unless those structures allowed Noun Phrases to occur separate from the propositions that they are involved in and to be constituents of sentences in which those propositions are embedded (McCawley 48, p. 225).

The word order in kill \((x(1),x(2))\) is justified by McCawley (48) on the grounds that many transformations in English can be stated considerably more simply if the verb-first order is assumed. The only alternative in English is a verb-second position which complicates matters.

In order to illustrate this point, let us look at a crude approximation of the passive transformation (articles and the introduction of the stative aspect of the verb "to be" in the passive are suppressed):

\(^5\) My apologies to all Norwegians. I am sure that neither Lakoff nor McCawley intended this coincidence, which is purely accidental.
In the verb-second structure both NPs would have to be moved. Thereby an important generalization is lost: move the focal NP into initial position where it becomes the topic of the surface sentence.

Recently it has been argued (e.g. McCawley 47) that the distinction of the traditional parts of speech is a characteristic of relatively shallow structure. Lakoff (39) has shown that adjectives and verbs may be considered as one form class. Furthermore, transitive nouns are formally identical to transitive verbs:
The only notion that remains for SEMANTICS is "predicate of" (more on this later).

The insight that tense and modals and other verbs like "usually" are derivable as higher (further up in the phrase marker) predicates introduce important simplifications.

Other work has concentrated on the nature of quantifiers, especially the similarity of the universal quantifier and the conjunction "and" and the existential quantifier and the disjunction "or." However, there are still many more unsolved problems and summarizing may be premature.

Finally, some as yet rudimentary efforts have been directed toward the inclusion of deductive rules in the theory. Some sentences can be clearly derived by deduction from other sentences and possibly from the set of their presuppositions. Lakoff (40) in particular has made some exploratory forays into applications of modal logic, which introduces predicates of possibility ("may") and necessity ("must") and can be extended to temporal predicates (i.e. "sometimes" and "always"). His insights are at this point largely provisional.

Summary.—It is apparent that generative semantics has made significant progress toward the solution of some of the problems that face ethnoscience. One of these is that lexical entries cannot be conceived simply as bundles of components. The notion of presuppositions converges with the notion of a lexical/semantic field. But while anthropologists have pointed out some of the characteristics of the lexical/semantic network, linguists have largely ignored it. Perhaps it can be justly said that the insights of generative semantics are still rooted in the notion of an autonomous syntax, which they rightly attack. Their methods, approaches, and arguments proceed largely from a syntactic base at the expense of the structure of the lexicon and the need for some deductive capacity for natural languages.

Semantic information processing.—Although many workers in this field are associated with Minsky (49) at MIT, they do not operate with a paradigm that can be easily characterized. The efforts I want to describe are all concerned with computer-simulated question-answering systems. In recent years these studies have assumed two relatively independent directions differing primarily in their emphasis rather than in overall orientation. The first approach is an attempt to model the structures that are necessary for the representation of large memories. The second concentrates on the deductive capacities of question-answering devices.

Memory models.—Among a dozen or so experimenters in artificial intelligence, Quillian (54, 55) is unique. He deals almost exclusively with the simulation of very large memory structures "... in which newly input symbolic material would typically be put in relation to large quantities of previously stored information..." (54, p. 220). "Actually most simulation pro-
grams . . . have not been primarily concerned with long term memory at all but rather with cognitive [deductive] processing" (54, p. 219). Quillian’s semantic memory model can be envisioned as a card file representation of the lexicon. Every dictionary entry has one card. Each card contains one entry name which Quillian calls the Type. Under the entry are paraphrases (definitions) in a special notation. Occurrences of words in these paraphrases are termed Tokens. The linkages of lexical-semantic-relations are as follows: 1. B names a class of which A is a subclass: A → B; corresponds to the relation of taxonomy; 2. B modifies A: A ← B corresponds to the relation of modification; 3. A, B, and C form disjunctive sets A or B or C; corresponds to disjunction; 4. A, B, C form a conjunctive set set A and B and C corresponds to conjunction; 5 and 6. B, a grammatical subject, is related to C, a grammatical object, in the manner specified by the relation A (verb) A → B ← C; 7. the associative link is the system of addresses (links) that relates the occurrence of a word to its occurrence in a paraphrase, that is, it connects every entry card to the occurrences of that entry in the definitions of other entries. Thus this system of relations makes Quillian’s model a general graph or network of associations.

The experimenter’s task is to select any pair of words from a store and to submit them to the program for comparison. He may check the machine’s comparison against the comparison made by a native speaker. That is, “he considers whether or not the machine’s output is one that might reasonably have been produced by a subject” (Quillian 54, p. 237).

Quillian’s second model (55), the TLC (Teachable Language Comprehender), is also an attempt to simulate input and output information in smooth English. As a result the canned phrases and ad hoc syntactic form tests (parsings) of the sample outputs give an extremely and deceptively human-like impression.

The basic parts of the program are (a) the memory network, (b) a processor, and (c) an on-line human monitor. The memory is structured similarly to Quillian’s first model but is somewhat more abstract: the dictionary part is simply a list of lexical items with a list of addresses following every entry. The addresses point to nodes in an abstract network. In my terminology one could look at his lexicon as a linear arrangement of items which connect to a giant switching circuit, the abstract network. The network consists of units which have one obligatory element, a pointer to its superset (superordinate taxon). In addition, the unit may have pointers to several properties. These have attributes and values which are also pointers. Roughly, the units are the Types of Quillian’s first model and the properties and values are the Tokens. The attributes are verbs and adjectives and the values are nouns that function as grammatical objects. Thus Quillian’s second model is essentially Quillian’s first with only the relation (1, 5 & 6) retained.

“... The comprehension procedure of TLC may find a number of properties related to a piece of text and, by using adapted copies of these (i.e. representing them in Quillian’s special notation), create a complex inter-
linked structure, in the same format as the memory (i.e. these structures are merged with the existing portion of the memory), representing the particular meaning of the current input string” (55, p. 475).

By relating the meaning of the current input string to already present information, the device is capable of producing one (? all Quillian’s examples contain but one) possible paraphrase (see below).

An intermediate model between Quillian’s and the models emphasizing the deductive capabilities rather than memory organization is PROTOSYNTHEX III by Schwartz et al (61).

This structure consists of four parts. First are the ordered triplets of the form X R Y where the R are generally verbs and the X and Y can be simple or complex. Second, there are special relations. These contain the relations of taxonomy and every concept is in a taxonomic relation to at least one other concept. Concepts may also be in the relation of equivalence to each other. The primitives are the most general concepts of the system. Third are the complex relations which form the backbone of the deductive system. These are abbreviations of complex systems (relations of relations) of the special relations and are called inference rules. Finally, there are the semantic event forms whose function is similar to Katz & Postal’s selection restrictions. That is, they are called upon to resolve ambiguities (?)

While the deductive capabilities of the system are impressive (it can, for example, deduce the problem of the monkey, a stick, and the bananas above the monkey and out of his reach) it seems in spirit closer to Raphael’s and Black’s systems of limited deductive capabilities. At no point is there an attempt to correlate a large lexical/semantic field. All examples given start with some simple input sentence inserted by hand and deductions based on such very limited input. For lexical/semantic fields the inference rules appear to be unnecessarily complex. For example, if AR1B stands for “A is brother to B” and CR2B for “C is father of B,” then the inference rule A[R1 C/P R2]B is constructed, that is, “A is uncle of B” (more precisely, “A is a kind of uncle of B”). It is not clear how this would constitute a simplification over: If (A)T (brother of C) and (C)T(father of B), then (A)T (uncle of B), where T stands for the taxonomic relation of the earlier formulation. This solution requires no new symbols nor new relations.

The program searches a question like “Who lost the battle of Waterloo?” in two steps: (a) “Who lost the battle?” and (b) “Battle of Waterloo.” In order for the device to function properly, auxiliary propositions are necessary if it is to solve more complex situations. If the input sentence was “Napoleon was beaten in the Battle of Waterloo,” then the above match will not work. Optimally one would expect the device to respond “if ‘was beaten in’ is equivalent to ‘lost’ then the answer is ‘Napoleon.” Please confirm first part of proposition.” It would be the human monitor’s task to make such confirmations. After validation of the equivalence, the two verbs become part of the permanent capability of the system. In this sense Quillian (55) is right in making his device teachable. Teachability, that is, the capacity to ask ques-
tions (or the confirmation of hypotheses as above) in the process of question answering, seems the only economical way to build such devices.

If none of these models represents a "great leap forward," it should be remembered that at present "almost any program able to perform some task previously limited to humans will represent an advance in the psychological theory of that performance" (Quillian 55, p. 459).

**Deductive models.**—Most of the models in this category deal with some form of symbolic logic. Much of the work in the field is brought together by Minsky (49). Later developments are described by Simmons (63).

Instead of going into a detailed exposition of symbolic logic, let me try to indicate by an example roughly what is involved. Part of the following example is from the R2 program (Biss et al 4) which contains in a verb-first logical notation a data base of some 2000 sentences of the Illinois Driver's Manual *Rules of the Road*. The device has thus by far the largest memory on which to base its deduction. Logical operators include "and," "or," "not," and "implies." Quantification may occur over any variables of this artificial language.

Following Bis et al (4), if the system receives the question (conjecture):

(i) Do cars always yield to pedestrians?

the resolution principle is invoked for the solution. Following Slagle (64) this means that "... a clause implies its factors ... [and therefore the solution is found by] ... working back from the conclusion toward the hypotheses of the theorem to be proved." In the above example this works approximately as follows:

First, the conjecture (question) (i) is negated ('always' assumed for all examples), which yields the 'clause':

(ii) Cars do not yield to pedestrians.

This is compared to the 'knowledge' of the system which states:

(iiiia) If X yields to Y then Y does not yield to X.

or for this example (insertion of x and y):

(iiiib) If a pedestrian yields to a car then a car does not yield to a pedestrian.

Because of the formula $p \Rightarrow q \Leftrightarrow \neg p \lor q$ ($p$ implies $q$ if and only if not $p$ or $q$). Therefore (iiiib) can be restated:

(iv) Pedestrians do not yield to cars or cars do not yield to pedes-

It follows from the identity of (ii) and the second half of (iv) that

(v) Pedestrians do not yield to cars
But the 'knowledge' of the system includes the statement:

(vi) Pedestrians yield to cars in crosswalks

Comparison of (v) and (vi) leads to a contradiction and the system replies to question (i): "No."

This problem as presented is almost trivial. However, with a large number of propositions (axioms) to select from, heuristic methods have to be found. Such heuristic programs operate generally as follows: At every step in the proof procedure there are numerous alternatives. The program tries each alternative on the first level (usually there are many levels). Some evaluation measure exists that assigns to the first step of each possible alternative some numerical value. The program selects the alternative with the highest value and checks all the alternatives at this point and so on. In some very sophisticated programs values of past evaluations are stored so that if a lengthy backtrack is necessary, the evaluations do not have to be recalculated. The device goes back and simply chooses the alternative with the next highest evaluation. Most chess-playing programs operate this way.

Summary.—The workers in the field of automatic answering systems usually use some simplified form of ordinary English. There is usually some kind of representation of propositions in a central memory. The notation systems are usually variations of notations of the first-order predicate calculus. All processes of deduction use variants of the propositional calculus of symbolic logic; some use the full power of the first-order predicate calculus. As soon as the problems reach some degree of complexity exhaustive searches are out of the question and heuristic programs have to be applied. These, stated most simply, use some evaluation procedure to reduce the number of cases which the device has to inspect in its path toward a solution. Both in the complexity of their deductions and the sophistication of their heuristic programs, the workers in this field are exceedingly advanced. However, their notions of the nature of the lexical field, the nature of lexical/semantic relations, and the fact that many of their solutions are simply "to solve the immediate problem" (i.e. ad hoc) rather than motivated by insights into the nature of language weaken their position. Much of the work in this area creates the impression that quick payoffs are the primary goal at the expense of theoretical insights into the nature of language. In this sense, although raising interesting questions, Dreyfus' (15) critique of "artificial intelligence" suffers from his tacit assumption that we have before us the ultimate in sophisticated computers, and more importantly, that our theories of language are anywhere near adequate to the task of answering questions in a humanlike manner.

It is premature to talk seriously about the limitations of computers except as part of the limitation of deductive systems (see Werner 72, in preparation). The relatively slow progress and failure to meet predicted achievements (Dreyfus 15) are poor indicators of the ultimate potential and/or limitations.
For anthropologists the cross-cultural validity of symbolic logic is an important question. Unfortunately, in anthropology the statement "The logic of the ABC" (where ABC is the name of a tribe or nation) is far from clear. It may refer to the fact that the ABC have a different set of basic axioms (propositions) about various parts of their universe than we do (see e.g., Malinowski: 44a), that the ABC use a different style of argumentation and do not accept some of ours. Finally, perhaps their rules of inference are different from ours. However, the evidence to support or refute the latter hypothesis is at best scanty. A very strong commitment to occidental symbolic logic and its extensions to modal logic seems to be the safest approach to the cross-cultural problem. The stronger the commitment and the more rigorous the application of European deductive systems to native systems of knowledge, the sooner will discrepancies surface and demonstrate beyond doubt that the deductive logic in question is truly different.

Sociolinguistics.—According to Fishman (19), there are two aspects of sociolinguistics: micro sociolinguistics, which is synonymous with the ethnography of communication, and macro sociolinguistics. In this paper only the former is relevant. Perhaps the most fundamental contribution of sociolinguistics is the emphasis on not merely the speech act but on the context in which it takes place. Although some work in this field assumes context as given or as easily inferable by the observer, a more sophisticated view is closer to our aim. According to Hymes (29, p. 27), "native terms are one guide," and Fishman (19, p. 43) makes "verification from within the speech community" (emphasis his) requisite. In other words, context is a series of native language texts which following Ruesch & Bateson (59, p. 276) describe at least the following four contributing factors: "(1) 'Perception of the other's perception,' or the establishment of the unit of communication. (2) The position of each participant and his function as observing reporter. (3) Identification of the rules pertaining to a social situation. (4) Identification of the roles in a social situation."

Except for contexts that are difficult to verbalize, descriptions of context in the native language are propositions about the use of language, hence, an integral part of a system of propositions which form lexical/semantic fields. Tyler's (67, p. 268) notion that "context itself is a part of the semantic system" is therefore correct. However, features of context in a lexical/semantic field are not separate from other semantic facts. An occasional sentence containing the referent "red" (in the literal sense) requires some red object to be present. Similarly, it implies that certain listeners are involved, or that these correlate to each other in some way, or what mode they use to communicate, or their style of delivery and the topic of discourse (based on Hymes 29, p. 216) depend on the presence of these factors. That these are modifiers analogous to "red" can be seen from the observation that the speaker and listener(s) must have past experiences against which to judge these external clues. Such internal representations are probably identical with attributes. It is
not necessary that attributes be simple, especially since the elicitation of contexts may be derived from the elicitation of long texts. But if higher order predicates (like "may") modify an entire sentence, there is no reason to deny the possibility that relatively simple sentences can be modified by several complex interlinked sentences. Since a general question-answering device should be capable of answering all kinds of questions, and since questions about the social, psychological, ecological, spatial, and temporal context are part of the method of getting at cultural rules (including rules for breaking rules, i.e. systems of priorities) there is no rational way to separate contextual variation from other kinds of variation. At least in part (as I show below) it is possible to control variation of the topic of discourse by a partitioning of the vocabulary.

Summary.—Micro sociolinguistics or the ethnography of communication is a young field, although Malinowski ought to be considered an important ancestor. It does not have an explicit metalanguage for the description of its major concern, namely "context." The demonstration of the importance of context is a major contribution to our understanding of some of the variations of meaning. Nevertheless, the field is at this point primarily descriptive rather than highly theoretical, and although it can as yet contribute little to question-answering systems its lesson, that context is semantic and needs to be represented, or at least be representable, is of the utmost importance.

Problems and Comments

In this section I select a few problem areas raised in my exposition and elaborate on them. The selection is nearly random and reflects mostly my own interests among those which may some day lead to a working question-answering device:

Predicate calculus.—According to Reichenbach (56), the notational convention of \( f(x) \) for intransitive sentences (\( f = \) predicate, \( x = \) argument) and \( f(x,y) \) (\( y = \) second argument) for transitive sentences rests on the fact that although both can be written \( \alpha \ [f,x] \), or \( \alpha \ [f,x,y] \) respectively, the function \( \alpha \) is a constant. I will demonstrate, however, that in fact \( \alpha \) does assume different values.

In many languages the relations of taxonomy, synonymy, and attribution (modification) are all expressed by the same surface syntax. For example, "A doctor is a professional" (taxonomy); "A doctor is a physician" (synonymy); "A doctor is well educated" (attribution). This can be restated briefly as follows: all three relations are cases of attribution. The only explanation that remains is what kind of a relation of attribution is the relation of taxonomy or why it should be considered as one. First, it is well known that the taxonomic relation is the result of taking a superordinate taxon and modifying it in some way. Thus, according to Denisson's *Loose Leaf Dictionary*, "A lion is a wild animal," or \( (\text{lion}) T(\ (\text{animal}) M(\text{wild}) \) where \( T \) is the
relation of taxonomy and $M$ is the relation of modification. By adding attributes (modifiers) a term is created that refers to a subclass of the superordinate taxon. It can then be argued that the superordinate taxon’s function is also an attribute of the taxon it governs. That is, lion not only possesses the attributes of “wildness,” it also has the attributes of “animalness.”

There is one more consideration before summarizing. In componential semantic analyses one encounters another kind of attributive (modification) relation. Note that in the more usual lexical modification the order is crucial especially when nouns are involved: “A dog house is not a house dog.” However, in componential analysis or modification by adjectives there is little or no difference. For most purposes a “big red ball” is equivalent to “red big ball.”

The above can be restated as follows. There are basically at least four relations of predication (attribution) that differ from each other in the following manner:

1. Attributions $\alpha(1)$, symbolized by $M(1)$ is
   
   - Reflexive: $(x) M(1) (x)$
   
   - Symmetric: If $(x) M(1) (y)$ then $(y) M(1) (x)$.
   
   - Intransitive: It is not true that if $(x) M(1) (y)$ and $(y) M(1) (z)$ then $(x) M(1) (z)$.

2. Attributions $\alpha(2)$, symbolized by $M(2)$ is
   
   - Reflexive: (see above, 1a).
   
   - Asymmetric: If $(x) M(2) (y)$ then $(y) M(2) (x)$ may or may not be true.
   
   - Intransitive: (see above, 1c).

3. Synonymy $\alpha(3)$ symbolized by $S$ is
   
   - Reflexive: (see above, 1a)
   
   - Symmetric: (see above, 1b)
   
   - Transitive: If $(x) S(y)$ and $(y) S(z)$ then $(x) S(z)$.

4. Taxonomy $\alpha(4)$ symbolized by $T$ is
   
   - Reflexive (see above)
   
   - Asymmetric: (see above)
   
   - Transitive: (see above, 3c)

Many properties of lexical/semantic fields can be characterized by these four relations.

Applying this notation to McCawley’s verb-first propositional form (p. 300 fn) and using $M$ for $M(2)$ and Polish parentheses free notation:

* Although I do not want to ignore semantic subtleties that may point to differences of the two phrases, these are at present beyond the range of semantic sophistication available in anthropology or elsewhere.
Note that the order of the arguments is changed. The first M links the verb to its object, the second links the resulting verb phrase to the subject.⁷

\[
\begin{array}{c}
S \\
\text{Proposition} \\
\text{NP: } x(1) \\
\text{NP: } x(2) \\
M M \text{ kill } x(1) \ x(2) \\
duckling \\
\text{by farmer}
\end{array}
\]

**Figure 10.**

**Compound relations.**—There are at least two reasons for discussing what I call here compound relations: (a) there is a need to show how all of Casagrande & Hale’s relations can be explained by taxonomy, synonymy and modification; and (b) some notational convention is needed to account for the speaker’s intuition that such compound relations are perceived very similarly to the primitive ones, or in other words, that lexical/semantic fields can be constructed from simple as well as from complex relations.

The procedure I want to follow is to show the nature of complex relations and a proposed notational convention first on one example and then to generalize it to others that are possible.

**Part-to-whole.**—This relation is based on a sentence frame in English roughly as follows:

\[\text{"1 is part of 2."}\]

There are two ways to arrive at the same result: (1) By making the above sentence into a taxonomic statement in the strict sense: "1 is a (kind of) 2-part." Thus by extension the first element is also a kind of a part, 2

⁷ The simplest justification of this arrangement of the relation of modification is obtained by nominalizing the above sentences three ways:

- *The killing* of the duckling by the farmer...
- *The duckling* killed by the farmer...
- *The farmer* who killed the duckling...

Farmer is deletable in the first two instances where it is not focal. There seems to be a general rule that a verb can be focal only in the nominalized form of a sentence, while only noun phrases can be focal in the declarative mode.
that is, the second element modifies 'part' and the first element is taxonomically (transitively) related to the second element plus part; therefore \( T M(\text{part}) \) (_____)(______). (2) Treating 'part' as a transitive predicate leads to the same conclusion: the second element is analogous to the object, the first to the subject; therefore, by considering transitivity, \( T M(\text{part}) \) (___)(___). A very similar argument can be made for most Casagrande & Hale relations (6).

Some of the Casagrande & Hale (6) relations translate into the three basic relations T, S, M (for M(2)) as follows:

<table>
<thead>
<tr>
<th>Class inclusion:</th>
<th>(<em><strong>)T(</strong></em><strong>) or T(</strong><em><strong>) (</strong></em>) is equivalent to T M(kind) (_<strong>) (</strong>__).</th>
</tr>
</thead>
<tbody>
<tr>
<td>Spatial:</td>
<td>(usually of the form) “_____ Preposition <strong><em><strong>” or M M(Preposition) (</strong></em>) (</strong><em>). Some of these ‘prepositions’ require dual or plural objects in position 2, e.g. M M(between) (</em>__ &amp; <strong><strong>)(</strong></strong>).</td>
</tr>
<tr>
<td>Attributive:</td>
<td>(<em><strong>)M(</strong></em><strong>), or M(</strong><em><strong>) (</strong></em>).</td>
</tr>
</tbody>
</table>

**Function and Operation:** Although the precise meaning of these designations is not clear, I have argued in (74) that the function relation is that between subject and verb, therefore M; and similarly, the relation of operation between verb and object, also M. Thus the sentence “Brewers make beer” becomes M M (make) (beer) (by brewers).

**Comparison:** “_____ is like _____,” or M M(like) (____) (____).

**Exemplification:** “_____ is an example of _____,” or “_____ is exemplified by _____. The first is equivalent to the taxonomic relation T, and since it is transitive is also equivalent to T M(example) (___) (____). (The transitivity needs amplification, i.e. “Grass is an example of green” is probably incomplete and should read fully “Grass is an example of a green object” and “Green is an example of color” is extended to “A green object is an example of a colored object.” The second is the inverse of the taxonomic relation (or T).

**Grading:** This is a relation not reducible to the basic T S M relation. It involves temporal and spatial order (the symbol for this relation is Q for Queuing):
<table>
<thead>
<tr>
<th>Left to Right</th>
<th>Right to Left</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proximal neighborhood (immediate successor)</td>
<td>Q(1)</td>
</tr>
<tr>
<td>Distal neighborhood (eventual successor)</td>
<td>Q(3)</td>
</tr>
</tbody>
</table>

**Figure 11. Queuing relation.**

Where $Q(1)$ is the inverse of $Q(2)$ and $Q(3)$ of $Q(4)$ and $Q(1)$ implies $Q(3)$ and $Q(2)$ implies $Q(4)$ but not vice versa. However, before further commitment more work is needed in the explication of this relation.

**Provenience:** "_____ comes from _____," "_____ is made of _____," and possibly "_____ is a stage of _____" (or "matures into _____"). This looks, at this point, like variations of the queuing relation: stage and provenience are related to the distal $Q(2)$. Apparently some more or less complex process intervenes between the first and second term. Again more work is necessary.

**Synonymy:** $(____)S(____)$ or $S(____) (____)$ is reserved for identity. It is possible that some other relation may be necessary for functional identity: for example, Lounsbury type expansion/reduction rules. In the Yankee kinship system $M M(____) (uncle) (vocative)$ is in direct address functionally equivalent to ‘uncle.’ In other words, any modifier of uncle is dropped in direct address.

**Antonymy:** Lyons (43) lists three types: 1. Complementarity, either "_____ implies not _____," or "_____ is equivalent to not _____" (or if not $= N$, unary relation, and implies $= F$, a binary one) $F N(____)$

$(____)$ or possibly $S N (____) (____)$. Whether the two cases are equivalent remains to be seen. 2. Antonymy par excellence, a relation not well understood at present but somehow linked to queuing: big $Q(1)$ bigger $Q(1)$ biggest, implies small $Q(2)$ smaller $Q(2)$ smallest. (3) Converseness, also somehow connected to queuing: “A sold B to C” implies "(A owned B)" $Q(1) "(C owns B)"$" and "(C bought B from A)" implies "(C owns B)" $Q(2) "(A owned B)"$ " Paul Kay's notion of 'contrast' (37) also sheds some light on the problem. However, much more needs to be done.

**Partitioning of the vocabulary.**—Partitioning of the vocabulary for ques-
tion-answering machines is extremely important. It accounts for the notion of context of discourse, and possibly can be extended to account for sociolinguistic partitionings and the reduction of searches to manageable proportions both in space and in time. The details will become apparent with the example in Figure 12 of a taxonomic representation: a hypothetical three-level taxonomy, highly regular and with four branches on each level in a tree representation (drawn only partially).

![Figure 12. Hypothetical three-level taxonomy.](image)

This taxonomy can be coded into a matrix. A three-level taxonomy was selected because it can be represented in three dimensions. Obviously for matrices of greater depth n-dimensional matrices will be necessary. These are simple generalizations of the three-dimensional case. The three-dimensional matrix is represented as a cube.

The rationale for this representation is to construct a space in such a way that every sibling and especially every ancestor node is an immediate neighbor (i.e. of one link distance). This is a condition of the transitivity of the relation of taxonomy.

Since node 0 dominates all subordinate nodes, it occupies an entire plane in the space (note the 0 plane in Figure 13—two rows have been excised in order to reveal part of the internal structure). The next level of the taxonomy is perpendicular to this plane and consists of the rows of 1, 2, 3, and 4, i.e. 1 T 0 (or 1 is a kind of 0), 2 T 0, 3 T 0, and 4 T 0.

The next level of the taxonomy is again perpendicular to these rows. These are the single cells—for example, 5, 6, 7, and 8 with 5 T 1, 6 T 1, 7 T 1, and 8 T 1—but since each of these is also dominated by 0 they are also perpendicular to the 0 plane. The illustration shows the terminal elements of the taxonomy being perpendicular to the element 7, i.e. 29 T 7, 30 T 7, 31 T 7, and 32 T 7. Note that the terminal nodes are also perpendicular to the 0 plane and the 1 row, as well as to element 7.

The major advantages of this representation are:

1. The transitivity of the taxonomic relation is a characteristic of the spatial arrangement of elements. In other words, syllogism, the simplest deductive capacity of the device, is part of the spatial arrangement.
2. Perhaps more importantly, partitioning of a large taxonomy, that is, the application of different search strategies, is simply a partitioning of the n-dimensional taxonomic space (three-dimensional in the example).

A depth-first search is, in the three-dimensional example, any proper subcube. For example, the following illustration shows the cube which represents the right-most branch of the tree in Figure 12. Searches that are depth first with a fanning out on the lower levels are sub-oblongs of the $5 \times 5 \times 5$ taxonomic cube (see dotted line in Figure 14).

A breadth-first search is the row shown in Figure 15. It includes only the first level of the tree. A breadth-first subtree of two levels is the plane represented in Figure 16.

A subtree search on a subtree of n-1 depth is a hyperplane (true plane in
the three-dimensional illustration) parallel to the hyperplane of the top node. Figure 17 illustrates a subtree of Figure 13.

Subtrees can be found in any search as subfigures of larger figures (submatrices of larger matrices). Thus Figure 18a is a subtree of Figure 16 and Figure 18b is one of Figure 15.

The need for partitioning taxonomies can be explained in detail as follows: It is a well-known fact about human information retrieval that it is fast rather than accurate; it is not as exhaustive as computer retrievals tend to be. Frequently used items tend to be more in focal awareness and hence more easily accessible than little-used items. Items in the device are moved up to a more focal position every time they are used for retrieval or in deductions. The corner facing the viewer in Figure 13 represents such a more focal position. Thus if the branch of node 2 should become more used or of greater interest, it could be moved to the left of the branch of node 1 (that is, branch of 1 and branch of 2 would change places, and so on). This moving to the left procedure explains what happens if discourse is domain limited. For example, if branch 1 is dealing with chemistry and if the discussion is about chemistry, the term "radical" would be interpreted by its chemical sense. If the branch of node 2 deals with politics, then an exchange of the branches of
node 1 with the branch of node 2 brings politics into focus and gives an appropriate interpretation to the term "radical."

Thus partitioning of the vocabulary accounts for use of speech in contexts bound to particular isolable cultural domains. Similarly, partitioning can be extended to include sociolinguistic contexts, e.g. intimate discourse vs formal discourse. It is conceivable that one could partition along two parameters at the same time: e.g. intimate talk about the domain of sex versus formal medical talk about the same domain.

In addition, the partitioning schema accounts for the indeterminacy of the length of paraphrases in the following way: the length of a paraphrase to a particular question is determined by the maximum amount of available information, i.e. the maximum extent of the subtree dealing with a particular topic plus its associations (i.e. the rest of the lattice) and the available time to answer the question. The available time is gauged by some perceived implicit sociolinguistic criteria. The computer device, however, would have to request clarification by an explicit "impatience factor" or other such indicators. The response of the computer to the answer of the above request for a time limit is also representable as a partition of the response matrix to the most commonly used pathways, i.e. some subsolid of the cube in Figure 13.
Since the mechanisms of such subpartitioning are not very well understood, the n-dimensional hypercube representation of a taxonomy seems particularly well suited for the investigation of this phenomenon. By varying different parameters of partitioning, comparisons can be made with the behavior of human beings.

Experimental work in psychology (e.g. Mandler 45) seems to indicate that the size of the taxonomies is somewhere of the order of $5 \pm 2$ items on each level, Assuming the maximum number, i.e. 7, the requirement is an $8^n$ matrix. For a maximal depth of $n = 7 \cdot 8^7 = 2,097,152$, which is ample for extremely large vocabularies [even considering that 262,144 (or $8^n$) cells are needed for the most general term—however, what seems wasted in space is gained in speed]. A reduction of storage requirements is achievable by breaking the large taxonomies into a series of interlinked smaller ones. This could account for the fact born out by observation that genus terms and species terms are more easily accessible than intermediate and very much more general terms. In Perchonock & Werner (52) we showed that the term "food" and specifically named foods were easiest to elicit. This argues for a relatively independent subtaxonomy of "food" loosely linked to the taxonomic aspect of the Navajo universe (Werner & Begishe 70). Such an arrangement
makes the retrieval of items in subtaxonomies like food, plants, or animals very efficient.

Attributes can be incorporated into the present schema in the following fashion: First I expand the previously given "definition" of (lion) in the following way:

"A lion is an African or Asian wild animal and a big cat."

Let us mark modifiers with a suffix M and as an edge pointing to the term they modify. Further we assume that every modifier with the modified repre-
sent a new taxonomic "double" node. The above sentence can be first represented as follows:

(lion) T (((animal) M (wild))) M (((African) D (Asian))) and (this could be expressed by a conjunction but that would complicate matters)
(lion) T ((cat) M (big)) (The truth of (cat) T (animal) assumed.)

Graphically this is representable as in Figure 19.8

In order to represent the above graph as a matrix visually it must be reduced in such a way that it can be represented in three dimensions. This is accomplished by substituting (A) for (animal), (Bo) for (wild African),

8 The McCawley type Polish and verb-first representation for the formulas is as follows:

T M(big) (cat) (lion)

In the first formula D is introduced following McCawley (47) as an n-ary relation:
(Co) for (wild Asian), (BC) for (big cat) and (L) for (lion). The matrix representation of the graph in Figure 20 is shown in Figure 21.

Both (lion) and (big cat) occupy two cells. Note that following the graph, (lion) is adjacent to (animal), to (wild African animal), to (wild...
Asian animal), to (big cat), and to (animal), etc (o(animal) o(Bo) should be read "wild African animal."

Whether it will always be possible in large taxonomies (multidimensional matrices) to move the end points (terminal nodes of different paths) into adjacent positions remains to be seen. Possibly polysemy will become definable by the impossibility of moving two homophonic items adjacent to each other.

The above model is not only plausible but very promising for the simulation of a large number of so far unstudied properties of lexical/semantic fields.

Variables.—A final point concerns the use of variables. In the preceding pages we have used x and y to denote the use of variable (usually) noun phrases. I do not think that in any simulation of human lexical/semantic behavior such use is justified or necessary. It is possible to utilize the natural characteristics of taxonomies, namely that every superordinate concept can be viewed as a pro-concept (in the sense of pro-noun for pronoun). All variables in sentence frames have ranges which extend over various subtaxonomies which are dominated by some concept. It is therefore natural to consider a convention which would view all superordinate nodes as variables for any concept under its domination. Thus McCawley's example may be represented as in Figure 22.

In fact, there is no need to represent the entry for the verb "kill" in this complex manner. The noun phrases for discourse on certain topics will come from the topic itself, and as long as this topic fulfills the requirement of the superordinate pro-concepts, insertion will be possible. Thus all we need is something like M M (kill) (animate) (entity). This is the full entry of the word "kill" in the context of this discussion. Lakoff (42) has argued that the above formula (or some variant of it) is not sufficient. Pronominalizations need to take into account the entire noun phrase, not just the head noun. Thus while "man" is insertable in a slot marked (animate), "dead man" cannot be. One solution is to treat (M) (dead) (man) as if (dead) was an operator that overrides the animateness of (man).
Summary

My emphases in this paper are the analytical aspects of ethnoscience. This is an important theoretical emphasis since no praxis is adequate without sound theoretical foundations. At present practical ethnoscience leads to better lexicography and better translation. However, our sophistication has not progressed much beyond the atomic definitions of Casagrande & Hale (6). Whether for analysis and critique of dictionary entries or for synthesis of new entries there is nothing better. The next steps appear to be much more difficult.

Relatively little has been done with the problem of individual variation. Here D’Andrade’s work (11, 13) stands out. Although his approach is averaging or statistical, the relationship of his methods to the usual lexicographic concentration on the knowledge of experts is poorly understood (however, see D’Andrade 14).

Computer exploration and experimentation with large lexical/semantic fields holds hope for the illumination of both: the problem of individual variation and the problem of practical applications.

The notion of context must be further explored. Partitioning of the vocabulary was one possible technique. Better methods and better techniques for eliciting subtle contextual variations must be developed. Neither this nor further advances in ethnosemantics through the use of computerized models of human memory are conceivable without a more thorough knowledge of the psychological literature. It is certain that the problem of computer simulation of lexical/semantic fields requires the integration of more knowledge than just the four fields discussed in this paper. For example, psychological experiments seem to establish the existence of a short-term memory. However, its utility for simulation is not clear and has consequently not been discussed in any of the proposed question-answering systems to date.

A general schema of the nature of language as described in this paper can be summarized as appears on the next page.

I would like to close this summary with a challenge: Kay (36, p. 30) has stated: “. . . cognitive models do not predict overt behavior. But when the cognitive model is supplied with the information it specifies as necessary for reaching a decision (emphasis added), it can predict overt behavior accurately.” Perhaps Kay’s statement is too strong. Surely not all behavior is predictable nor is all necessary information elicitable. But the basic question remains. If cognitive models can predict behavior, then where is the boundary between ethnoscience and the study of social systems?

Conclusion

About 10 years ago the late Uriel Weinreich (68) said hopefully, “[In semantics] almost everything remains to be done.” Some progress has been made but basically the situation remains unchanged. Eighteen months ago I (Werner 73) predicted the turn of the century as a possible target date for a working general question-answering device. I envisioned the day of a three-
MASTER MEMORY

Contents: Associative lattice or tacit knowledge
Equivalence rules (e.g. "If A parent of B, then B child of A")
Characteristics: Not necessarily logically coherent
Compartmentalized
Contradictions between "compartments" possible simply because they do not arise. At this loosely integrated level compartments do not interact and can interact only in focal memory.

Movement of subsystems from Master Memory to Workspace by partitioning; which may be dynamic, i.e. varying as speech act or discourse proceeds. Movement makes tacit knowledge focal.

LOGICAL WORK SPACE OR FOCAL MEMORY

Contents: Rules of deduction, Focal Knowledge related to current discourse.
Characteristics: Logically coherent
Contradictions not tolerated.
Deductions are calculated here.
Questions are processed here for answering.

P(O)
  .
  .
  .
  .
  P(n)

Domain of TRANSFORMATIONS

PHONOLOGICAL COMPONENT

ACTUAL SENTENCES
way interaction at a computer console between an informant, an anthropologist, and a machine that tries to mimic the informant's verbal behavior. Such interaction will be instructive to the anthropologist as well as the informant, and will allow for improvement of the internal workings of the machine and hence anthropological theory. For the first time it will make falsification of anthropological (or ethnoscience) hypotheses possible. That will revolutionize anthropology. However, I am less optimistic today than I was 18 months ago. Almost everything still remains to be done.
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KINSHIP SEMANTICS

Harold W. Scheffler

Department of Anthropology, Yale University
New Haven, Connecticut

"Accounts of meaning usually throw a handful of putty at the target of sign phenomena. . . ." (Morris 43, p. 19)

INTRODUCTION

The study of systems of kin classification has occupied a prominent place in anthropological theory since the publication of Morgan's Systems of Consanguinity and Affinity of the Human Family in 1871 (42). Yet, in the view of a number of anthropologists, many of us have still to learn the lesson taught by Morgan's early critics: that many or all of the terminological systems Morgan dealt with are not systems of kin classification, that their terms do not designate categories of kin, or if they ever do, this is only one of their semantic functions, and perhaps a relatively trivial one at that.

Elsewhere (59) Lounsbury and I have attempted to demonstrate that this assessment of the ethnographic facts is mistaken, that the designations kinship, kinship terms, and systems of kin classification have not been misapplied by most ethnographers in the way that their critics (47, 66) would have us believe; and we have given some indication of the concepts and methods we think are appropriate to the description, analysis, and comparison of such systems. This essay recapitulates the major points of our earlier argument and reviews some of the more recent, as well as very old, literature on the subject of kinship semantics.

The focus here is on what I think are the theoretically most fundamental issues in the study of kinship semantics, i.e. of the meanings of kinship terms. Little is said about the study of systems of social action whose norms are grounded in relations of genealogical connection. [A masterful review and critique of three principal "styles in the study of kinship" in this, the broader sense of the term, is now available (2).] This considerably reduces the number of publications that need to be noted, but I have reduced the number even more by concentrating on the works of a few scholars (not the only ones, of course) who have dealt with a wide range of topics and whose writings have significantly influenced the work of other anthropologists. If it

1 This review was prepared while the author was a Visiting Fellow at the Research School of Pacific Studies, Australian National University, Canberra.
seems that a few scholars are singled out for undue critical attention, it is
only because I have found their work stimulating and provocative. D. M.
Schneider deserves more than the conventional footnote by way of acknowl-
edgment. He has forced me to re-examine my own assumptions when I
thought I knew and understood them, but did not, and he has encouraged me
to subject his assumptions and interpretations to critical scrutiny even though
he could be virtually certain that I would continue to disagree with him. He
has generously permitted me to quote from a still unpublished though widely
circulated paper (65).

The reader should be advised that the terms denote, designate, signify, etc
are used in special technical senses in the following discussion; if their use is
not understood, much of the argument will not be understood either. For the
appropriate definitions see (59, pp. 3–12) or (12, 13, 34, 35, 43).

THE MEANINGS OF KINSHIP TERMS

The lengthy anthropological dispute about the “meaning(s)” of kinship
terms has been, by and large, both needless and relatively unproductive. It
has been needless because it has been conceded from the outset, even by
some of the most outspoken proponents of the antigenealogical, antikinship
point of view, that many if not most of the words in question are used to
designate kin categories. Spencer & Gillen, for example, asserted on numer-
ous occasions that Australian Aboriginal languages have no words “equiva-
 lent” to English father, mother, brother, etc. Of the Arunta in particular they
explain: “The whole classificatory system and social organisation is based on
the existence of . . . exogamous intermarrying groups, and . . . the fundamen-
tal feature of the terms used is that they are indicative of group relationship,”
but they added, only in passing, the same terms are indicative “sometimes, in
a secondary way, of individual relationships” (71, p. 44). Unfortunately,
they did not bother to elaborate on what they meant by “secondary,” but
extensive documentation of the apparently relevant ethnographic facts ap-
ppears in earlier studies by C. Strehlow (72, pp. 66–70) and Schulze (68).

It is not necessary at this point to inquire into the exact meaning of
Spencer & Gillen’s assertion that designation of “individual relationships” is
“secondary” to designation of “group relationships.” The question of the na-
ture of the relationship between these designata is logically secondary to the
questions of whether or not the terms each have more than one designatum,
and, if so, whether or not one of these designata is genealogically defined.
Spencer & Gillen answered “Yes” to the first question, and “Yes” in effect to
the second, at least in regard to some of the Arunta words in question. Their
mistaken notion that Arunta have no “knowledge” of physical paternity (cf
73, p. 730) prevented them from acknowledging that all of these words (ex-
cept those which are perhaps strictly affinal terms) have genealogical desig-
nata.

Once it is acknowledged that certain words may be used to designate ego-
centric, genealogically defined categories, it is difficult to see what legitimate
objection there can be to describing them as kinship terms and to acknowledging that their use in this way constitutes a system of kin classification. To describe them in this way is not necessarily to imply that their genealogical or kinship designata are their only designata or even their "primary" or most "important" designata. It must be kept carefully in mind that to describe a word as a kinship term is merely to assign it to a descriptive-semantic category on the basis of what we know about its meanings. Such categories are functional and not necessarily mutually exclusive with regard to the phonological forms (words, lexemes) which serve as vehicles for meanings. It may be possible to assign the same phonological form to several such categories. The only possible justification for thinking of descriptive-semantic (and analytic) categories as mutually exclusive would be to assume that all words are monosemic (i.e. have one and only one significatum), though perhaps with various shades of meaning. There are many reasons to suppose that this assumption is false, certainly about kinship terms, which may designate genealogically defined categories (thus our description of them as kinship terms) and other kinds of categories (e.g. categories of kin-like persons) as well.

Further still, once it is acknowledged that certain words may be used to designate genealogically defined categories, though they may have other designata as well, there can be no legitimate objection to independent analysis of the content and structure of that kind of usage (cf 63, 65, 76). To the best of my knowledge, no careful student of the subject today assumes that genealogical designata are the only referents of kinship terms or that analysis of such referents exhausts the potential for semantic description and analysis. What is commonly assumed (as in 12–14, 34–37) is that we call certain words kinship terms because they have egocentric genealogical designata, and that these may and must be analyzed independently of any other meanings those words may have. It has yet to be shown that these assumptions are ill founded.

If this much is conceded, it is clear that the dispute over the meanings of kinship terms has been misconceived from the outset. The real problem is not what kinship terms mean but the nature of the relations among the genealogical designata and significata of certain words and between those designata and any other designata those words may have.

If the argument has been conceded from the outset—sometimes only by implication—by some of the disputants, others have taken the stronger position that the words in question (or some of them) never are used to designate kin categories. Unfortunately, some have persisted in describing the words in question as kinship terms even though they maintain that these words do not designate kin categories (see Needham's early studies of prescriptive alliance systems of social classification). Thus what began as an argument about the ethnographic facts became also a largely unacknowledged dispute about the meanings of words used by anthropologists to describe such facts. This confusion aside, the argument has been that the words in question refer only to kinds of intergroup relationships, or only to kinds of jural ("le-
gally" sanctioned) relationships, or perhaps to both, but not to interpersonal relations of genealogical connection.

The jural relations interpretation leaves open the questions of the criteria by which these jural relationships are ascribed, allocated, or transmitted, and of whether or not the terms necessarily refer also to these criteria. But, more important, most of the words for which such interpretations have been offered have been described by the ethnographer(s) as kinship terms or, by implication, acknowledged to function as such. To argue that the words in question are not kinship terms, certain theorists have had to ignore abundant linguistic and ethnographic evidence to the contrary, even while claiming that their analyses are "factually comprehensive" (46, p. 259). Thus Needham's (46) analysis of Wik Munkan kinship terminology as a "two-section system of social classification" does not even mention the bulk of Thomson's (74, 75) detailed evidence to the contrary; and his several attempts to analyze the Siriono system of kin classification as an asymmetric prescriptive alliance system take no account of Schermair's linguistic data (references given in 59). Korn's (27) treatment of the Dieri data wholly omits consideration of Howitt's (24, pp. 46, 58, 70) report that Dieri distinguish terminologically between "own" or "real fathers" and "little [classificatory] fathers" and other classes of kin. It is not relevant at this point that considered analysis of these terminological distinctions disconfirms the results of analyses that have not taken them explicitly into account. If proponents of the "social categories" interpretation of systems of kin classification have not conceded the argument by failing to deal adequately with these data, neither have they proven their point.

Origins

We should not pass over Spencer & Gillen's reasons for arguing that Australian languages have no words equivalent to English father, mother, brother, etc, even though they acknowledged that in some of these languages there are words that refer "sometimes, in a secondary way, to individual relationships." One of the reasons was an implicit assumption of monosemy, or "one word: one meaning" (one lexeme: one significatum), which made it difficult if not impossible for them to deal adequately with their own observations by forcing them to look for the "common denominator" in all usages of a term and to regard that as the meaning. Without this assumption, it is difficult to see what objection they could have had to saying that some Aboriginal words are "equivalent" to English father, mother, brother, etc in some but not all of their respective meanings (especially their respective structurally primary designata).

The quest of early (and not so early) anthropologists for the "original" meanings of kinship terms owes much to their uncritical adherence to the assumption of monosemy. Faced with the fact of current polysemy (see below), they were forced to offer an "historical" explanation. Thus the dispute raged over which were the temporally prior or even "original" meanings of
the words, their "personal kinship" or their "group kinship" meanings? Some critics (9, pp. 118–42; 30) pointed out that some scholars were confusing questions of etymology with questions of what the terms now mean (i.e. diachronic and synchronic relations) and thus falling into self contradiction by sometimes speaking of the alleged original or prior sense as though it were still the only sense of the word.

Lang (30) took note of Spencer & Gillen's and Howitt's linguistic documentation of the fact that Arunta, Dieri, and other kinship terms are each used to designate more than one genealogically defined category. He argued that in the absence of historical documentary evidence, or of the careful regional comparative studies required to support an argument, there was no solid basis for arguing one way or the other about the "original" meanings of Australian kinship terms. However, for Indo-European languages where documentary evidence and comparative studies were available, he argued that the evidence suggested that kinship terms were often derived from nonkinship terms such as those signifying "woman," "man," etc. (by narrowing of their significata in certain contexts of usage), and in some cases were then extended from close to more distant kin (by expansion of their genealogical significata). On the basis of this comparative evidence and on the available Australian linguistic evidence (which he acknowledged was inconclusive), it was plausible, he argued, that the same semantic processes, resulting in the same contemporary semantic structures, had occurred in Australia. In Australia, he suggested, extension of the terms in their derivative kinship senses, so as to make them also "terms of tribal status" (i.e. terms signifying relative positions in subclass or section systems) entailed extension of the "legal statuses" to which they also referred.

Lang's hypothesis was both diachronic and synchronic, or about the temporal and the logical or structural relations among the various coexistent meanings of kinship terms. While I do not doubt that Lang's hypothesis is essentially correct, it should be noted that its diachronic and synchronic aspects need not be regarded as necessarily related. The assumption that diachronic and synchronic semantic structures are necessarily directly correlated is rejected by most modern philologists and semanticists (78, 79), and the linguistic evidence is more than sufficient to establish the validity of Lang's synchronic hypothesis.

**Polysemy**

The semantic condition in which two or more significata of a word are related, sharing some of their distinctive features and suggesting derivation one from another, may be described as polysemy (59, p. 6). We should be careful to distinguish polysemy from connotation and metaphor, which are described below (but cf 65, pp. 5, 36, 51–56; 6, p. 45). It is necessary also to distinguish two kinds of polysemy: one based on narrowing of reference by the imposition of additional constraints on the use of the word; the other based on widening of reference by the suspension of certain constraints. Nar-
rowing (or specialization) and widening (or extension) as diachronic processes must be distinguished from narrowing or widening as synchronic processes (79). Structural semantics is concerned with the latter—with the logical relations among the several senses of a word as it appears in a variety of specifiable linguistic and social contexts of usage. Etymological or diachronic semantics is concerned with changes in the structure of usage as described in structural semantic studies; valid diachronic studies depend on valid studies of the structure of word use at different time periods.

A polysemous word is frequently, sometimes obligatorily, marked by the addition of a term or phrase specifying which of its several possible senses is intended. [Other kinds of “marking” are possible (39, p. 29)]. The existence of such markers has often been ignored in kinship analyses, and some anthropologists seem to be unaware of their existence. Fox, writing of Hawaiian-like systems, says:

... all the men of the parental generation on both mother's and father's side are called by the same term. We tend to write this as 'father' but of course this is begging the question of what it really 'means'. All we know is that they are called by the same term (11, p. 240).

Taken literally, this statement about “all we know” is false and seriously misleading. Lang (30) and Hocart (23) knew better and at least attempted to take the full range of known linguistic facts into account. Hocart's attempt deserves further notice here since it has an important bearing on the question of how we tell which kind of polysemy we have to deal with, that based on narrowing or that based on widening (or extension).

Hocart (23) noted that in contemporary Fijian, as in many other languages, kinship terms are used in both narrow and broad senses. Thus Fijian tama may mean “father” or “male of the previous generation on the father's side.” He asked which of these was the “original” or prior meaning of tama and suggested that, contrary to the assumptions of certain other scholars (unnamed, but perhaps Lang), it was not improbable that the broader sense was also the prior one. It could be, he argued, that the expression “my tama,” meaning ego's father in particular, designates a special case of tama in the broader sense of the term, and that this specialized, restricted use of the term is a recent development. Hocart's etymological hypothesis implies a synchronic hypothesis: The sense of tama in which it designates the category “ego's father alone” is related by narrowing or specialization to the sense in which it designates the category “man of the previous generation on the father's side.” On this hypothesis, the latter broad sense of the term is its structurally prior or primary significatum.

A moment's reflection will show that this is not true. Note that the broader alleged primary sense not only includes ego's father but is defined by reference to ego's father. The category which includes only ego's father is logically prior to the more inclusive category defined by reference to ego's father. Therefore, we may conclude that the narrow sense of tama is its logically prior or structurally primary sense, to which the broader sense is related.
by widening or expansion (see also 56). Of course, it is conceivable that Ho-
cart’s definitions are wrong and that the broader sense of tama may and
should be specified in some other way that would avoid this objection. Per-
haps, but no attempt to meet this objection in similar cases has succeeded. All
such attempts have appealed, explicitly or implicitly, to egocentric genealogi-
cal criteria. Consider, for example, Needham’s (45) contention that the
terms of “prescriptive alliance systems of social classification” are “defined by
relations of descent and alliance.” To begin with, Needham has not yet speci-
fi ed what he means by “descent,” and we are forced to speculate on what he
might mean. I can imagine only two possibilities. He may refer to member-
ship of descent groups, in which case relations of genealogical connection are
relevant, for what is a descent group if not a special kind of genealogically
constituted social unit? Moreover, the kinds of descent group in question
have to be specified as ego’s father’s or mother’s etc, thus again specifying
these kin as the focal or primary designata of certain terms. Finally, the se-
monic content of the lexical markers which may accompany these terms
shows that father, mother, etc are their focal or primary designata. What else
are we to make of expressions such as “own,” “real,” and so on?

Alternatively, Needham may refer to the transmission of rights and duties
from one generation to another, i.e. to a jural system whose structure may be
describable in quasi-genealogical terms but which is not necessarily connected
to “the biological facts” (47; cf 27, p. 74). The “biological facts” aside—
they are relevant insofar as the people concerned posit any such facts (54,
58, 60)—it has to be added that the transmission or allocation of the jural
status with which Needham has been concerned is usually governed by con-
siderations of ego’s parentage (40, p. 191), again a strictly genealogical mat-
ter. The fact that the relevant statuses may be assumed by people who are not
genealogically entitled to them (as in adoption as a kind of kinsman or as a
member of a descent group) does not lead inevitably to the conclusion that
we are not dealing with genealogically ascribed jural statuses. Ascription by
genealogical criteria does not preclude voluntary assumption of the same sta-
tuses by individuals who do not assume that they are kinmen.

If by “descent” Needham does refer to the transmission of rights and
duties, i.e. to a jural system, his argument about “what kinship terms (so-
called) really mean” is doubly deficient because, again, it leaves quite unan-
swered the most fundamental sociological question: By what criteria are the
statuses distributed, allocated, or transmitted? Wherever this question has
been given the attention it deserves, it has invariably turned out that kinship
terms may refer to relations of genealogical connection, or to jural statuses
ascribed with reference to such connections, or to both at once. Any attempt
to ascribe analytic insignificance or unimportance to one or the other of these
referents is bound to foreshorten ethnographic understanding and to lead to
an impoverished and self-deceptive theoretical system. It does not follow,
however, that we must suppose that they are structurally equal referents (see
below under Connotation).

It would be foolish to assert that all kinship terms are polysemic or that
the relationship between two signifcata of a kinship term is always one of extension or generalization. It has been noted on numerous occasions that certain words function both as kinship and nonkinship terms (12, 23, 30, 59) and that their kinship senses are related to their nonkinship senses by narrowing or restriction of reference. It may be useful here to cite an example of narrowing within the domain of kinship itself. Silverman (70, pp. 242-43) reports that Banaban tari may designate the category "same-sex sibling" or the category "sibling," while mane may designate only the category "opposite-sex sibling." Since the category "sibling" is logically prior to the categories "same-sex sibling" and "opposite-sex sibling," it follows that the primary sense of tari is "sibling" and that the special restricted subcategory "opposite-sex sibling" is singled out and given the special designation mane. This leaves tari with two senses: its primary sense in which it designates the category "sibling" and the derivative (narrowed) sense in which it designates the residual subcategory "same-sex sibling." The lexical evidence, by the way, offers no reason to assume that same-sex siblings are regarded as the siblings par excellence (cf 70, p. 243).

Connotation

In 1907 Rivers (49, p. 322) attempted to resolve the protracted dispute over whether kinship terms refer to genealogical or social relationships. He argued that the weight of the evidence is that kinship terms, as they are now used, often have both kinds of meaning, and he spoke of the "double nature of the classificatory system as an expression of status and of consanguinity." Rivers' distinction has been accepted by most anthropologists, and the dominant tendency, as illustrated in the works of Malinowski (39), Radcliffe-Brown (48), and many of their students, has been to suppose (not without seemingly good reasons) that the relationship between the two kinds of meaning is one of normative ascription and therefore hierarchical.

As Radcliffe-Brown (48) saw it, the typical if not universal arrangement is this: A more or less distinctive set of rights and duties is ascribed to the primary (closest) denotatum or denotata of each term, and along with the extension of the term to more distant kinsmen goes the extension of those rights and duties, or of some subset of them (see also 38). That is to say, the jural status referred to by a kinship term when used in one of its extended senses is most often an attenuated version of the jural status to which the term may also refer when used in its primary sense.

We need not consider Radcliffe-Brown's generalization about jural-status extension any further than to note that exceptions to it have been recorded (59, p. 153) which may only limit the range of its validity, and that Keesing (26) has noted how difficult it may be to distinguish the rights and duties ascribed to categories of kin from those ascribed to the same persons acting in other capacities or "social identities." The more important question concerns the validity of the assumption of a hierarchical relationship between reference to kin categories and reference to jural-status relationships, especially at the level of the primary senses of the terms.
In technical semantic terminology, the assumption in question here is that the status relationships to which kinship terms may refer are features of connotative rather than definitive meaning or, in other words, they are not among the distinctive features of a term's significatum (or significata if it is polysemic). It is important to make this distinction between signification and connotation as clear as possible, for it has been the source of some confusion and misunderstanding (cf 4, 32, 65, 70).

The significant, defining, or distinctive features of a class are those features or qualities which an object must possess if it is to be regarded as a member of that class; they are the necessary and sufficient conditions for membership of the class and for designation by its linguistic sign (34, pp. 192–93; 35, p. 1074; 43). In contrast, features of connotative meaning are those additional attributes possessed by some or all members of the class but which are not proper defining features of the class. In logical terms, they are accidental or nonessential conditions of membership, rather than necessary and sufficient conditions for membership of the class. Such features may become associated (in the minds of the speakers of the language concerned) with a class of objects, or some part of the class, through repeated dealings with members of the class in various behavioral contexts; and the society may set norms or standards for dealing with members of the class, thereby tending to standardize the connotations of the term which designates the class.

If entitlement to the jural status to which a kinship term may refer rests on prior entitlement to designation by the term (i.e. to membership of the category or categories designated by it), that jural status is a contingent condition of membership of the designated category. It is a normative implication of membership of the category, not the grounds for membership and designation by the term. The features of jural status are not among the defining or distinctive features of the class; they are features of the class, but, being accidental or contingent, they are features of connotative rather than definitive meaning.

No elaborate argument is required to show that this is true of English kinship terms, and there is much evidence to show that the same arrangement is characteristic of the kinship terms of many other languages as well. This is clearly revealed by the fact that in English and other languages, my father is my father whether or not he honors his obligations as such (13, p. 2; 54; 62, p. 24). Of course, people may describe their fathers as the men who rear them, or even respond to open-ended questions like "What is a father?" with similar observations; but it is a mistake to suppose that in so doing they are stating the definitive meaning or signification of the term. They are describing what it means to be a father or an aspect of the social significance of being a kinsman of the father class.

At first glance there would seem to be little reason to suppose that the relationship between reference to kin categories and reference to jural (or affective) relationships could be otherwise. Indeed, there are many seemingly good sociological and psychological reasons for the two kinds of meaning to be associated in this way. Yet it is not inconceivable that it could be other-
wise. Some anthropologists have suggested that the relationship may vary
from one society to another, that in some cases the two kinds of reference
may be "independent" (10, p. 132). Schneider & Roberts (67, p. 18) once
argued that reference to "roles" is the "dominant" or "primary value" of
Zuni kinship terms (see also 69). Bloch (4) has argued that Merina kinship
terms refer to "moral concepts" which may or may not "contain an element
of genealogical denotation"; and Schneider (61–66) and Silverman (70)
have argued that genealogical and social relationships are both "distinctive
features" of American and Banaban kin categories. Finally, Needham (45, p.
87) and Korn (27, p. 47) have argued for "the primacy of classification over
consanguineal ties" in a number of societies.

There is much that needs to be said about each of these claims, but I
confine my remarks to noting that for none of the cases in question is there
any reason to doubt that certain right and duty statuses (not always clearly
specified in the ethnography) are ascribed to certain categories of kin, at
least at the level of the primary senses of the terms. For most of the cases in
question this is clearly stated by the ethnographers. What then is the prob-
lem? The problem, in brief, is the failure of the theorist to distinguish be-
tween kin classes and nonkin but kin-like classes, both of which may be des-
ignated by the same terms, or in other words between simple extension within
the domain of kin classification and metaphoric extension.

**Metaphor**

It is a common ethnographic observation that kinship terms may be used
to denote individuals not presumed to be genealogically related to the
speaker, or to denote someone who is presumed to be genealogically related
to the speaker but in some irregular way, as when a man calls his daughter
"sis" or, when speaking to his daughter of his wife, refers to his wife as
"mom" (cf 65, p. 26). The usages in question here are those based on the
jural or affective connotations of kinship terms, and where the speaker could
appropriately assert that the designated party "is not a kinsman" despite his
or her designation by a kinship term, or, as in the examples given above, that
the girl spoken to "is not his sister" and the woman spoken of "is not his
mother." We need not consider those usages of kinship terms based on "pre-
sumption of kinship," in turn based on common membership of genealogi-
cally constituted social units such as descent groups, for these are based on
partial genealogical knowledge and are still extensions within the domain of
kinship, i.e. genealogical connection, proper (59, p. 142, f.n. 3).

The usages under consideration here have occasioned much discussion
and dispute among anthropologists and have provided part of the basis for
the contention that the distinction between signification, connotation, and
metaphor is untenable, at least in relation to the meanings of kinship terms
(32; 65, pp. 51–56). Before we consider this contention we must be clear
about what we mean by metaphor.

Metaphor is often described as "simile without the 'like'," but we must
consider the underlying semantic structure of such a comparison. It consists in suspending one or more of the defining features (criterial attributes) of the primary sense of the word and substituting in its place some feature of connotative meaning which is associated with the primary sense or some simple widened sense of the word. In the process connotative features become criterial (34, pp. 192–93; 59, pp. 9–11). By this means words may be transferred from one semantic domain to another, for example, from the domain of kin classification to the domain of classification on the basis of kinds of social or affective relationships. As noted above, transfer from one domain to another may occur also through narrowing or specialization of a term’s significatum. Although narrowing, like metaphor, requires the addition of features, in narrowing no prior features are suspended (59, p. 8).

Now if we distinguish, as we must, between what kinship terms signify and what they connote, we may say that designation of a nonkinsman by a kinship term—specifically to indicate that a kin-like social relationship has been established between the speaker and the designated party—constitutes a case of metaphoric extension. We may say so because features of connotative meaning have been taken as criterial for the use of the term.

Of course, if we argue that the genealogical and jural-status referents of kinship terms are structurally equivalent, that both are distinctive features of the categories designated by those terms, we have no basis for speaking of metaphoric usage in such a case. This, as I understand it, is the crux of Scheider’s argument about American kinship terms and the basis for his argument that the use of “father” in reference to a priest, or of “den-mother” in reference to the supervisor of a Cub Scout pack, or of “aunt” in reference to a close friend of a parent, are not cases of metaphoric extension (62, pp. 99–106; 65, pp. 20, 52).

The important question here is: What grounds do we have to argue that genealogical features and jural-status features (Schneider’s “code for conduct”) have an equal status in the significata of American or any other kinship terms? Schneider (62, pp. 23–29, 32) argues that both are distinctive features because two people may designate each other by kinship terms either because they are genealogically related in a certain way, or because they have a certain kind of social relationship, or for both reasons. In Silverman’s terms, they are “alternate necessary conditions” for kinship terms to denote (70, p. 233). It would seem, however, that on Schneider’s and Silverman’s accounts of the relevant ethnographic facts, the two kinds of features are both sufficient conditions; that is, the existence of a certain kind of genealogical relationship or of a certain kind of jural relationship (one of the kind ascribed between kinsmen) between two persons is sufficient grounds for them to denote one another by certain kinship terms. This does not rule out the possibility of metaphor, however, since the metaphoric process consists in treating accidental or nondistinctive features as sufficient (but not necessary) conditions (34, pp. 192–93). Lounsbury (34, pp. 193–94) has noted that it is conceivable that, in “some areas of lexicon, semantic structure may be so
complex” as to force us to abandon “the clear distinction between ‘essential’ and ‘accidental’ features” in our attempts to describe semantic structure within those areas. Apparently, this is what Schneider and Silverman would have us do, at least with regard to the two cases in question here.

Before we do so, however, we should ask whether or not (in these two cases) there is any context of kin-term usage in which either certain kinds of genealogical connections, or certain kinds of jural relations, or both, constitute necessary and sufficient conditions for designation by a kinship term. The question is: Is there any context in which a person is denoted by a specific kinship term, or by the generic term “kinsman” or “relative,” if and only if that person is acknowledged to be genealogically connected to ego or the propositus, and in which it is not relevant that he or she does or does not maintain a social relationship with ego or the propositus?

It should be clear that there is such a context, and it is specifiable as that context in which we desire to indicate that the designated party is or is not a kinsman, or is a kinsman of a particular kind (not merely that we may call or address him by a kinship term or by the generic label “kinsman” or “relative”). It is, in other words, the context of kin classification. As noted above, a person’s genitor is regarded as his father whether or not he honors his obligations as such, and even if his identity is not known or openly acknowledged. If a person wishes to refer to his genitor, whom he has never seen and whose name he may not know, he speaks of his “father.” Similarly for all other English kinship terms and the expression “relatives.” Schneider (62, p. 24) has pointed out that it is genealogical connection or “blood relationship” that makes people kin (or “relatives”) in American culture, and that the absence or termination of social relations between them cannot alter the fact that they are kin (or “relatives”). In English it is tautological (but informative) to say that one’s father, mother, brother, etc are one’s relatives; tautological because by definition to be a father, mother, brother, etc, is to be a relative. In contrast, it is false to say that one’s priest is one’s father (unless he happens to be that too), or that a den-mother is the mother of the boys under her supervision (unless she happens to be that too), or to say that one’s priest or den-mother is one’s relative (62, p. 100). We say only that priests and den-mothers are like fathers and mothers, and then only in very limited respects.

In short, relationship by genealogical connection is the necessary and sufficient condition for inclusion in the category “relatives” and for inclusion in one of its subcategories (e.g. “father”). In contrast, the existence of a kin-like social relationship between two persons is a merely sufficient condition for designation by a kinship term, which is not the same as inclusion in a kin class. Designation by a kinship term may indicate inclusion in a kin class or inclusion in a kin-like class. This distinction between kin classes and kin-like classes is not merely analytical; it describes a distinction made within American culture. Of course, our informants may not make this distinction when talking to or of the relevant nonkinsman in his presence (cf 4, p. 81), but it
is the very essence of metaphor that its comparison should remain implicit. Where this distinction may be made it is incorrect to describe jural (or affective) relations as distinctive features of kin classes. They are features of kin classes (where ascribed between categories of kin), and they may be distinctive features of nonkin or kin-like classes, but they are not components of a term's significatum when it is used to designate a kin category.

It follows that we must be careful to distinguish between kin terms and kin categories. We have seen that a particular term may designate more than one kin category and that it may designate a nonkin or kin-like category. If, when we describe a word as a kinship term, we do not necessarily imply that every category designated by it is a kin category, there can be no legitimate objection to independent analysis of kinship terms as designators of kin categories. All objections to this procedure (4, 31, 32, 61–66, 76) rest on the fallacious identification of kin terms with kin categories.

We may ask, of course, whether or not the distinction between kin categories and kin-like categories designated by the same terms occurs in other cultures and languages and if so how we are to recognize it. Some indication of how these questions may be answered is given in a previous study (59) and above. Several recent studies (3, 4, 7, 8, 25, 69, 70) provide excellent data on the use of kinship terms in various ways and on the lexical markers used to distinguish the occurrence of a specific category label, or of the cover term "kinsman" or "relative," in a primary, simple extended, connotative, or metaphoric sense.

What do we make of English expressions like "old woman" when the denotatum is the speaker's wife or mother? Is this a kinship term? Schneider (63, p. 89) describes this and similar English usages as "the use of a nonkinship term for plainly kinship meanings" and adds that it is worth some attention that this expression "classes mother with wife." Certainly the denotatum is the speaker's mother or wife, but is the genealogical or marital relationship that which is signified by "my old woman," such that we must say that the expression functions as a kinship term? Plainly not. The denotatum is also a woman, so that "woman" need not be understood in any special sense. She may or may not be old in some sense of the term; but women often have acknowledged domestic authority over their sons and husbands, which the latter may find more or less irksome. After all, the expression is mildly derogative, and the mild derogation is expressed by "old" in this context, since "old" has connotations of respect and authority as well as perhaps of demanding too much. [Of course, the connotations of similar expressions in other languages need not be the same (cf 70, p. 246).] Thus what the expression signifies is that the denotatum is a woman, and perhaps older than ego, and what it connotes is her rightful or acknowledged domestic authority over him. It should be clear that the expression does not "class mother with wife" but designates a class of women which, it just so happens, includes a man's mother and wife. To paraphrase Schneider (62, p. 59), the relative as a person (e.g. an "old woman" kind of person) is quite different from that same
individual regarded as a relative or as particular kind of relative (e.g. a mother or wife).

Finally, we should note that, contrary to Leaf's (32, p. 545) judgment, the applicability of the concept of connotation, and therefore of metaphor, does not depend on a belief that kintypes are "quasi-physical objective referent[s] that can be cut off from other nuances that are somehow less real." To suppose that it does one would have to accept the absurd assumption that kinship terms refer to biogenetic relationships of the sort known only to the sciences of biology and genetics. The only tenable assumption is that they refer to relationships "known" to or posited by the people who use the terms. The components of their significata, like the components of all significata, are cultural constructs; and similarly for the components of their connotative meanings. This was made quite clear by Charles Morris sometime ago, and I can do no better than to refer the reader to his discussion (43, p. 19). Certainly, some practitioners of the art of structural semantic analysis have confused the issues and misled others by describing kintypes as "objective" entities and by asserting that structural analysis of systems of kin classification has been facilitated by the availability of an "etic grid" against which such systems may be analyzed. But if we think of ourselves as dealing with semantic systems, such a "grid" may be described as "etic" only in the sense that it represents something intrinsic in a wide variety of cultural (ideational) systems and therefore external but not alien to them (see also 59, pp. 69–70).

**Vocative and Referential Systems**

Some anthropologists have questioned the validity of conventional distinctions between vocative and referential terms and systems (cf 44, pp. 97–98, 106–7) and have argued that the validity of many published analyses of systems of kin classification depends on them (63, 65). However, virtually the whole of the argument against these distinctions, and against independent analysis of the systems of classification characteristic of different socially defined contexts of usage, rests on the alleged unacceptability of the distinctions between signification, connotation, and metaphor. Once this misunderstanding has been corrected there need be no doubt about the value of distinguishing between different socially defined contexts of usage. Again, the value of making the distinction is not merely a matter of analytic convenience; it is a simple matter of ethnographic necessity since our informants often make such distinctions (67, p. 12; 70, p. 240).

As Goodenough (14, p. 262) and others have observed, it is difficult to analyze vocative usage because "personal considerations affect the use of kinship terms in address." As I understand it, this is not to claim that kintypes "are not the denotata of terms of address" and so "terms of address" may be excluded from an analysis of Yankee kinship terminology, or to imply that "terms of address" are not kinship terms (65, pp. 26–27). Note that in this context Goodenough wrote not of "terms of address" but of the "use of kinship terms in address." There need be no doubt that he meant to imply only
that considerations of their connotations are more likely to affect the use of kinship terms in address, thus making it difficult to know with certainty whether they are being used to designate kinsmen, or to stress their connotations, or both at once, or metaphorically to designate nonkinsmen. This is why it is essential to control for context of usage when analyzing the meanings of kinship terms and why control questions like the one used by Goodenough are essential ethnographic tools. There is no suggestion here that vocative and referential are the only kinds of social contexts that may have to be taken into account in studies of kin term usage, or that the use of kinship terms in address, either to designate kin categories or to designate kin-like categories, is not amenable to structural semantic analysis. No good can come, however, of confusing their use in these quite different ways in either vocative or referential contexts.

Of course, the use of such control questions “rigorously and narrowly,” that is to say, mechanically and unimaginatively and on the assumption that they are the only way to acquire reliable data on the meanings of kinship terms, would be the height of folly (63, pp. 89–90). It remains to be demonstrated, however, that anyone has done this.

**Alternative Models**

Anyone who ventures for the first time into the contemporary literature on systems of kin classification may be excused for wondering if he should take any of it seriously. We have seen that some analysts view the classificatory systems in question as systems of social classification, while others view them as systems of kin classification whose terms may have other semantic functions (such as social classification) which need not be taken into account when analyzing their genealogical designata. Still others take no clear position on either side of this argument and are content to practice one or another kind of analysis without being clear about why that kind of analysis is necessary or how it might prove relevant to one or another of the broader interests of anthropology, linguistics, or psychology (to all of which semantics is relevant in one way or another). Further still, styles of analysis vary greatly from the relative simplicity of conventional componential analysis to the (perhaps only apparent) relative complexity of mathematical and partly mathematical models, and bear no simple correlation with positions in regard to the dispute about the nature of the object of analysis. It is not surprising that some anthropologists express impatience and outright contempt for the whole lot and imagine that the analysis of kinship terminologies has become for some “an end in itself, to which the original facts on the ground are related only as a tiresome and perhaps misleading irrelevance” (31, p. 96).

There is some justice in Leach’s remark. Too many studies of systems of kin classification or, more vaguely, of “the meaning(s) of kinship terms” have been dominated by methodological considerations (thus the charge of “hocus-pocus”), and the goals of the analytic effort, short of “demonstrating a method,” often have remained unclear. Most important, it has not often
been made clear why a particular method has been chosen, or why it should
be chosen, or how it relates to a conception of the nature of the object under
analysis. In other words, too many studies of kinship semantics have been
based on ad hoc, unsystematic, and largely implicit semantic theory or—what
is worse—on no clearly semantic conceptions at all. However, if the assess-
ments presented above—of the nature of the object under analysis and of the
semantic concepts and theory relevant to analysis of it—are substantially cor-
rect, we are forced to the following conclusions.

Most “social categories” interpretations of the meanings of kinship terms
are false and misleading insofar as they purport to tell us anything about the
structurally primary and simple extended senses of those terms (for example,
that this distinction is inapplicable or generally untenable) or about the struc-
ture of their connotations at these levels of kin-class designation. Such inter-
pretations tend to be based on data about the extension of kinship terms into
the domain of descent-group structure and to ignore or explain away much if
not all data about their structurally more basic meanings. It does not follow
that such interpretations are wholly misleading and false, for they may still
tell us some anthropologically interesting and important things about seman-
tic content and structure at those levels of semantic structure which are taken
into account. Yet we must be cautious in accepting the validity of one or
another aspect of any such interpretation and continually inquire into the
extent to which the validity of that aspect depends on the more basic assump-
tions of the analysis.

Most practitioners of the art of conventional componential analysis have
avoided the trap of failing to distinguish clearly between signification, conno-
tation, and metaphor—though as critics have noted, not always with suffi-
cient understanding of how or why they should have avoided it (cf 10, 20).
Yet even the most recent studies in which such analyses are presented (8,
16) are dominated by the search for single conjunctive definitions; the possi-
bility of polysemy within the domain of kin classification is not even consid-
ered, and some anthropologists appear to regard the admission to analysis of
disjunctive definitions as an admission of failure (10). We need not go into
the reasons for this (see 59, p. 138), but it should be noted that they are
fairly complex since Goodenough (12, p. 212) and Lounsbury (34, p. 175)
noted the possibility of polysemy within the domain of kin classification, and
of the consequent necessity to admit disjunctive definitions to the analysis, as
early as 1956. The important point here is that most componential analyses
have glossed over much of the underlying structure of the systems under
analysis. In some cases, it seems, their results (as specified in the stated com-
ponential definitions) are not inconsistent with the polysemic nature of terms
under analysis; they express only the broadest kinship significata of those
terms and need to be supplemented by definitions of the narrow or primary
senses and by rules of extension of those senses (56). In other cases, how-
ever, it is doubtful that their results are valid even within these limits. In any
event, most published componential analyses cannot be taken at face value, including my own early efforts (52).

Numerous (perhaps all) attempts to apply the "methods" of reduction-rules (21, 51) or equivalence-rules analysis (36, 37, 59)—the latter of which was designed specifically to deal with the facts of polysemy—likewise suffer from a number of defects and need to be reconsidered at least in part (59, p. 75). Perhaps because the underlying semantic postulates of the theory were not fully explicated when analyses based on it were first published, many anthropologists have been tempted to use it as a mere methodological device or even "gimmick"; many who have attempted to apply it have misunderstood aspects of it or have failed to recognize that polysemy within the domain of kin classification is really what it is "all about." Some of the consequences of these misunderstandings have been noted elsewhere (53, 59, pp. 73–78).

Wallace's recent (80) "relational analysis" of American kinship terminology (see also 7) takes the facts but not the concept of polysemy into account in an effort to more closely approximate psychological reality. It is untrue, however, that the result of his analysis is a unique solution or that it is "no more correct in a structural sense" than previous componential analyses which considered neither the facts nor the concept of polysemy (cf 59, pp. 136–50).

Finally, what of the mathematical and partly mathematical models now so numerous? Korn & Needham (28) recently reviewed and passed judgement on some of this work, particularly that bearing on one ethnographic case. Their verdict on the practice of analyzing someone else's interpretation of the original ethnographic data, sometimes even second-hand interpretations, appears to be more broadly applicable. This has been done all too often (5, 18, 33) in cases where the ethnographic facts are seriously in dispute because the available data are weak to begin with (1, 55). Of course, the attempt to construct a mathematical model of the available data may reveal that the data are internally inconsistent or incomplete, and thus the possibility that the facts or all of the facts are not yet known; but this usually is not what the analyst has claimed.

It would be foolish to object to mathematical analyses and models as such. Surely it may be assumed that a number of levels of structure may be discerned in any cultural system and that each such level may have its appropriate mathematical representation(s). Lamb (29) and Hammel (22) have argued that the Crow- and Omaha-type skewing rules described by Lounsbury (36, 37) have common structural features at a higher level of abstraction than that dealt with by Lounsbury himself, and there are ample reasons to assume that higher (more abstract) levels of structure may be discerned both within and among systems of kin classification (17, 57). It is worth noting, however, that our understanding of even the less abstract structural features of such systems is not as good as it might be, for we have only begun to take the facts of polysemy and superclass structure (36, pp. 364–66; 59,
pp. 89 ff.) into account. Few if any of the mathematical models published to date have taken such facts into account. As in conventional componental analysis, there has been an implicit assumption of monosemy (within the domain of kin classification). It must be left to those more competent in mathematics than I am to assess the extent to which this often fallacious assumption reflects negatively on the models premised upon it.

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STRUCTURALISM IN CULTURAL ANTHROPOLOGY 9512

PIERRE MARANDA
Department of Anthropology
University of British Columbia, Vancouver, B. C., Canada

INTRODUCTION

This chapter consists of five parts. The first proposes a definition of structuralism against the background of other anthropological approaches. A brief diachronic sketch is found in Part II. Part III reviews some very recent contributions. A theoretical summary follows in Part IV. Finally, Part V focuses first on transformational analysis before examining a few testable hypotheses and their verification.

I. STRUCTURALISM IN ANTHROPOLOGY

The position of structuralism among other approaches in anthropology can be located by means of the Aristotelian notions of causality. Aristotle, and many after him, look at epistemology in terms of etiology. To know means to be able to map the different causes of a phenomenon. There are four causes according to traditional European philosophy:¹ the material cause, the efficient cause, the formal cause, and the final cause. The material cause answers the question “What is the phenomenon made of?” The efficient cause answers the question “What/who made it, what is its origin?” The formal cause answers the question “What is it?” And the final cause answers the question “What is the phenomenon for?”

Applied to social anthropology, the grid yields the following, where neatness does not necessarily mean oversimplification. Material cause: biological and physical anthropology and ethology in so far as they bear on social anthropology—generally speaking, cultural ecology. Efficient cause: evolutionary theory. Formal cause: structural theory. Final cause: functional theory.

This is not the place to deal with the epistemological implications of the model or with the relationships between the different causes. All are different mappings of the same phenomenon, and therefore all approaches reveal different configurations. One can look at a piece of chalk or at the element of kinship from different viewpoints and map them accordingly. Thus, the definition of a chalk crayon in terms of limestone (material cause), or of its manufacturer (efficient cause), or of its cylindrical shape (formal cause), or

¹ It is appropriate to use this as a frame of reference as anthropology belongs to the European world view.
of its use (function) depends on the types of questions asked. Similarly, the
definition of the element of kinship in terms of biological components, or in
terms of its origin when passing from nature to culture, or in terms of its
configuration, or in terms of its role in social organization, also depends on
the type of questions asked.

Structuralism focuses on the formal cause; the web of relationships be-
tween terms. On the one hand, it seems to be only minimally concerned with
biological components, their origins, and their actual social functions. Yet it
considers essential the substance of components and thus looks at the mate-
rial cause from a specific viewpoint. It also pays attention to process and
therefore considers efficient causes in connection with this form. Teleology
(final causes) is also taken into account, but here again with respect to actual
sets of relationships between components. This is because form cannot be
separated from contents: Aristotle and Lévi-Strauss insist that form is noth-
ing other than the shape of contents in a given state of a system. Content
structures form, in the sense that a grizzly bear, for example, cannot be in-
vested with any meaning but only with those a specific tradition makes avail-
able. (However, note that coyote, in the same traditions, is much more versa-
tile and is thus a more powerful semantic operator.) There is no such thing
as a form which could freely and arbitrarily shape up any contents. Lévi-
Strauss is explicit on this in La Structure et la Forme and in the series Mytho-
logiques (39–43). Then, the position of the phenomenon in the general sys-
tem implies considerations of its origin, hence the emphasis on the contrast
between nature and culture if not on the passage from the one to the other.
Finally, to generalize from a statement about kinship in Structural Anthro-
pology (37), “social facts exist only to perpetuate themselves,” i.e. their in-
terpretation is teleological and there is no need to worry about that (Mar-
anda 47, Lévi-Strauss 41).

The definition of the field of research commands, however, that consider-
ation of material, efficient, and final causes be on a level different from that
of, for example, evolutionary theory. This implies that structure will always
evade those who like many British social anthropologists, pursue empirical
patterns on the ground. To structuralists, social organization belongs to the
realm of material causes. Structuralism is not primarily a question of pattern
recognition, time series, and stochastic processes; it is above all the study of
those properties of a system which remain invariant under a given group of
transformations. The last clause is a close paraphrase of Klein’s definition of
modern geometry and topology.

The analysis of formal causes presupposes the existence of an order
which is to be discovered or read into the phenomena. Order here means a
system whose properties can be mapped in terms of a constant set of related
propositions. The set of related propositions is itself defined when the rules
which generate it are stated. Thus, cultures are seen as logical mechanisms

2 Except Leach, V. Turner, some of Gluckman’s and Worsley’s works.
for reducing the randomness of history. Unexpected events occur which have to be faced, defined, integrated into a world view, or else the society disintegrates and/or has to be revamped. Actual solutions vary from society to society, but because the mechanisms are essential and universal features of mankind, they remain constant.

To return to the opposition alluded to above between organization and structure, one can say that the study of organizations is an attempt to define set membership by intersecting listings. For example, a kinship terminology in a given society has a number of features which the anthropologist will list; then he will search other social data in order to find points of intersection with kinship terms, like residence, inheritance, etc; then the overlapping areas will be proposed as the nodes of a network describing the society under investigation. In contrast to this procedure of defining class membership by listing, a structuralist arrives at a definition of class membership by rule. Under what conditions can a “word” be considered a kinship term? What is the rule to define the domain of residence, etc? This does not mean discovering the norms of the society in question; such native models are devised only to handle behavior, whereas anthropological ones are devised to handle intelligibility. In essence, structuralism seeks to understand how societies preserve their identity over time. Structuralism emphasizes therefore not the study of inertia as a cultural fact but, by analogy with information theory, the study of negentropic processes. [For clarifications and developments, see Maranda (48, 49, 53, 55); for contrasts between functionalism and structuralism along the lines sketched above, see Leach (30) and the comments in Maranda & Königäs Maranda (58); for contrasts between psychological anthropology and structuralism, see Hymes (19); for ethnographic applications, see Maranda & Königäs Maranda (56).]

The assumption that “societies exist to perpetuate themselves” implies teleology. Obviously, it also implies a concept of dynamic permanence. What is the framework within which things can change without shattering the society that strives to perpetuate its identity despite the repeated blows of history? A brief consideration of approaches to kinship will perhaps help here as an example.

Lévi-Strauss' *The Elementary Structures of Kinship*, or Dumont's *Homo Hierarchicus* do not define kinship systems so much as they define the semantic parameters within which kinship operates. The structural analysis of kinship attempts to lay bare a mechanism and is thus both broader and narrower than strict kinship analysis. It is dynamically related to politics and economics, in the sense of Mauss' *The Gift*, and does not aim at producing a description such as those found in the works of componential analysts. Bulmer (7) and Lévi-Strauss (44) state it clearly: the former in his critique of ethnoscience, especially pages 1081–88, and the latter in his conclusion of The Deduction of the Crane:

Such a view [as proposed in the article] allows us to see the possibility of
a mythical typology which would renounce all external criteria. Instead, it
would use a single internal and formal criterion, namely the 'degree of order'
at which the myths of a region or a population (or for a single population
certain myths will thus be distinguished from others) cease the process of
composition which proceeds from the indigenous ethnobotanical and ethno-
zoological base. This base may well be called 'ethnoscience' as long as we do
not forget that it is the first step in a dialectic destined by its very nature to
blossom into a logic and a philosophy (Lévi-Strauss 44, p. 20).

And what I consider a key statement in The Elementary Structures of
Kinship makes the same point more specifically:

These [ethnographic] facts are important for several reasons. Firstly, they
emphasize that matrimonial exchange is only a particular case of those forms
of multiple exchange embracing material goods, rights and persons. These ex-
changes themselves seem interchangeable, viz., a woman replaces a payment
for a debt which was in the first place completely different, say, a murder or
ritual privilege; not giving a woman takes the place of vengeance, etc. Furth-
ermore, no other custom can more strikingly illustrate the point, which seems
crucial to us, concerning the problem of marriage prohibitions: the prohibition
is defined in a fashion which is logically prior to its object. If there is a prohibi-
tion it is not because there is some feature of the object which excludes it from
the number of possibilities. It acquires these features only in so far as it is in-
corporated in a certain system of antithetical relationships, the role of which is
to establish inclusions by means of exclusions, and vice versa, because this is
precisely the one means of establishing reciprocity, which is the reason for the
whole undertaking (33, pp. 113–14).

It must be pointed out that in this passage Lévi-Strauss emphasizes the
positional nature of elements in a set, while in La Structure et la Forme, as
said above, he lays the emphasis on the structural constraints which come
from the nature of the elements themselves, as he does also in the concluding
chapter of The Elementary Structures of Kinship where he proposes a definition
of the term "woman."

To conclude this first section, I should like to repeat that structuralism in
anthropology can be seen as a study of formal causes, and that this implies a
consideration of other causes sub specie causae formalis. Structuralists opt
for a more abstract level of analysis than other anthropological approaches,
and the consequence is a concern for general models and their rules of opera-
tion—i.e. for formal cross-cultural philosophy in the sense that I used the
term in the Introduction to Echanges et Communications (62).

II. BACKGROUND

Let us not go back too far. I shall begin with Tylor. A programmatic
statement of modern structuralism is found in his Primitive Culture published
in 1871. His mechanistic propositions on the nature of the human mind are
basic [for a more elaborate discussion, see Maranda (53)]. To him, the
function of the mind is to combine and derive, not to invent; and he objects
to the popular and unfounded conception of the "limitless" creative power of the human brain.

At the turn of the century, Hubert & Mauss (17, 18) developed the same approach in their analyses of magic and sacrifice which van Gennep was to apply a few years later to rites of passage (15), Hertz to the conception of death (16), etc. The basic propositions of structural analysis laid at that point were brought to bear on linguistics by de Saussure [for a more detailed discussion, see Maranda & König Maranda (58)]. At approximately the same time, Boas published his important thesis on language and semantics (5) where he introduced what are called "cultural idioms"—cf Chafe (9).

When de Saussure and Boas were defining their views, the Russian Formalists were working along similar lines. In folkloristics, this culminated in the publication in 1928 of the now well-known and very influential monograph by Propp, _The Morphology of the Folktale_ (63). After the models proposed by Hubert & Mauss in their two monographs, Propp's provided a definition of variable and constant elements in folkloric discourse; additionally, he created the technique called afterwards "content analysis" by showing how lexical diversity can be reduced to categorical descriptors. Furthermore, in a paper published the same year as his _Morphology_, Propp initiated transformational analysis in folkloristics (64).

The rapid expansion of structuralism with the works of Lévi-Strauss bears witness to a continuing interest in the view found in Tylor's _Primitive Culture_. The opening paragraph of Chapter 17 of _Tristes Tropiques_ (36) actually reiterates and expands Tylor's fundamental proposition. The combinatorial model of human societies and cultures which Lévi-Strauss envisages there, "similar to an anthropological Mendeljev's table" (and which is not without similarities to Kroeber's "cultural traits"), assigns an ambitious objective to anthropology.

Thirteen years after the publication of the founding paper of modern structuralism, Structural Analysis in Linguistics and Anthropology (31) and 3 years after that of The Structural Analysis of Myth (35), the first English translation of Propp's _Morphology_ contributed importantly to further developments [for references to the secondary literature as well as for other orientations, see König Maranda & Maranda (26)]. Figure 1 summarizes my point.

Over the last 10 years structuralism has become a consolidated field. Courses are now devoted exclusively to structural anthropology in many universities in North America as well as abroad. This probably indicates the need for a renewed theoretical framework or at least for a complement to other theories in anthropology—without forgetting that the structural myth is now part of the intellectual establishment's system of values.

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3 See also the high proportion of papers revolving around structuralist issues in the annual meetings of the American Anthropological Association over the last few years.
III. Some Recent Contributions

The most noteworthy event in the field is doubtless the completion of Lévi-Strauss' monumental *Mythologiques*, whose fourth and last volume appeared in 1971 (43). Other significant contributions were the English translation of Piaget's book *Le Structuralisme* (61), and Turner's *The Ritual Process* (69). The Lévi-Strauss *Festschrift, Echanges et Communications* (62) contains over 80 chapters from all continents illustrating applications of the structural method. Over the last few years in the Soviet Union* a strong impetus was given to structuralism by Meletinski and his associates. Finally, *Structural Models in Folklore and Transformational Essays* (26) and *Structural Analysis of Oral Tradition* (57) appeared almost simultaneously. These and two recent readers will be the works examined briefly in this section. The selection is restricted by the limits of this reviewer's knowledge and by the criteria of significance adopted. Significance is defined in terms of the headings of the two next parts, viz. theoretical contributions and testable hypotheses.

Lévi-Strauss' *L'Homme nu* consists of two parts. The first, to page 558, is a continuation of his analysis of Amerindian myths. It culminates in the Northwest Pacific Coast area. Lévi-Strauss finds there the key to the interpretation of the some thousand myths he tackles in his four volumes. It is note-

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*Under the influence of Soviet space engineers who discovered and rehabilitated Propp's works.*

**Dans une telle hypothèse dont on se gardera d'exagérer la portée, les mythes sur lesquels s'achève notre inventaire représenteraient les formes toujours vivantes,*
worthy that he summarizes his findings by focusing on a technological fact, the earth oven. This constitutes most significantly "a formal and intimate homology between infrastructure and ideology" (43, p. 557). But the latter may be more determinant than the former, "if the peoples who have the earth oven do not make pottery, it is because of the incompatibility of a philosophical order, as it were, which they conceive more or less consciously between these different techniques" (43, p. 553).

The second part consists of a critical reflection on the method followed in the analysis. The style switches from the impersonal "we" to the engaged "I," and Lévi-Strauss begins by considering the critiques addressed to volumes 1–3. He divides them into two sets, the not too enlightened ones, which he deals with first, and those he considers significant, which he discusses carefully. The work ends with a philosophical statement in line with similar passages in *Tristes Tropiques* (36), and the very last sentence, 20 lines long, proposes a world view implicitly in agreement with the second law of thermodynamics (cf the conclusion of *Tristes Tropiques*: and see below, Part III).

Piaget's synthetic review and discussion of structuralism (61) in the social and behavioral sciences is perhaps the most useful in the field. For Piaget, and this is a restatement of his *Traité de logique opératoire* [see my application of it to anthropology in (55)], a structuring action is essentially a system of transformations. In effect, phenomena are structured by "laws of composition"; they are therefore "structuring" by their very nature at the same time as they are structured. He does away with Chomsky's innateism to hold instead that constricting equilibrating processes are enough to describe the system, and he opts for constructivism against those who prefer an axiomatic approach.

The three "mother structures" distinguished by Piaget are fundamental and all are relevant to anthropology. The first is algebraic, the second order relations (e.g. a lattice), and the third topological. The theory of categories then is important for the constitution of morphisms, which puts the emphasis on the operations performed on constitutive operations. It is impossible to give an adequate idea of Piaget's valuable methodological reflection in the present format. I should like to recommend it to all those who wish to take stock of the recent developments of the field. Chapters 1, 2, 5, 6, and 7 are especially relevant to the problems anthropology deals with.

Leach's theory of tabu [Animal Categories and Verbal Abuse (29), *Kimi*: A Category of Andamanese Thought (30)] was developed by Mary

les plus riches et le mieux préservées aussi, d'un système qui, en se diffusant vers l'est et le sud, se serait progressivement décomposé, et dont nous n'aurions fait que retrouver, jusqu'au cœur de l'Amérique du Sud, les débris charriés et éparpillés au cours des siècles par le flux des migrations. Recueillant et mettant bout à bout ces morceaux, nous aurions patiemment reconstitué ce système tout au long de notre entreprise, remontant pas à pas jusqu'à sa source où, sous un état encore relativement intact, nous l'aurions enfin retrouvé" (45, p. 536).
Douglas in her *Purity and Danger* (11) and *Natural Symbols* (12) and still further in Turner’s *The Ritual Process* (69). Although the latter refers to Leach’s theory only indirectly through a few lines in connection with Douglas (69, p. 109), his concept of liminality expands what Leach calls his theory of tabu. According to it, indeterminate margins surround discrete elements in a symbolic field, and such margins are the danger zones in which tabu is imposed. These convergences between the works of Leach, Douglas, and Turner seem to indicate the emergence of a new form of British structuralism.

I cannot give here even a dim idea of the contents of Lévi-Strauss’ *Festschrift*. It ranges from structural ethnographies to political analyses (e.g. Peacock’s “President Sukarno as Myth Maker”); the first volume contains a drawing by Max Ernst and a piece by Michel Leiris. The *Festschrift* is structured after the works of Lévi-Strauss himself. The eight parts correspond each to one or more books, from *La Vie familiale et sociale des Indiens nam- bikiniara*, an ethnography published in 1948, through *The Elementary Structures of Kinship, Race and History, Tristes Tropiques, Structural Anthropology, Entretiens avec C. Lévi-Strauss, The Savage Mind*, to Mythologiques. The diverse contributions are grouped under each of these headings according to their subject matter methodology.

The publication in Russian, German, Italian, and French of monographs and papers by the Soviet and East European structuralists bears witness to the fecundity of their approach. I do not think a list of those works would be very useful as most of them are written in languages little known by anthropologists. Instead, I should like to mention that the series *Approaches to Semiotics* (ed. T. A. Sebeok, Indiana University and Mouton) will publish English translations of some of the contributions by Meletinski, Nekludov, Novik, Segal, and Pop. The journals *Uomo & Cultura* (University of Palermo); *Semiotica; L’Homme et la Société* (Paris); *Langages* (Paris); *Annales-Economies, Sociétés, Civilisations* (Paris); *Communications* (Paris); and the publications of the Soviet Academy of Sciences in the Social Sciences all contain new and original studies. Perhaps most significant so far from Eastern Europe and the USSR are the drastic revisions of linguistic models on the one hand and elegant mechanistic description in folkloristics on the other. The researchers in the Soviet Institute for Applied Mathematics and in the Institute for Mathematical Linguistics have proposed, for example, a definition of synonymy in the framework of discourse analysis and conduct field tests in folkloristics to refine their protocols. Propositions made along the same lines elsewhere are inspired by transformational linguistics. For a summary of results by the Eastern Europeans over the last few years, see Rozentsweisg (66).

*Structural Analysis of Oral Tradition* (57) contains an introduction and 11 chapters, several of which mark new developments in the field. Myth, ritual, folk drama, folk tale, riddle, folk song and myth in culture contact are the seven areas which specialists tackle after brief presentations of their analytic concepts. Lévi-Strauss uses the Kantian notions of empirical and tran-
scendental deductions as tools for the analysis of myth. Leach contrasts structuralism to functionalism, reanalyzing Radcliffe-Brown's Andamanese data. Hymes uses the method presented by Lévi-Strauss in Totemism to test Jacob's psychoanalytic interpretation of Northwest Pacific Coast myths. Greimas introduces the concepts of "posed" and "inverted contents" to characterize mythic thought. A metalanguage for semantic analysis in the field is presented by Turner (70), who applies it to ritual. Peacock merges Burke's and Parsons' categories to map out the structure of Javanese folk dramas. Continuing his application of the Proppian model, Dundes shows its utility for comparative analysis by contrasting a theme in North Amerindian and African folk tales. Metaphor, metonymy, and transformations are the concepts used by Königäs Maranda in her analysis of a corpus of Finnish riddles. Lomax and Halifax adopt content analysis procedures to conduct a broad cross-cultural piece of research in folk song texts. Culture change—see also Savard's recent monograph (67)—is investigated by da Matta in terms of a relational analysis which implicitly brings together Propp's and Lévi-Strauss' methods. Finally, a test of the validity of desk analyses—armchair anthropology—is successfully carried through by Dundes, Leach, and the present author. Maybury-Lewis, who had collected the documents analyzed in this exercise, concludes that, after all, it seems to be possible to say something meaningful about a society totally unknown to the analysts using a well-defined method. His final statement shows that the new theory of myth being elaborated by structuralists is indeed needed.

Chapter 2 of Structural Models in Folklore and Transformational Essays (26) is a revised and enlarged version of a monograph originally published in 1962. In it the authors devise a taxonomy of plots and find that five models are necessary and sufficient to describe adequately myths, folk tales, riddles, proverbs, rituals, as well as other genres. Thus the actual number of possible plot structures is presented as finite—this is both a modification of Propp's single model and a new development. The five models are in fact paths followed by the carriers of oral tradition, and they also represent learning steps in the process of mastering more and more complex structures (experimental approach, Chap. 2, Sec. 8). A typology is therefore set up of rules that govern the generation of folkloric items. Furthermore, the authors argue that statistical predominance of the different models in different cultures are indicative of value orientation (see below, Part V).

Transformational analyses are used in Chapters 3 and 4. In Chapter 3, on myth, semantic domains remain constant while a structure is transformed into another structure. In Chapter 4, on riddles, structure remains constant while a semantic domain is transformed into another semantic domain.

Königäs Maranda's work on riddles should be read along with the papers by Morin on jokes (60) and by Milner on proverbs (59). Developed independently, these show a remarkable convergence. See also Maranda & Königäs Maranda on proverbs (26, Chap. 2.6), the Soviet Permiakov's monograph, still untranslated, From Proverb to Tale: Notes toward a General Theory of
the Cliché Text, and the continuing team research conducted at the Centre d’Ethnologie Française in Paris by Loux, Charraud, Richard and Virville. Units are defined in the same way; metonymic and metaphoric processes are shown to be basic mechanisms in the constitution of the data. Königäs Maranda’s transformational analysis [like Maranda’s in his algebraic approach in Structural Models (26, Chap. 3)] shows that semantic analysis is not a matter of atomization. This bears out the propositions of Lévi-Strauss (39) and Leach (29) on the discrete and the continuous and corroborates Schneider’s objections to componential analysis.

Finally, two important readers must be mentioned: Structuralism, edited by Lane (27), and Anthropologie et calcul, edited by Jaulin & Richard (20). Both contain theoretical chapters and applications. In addition to papers of historical interest by de Saussure, Prague linguists, and Jakobson, those by Abell and Barbut are especially valuable in Lane’s reader. The first one applies, after Flament (14)—a relevant book Abell does not seem to know—graph theory, to the “element of kinship.” The second, written by a mathematician specialized in the social sciences, provides a good introduction to Klein’s groups and to the most fundamental concepts of isomorphism and homomorphism [on their role in structural analysis, see this author’s Anthropological Analytics (55)].

Anthropologie et calcul contains original and critical essays in addition to reprinted papers. Lucid assessments of formalization and structural approaches are made available in a convenient and sober form. I hope this reader will be translated into English in the near future, for it would be most helpful in many anthropology courses.

IV. Theory

I propose to summarize structuralism’s main recent theoretical contributions in terms of a parallelism with the second law of thermodynamics (see above). This may be my own reading. To support it, I would argue that it should not be unexpected to find modern analyses of myth and ideological systems by members of our societies, partaking in our societies’ scientific world view. Natural scientists see the universe as a process of increasing disorder (entropy). Since there is a gradual loss of available energy in the physical world, total entropy increases with the flow of time; in sociological terms, history would be the description of mankind’s increasing entropy despite its efforts to counteract it (negentropy).

If we agree that in order to communicate (a) people must share common mythic conceptions, as I have argued elsewhere (53); (b) that members of our own societies share a common mythic conception of science as a dynamic set of beliefs; (c) that this scientific myth rests on and is expressed by the second law of thermodynamics (entropy increases over time); then how could anthropological and other theories free themselves from these thought

*Lévi-Strauss refers to this type of formalization in Mythologiques III (42).*
parameters and from their culturally defined conceptions of their own intellectual substance? I do not mean that anthropological and other sciences derive their theoretical patterns from thermodynamics; I mean that structural anthropology as well as physics and other sciences would all be molded by our mythic, i.e. entropic, conception of the universe. The story of the Original Sin in the Bible already laid the ground for this. According to the Biblical semantic charter, knowledge can be achieved only at the expense of a loss of order, Sin. If Eve had ranked immortality higher than knowledge, mankind would still be immortal—and ignorant—and there would have been no loss of the original stable order, i.e. no entropy.

My proposition is therefore that the structuralist interpretation of mankind and of its operations and processes is congruent with the second law of thermodynamics as the second law of thermodynamics is congruent with the myth of the Original Sin and with a great many of our cultural axioms, and that it is so because both physics and structural anthropology are products of the same culture, because they both rest on the same myth. (Whether the same could be said for other approaches in anthropology or whether only structuralism comes close enough to the natural sciences to meet them on the level of our basic myth is beyond the scope of the present review.)

The debates between historians and structuralists, the oppositions built between synchronic and diachronic approaches, converge in fact on the concept of time. Again, this is not the place to discuss the debate's philosophical implications, not even the supposedly "cyclical" conceptions of time in non-literate societies (cf Eliade's works, especially Cosmos and History, and Leach's Rethinking Anthropology, Chap. 6). Structuralism sees history as irreversible and mankind's operations on this flow as vain attempts to slow it down if not to stop it. In the physical world, some processes can reverse entropy. Thus, freezing increases the internal order of a liquid and reduces entropy—a process called negentropy (cf Lévi-Strauss' concept of "cold" vs "hot" societies in Race and History). The same concepts are used analogically in Communication Theory, and it is possible to measure the entropy of verbal messages (see 48, 52).

In effect, the conclusion of the last volume of Mythologiques, and more or less explicitly the other works briefly reviewed in the preceding section, see mankind's ideologies as the classical Greeks did: devices to pass from chaos to cosmos, to cancel disorder, to negate entropy—i.e. to nullify the complex and threatening future by resorption into the past. Thus science and philosophy are attempts to reduce the randomness of history to a pattern, that of one's own culture's myth (see 53).

Administer a Word Association Test to a sample of Overseas European (Murdock's name for "Americans"). Use the word "tobacco" as one of your stimuli. You can predict a high association score with responses like "cancer," "pollution," "heart condition," and the probability is extremely low that you will get "Pleiades." But it is the other way around with Plains Americans. The reason is that Overseas European thinking habits are conditioned
FIGURE 2. Model III (see 26).

by a mythology very different from that of Plains Amerindians. The former is continuously molded by "scientific" statements and is structured by a set of beliefs idiosyncratic to technologically overgrown societies. The latter's thought patterns, as shown by Lowie (46), are molded by a dynamic cosmological view which has more to do with prayer than with the medical arts.

This is to say that mythology conditions thought, and consequently language, like language conditions speech acts. De Saussure's distinction has thus to be read in the full context it implies, viz.

myth : langue :: langue : parole

In this view, myth is defined as a charter—not a charter of society as Malinowski's narrow functionalism would have it, but a cognitive charter. It delineates the parameters within which the members of a society can communicate and beyond which they are lost—"out of the way" poets, "foreigners," or crazy (on this, see Mallarmé's works and T. S. Eliot's Tradition and the Individual Talent in his Points of View). In other words, myths (and other folkloric genres as well) map out the grooves along which thought can move in a linguistic and cultural community. They also, by virtue of this, teach which associations are permitted and which ones are not, within specific parameters. The study of cargo cults is very instructive in this respect as well as that of culture change [see Burridge (8), Hymes and da Matta, both in (57), etc]. As pointed out in Part I, this theory of myth stems from Tylor, the French School of L'Année Sociologique, Boas, and Lévi-Strauss. It can now be considered well established and supersedes the Sapir-Whorf hypothesis which in fact it pulls inside out.

Take a cultural system in a state $S_1$. An event occurs that alters the state to $S_2$. Homeostatic, cybernetic, or other devices (according to the analyst's theoretical inclinations) attempt to bring it back to $S_1$ [Model III in the terminology of (26)]. However, it may be impossible, attempts fail, and the system collapses (Model II in the same terminology). Or it may be possible to bring it back to a state approximately equivalent to $S_2$, say $S_{1'}$. To use Lévi-Strauss' metaphor, the outcome of the process will be on the same longitude but at a different latitude. This twist over time generates a helicoidal
STRUCTURALISM IN CULTURAL ANTHROPOLOGY

structure (Model IV)—social change—towards either a temporarily more stable or unstable pole, with instability prevailing ultimately. Figures 2 and 3 represent the structure graphically.

The distances $S_i - S_i'$, or $S_i - S_2$ can be positive or negative in reference to the number of elements subtracted from or added to $S_i$ and thus provide a measure of entropy if negative and of negentropy if positive. [For a more elaborate discussion, see (26, 42) and for technical applications, see (26, 48, 52), where the concepts of structural strength and entropy are discussed.]

In this respect, mediation and inverted symmetry appear to be fundamental analytic concepts. Lévi-Strauss has drawn extensively on them for many years (32, 35, 38). Gluckman, Turner, and others have also shown their relevance as general mapping devices, i.e. as transformers.

More specifically, one of the main methodological consequences of structural theory has been to reactivate Mauss' concept of total social system. Thus, as implied above (Part I, references to Bulmer and Lévi-Strauss), myths structure philosophies. This means that ethnosciences and componential analysis are unproductive exercises as far as comprehensive interpretations are concerned—and let us bear in mind that they are not proposed as comprehensive by their founders—and that, in this respect, the approach by the metaphor must be preferred. Schneider's position is thus corroborated: instead of being considered peripheral (Lounsbury), metaphor must be taken as the central core of the phenomena under study (see 10, 22–25, 29).

V. Transformational Analysis and Testable Hypotheses

I shall take up two topics in this part. The first one, transformational analysis, will be examined in a different way than it is in linguistics. Then I shall review some structural hypotheses and the procedures used to test them.

Transformational analysis.—I want to make it clear at the outset that there may be very little in common between Chomskyan linguistics and transformational analysis in structural anthropology. The notion of transforma-

![Figure 3. Model IV (see 26).](image-url)
tional analysis in our field was first used by Propp in 1928 (64). Then Lévi-Strauss reintroduced it independently in 1949 (32) and developed it further in 1956 (34). For Propp, the transformers are the sociological contexts of folkloric items, and the transforms are terms or dramatis personae [for a summary and discussion, see Maranda & Königäs Maranda (58)]. Lévi-Strauss used the term first in the analysis of art and in the context of symmetry. This may be a coincidence, but it so happens that in mathematics symmetry is also the point of departure of isomorphic mappings (= transformations). It may be that the consideration of plastic symmetry led Lévi-Strauss along a path parallel to that followed by mathematicians. A simple form of transformation is repetition in a reverse order or reflection (in Königäs Maranda’s terminology, transformation by renversement; in Lévi-Strauss’ terminology, by inverted symmetry). We then have bilateral symmetry. The next one on a plane is rotational symmetry, and sequences of rotations and reflections can generate a number of figures or myths. Two-dimensional symmetry is more complex but is essentially based on the same operations of transformation. [Compare the transformations in folkloristics described by Propp (64) and Königäs Maranda (23).]

In structural anthropology, therefore, as in mathematics [see Barbut in (27) and Maranda (55)], transformation means mapping. Mappings are either one-to-one (isomorphism, following the rule called bijection) or one-to-many/many-to-one. An example of the former is the Arabic and Roman numerical systems (except for zero); an example of the latter is spoken English and written English—e.g. the ten graphemes in /gloucester/ are mapped respectively seven-to-four and three-to-two in the two syllables of spoken English, and this shows that graphemes are irrelevant units for the analysis of speech.

As Propp pointed out several decades ago, transformational analysis is a useful tool to reduce the multiplicity of empirical data to explanatory simplicity. But it can also be used to proceed in an inverse way, viz, to show the depth of mental processes at work in the construction of a mythology (44). The operations Tylor refers to (combination, derivation) can indeed generate extraordinarily complex products which contain implicitly advanced pieces of higher mathematics. Not only Australian and Oceanic kinship systems but also myths from all over the world bear out that if the human mind can barely do more than derive and combine, it performs these simple operations with great virtuosity. We may hypothesize that, like Arabic art and divination, myths exhaust all the possibilities tolerated within a semantic universe.

A word about culture change in this connection. Whenever societies are under stress, they draw on all their semantic resources in ritual and in myth to interpret the situation. Culturally defined mediators are then revamped and surcharged, as it were, with all the semantic resources possible, to face the emergency. They often fail (Königäs Maranda & Maranda Model II). But the very mobilization of resources also triggers all available transformation processes available in the semantic repertoire, and the society in question lays
bare its most fundamental mechanisms. Similarly, when the world is to be ordered, all resources are grouped and put at the disposal of a versatile culture hero. Savard, for example, has shown this at work among the Montagnais-Naskapi Indians (67), and da Matta is tackling the same ethnographic situation among the Timbira in (57).

Transformational analysis and the generative approach in structural anthropology, therefore, are essentially a matter of mapping rules which reduce empirical diversity to cognitive manageability. One can then pass from the discrete to the continuous and vice versa in a hierarchical system whose nodes are indicative of semantic depth, as suggested by Buchler & Selby after Yngve (6); see also (49).

*Testable hypotheses.*—Word Association tests (see above, Part IV) are useful tools for investigating the semantic structures which underly cognition and its parameters as expressed in mythologies. Sophisticated approaches have been developed over the last decade by psycholinguists and semanti-
cians.

Word Association tests enable students of myth and cognitive systems to test hypotheses formulated on the basis of narratives, taxonomies, and other folkloric documents. I have modified the approach into Sentence Association tests and Plot Association tests (50, 54). These strategies make it possible to define the idioms or semantic units larger than "words" which are the components of discourse (cf 9). Patterns are thus identified, parameters are hypothe-
sized, and transformers are shown to be either productive or not in a given cultural universe.

Two examples will illustrate the point. The first one is an analysis of riddles and the second, an analysis of myth. In her papers on the structure of riddles, Köngis Maranda (22–25) can generate riddles by using the mechanisms which describe their structure. The analysis of a sample enables her to formulate a core structure and its mappings. By using the rules formulated on the basis of the sample, she generates new items. Standard collections are then searched to verify the acceptability of the riddles artificially generated.

While Köngis Maranda remains within well-defined linguistic and cultural areas (Finnish; Lau of the Solomon Islands), Lévi-Strauss works on a higher comparative level. To take the case of one of the "zooemes" in the Amerindian corpus, the positional definition of the north American "pheasant" corroborates the results of the analysis of the south American "partridge." Thus, a structure defined deductively in *Mythologiques I* (39, pp. 209–13) is confirmed empirically in *Mythologiques IV* (43, pp. 353–54). In other words, the North American data provide evidence to assess the interpretation of the South American ones: the definition by a series of commutations of the "partridge" as mediating between the world of the living and of the dead is borne out by the North American conception of the "pheasant" in the same role.

In the Soviet Union, Meletinski and his associates combine synchronic
and diachronic analysis to investigate the dynamism underlying the evolution from myth to folktale. Although restricted mainly to the European area, their works occasionally refer to nonliterate societies [for an application of their approach by one of them to Tsimshian myths, see Segal (68)]. As mentioned above, they base their model on Propp's fundamental contribution. Such operational concepts as the constant amount of power distributed between dramatic personae, the function of tests in the structure of folktales and myths, and that of magical objects (inverted from myth to folktale), are all testable propositions. In fact, several were tested on Okanagan and Kwakiutl data and on the lais of Marie de France (where folklore and early literature merge), by the Jileks (21), Reid (65), and Layton (28). The conclusions reached show clearly that structural folkloristics has reached a high level of sophistication in anthropology.

The society-specific prevalence of one or the other of the Marandas' models as indicative of cultural and social orientations has also been tested on different corpora and provides a broad basis for semantic taxonomies. The semantics (cognitive parameters) of interaction with the physical and social environments, and competition versus coalition in both cases, culminate in self-assertive exploitation strategies ("capitalistic" orientations, as in European folklore, or cooperative strategies, as in Eskimo folklore) which can be typical of more or less successful societies in the face of culture contact or under other forms of stress. Mythic structures thus provide predictive models: closed, sterile structures (see above, Model III) have only survival power, while open, productive ones (Model IV) allow for versatile and winning combinations.

The same models are also useful for investigating culture-learning behavior. As experiments with children have revealed, structural competence is achieved only at age 9 in well-to-do areas in our societies. Additional tests will show whether this varies with socioeconomic factors and cross culturally (26, Chap. 1.8).

Finally, the growing use of Digraph theory in the field yields more and more systematic and precise formulations of problems, of testable hypotheses, and of their verifications (1–3, 6, 14, 51, 52, 56).

To conclude this part, I should like to say that perhaps two of the major results of structural analysis are: (a) the definition of basic mechanisms at work in ideological systems which rests on the construction of homomorphisms (transformations); and (b) the hypothesis that myths, like other major semantic mechanisms, are ergodic systems—in other words, Propp's approach leads to valid predictions.

**Summary and Conclusion**

The five parts of this survey have presented some aspects of structural analysis that are salient according to the present reviewer. Themes have been briefly developed as well as summarized. Emphasis was put on semantic systems as chartered in myths, which are viewed as negentropic devices. These
function as mechanisms to annul history. The fact that they fail to do so in no way affects the methodology. The Sapir-Whorf hypothesis and componential analysis should thus be pulled inside out. To state it in topological terminology, which is appropriate, anthropological domains are not simply connected, as Sapir and Whorf, functionalism and componential analysis would have it, but they are multiply connected.

Testable hypotheses were also considered. On this front, it seems that structural theory has been perhaps one of the most productive fields in our discipline over the last decade. The next 10 years should see still more valuable results.

In conclusion, I should like to say that a negative picture might very well have been depicted. I could have emphasized a number of flaws; I could have dealt at length with shortcomings, oversimplifications, trivia, and I could have presented the field as a futile exercise in pseudo-mathematical pretensions. I do not deny that structuralism lends itself to such critiques. Yet I opted for a more positive evaluation.
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In one sense "linguistic theory" is whatever linguistic theorists choose to do. In the late 1940s, for example, they sought to characterize the relationship between some corpus, whether fixed or potential, and an analytic statement of structure. With the self-proclaimed revolution in the field during the past 15 or so years, linguistic theory in the dominant school has been defined as the construction of a universal theory of grammar in the sense of necessary universals of linguistic "competence." This is from the point of view of what linguistic theorists are doing. Clearly all linguists at all times have as a general aim the elucidation of language in every respect; this is the ideal linguistic theory.

To a great extent, the issues of theory in syntax and semantics are the same as those in phonology and morphology. These include criteria of category and rule types, rule applicability, naturalness of linguistic generalizations, diachrony versus synchrony. Inasmuch as the most intense recent controversy has detailed areas of "syntax," broadly conceived, and "meaning," and because of severe space limitations on the current report, I limit my exposition to these concerns. In addition, this discussion of the more restricted area is based principally on the last two bibliographical years, with emphasis intended to be in proportion to the work reported. During this period linguistic theory as defined by the transformational generative point of view continued to dominate the field as the most formalized, most developed, and most explicit framework.

**Competing Theories**

While it is true that the transformational generative school is by far the dominant one in linguistics, continuing work is reported on several other approaches. It is interesting that once such a theory as generative grammar becomes dominant, then all discussions of other theories become polarized to arguing with this dominant one. This is clearest in stratificational theory, as developed by Sydney Lamb and others. There was a typically vicious attack on stratificational phonology, for example, in Paul Postal's *Aspects of Phonological Theory* (133), and this has led to a standard disclaimer for the ap-
proach by stratificational linguists. Peter Reich, for example, indicates that "the goal of this grammatical system differs from that of the transformationalist. Our goal is to model natural language behaviour" (143, p. 21). This is mirrored in the distinction that Sampson (150) makes between a "generative" description of a language in transformational terms and a "communicational" description in stratificational terms. If we are to equate this with a "performance" model, it is curious that no performance data are given. Early criticisms of stratificational grammar indicated that formalizing this approach would result in an extreme form of post-Bloomfieldian structuralism: language as a set of independent structural levels related by rather arbitrary "realization" rules to account for alternations. This argument has not been met; instead, as we saw, the stratificationalists have simply said that their goals of description are different. Hence, they have recently eschewed rules of grammar in formalized symbolic terms. Reich claims that language is representable in terms of "relational networks" (142), diagrams of nodes for alternations connected by lines for realization and concatenation, through which one moves from level to level to produce a sentence. This avoids the difficulty of having one's rules criticized, as well as the difficulty of predicting the wrong generalization about alternations. Under the circumstances, there can be no enrichment of a linguistic theory of competence in general from stratificationalists, since they seem to respond to criticism of their formal mechanisms by dropping them.

Tagmemics, originally the creation of Kenneth Pike, is another taxonomic approach in which theoretical statements carefully take account of transformational grammar. Recently tagmemicists have embraced the concept of deep structure though the way in which this is to be integrated into the theory is not at all clear. Longacre, in his theoretical discussion of "hierarchy" (109), makes no use of deep structure, though he describes in an interesting way the kinds of nestings of syntagmemes of one level into those of another. For example, a relative clause is a clausal-type syntagmeme functioning as a word-level one when it modifies a noun. The tagmemes noun and adjective have as exponents the syntagmemes noun and clause respectively. In a transformational treatment, this recursion would be treated at the underlying level as an embedding of a sentence node in the noun phrase where it occurs as a relative clause. But what would a tagmemic description consider to be the deep structure in this case? It is clear that one way in which tagmemicists have used deep structure is to describe paragraphs. Here for purposes of pronominalization and other anaphora "the boundaries of deep structure sentences and surface structure sentences are not always co-terminous" (Wise & Green 160, p. 279). There are programmatic statements about this by Ballard, Conrad & Longacre (10), but I suspect that the nuclear and peripheral syntagmemes of Longacre (110) achieve a consistent deep tagmemic representation that is antithetical to "deep structure" as it is currently understood by transformationalists.

So far tagmemicists have provided two different catalogs of sentence
types, referring to them as deep and surface representations, but the point of deep structure originally was to explain the several types of surface representations of an underlying structure by automatic rules of transformation. The only explicit use of transformations I have seen is a deletion of an underlying repeated element that Longacre (109, p. 194) employs to explain the sentence *Green frogs do not have great intelligence, but jellyfish have less*. Tagmemicists in general still extol the system they set up in terms of sentence-type formulae which "are partially similar as well as contrastive" (Elkins 47, p. 219), without really explicating the basis for this partial similarity. Yet none has denied its theoretical significance.

There has been continued work recently on the kind of transformational grammar generally associated with Zellig Harris, out of which the Chomskyan type evolved. In his *Paraphrase Grammars* (151), Smaby tries to come to grips with a description of sentence structures as "paraphrases" of others. There are many interesting questions of constraints that must be imposed to give the correct paraphrase relations; the devices suggested remind one of the global derivational constraints and indices used in generative semantics, though the systems are very different. More in the Harris tradition, as elaborated by Henry Hiz, is a work by Robbins (144) on definite articles, reviewed by Jackendoff recently (70). Definite articles come under the rubric of anaphora and specification generally, and pose some exasperating problems for generative grammar. The problems for the Hiz-type analysis seem to be even more severe. Here we start with kernel sentences and derive the complex sentences with *the*, for example, from combinations of kernels. But, as Jackendoff notes, a grammarian’s question should be why all these different sources (one can easily imagine them) should wind up with the same definite article: is it accidental? In general, this is known as the "naturalness" problem in linguistic theory.

These several traditions are independent of transformational generative grammar. Because they lack widespread familiarity and devoted workers it is not at all surprising that they should leave some crucial issues vague and undeveloped.

Transformational generative grammar has been dominant because of the two-sided approach that it has taken since the beginning: constructing an explicit general theory of grammar along with the description of facts in terms of it. In this sense, for example, Chomsky (33, p. 55) discusses the justification for a phonetic representation in grammars:

*There is no a priori way . . . to justify the postulation of the level of phonetic representation. . . . The most that one can hope to show is that an interesting range of phenomena can be accounted for by a theory that incorporates a level of phonetic representation of the sort postulated, that there is no crucial counter-evidence, and that there is no reason to suppose that some alternative form of theory will be more successful.*

And in this paradigm for theoretical discussion the difficulties with alternative proposals begin. Linguists had at first thought that there was some absolute
notion of simplicity that was being appealed to in claiming the superiority of transformational grammar. The necessity for comparability of theoretical formulations in ascertaining simplicity should not be lost sight of in the ensuing discussion of the schools of generative grammar. The discussion can never be in terms of absolute simplicity; simplicity is defined only with respect to one given theoretical formulation. Instead, discussions recently have focused on the empirical consequences of the alternatives proposed. Can we or can we not explain phenomenon X—and is it a phenomenon to be explained—with the old and/or new formulations?

The theory of transformational grammar that was codified by Chomsky's *Aspects* (31) consists of a "base component" that includes categorical rules of several forms, and certain kinds of "local transformations," related to lexical markings. We have context-free phrase structure rules that give the constituency of sentences (S) in terms of categories noun phrase (NP), verb phrase (VP), and so forth. In this way, labeled hierarchies of these constituency relations are generated. They are perhaps more familiarly represented as tree diagrams. Many of the lexical categories such as nouns (N) and verbs (V) show specific patterns of co-occurrence restrictions. For example, verb-object restrictions are violated in the deviant *John intimidated the book*, but are not violated in the acceptable *John intimidated the neighbors*. These are provided for by marking the nouns of a derivation with classificatory distinctive features and then using these feature specifications of nouns to define frames in which different classes of verbs may be inserted. All of the lexical categories are represented in a dictionary or lexicon, where individual lexical items are composed of syntactic, phonological, and semantic features (the formalism need not concern us). By matching the syntactic features of a lexical item to those of a lexical category in the derivation, we can insert into the derivation lexical items of just the proper lexical classes. The resulting structure is a "deep structure," fully specified for its lexical content, and with all grammatical information necessary for semantic interpretation by a system of rules which are not worked out.

It is important to note that deep structures here determine the semantic interpretation of sentences. This has sometimes informally been referred to as the requirement that "transformations preserve meaning" in the subsequent theoretical discussion (e.g. Partee 126). See Kuroda (89) on ways to interpret this. The phrase as such is meaningless, quite clearly, since the output of transformational rules, a surface structure, does not have a meaning by this theory, but its corresponding deep structure does. What is meant by the phrase is that surface structures do not contribute to semantic interpretation. This implies that a difference such as negative vs declarative surface structures with the same subject, object, and verb, e.g. *John is not chasing the ball* vs *John is chasing the ball*, do not share the same deep structure but instead have related deep structures as follows. In the first there is a negative element, Neg, that determines two properties, the semantic interpretation as a negative, and the automatic application of the sequence of transformations whereby Neg (which
winds up as not in the surface form) is placed just after the tense-bearing element of the verb phrase. In the second no such element occurs. Thus, a deep structure which can be abbreviated s[Neg NP[John] VP[Pres Prog [chase NP[the ball]]]] becomes the negative sentence, while one of the form s[NP[John] VP[Pres Prog [chase NP[the ball]]]] becomes the declarative one.

A deep structure serves as the input to transformational rules, which operate obligatorily just in case the "structural description" of the rule is matched by that of the input structure. Transformational rules are ordered, in the sense that the output of one may serve as the input to another. Each rule depends on the specification of various constituency relationships in a tree for its operation, and it changes this constituency by insertion, deletion, or permutation of constituents. One of the major innovations of this model over that of the earlier transformational one is that it provides directly for embedding sentences into sentences to form complex sentences at the surface level. The transformational rules are to be applied cyclically, that is, on the domain of the lowermost embedded sentence and, having run through all applicable transformations in order, then all applicable rules are applied to the next higher one, working up to the topmost sentence. A relative clause may be passive, for example, embedded within a passive sentence. This is reflected in the fact that passivization applies twice in the transformational derivation of the surface structure from the deep structure: first in the domain labeled by the inner relative S, then, after relativization, in the full S. So we derive a surface structure roughly of the form s[The dog s[that was bitten by the bat] was discovered by John] from s[John discovered NP[the dog s[the bat bit the dog by Pass]] by Pass] (this last extremely sketchy).

There are devices to mark deviance of the output of a transformational rule. A well-formed output of all applicable transformations is a surface structure. In this sense, some structures generated by the base component are not proper deep structures because they do not correspond to well-formed surface structures. For example, in our relativization example, one of the conditions on the input tree is that the noun phrase on the inner S be in some sense "identical" to the antecedent noun in the higher sentence. In this example, we can relativize on the dog; were one of the nouns of the inner S not "identical" to the antecedent, well-formed relativization would not be effected. Chomsky called this the 'filtering' function of transformations. The question arises of whether or not such derivations as violate conditions are semantically deviant as well. In this connection, we might say that grammatical vs semantic deviance is a very difficult issue. The assignment of grammatical and semantic deviance is of course relative to the theory proposed. Hence some sentences are grammatical but semantically deviant (they violate some implicational relationship among features: *These three boys are these two girls); while some are semantically sound but grammatically deviant (they violate conditions on transformations: *Five pounds are weighed by this fish); and some are both grammatically and semantically deviant (by combining these two types). It is clear that the notion of grammatical and seman-
tic soundness is independent of the notion of "acceptability" of sentences, which is defined in purely behavioral terms. What are the relations between the two, however, and how do we in fact determine what is intuitively felt to be semantic deviance as opposed to grammatical?

It is this Aspects model that has served as the background for the discussion of transformational theory in the last several years. Various of its claims have been attacked. At first the use of features for subcategorization of nouns (and, by the mechanism of selectional restrictions, of verbs) was questioned (McCawley 111, 113; Kuroda 88). Then, as more data were examined with this model, it became clear that deep structure, defined on the basis of certain syntactic relations apparent in sets of surface sentences, was inadequate as a complete semantic input. [Recall that "transformations preserve meaning," but see S. Anderson (2) for a case where the proper generalizations are made at the level of deep structure.]

These questions first reached prominence in connection with the derivation of nominal phrases of the type of English gerunds. We frequently have alternative ways of expressing the nominalization corresponding to a verbal relation: John's refusing the offer, John's refusal of the offer, John's refusing of the offer. A 'transformationalist' position on this question was that the apparent nouns were transformationally derived so that the lexical specification of an item [refusal] was unnecessary. Instead, we could derive all the nominalizations from some transformation on an embedded nominal sentence NP[N[John refuse the offer]]. This would provide the proper input for the semantic interpretation of the nominal phrase, since native speakers perceive the same relation of subject-verb-object in these cases (see Fraser 57, Newmeyer 121). Note that the difference between the types, nominal and gerundial, must be specified somehow in the deep structure to trigger the proper transformations.

The alternative hypothesis, which Chomsky defended at some length (34), is called the 'lexicalist' hypothesis, whereby noun phrases of the form N[P[N[John's] N[refusal] of the offer]] are generated directly in the base component, whereas the gerundial forms are productively derived by transformation. [See also the interesting arguments of Wasow & Roeper (156) that distinguish the two types on the basis of the (controversial) node VP, contrasting with S.] Here Chomsky captures the intuitions about English that the derived nominals, so called, are fossilized and irregular in their relation to the verb forms, whereas the gerundial forms are quite regular in an unrestricted way. This leads into the question of lexical decomposition in general in a generative grammar, which has become one of the major differences in the next incarnation of the discussion, "generative semantics" vs "interpretive semantics."

Consider a generative grammar in which the insertion of lexical items into a hierarchical tree structure is not all accomplished before the tree has undergone any grammatical transformations. It follows that some lexical items must be inserted into structures that are outputs of transformational rules. Now if
some of these grammatical transformations create structures for lexical insertion, then the lexicon consists at least in part of transformations substituting single items for subtrees. For example, Postal proposes (136) that the lexical item [remind] is derived by a sequence of transformations from [perceive $s$— similar to $-$]. (So The ambassador reminds the Queen of a gorilla derives from something of the form $s$[The Q. perceives $s$[the A. is similar to a g.]]. The crucial stage of the derivation has the nodes to be conflated all dominated by a single node and in a certain constituency.) The consequences of such an hypothesis are several: (a) the notion 'deep structure' in the sense of the Aspects model is not defined; (b) the empirical basis for this claim must demonstrate that some grammatical transformations apply before certain lexical items of the Aspects model have been 'formed' by the lexical conflation transformations; (c) semantic interpretation on the basis of "deep structure" loses its motivation. In addition, one can see that the decomposition of lexical items into the complex tree-like structures might lead to a reduction of the underlying categories of sentence formation. Are all derived nominals from underlying verbal phrases, and, if so, are all nouns derived from relative clauses from intransitive verbs like [man] ("the x such that x mans," if you will permit the vulgarization of the hypothesis), as Bach (5) proposes? This systematic building of very abstract underlying forms for sentences, sometimes claimed to "be" the semantic representations for them, has characterized generative semantics.

In contrast, Chomsky and several of his students have been working on an alternative way to salvage what one can of the Aspects model. They abandon the requirement that "transformations preserve meaning." It follows that surface structure representations—or intermediate ones—are relevant to semantic interpretation as well as the deep structure representations, since they now reflect the cumulative meaning "changes" effected by the transformational rules. Chomsky calls this position the "extended standard theory" (35, p. 15). Such sentences as The boy hit the ball and The ball was hit by the boy were related to each other in the Aspects model by the absence vs presence in the deep structure of a passive device, and the operation of the transformation itself contributed nothing to semantic interpretation. However, in sentences with quantifiers all, each, none, etc, the active and passive senses seem to differ in interpretation with different relative placement of the quantifiers. Chomsky, in Aspects (31, p. 224), already pointed out Everyone in the room knows at least two languages vs. At least two languages are known by everyone in the room. The senses differ in more than active vs passive relationship, and he suggests that "the reason for the opposing interpretations is an extraneous factor—an overriding consideration involving order of quantifiers in surface structures—that filters out certain latent interpretations provided by the deep structures." In more recent work (33) he suggests formally that the pair (deep structure, surface structure) together determine the semantic interpretation of sentences. Jackendoff and Dougherty have provided a great deal of the motivation for this kind of semantic theory in many im-
portant papers which we will come to below. In fact, Jackendoff (68, p. 489) suggests that "there are two independent hierarchical structures in the semantic representation": a 'functional structure' based on grammatical relations, and a 'modal structure' giving the referential conditions.

Unless we do resolve the different semantic contributions in this fashion, we are left with a very difficult question in trying to construct an interpretive semantics. If paraphrase relations among sentences are the source of grammatical information in terms of deep structures, and these deep structures determined the application of transformations, then there is some formal link between the surface arrangements of sentences and the intuitions of natives about the underlying relations. These are expressed as transformational rules. If semantic interpretation depends now on both deep and surface structures, with no restrictions on the kinds of contributions each may make, how are we to determine which level, deep or surface, determines certain paraphrase relationships and co-occurrence restrictions? Granted, once we have a system of transformations and a principle that meaning depends on deep structure, we can revise this so that scope of negation and quantifiers depends on surface structure configurations. But how else do we determine what else is a surface phenomenon, for example the feeling that refusing the offer and refusal of the offer have the "same" reading, while refusing the offer seems to differ slightly in interpretation? Is this a surface structure contribution of some optional insertion of of? The approach of interpretive semantics, as reflected in the Jackendoff proposal above, suggests that "grammatical" relations are determined by deep structure. We must construct then a theory of possible grammatical relations in the form perhaps of a universal base hypothesis. This has its own problems (see below). Further, if we wish to restrict the surface structure contributions to such elements as quantifiers, how are we to determine what may count as a quantifier? One might say, by the universal theory of what a quantifier may be. It does not seem that logical quantifiers provide enough of a repertoire, nor do the traditional modalities of European languages. Interpretive semantics is certainly a possible way to save the notion of grammatical deep structure, but unfortunately, I see no way of making it an empirical hypothesis at this point.

These two schools, generative semantics and interpretive semantics, emphasize different aspects of the so-called "standard theory" [the terminology is Chomsky's (33, p. 55)]. As Partee (125, pp. 656-57) outlines, one would do away with deep structures in order to keep the notion that transformations "preserve meaning"; the other would do away with the hypothesis that meaning is determined from the deep structure uniquely, without significant contribution from the surface structure. But there are other versions of what seem to be transformational grammar, if we can intuit their formal properties from the discussions of them. One is associated with the work of Charles Fillmore and the other with that of Wallace Chafe. Both of these approaches to linguistic structure seem to point out an inadequacy in the standard theory in its treatment of the so-called "case" relations of sentences. By these are meant
the grammatical properties relating certain of the noun phrases in the underlying structure to the verb phrase, to the sentence, and so forth. These theories wish to indicate in some way that the relationship involved between "object" and verb in *The man opened the door* and the relationship between the "subject" and verb in *The door opened* are the same.

Fillmore conserves more of the framework of the standard theory in this respect, since after the assignment of the case relations to the constituents making up a sentence with a verb, there follow several rules of "promotion" of the grammatical subject, with some changes of the verb concomitant with these. This creates a constituency much like that of the standard theory. Then the transformational rules of the more usual sort, e.g. embeddings, would operate as in the standard theory. This version of transformational grammar has not been tried out on a large corpus of data; indeed, like Chafe's it is fine for very simple cases, but untried and formidable looking to test with very complex ones. In the first paper proposing his case grammar (49), Fillmore gives the basic constituency of sentences as $S \rightarrow \text{Modality} + \text{Proposition}$, where the proposition consists of a verb with several types of case markers following: $V + C_1 + \ldots + C_n$ ($C$ stands for case), e.g. Agent, Object, Dative, Locative, etc. (Whether or not there is a theoretical limit on the types of such cases would presumably be resolved by a universal base hypothesis.) The specific verbs of the language are classified for occurrence in the different case configurations made possible by the constituency rule, e.g. [___ A (I) O] for a transitive that optionally takes an instrument; on this basis, a proper form is inserted in the environment. Then rules apply for subjective topicalization (promotion) of each of the different cases in a preferential order, depending on the total configuration of case elements present. For example, the most general rule: "If there is an A[gent], it becomes the subject; otherwise, if there is an I[nstrument], it becomes the subject; otherwise, the subject is the O[bject]" (49, p. 33). Note the underlying structures: [open $A$[the man] $O$[the door]] vs [open $O$[the door]]. In order to have the $O$ as subject in a sentence with $A$ present, the verb must be marked with a special "passive" feature; this will be spelled out at a later time as the familiar *be* plus participle construction. The cases other than subject are spelled out in English by prepositions determined by the particular verb.

Several studies have incorporated this framework in presenting grammatical material, including Fillmore's own discussion of types of lexical information (51) necessary in a generative grammar. For example, Babcock (4) proposes that at most one case of a given type be allowed per sentence in underlying form; if this is true, it eliminates conjunction-reduction as a source for certain compound nominals. Miller (119) proposes that a new case "Comitative" be recognized for Russian conjunct noun phrases of the type *my c Ivanom poshli*, literally "we with Ivan went (pl)," that is, "Ivan and I went." Walmsley (155) proposes similarly a "Comitative" to distinguish these from examples of the type *Seymour sliced the salami with a knife*. Chapin (30) simply assumes the Fillmore model to discuss pronominaliza-
tion in Samoan. A somewhat strange version of these case relations is incorporated into Hutchins’ (65) “semolexemics,” and similarly, J. Anderson (1) wants an “Ergative” case function of nouns, although in a kind of dependency-tree (see Robinson 145). This is antithetical to Fillmore’s own work, which explained nominative vs ergative surface systems as produced by variant rules of promotion in languages. Perhaps the most interesting of these articles is Lees’ reexamination of English nominal compounding (106), which he investigated first in his famous monograph of 1960 (105), the first application of transformational theory. With the addition of a specific case marking to inputs of compounding transformations, Lees is able to disambiguate precisely many more compound types.

However, there have been a number of criticisms against this approach in its most general form which are so far decisive. First Dougherty (44), in a long review of the original Fillmore article, points out the difficulties of sentences with animate possessors: Caruso broke the window with his voice vs Caruso’s voice broke the window. These two sentences would be assigned the same underlying case structure by Fillmore’s grammar. But the first of these has two distinct underlying forms by Fillmore’s proposals. There are other such examples, which leave us with the feeling that the case system as such is too general to be of direct syntactic relevance, and not quite proper semantically either (see Fletcher 55). Chomsky mentions the existence of case grammar in his “Deep structure . . .” (33) in a footnote, and deals with it somewhat along the lines of Dougherty in his latest survey (35), coming to the conclusion that in fact the level of deep structure explains the syntactic and semantic properties of case-like relations better than Fillmore’s case system. The crucial facts are simply that under Fillmore’s hypothesis almost every verb in the language shows idiosyncrasies in interpretation with different case markers preposed by the promotion rules.

And yet the correlation of subject, object, and other case-like relations in sentences with semantic properties of noun phrases needs exploration by grammatical theory. There are syntactic facts that correlate with this: for example, in ergative languages there are split systems [Chinook, Dyirbal, Wichita (see Rood 146)] where first and second person agents function with nominative-accusative surface case, while third person agents must be ergative; the reverse does not seem to occur. First and second persons are always animate, “natural” agents; third person is not necessarily animate, and so receives an ergative surface mark. Several series of psycholinguistic experiments bear out this naturalness in subject-object pairs. See Dubois & Irigaray (46) on French, and Clark & Begun (36) on English, with the same results. The hierarchy is: “Human nouns, Animal nouns, Concrete-count nouns, Concrete-mass nouns, Abstract-count nouns, and Abstract-mass nouns” (Clark & Begun 36, p. 42). It should be possible to account for the preferential promotion of one noun phrase among several to be surface subject if the standard theory incorporates a relational notion defined on pairs of noun phrases in the deep structure.
Fillmore's innovation is particularly at the level of representation of these case-like relations, and he keeps the rest of the system, apparently, more or less intact. Chafe, on the other hand, has presented in very strong language the conceptual outlines of a system that rejects, or seems to reject, the major constraints on transformational grammar that have been developed. The idea here, "language as symbolization," is to capture the production of speech by a system that starts with "semantic representation" and generates, unidirectionally, phonetic forms. The standard theory is not directional in this way. For example, "well-formed deep structure" is not defined until we have mapped it onto a surface structure. Chafe has outlined the system for English in a lengthy work, Meaning and the Structure of Language (27), and has applied the system to one of the Iroquoian languages, Onondaga (28). Rood, in his interesting paper on Wichita (146), makes an argument for something like Chafe's "agent" and "patient" underlying structures as relevant to the determination of verb agreement.

Chafe's system starts with a verb as fundamental to the structure of a sentence: "a verb is always present, though it may in some instances be deleted before a surface structure is reached" (27, p. 97). The verbs are semantically subcategorized, as for "ambient," "process," "action," etc [the features are marked minus (−) or present (+)], and these different subcategorized verbs generate a constituency which includes the obligatory accompanying units, agent, patient, instrument, etc. Some of these rules are obligatory, others optional. (In presenting these examples, I linearize his system of transcription.) Compare Fillmore: "One is almost willing to allow these facts to be expressed by a generative process which chooses a verb, then the cases required by that verb, then the other cases that are compatible with the cases originally chosen" (49, p. 87).

Thus for example, \( V_{[\text{process, action}]} \) generates obligatorily patient and agent nouns, and optionally an instrument. This gives a constituency \( [[[V_{[\text{pro, act}]} N_{[\text{pat}]}] N_{[\text{ins}]}] N_{[\text{agt}]}] \). Inasmuch as Chafe writes his constituency rules one at a time with \( V \), it never becomes clear how we arrive at this constituency, rather than say, \( [[[V_{[\text{pro, act}]} N_{[\text{ins}]}] N_{[\text{pat}]}] N_{[\text{agt}]}] \). He clearly appeals to some intuitive judgments about the constituency of these relations in a native speaker-linguist. If these intuitions are clear, they should be built into the system. Further, it should be noted that the initial symbol of all his rules of constituency is not 'S' (for sentence) as in the standard theory, but the verb itself. Thus these are not by any means phrase structure rules, as Chafe would be the first to point out; they are of the form \( X \rightarrow X + Y \). After these relations are defined, each of the constituents is subcategorized with further features such as count, potent, animate for nouns. The verbs undergo optional derivation, with a wide variety of semantic markers, such as inchoative (\( \text{wide} \rightarrow \text{widen} \)), resultative (\( \text{break} \rightarrow \text{broken} \)), causative (\( \text{hot} \rightarrow \text{heat} \)). These are represented as elements conjoined to the root of the verb in underlying form (\( \text{root} + \text{inchoative}, \text{etc} \)). But how are we to distinguish what should be treated as a feature on the verbs from what should be treated as a derivational suffix or
prefix in underlying form? Chafe’s rules of postsemantic “literalization” to spell out feature complexes (e.g. $V_{[p_{part}, p_{proi}]} \rightarrow \left[[V \ be_{[p_{part}]} \ have_{[p_{proi}]}\right]$ are ideally adaptable to this purpose; again, is it the intuition of the speaker-linguist that makes the distinction? The formalism one can reconstruct from his informal discussion is so powerful that it will make it possible to generate a vast number of surface elements from alternative kinds of underlying forms. Is there a restriction on this potential that will give the right solution in each case?

After rules of derivation, Chafe separates out a semantic layer of “infection” in nouns and verbs, where “infectional units . . . do not influence the choice of a lexical unit, and are not redundant if the lexical unit is known” (27, p. 168). Here are such features as perfective, progressive, anticipative, inferential on verbs, and definite, generic, plural on nouns. These infectional features are added to the specifications derived by the derivational component, and are to be spelled out afterwards by the literalization rules, as we saw. The interaction of infection and derivation is a subject so vexed, I cannot go into the details here, but it should be noted that every language has an interaction of the inflectional possibilities with the nature of lexical items; deponent verbs in any language are an example (e.g. oida in Greek occurs only as a perfect). Of course, we can say, as Chafe seems to be doing, that this is a semantic process not directly reflected in the surface forms of language; this merely points up the power of the machinery.

Central to the system at this point is the specification of what information is “new” and what information is “old.” This has obvious parallels to the Prague School distinctions of ‘theme’ and ‘theme’ respectively, as outlined for example in Daneš (37) and Kiefer (85). “New” information constituents are related to the “focus” of the extended standard theory, and “old” information to the “topic.” Chafe gives a series of rules for marking certain constituents as containing “new” information, and these markings will determine certain properties of surface structure such as the topic and comment of a sentence. In passive sentences, for example, with $V_{[p_{passive}]}$, it is obligatory to mark $N_{[p_{act}]}$ as “new.” This will determine that $N_{[p_{act}]}$ (the “object” in underlying structure) will be come the surface subject of the sentence, by later rules of linearization. There are also markings for “contrastive” information, which have their own effects on the surface structure. I think Chafe is confusing here the difference between topicalization of elements of a sentence, where languages indeed differ very much in the surface realizations, e.g. preposing or postposing an item, stress, subordination of the rest of the sentence (French, Yiddish), with what we might call, for lack of another cover term, “topicalization.” By this we mean alternations in surface case relations that occur in sentences that paraphrase each other, up to scope of quantifiers, etc. There are clearly tendencies in languages to conflate the two, but I think they must be kept distinct for purposes of semantic interpretation.

After a sentence is formed by these various semantic processes, there are “post-semantic” processes to “literalize,” i.e. spell out in ordered constituencies of syntactic formatives, the information coded by features on the lexical
items. Note the English past progressive quoted above. These formatives are “linearized” into the surface order, “symbolized” in a phonological transcription, and then phonological processes will convert them to phonetic forms.

Chafe’s system has been reviewed by Nilsen (122), who is most disturbed by the “atomistic” and “arbitrary” nature of the post-semantic processes. This is another way of asking for clarification of the restrictions on alternate treatments of a given phenomenon. In a lengthy review of Chafe’s book, Langacker (103) brings up some of the same questions, trying to understand the motivation for the analysis Chafe presents. Clearly, Langacker believes, part of the motivation comes from Chafe’s view of the evolution of language, outlined in the first chapters; these speculations themselves are open to doubt. In addition, there is the odd feel of the standard transformational theory in dealing with the relationship of syntactic form to semantic structure:

In its classic form, transformational doctrine tends to strike the uninitiated as a highly unnatural way of viewing language. Syntax is its prime concern, and meaning (to the extent that it is treated at all) is seen as the output of a mysterious ‘semantic component’ that no one knows very much about (103, p. 135).

He praises Chafe’s concern for the directionality of well-formedness, semantics over syntax, and Chafe’s concern for “idiomaticity,” a neglected area of transformational grammar [though see the contributions by Weinreich (158) and Fraser (56) within the standard theory; Malone (117) presents an ad hoc mechanism]. However, in considering the little formal motivation for the analyses proposed in Chafe’s system, and the vast power of it, Langacker too rejects it as an empirically based alternative.

Chafe’s most recent contribution, written from his framework in mind, suggests that “true paraphrase exists only in those relatively few cases where random or optional choices in the application of post-semantic processes lead to alternative surface structures for a single semantic structure” (29, p. 25). Given the primacy in Chafe’s view of intuition over any formal evidence, how are we in fact to determine that a given process, say reordering, is indeed “post-semantic”: we feel that He looked up his old classmate vs He looked his old classmate up are different in some way. The grammar must assign this difference to a certain level. Without some theoretical restrictions, is intuition enough?

The central theoretical concerns within generative grammar do not lie in the validation and elaboration of the Fillmore or Chafe frameworks at the present time. Instead they center around the controversy alluded to before between the school of generative semantics and the school of interpretive semantics. A number of controversies have developed around the notions of what constitutes a “generalization” or a “unified phenomenon”; these might be terminological issues. They bespeak a concern with what we might term “accountability” of linguistic theory. Our “gut” feelings on this must be cast into some theoretically coherent form before they can be tested one against another. The problems this entails are not insignificant.
PROBLEMS OF ACCOUNTABILITY

The conception of what it is linguistic theorists must do obviously determines the acceptable and unacceptable data to be taken into consideration. One of the strong trends recently has been the search for a theory with a high degree of "naturalness" in directly explaining certain intuitions. This is fine; but one theory's naturalness is another's epiphenomenon. For example, in Language and mind (32), Chomsky characterizes the Lévi-Straussian analogizing to structural phonology as misguided, in the view of contemporary linguistic theory:

... the structure of a phonological system is of very little interest as a formal object; there is nothing of significance to be said, from a formal point of view, about a set of forty-odd elements cross-classified in terms of eight or ten features ... the real richness of phonological systems lies not in the structural patterns of phonemes but rather in the intricate systems of rules by which these patterns are formed, modified, and elaborated. The structural patterns that arise at various stages of derivation are a kind of epiphenomenon (32, p. 65).

What determines this? Certainly speakers can hardly be said to have intuitions about the interactions of rules of phonology any more than they can be said to have about the formal patterns of phonemes. In fact, it is the experience of field workers that the case is just the opposite in terms of behavioral responses: Sapir's classic papers on sound patterns are of this nature. The explanation for the patterns in terms of generative phonological rules constitutes an interesting hypothesis. Similarly, patterns that by hook and crook are maintained or reestablished in phonological sequences are static arrangements, not rules. They have lately been explained as rule "conspiracies." (Chomsky's rebuttal of this would turn on the expression "formal object.")

Hockett, in his State of the Art (63), seeks to demonstrate that all mathematical regularity in language is a kind of epiphenomenon, and that language is not, in fact, subject to generative treatment in a formalized approach. He concludes that language is not a well-defined system for generating utterances. While I cannot summarize the argument here (see the review by Palmer 123), I should remark that Hockett is to a great extent talking about the performance aspect of the dominant theory, rather than competence, and so his comments are beside the point. This agrees with the standard theory that also claims in essence that performance is not well defined. Householder, in his review (64), seems to find congenial the point of view that competence may not be well defined. Methodologically, the question can be asked differently, namely, what is the relationship of competence to performance? It seems to me that this is a fundamental question needing answer before competence has empirical content. Do the facts explained by a characterization of competence bear any regular relation to performance?

This is the approach taken by Weinreich, Labov & Herzog in their remarkable study (159) of the synchronic bases of linguistic change, and again
by Labov in his papers on Methodology (93) and on sociolinguistics (92). Here are explored the consequences of accepting, as linguists from Paul to Chomsky have, such a distinction:

Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance. . . . Observed use of language or hypothesized dispositions to respond, habits, and so on, may provide evidence as to the nature of this mental reality, but surely cannot constitute the actual subject matter of linguistics (Chomsky 31, pp. 3-4).

As was mentioned above, the data for linguistics are constituted by speakers’ intuitions about sentences, and so it devolves upon the use of these to establish the structure of linguistic competence. Methodologically, Labov has called this position “the Saussurian paradox: that the social aspect of language can be studied by the theorist asking himself questions, while the individual aspect can only be studied by a social survey” (93, p. 437). The social aspect is the supposed shared norm, Saussure’s langue, Chomsky’s competence. In fact, it has become clear by simply noting the judgments of grammaticality and ungrammaticality (which are distinct, be it recalled, from acceptability and nonacceptability in performance) made by linguists in their presentations, that these vary so widely as to make it impossible to apply the notion in practice. A recent contribution concludes that the solution to the problem presented “will be resolved by speakers whose intuitions about the sentences in question are sharper than mine, which have been blunted by frequent worrying about these cases” (Fraser 58, p. 178). Indeed, this is one of most remarkable aspects of the current scene. As judgments become more “delicate” (the term is Halliday’s) the solid basis of intuition vanishes. Bever has called attention to this perceptual problem in several recent contributions on developmental psycholinguistics:

there is no reason to believe that [our linguistic intuitions] are direct behavioral reflections of linguistic knowledge. The behavior of having linguistic intuitions may introduce its own properties; that is, there is no guarantee that a linguistic grammar itself is either a direct or an ideal representation of the linguistic structure (17, p. 343).

The data we have so far, if they reflect competence directly, seem to show variation.

One would guess that Chomsky has already declared such objections irrelevant in several crucial passages of Aspects. He notes that “it is quite apparent that a speaker’s reports and viewpoints about his behavior and his competence may be in error. Thus a generative grammar attempts to specify what the speaker actually knows, not what he may report about his language” (31, p. 8). If this concedes the operational indeterminacy of the enterprise, he still maintains that “the theoretical (that is, grammatical) in-
vestigation of the knowledge of the native speaker can proceed perfectly well [in order to] . . . account for masses of evidence that are hardly open to serious question . . . the enormous mass of unquestionable data concerning the linguistic intuition of the native speaker (often [the linguist] himself)” (31, pp. 19–20). The problem seems to be resolved in a pile of adjectives about the data.

To return then to the conditions disputed by Weinreich et al, the first principle which they find abundant evidence against is that any speech community can be homogeneous. This is not in the trivial sense of memory limitations etc, but rather in the discovery that there are very strong statistical regularities in a number of significant points of variation within every speech community studied, and that these regularities are correlated very directly with such factors as age, socioeconomic class, and so forth. Moreover, native speakers control and have intuitions about functional varieties of language; we might ask if it is correct to ignore these in dealing with paraphrase relationships. This kind of “orderly heterogeneity” for them is a prime datum to be accounted for by a theory of competence, inasmuch as it is replicable, for the same linguistic feature, in comparable communities (Labov 91, p. 761). We return to variation below.

Antal, in a somewhat intemperate paper (3), makes the interesting observation that frequently a speaker “makes errors not from his own point of view but from that of the general norm” (3, p. 180). This is presumably taken care of by Chomsky’s disclaimer about “characteristic” errors in applying linguistic competence to the production of utterances. Yet is is not clear how we are to interpret the intuitions about grammaticality of such a speaker. To judge from the assignment of grammaticality in recent publications, this is indeed a problem. [See also the amusing contribution on bilinguals by Lehiste (107).]

Pitkin expresses the difficulty in accountability succinctly, I think, in his paper on anthropological perspective: “speech, meaning, and behavior are not discretely segmentable as separate entities to be considered wholly independently from each other” (132, p. 31). Each hypothesis about the competence involved in producing an utterance entails necessarily hypotheses about the situation of discourse, the function of the utterance in the sense of the cultural variables that determine its acceptability (notice that this is a performance term), and so forth. It is clear that we are a long way from understanding this. But the point is that we will never understand competence either until we tackle these questions as part of the same problem. In part, it seems, we must first abandon the myth, in Labov’s term, of the “degenerate” nature of speech:

In the various empirical studies that we have conducted, the great majority of utterances—about 75 percent—are well-formed sentences by any criterion. When rules of ellipsis are applied, and certain universal editing rules to take care of stammering and false starts, the proportion of truly ungrammatical and ill-formed sentences falls to less than two percent (92, p. 42).
Acceptability restricts grammaticality; it does not abandon it.

Since competence consists, in the standard theory, of a grammar that includes semantics, the question of the nature of meaning looms large for methodology. I think that Bloomfield already presented the paradigm for the linguist's approach in his *Language*:

A starving beggar at the door says *I'm hungry*, and the housewife gives him food: this incident, we say, embodies the *primary* or *dictionary meaning* of the speech form *I'm hungry*. A petulant child, at bed-time, says *I'm hungry*, and his mother, who is up to his tricks, answers by packing him off to bed. This is an example of displaced speech. It is a remarkable fact that if a foreign observer asked for the meaning of the form *I'm hungry*, both mother and child would still, in most instances, define it for him in terms of the dictionary meaning (23, pp. 141–42).

The direct contradiction here to Bloomfield's stimulus-response theory of meaning is beside the point. What is important for us is the fact that there are ranges of culturally determined speech situations in which the primary meaning is evident. The theory of meaning then goes hand in hand with a theory of the speech situation in which a form is used. It is clearly to be separated from the meaning of the form, and constitutes a third term always implicitly present in the segmentation of grammatical forms, meaning, and situation. Keenan (83) distinguishes essentially the same area as "presuppositions" about the speech act.

Generative grammarians have by and large tacitly agreed that presuppositions and entailed propositions of a sentence should be handled in some way in giving its meaning. It is not clear then how to handle the presuppositions of the particular culture. Lakoff has devoted papers (98, 99) to the notion of grammatical well-formedness relative to beliefs about the world and presuppositions in general. However, as Chomsky observes (35, pp. 2–3), this is merely a redefinition of the term grammatical well-formedness, as opposed to semantic well-formedness. As we noted above, the linguistic theory provides a division of grammatical and semantic.

The notion of meaning in the standard theory was based upon a form of "compositional semantics," whereby the meaning of a construction $e[A + B]$ is calculable from the meanings of A and B. Idioms of course are a notable exception to this, and there has been some effort at incorporating them into a standard-type theory (as was noted above). For Chafe, "idiomaticity" in its broad sense is a major reason for abandoning the standard theory. Defending a purely compositional theory of meaning on the basis of deep syntactic structures has been Katz, one of the proponents of the original conception of semantics in a generative grammar. In a series of papers specifically directed against the generative semantics point of view, he tries with all philosophical cunning to show that the results of generative semantics are either inadequate (78), or show that generative semantics is equivalent to interpretive semantics (79), as he calls it. (It should be clear from our characterization that what Katz calls interpretative semantics is distinct from the use by
Chomsky, Dougherty, or Jackendoff in the extended standard theory.) Katz argues inter alia that if the underlying structure of all sentences is their "logical form," then there is no clear notion in this generative semantics of semantic ill-formedness due to such things as conflicting presuppositions of two conjoined clauses (even if the whole be grammatically well formed). This criticism is accepted by McCawley, who, however, continues: "but that is no objection: an adequate account of such sentences requires interaction between grammar and (natural) logic, and Katz has exhibited no defect in the grammar which generative semantics can provide for natural logic to interact with" (114, p. 294). Are we to conclude that McCawley excludes the presuppositions and entailments from the underlying representations of generative semantics? Such a position would contrast with that of G. Lakoff, who explicitly defines it in his paper on generative semantics (97). In Lakoff's notation, the transformational derivation of a surface structure is indicated by a series of 'P-markers':

Given a syntactic structure \((P_1, \ldots, P_s)\) we define the semantic representation SR of a sentence as \(SR = (P_1, PR, Top, F, \ldots)\), where PR is a conjunction of presuppositions, Top is an indication of the 'topic' of the sentence, and F is the indication of the focus of the sentence. We leave open the question of whether there are other elements of semantic representation that need to be accounted for (97, pp. 234–35).

He later shows a method of describing topic and focus with "global derivational constraints" (see below). The more recent contributions of Lakoff, such as his "natural logic" paper (95), use the framework of underlying structure plus presuppositions as the "semantic representation" of the sentence.

The question of presuppositions in linguistic structure seems to have developed out of a discussion of Chomsky's "selectional restrictions" in the standard theory. Sentences such as "My buxom neighbor is the father of two," argued McCawley (111), are not anomalous because of any syntactic selectional violations of subject and predicate noun co-occurrence, but rather because of a violation of the "presuppositions" attached to the lexical items buxom (female) and father (-female). Kuroda (88) disputed these claims with some well-chosen examples from French, as did Moravcsik using English (120). It is also the case that lexical items such as verbs which take complement clauses are associated with presuppositions about the complement. This view was put forth by Fillmore, for example, in his criticism (50) of the important Bendix monograph on empirical investigation of semantics (14). Kiparsky & Kiparsky, in their paper on "factive" verbs (86) that presuppose the truth of the complement clause, develop a formal description of presuppositions in relation to the syntactic properties of the structure containing such verbs. For example, It is significant that he has been found guilty contains a factive predicate (be significant) that presupposes the truth of the embedded clause; it participates in certain transformational operations, to the exclusion of others.
More generally, certain sentences as wholes seem to have presuppositions associated with them, as G. Lakoff pointed out in his paper on relative well-formedness (98). And further, there are interesting phenomena of ill-formedness resulting from contradiction of presuppositions in complex sentences. This has all pointed up the need for some treatment of the questions in achieving a semantic representation of an utterance. Lakoff simply assumes, as do Kiparsky & Kiparsky, that the presuppositions are expressed as trees in the same form as underlying structures of sentences. It is not clear, however, that the sentence, which may or may not be a “proposition,” in the logical sense, should have the same underlying form as a presupposition, which clearly must be a proposition if it is to have a truth value. Karttunen notes this for “factive” verbs (77); he proposes “meaning postulates” to get around this difficulty. The question is complicated by the relationship of presupposition to focus and topic in surface structure, where these may or may not be well-formed constituents, but rather disjoint sequences of constituents. How to resolve this naturally has not become clear.

If this report on the status of the entities postulated as components of meaning seems obscure, I think that it accurately reflects the status of the question. If presuppositions are part of the underlying semantic structure of the generative semanticists, Chomsky notes (35), then the underlying structures are certainly not phrase markers (single branching trees) in the sense of the standard theory. Some concrete proposals are in order. Furthermore, if we take into account various presuppositions of the situation of discourse, as I have urged above, the semantic representations of the generative semanticists are inadequate anyway. Presuppositions are slippery to investigate “in vitro” so to speak, as opposed to “in vivo.” I find it difficult to conceive of a theory of presuppositions which does not relate to ethnographic evidence, currently lacking in discussions of it.

This becomes more acute, I believe, in trying to account for the relationship between linguistic structure and “natural logic,” as G. Lakoff has publicized the term. A natural logic differs from the usual sort of predicate calculus of logicians in that (a) it incorporates devices such that “all the concepts expressible in natural language can be expressed unambiguously, that is, in which all nonsynonymous sentences (at least, all sentences with different truth conditions) have different logical forms”; and (b) it “is capable of accounting for all correct inferences made in natural language and which rules out incorrect ones” (95, p. 54). The question of inferences made in the course of the natural use of language is so vast, one wonders how in fact it can be distinguished empirically from all cultural knowledge. I do not think that such information is formulative within a theory of semantics qua logic.

But is classic logic enough to express the kinds of inferences abundant in any discourse situation? Bar-Hillel (11) seems to indicate that a good deal of semantic universals are nothing more than logical ones. Recently, he criticized (12) a number of contributions on the grounds that they were dealing not so much with semantics as with “pragmatics,” to use the standard term. But it is
clear that speakers are always violating the laws of logic, so to speak, in conversing. Geis & Zwicky discuss “invited inferences” (60) in conversation that make of sentences with the logical form $P \supset Q$, plus the “suggestion” that 
~$P \supset ~Q$, a bidirectional $P \iff Q$; similarly (X or Y) $\supset Z$ “suggests” (X and Y) $\supset Z$. Karttunen builds on this principle to give (74) an explanation of certain logical difficulties with counterfactual conditionals. Now it is clear that if a natural logic wishes to account for these kinds of conversational facts, then a rather interesting distinction between “correct” and “incorrect” inferences develops out of Lakoff’s conditions quoted above. Are we to relegate this kind of phenomenon to performance, since, as Bloomfield noted, a speaker, if questioned about the logical entailments of propositions, would presumably give the “primary” logical relationships? Maratsos (118) has pointed out another usage, with definite articles in English, where marking a noun [+ definite] seems to follow from “the speaker’s consideration of the reasonable range of possibilities for his listener,” rather than from definite reference or anaphora, as in The Red Sox will win if the pitcher is okay.

The philosopher Quine has for some time characterized these kinds of approaches to description of meaning as the “museum” theory, which posits meanings that exist in the mind of the user of the language: “the myth of the museum in which the exhibits are meanings and the words are labels” (140, p. 186). In several methodological papers, he has attacked this as a superfluous feature of linguistic theory. Most importantly, it seems to me, his insistence that “there is nothing in meaning that is not in behavior” (141, p. 12), while insufficient to account for meaning entirely, necessitates an approach to empirical semantics that asks first what in meaning is behavior. Quine’s argument for the sufficiency of behavioral data fails to account for the properties (in the use of speakers) of such items as unicorn, ambrosia, etc. He is forced, I believe, to resolve the issue by appeal to language ontogenesis, whereby a child learns the “clearer” terms—those with ostensive definition—and then constructs from these by the use of language the more remote or fuzzier terms. Does this in fact indicate that no two speakers may be expected to construct such terms in the same fashion? We would indeed then have extensionally equivalent grammars which differ in structure. Bever, in the paper cited above, makes an equivalent point in saying that “competence” and the acquisition by the child of “performance” cannot in principle be separated from each other. There is always an ontogenetic component present in the empirical investigation of competence: “our first task in the study of a particular structure implicit to adult language behavior is to ascertain its source rather than immediately assuming that it is grammatically relevant” (17, p. 280).

This brings up the more general question of variation in language, which Chomsky rules out of the theory of competence, but which Weinreich et al claim is essential to any language. As was mentioned above, variability is patterned, it is observed in every linguistic community, and it is correlative for certain features. It would be conceivable to say, therefore, that this variability
is the result of complex rules of performance which distort an otherwise homogeneous competence within a community. In fact, given the basis for grammaticality in Chomsky's statement quoted above, this follows necessarily. Two proposals of note have been made for extending the notion of competence to account for this patterned variation, one by DeCamp, the other by Labov; they would extend the formalism of generative grammar in different ways. These proposals seem to depend in some way on the prior establishment of a reasonable generative grammar of the scope of the standard theory in terms of which the data they account for is handled. We should probably call the notions they define types of "extended competence," which is distinct from performance in that it is not due to quasi-random factors excluded in Chomsky's quotation above.

DeCamp's innovation is to control for variation expressible as implicational relations among rules generating sentences. He suggests (38, 39) that the scalogram technique, which he developed for language independently of Guttman, captures the fact that stylistic levels and sociologically sensitive variation in general show certain features linked with others in a unidirectional fashion. In this way he presents the "post-Creole continuum" of styles in Jamaica, for example. In sampling speech, he notes, it is necessary to assume "that each sample is relatively homogeneous, or may be subdivided into homogeneous subsamples; this is a necessary but indeed questionable assumption, for a speaker's stylistic level keeps varying during an interview, no matter how hard the interviewer tries to keep the atmosphere of an interview constant" (39, p. 354). If the features of performance vary, then in actuality one would wind up with data on the statistics of occurrence of certain features, rather than a categorical yes or no answer on occurrence. This was brought up by Fasold (48) in his discussion of sociolinguistic models. It is not clear that a principled basis can be given for categorical interpretation of a sample, since there are different frequency ranges for different features co-occurring at a given stylistic level.

In any case, DeCamp proposes an extension of competence to account for the grammar of a community as a set of stylistic features on which all draw, but which are not all jointly present in the competence of any one speaker. (This harks back to the Whitneyan conception of a language, as well as to Mathesius' "potential" of a language community.) Just as phonologists propose marking certain lexical items in English as [+ Romance] to trigger certain phonological rules, so DeCamp postulates such abstract features in stylistic variation: "A feature may be opted in the base rule whose presence triggers all the necessary changes which the subsequent components of the grammar must make in any derivation marked with that feature . . . the potential variation is built into the rules themselves" (39, p. 353). But the phonological example shows lexical governance of rules, where some single items are exceptions, and this "arbitrary" information must be coded. DeCamp uses sentence-level arbitrary features in the base structures of the several stylistic alternants of a language to block or trigger rules elsewhere in the
grammar. This approach ignores the relationship found by DeCamp himself among stylistic levels, that there seem to be implicational regularities on the joint presence or absence of features. Neither the linking of the rules nor the distinct configuration of each style is captured by the proposal. Part of this is remedied in a more recent work (38) [see also Bickerton (19) for implicational relations]. Styles involve rules and elements from all parts of the grammar; what makes the collection more than arbitrary?

We might ask if functional alternants present configurations at the surface that are epiphenomena, in the sense described above, or present true cases of "conspiracies" of several grammatical systems to produce a distinct configuration. Clearly, a highly restrictive hypothesis, making use of DeCamp's actual results, would deal in the inclusion relationships on sets of rules that could be postulated for increasing stylistic elevation. This fits with Jakobson's suggestions about a "maximally explicit" grammar (code) and a sliding scale of contraction to the maximally reduced code. This hypothesis is easily subject to falsification of an interesting sort, since it makes the claim that varieties of a linguistic system are indeed "functional," i.e. have an internal cohesion as well as external criteria for switching. (This brings up the vast, unexplored area of the adaptive character of linguistic structure.)

The statistical difficulties, at least, of DeCamp's analysis are avoided in the approach of Labov, who incorporates a probabilistic index of actual community use into the concept of the "variable rule." Labov does not actually incorporate such an index into the rule itself, but associates with the rule a probability value computed on the basis of both the external influences (age, sex, etc) and the internal influences (linguistic environments in rules). The precise formalism changes from the paper by Weinreich et al (159) to an improved version in the paper on the English copula, but there are still serious difficulties with the representation. Since in the latter work (91) the features of a phonological rule (such as the presence of a word boundary) are distinguished for their effects on the application of the rule, there seems to be provision for a distinction between a rule that is always applied (that is, a "nonvariable" rule) and a variable rule that is always applied because a feature in the environment of the rule triggers it categorically. Furthermore, the formalism does not naturally provide for the case that a rule should not be applied normally, but the presence of a certain factors makes it more likely to be applied. I have in mind the moribund status of the ablaut rules in English, which, however, are frequently applied for certain phonological shapes (*bring → brang, brung*). I think that in addition to variability expressed by statistics, we need a notion of productivity built into competence. In this fashion, with a notion of the "marked" or "unmarked" character of the implementation of a certain linguistic rule, we can have a principled means of knowing what our frequency data indicate with respect to the affective values and hypercorrection phenomena associated with different functional variants.

What is the relation of his kind of extended competence to the competence of the standard theory? Labov notes that "the variable rules are rules of
production” (92, p. 59). This is a terminological issue, then. In other words, Labov’s work answers part of the question about the relationship between the standard theory’s competence and performance: there are regularities stateable by means of statistics of individual rule probabilities within a population of speakers.

But consider further that perception of speech is categorical in its association with “strong social values”; I would say that in those variables lies a principled basis for extending the notion of competence in its intended sense. The statistical regularities might very well be considered a combination of factors that distort the competence of the individual in some systematic way. But our perception of some of them—associated with affective value—on a categorical basis cannot be so analyzed. Our competence includes this categorical indexing of situations, variables of class and sex, etc, by features of language; were these not nonstatistical norms, there would be no embarrassment, no hypercorrection, etc. I do not think that Labov has made a strong case for the nonterminological extension of competence into the realm of statistics. As DeCamp says:

Sociolinguistic competence does not consist of meters and variable controls designed to maintain and to recognize a specified frequency. The speaker does not monitor his own frequencies. . . . Rather he faces a complex set of discrete decisions (to ain’t or not to ain’t), and a set of implicational consequences of those decisions (38, p. 34).

In this sense language competence, even extended, is not quantitative and still abstract, and we still have a methodological problem in its investigation. I want to leave this methodological question unresolved and turn to some of the specific debates within generative grammar as practiced.

Problems of Generative Grammar

The theory of transformational generative grammar must come to grips with a new set of mathematical results. If we consider intuitively a framework such as the Aspects model outlined above, with its transformational rules that add and delete (and hence permute) constituents of trees, it should be clear that an arbitrary tree can be transformed into any other, and hence any deep structure will serve as input. Given no restrictions on the operation of rules, this reduces the theory to an untestable vacuity, since we could always construct some such sequence of rules to accomplish the task. Recently, we have from Peters & Ritchie (130, 131) results that seem to show that this is the case. Using the concept of a recursively enumerable language, one that is generated by a rewrite schema of the form A → B (A,B arbitrary strings), they showed that several versions of transformational grammar incorporating features of the standard theory generate such languages. (This is in terms of generating strings, not providing structural descriptions, however.) They find also that “restricting the base component, even very stringently, does not restrict the class of languages generated by transformational grammar” (131, p. 375).
486). Given that natural languages are recursively enumerable, then the hypothesis of a universal base, that all natural languages share the same underlying context-free phrase-structure (base), is not subject to testing with an unconstrained transformational approach.

These results indicate that restrictions on the form and applicability of the transformational rules themselves are necessary (see also Partee 125, Wall 154). In fact, this is the position taken in a number of recent papers. Chomsky argues again and again in his “Empirical issues . . .” (35) that any proposals for devices more powerful than transformations will make the situation worse. Such devices as “global rules” and “transderivational constraints” (see below) are much more powerful than transformations; it would seem to follow that they must be more highly constrained. The proponents of such machinery do not have precise enough results to accomplish this so far, though several proposals have been made.

One might put the question differently and ask what are the ways in which surface structures may differ from deep structures. Transformational grammar has attributed the “boundless variety” of languages that so impressed William Dwight Whitney to the operation of transformational rules. But it is clear that surface structures are themselves far from boundless in number, but present rather interesting recurrent types. Transformational theory certainly has not emphasized this. As Bach recently remarked:

current linguistic theory faces two distinct problems: an inability to capture or explore similarities among languages in both the deep and surface levels. But the two problems seem to point in the same direction: we need to devote more attention to hypotheses that limit the power of transformations in a substantive way (6, p. 154).

Bach then formulates hypotheses about universals of question sentences. Similarly, Ross, in his study of “gapping” (147), the deletion of a repeated lexical verb (e.g. English I drank wine and you chocolate malts), proposes a method of inference about underlying constituent order based on assumptions about gapping rules. Maling (116) has replied with a more restrictive hypothesis about the rules, which, however, yields a weaker inference about deep structure. McCawley argues from conditions on transformations that English is underlying a “VSO language” (112) (verb-subject-object, in that order). Bresnan’s (26) discoveries about stress cast some doubt on this hypothesis, however.

I think it follows from not dealing seriously with surface pattern that in the search for underlying structures there is little concern for shallowness. By this I mean minimal proliferation of rules and rule-complexity (Chomsky’s remark quoted above about phonology says just the opposite). It is thus very rare to find statements like Dougherty’s: “It seems natural to assume that deep structure looks like surface structure until proven otherwise” (43, p. 874). Given the results about the power of transformations, whereby we can make languages look alike in underlying form, it should always be possible to
provide an analysis in universalistic terms. This is an avowed aim of the method [see Postal (134) for an example of forcing Mohawk and English data into a particular form].

From a somewhat older point of view, any hypothesis about the relatedness of two alternants entails an hypothesis about the level of structure at which they are "the same." Thus [wayf-] and [wayv-] are alternants under an hypothesis of sameness of morphological form. In syntax, statements about paraphrase equivalence of surface structures seem insufficient to characterize what generative grammarians are describing about sentence structure. I see no way of justifying the search for underlying structures except in making explicit the dependence of all syntactic argument on a theory of meaning structure.

In this we cannot but agree with the generative semanticists, who insist that their results show that logical forms are approximations to an adequate notion of underlying unity of sentences. We saw above that there are claims that natural logic provides the basis. I have argued that in addition we need an explicit accountability for variables of the speech situation. Certain surface structures can be said to share deep structure representation by a paraphrase criterion, say, if and only if the variables of speaker and hearer (age, status, sex, etc) are controlled separately. This implies a separation of honorific vs nonhonorific alternants as different in kind from the cognitive paraphrase relations.

The current debate has shaped up around three sets of issues: first, the nature of lexical representation, including the decomposition of the lexical items of the standard theory; second, the method for handling anaphora, focus, scope of quantifiers and modal elements; third, the existence of and necessity for global rules, transderivational constraints, and other expansions of the standard theory with more power.

I discussed part of the lexical controversy above. In the standard theory, deep structures with lexical items are input to all grammatical transformations; lexical items are simplex (no internal syntactic structure). (Curiously, the problem of "derivation," in older terms, has never been explored systematically for languages where this is a major feature.) It was at first concluded from this that the meanings of lexical items could be given as con- and disjunctions of "atomic" predicates, with some conditions of co-occurrence also specified. However, Weinreich, among others, came to the conclusion that "every relation that may hold between components of a sentence also occurs among the components of a meaning of a dictionary entry. This is as much as to say that the semantic part of a dictionary entry is a sentence—more specifically, a deep structure sentence" (157, p. 446; see also Spa 152).

Meanings themselves show an internal syntax. Many interesting proposals along these lines have been forthcoming. Bierwisch criticized Weinreich's formalism (20), but gives an alternative typology (21). Dixon (42) takes advantage of a Dyrribal (Australia) taboo lexicon to isolate one-to-many semantic mappings between lexical items of this and the everyday subcode.
This method yields two sets of lexical primes, those with minimal componential definitions ("nuclear" items) and those with internal syntax ("non-nuclear") in terms of the nuclear lexicon and the grammatical pattern of the language (identical in both subcodes).

Of course, in languages which lack a behaviorally controllable semantic mapping, other tests must be sought. For English, Bendix (15) uses behavioral tests in terms of the effects on acceptability of conjunction and disjunction (e.g. The girl giggled but did not laugh.) This method of achieving minimal componential definitions, atomic or not, has been criticized by Fillmore (50), who asks instead for a concentration on the presuppositions of the constituents occurring with a given lexical item (51, 53). He takes his inspiration from the Oxford philosophers in asking for felicity conditions on the use of items. Yet in describing "verbs of judging," he too must give a 'meaning' in terms of a calculus of more elementary propositions: 'blame' is roughly THINK (judge, 'X'), where X = RESPONSIBLE (defendant, situation); judge, defendant, situation have the obvious referents on the analogy of legal concepts (53). All verbs of judging that he discusses turn out to be defined in terms of THINK and SAY (except forgive, which he queries).

The interesting result here is that a class of verbs shares properties in terms of both syntax and semantics. Zwicky (161) shows the correlation of semantic class of shout, scream, etc with their syntactic properties, and likewise Karttunen (76, 77), following upon Kiparsky & Kiparsky (86), discusses the syntactic properties that are common to factive and implicative verbs defined by common presuppositional and entailment structures of clauses they govern; there are a great many exceptions to be reckoned with, however. If in some sense features of meaning of these items unifies them as a syntactic class, then there is some support for not distinguishing the semantic features of the lexicon from features that trigger transformational rules, i.e. that determine surface syntactic properties.

In this fashion, there is support for more far-reaching revision of lexical structure, along the lines of Postal's (136) derivation of Y reminds X of Z from [X perceive [Y similar to Z]], referred to above. Bolinger (24) has criticized this as "overloading" the analysis with material that is inferential for the native speaker; but where do we get a handle on the distinction of meaning and inference that is valid for the whole speech community? G. Lakoff gives several examples of this decompositional technique in his paper on natural logic (95). Chomsky rejects these decompositional attempts (35) on the grounds that the syntactic consequences are unacceptable, and that the transformations employed to conflate the separate parts of the underlying form to a single complex constituent are ad hoc. But an avowed purpose of the work is to use as natural-looking transformations as possible. Thus, McCawley rules out predicates of natural languages of the form *flimp = "[kiss v_{RP} a girl s[who is allergic to _]]]" as in *Bert flimped coconuts. This is in accordance with the syntactically motivated constraint on constituents with nouns and relative clauses, ruling out such extraposition. But do we not have cuckold in English, "copulate with a woman who is married to," of exactly parallel structure?
This approach by syntactic motivation for the decomposition of lexical items in underlying form has not, in general, concerned anthropologists and lexicographers who eschew considerations of a syntactic nature in arriving at taxonomies, folk or otherwise. Lehrer (108) notes that there are surface taxonomic gaps, and suggests that some are accidental, some systematic; this must be defined at least in part on the basis of possible lexical items, given a linguistic theory. Kay’s (80) elegant treatment of taxonomy, on the other hand, shows little awareness of the syntactic problem.

The second area of controversy might be termed in general “deixis” since the items in question point to objects either of language or of the external world. Pronominalization is a case in point. First and second person pronouns are referential items, not anaphoric; they do not indicate that there must be another coreferential noun phrase in the relevant utterance segment. Third person forms are always anaphoric and sometimes referential (see Benveniste 16, pp. 225–36, 251–66). Postal wishes in essence to reduce this to a uniform syntactic schema (135), which turns out to be unwise on purely syntactic grounds (Delorme & Dougherty 40); the same may be said of Ingram’s terminological treatment of pronouns (67). There are a number of different kinds of third person pronouns, involving a great number of syntactic difficulties for transformational accounts of anaphora: missing antecedents (Grinder & Postal 61); referential identity paradoxes (Karttunen 75, Kuroda 90); “sloppy identity” of coreference (Keenan 81); global constraints (Postal 138); constraints on transformations moving anaphoric elements (Postal 137).

These studies, dealing with English, have attempted transformational separation of the several types of underlying anaphora that are frequently overtly differentiated in other languages: third person pronouns, definite and indefinite articles, cross-reference, deletion (to what extent does this merge with “gapping”?). Pronominalization has been a rich source for arguments about underlying identity of constituents. For example, G. Lakoff (96) argues that sentences of the form Goldwater won in the west, but it wouldn’t have happened in the east have anaphoric it indexing a sentence constituent of the form [G. won], from which it would follow that the adverb in the west originates in a “higher sentence.” However, we might contrast a sentence with identical first clause, . . . but it wouldn’t have happened with Scranton, to see that the anaphoric it can index any sequence potentially cast into nominalized form. (This would have to be formulated as a transderivational constraint, for which see below.)

The use of definite and indefinite articles is related to pronominalization. Bellert (13) discusses logical form of definite referential noun phrases, while Perlmutter (128) wants to relate English a to underlying [one]. Generics are a difficulty here, as Kuno’s study (87) of nonreferential noun phrases in English shows. Kuno in fact shows several interactions of articles and anaphoric pronouns that require discourse motivation, that is, they operate across sentence boundaries.

This brings up the larger question of deixis as an “illocutionary act” in
the Austinian parlance that Fillmore (52) adopts. Ross (148) analyzes all sentences (S) as embedded in a “performative” clause of the form I SAY (ORDER, REQUEST) S to you, and derives many of the properties of reflexives of first and second persons from this underlying form. Such “abstract verbs” provide an interesting approach by generative semantics to such deictic elements as point out beliefs or presuppositions of speech-situation participants. For example, McCawley (115) and R. Lakoff (101) discuss this for “tense”; Heidolph (62) for negation; R. Lakoff (102) for conjunction; Keenan (82) for quantifiers. It is not surprising that the interaction of these last three categories has provided some vexing problems for grammatical theory.

There have been alternatives offered to these abstract analyses of negation, quantifiers, and conjunction by defenders of a more directly syntactic approach (necessitating an extended “interpretivist” semantics), Dougherty (43, 45), Jackendoff (69), Partee (124). These require considerable categorical and lexical enrichment of the base component of transformational grammars in order to obviate the need for more abstract constituency. The last abandons the requirement that “transformations preserve meaning,” which must lead to the conclusions about referential scope of negation and quantifiers that Jackendoff (68) articulated, as we noted above.

This tradition of work, epitomized by Chomsky as the “extended standard theory,” deals with many phenomena as syntactic, rather than abstractly semantic or pragmatic in underlying form. Baker’s abstract Q(question)-marker (8) contrasts with a performative analysis of Ross in terms of I ASK S of you. Bresnan (25) distinguishes several complement types by syntactic markings rather than, for example, the semantically motivated ones of Kiparsky & Kiparsky (86). Langendoen sees transformations as obscuring or preserving essentially syntactically motivated deep structures (104) in concerns really bordering on the theory of “performance.” Indeed, he and Bever (18) employ perceptual strategies as the explanation for certain systematic changes in relative clause formation in English. Jackendoff & Culicover (71) patch up a purely syntactic explanation of dative movements (John gave a book to Mary → John gave Mary a book) with a theory of “perceptual strategy” in performance that sets up expectations of surface constituent order. This comes close to begging the methodological point of separating “competence” from “performance,” for judgments of grammaticality are no longer to be accounted for exclusively in terms of syntactic competence.

In the third area, that of powerful new rule mechanisms, we have had proposals from Perlmutter (127, 129) for template-like constraints on both deep and surface structure, which eliminate certain formations that are produced by rule. These add to the ‘filtering’ function of grammars and permit simplification of the statement of transformational rules themselves. Recall that in the standard theory, transformations were the filtering device for defining the notion “underlying structure” of a sentence. G. Lakoff generalizes from this filtering function of transformations by first observing that transformations
“are well-formedness conditions of successive phrase-markers... in that they
‘filter out’ derivations containing successive phrase-marker pairs \((P_{i}, P_{i+1})\)
which do not meet some well-formedness condition on such pairs” (97, p.
233). He proposes in addition to these “local derivational constraints” a class
of “global derivational constraints,” or, as they have been called elsewhere,
global rules, in that they specify well-formedness conditions on phrase-markers
that are not successive in a derivation.

Thus Ross (149), in explaining why there are no sentences of the form
*It is continuing raining, finds himself forced to postulate constraints on deri-
vation relating “remote structure” (= approximately the deep structure of
the standard theory) to surface structure. A surface configuration

\[ s[X_{\text{V}_{a}}[V\text{ ing}]
N\text{P}\{s[V_{b}[/V\text{ ing}) Y]] Z \] where NP is the complement of \( V_{a} \) is ungrammatical if in
remote structure \( V_{a} \) and \( V_{b} \) were dominated by nodes of certain types. Note
the negative statement, whereby certain derivations produced by transforma-
tions are thrown out as ill formed. Postal has tentatively placed certain con-
straints on global rules, generalizing from the few good examples (138, pp.
35–36), rightly observing that “it is necessary in a serious account to propose
the narrowest possible constraints on the class of rules to be allowed, if empiri-
cal content is to be retained.” To this end, he tentatively says that global con-
straints should mention no more than three or four stages of derivation, where
the third is “semantic representation,” they should refer only to two constitu-
ents of these structures, and should be stated in terms of the same constituency
properties as transformational rules. The question here is the degree of “nat-
uralness” achieved in underlying representations and transformational rules by
the use of such powerful devices. Indeed, after G. Lakoff presented these
global devices informally (94), Baker & Brame countered (9) with numerous
modifications of the standard theory to account for the data, which Lakoff
then (100) denounced as “arbitrary” and ad hoc.

If transformations innovate over phrase structure rules in referring to con-
stituency (that A and B are dominated by or are constituents of C figures in
the structural description of a rule), global rules innovate over transforma-
tions by permitting reference to derivational history (A and B were domi-
nated by C more remotely). Yet a further device, transderivational con-
straints, has been proposed, which permits reference to possible constituency
in some other derivation (A and B could be dominated by C in another deri-
vation from essentially the same underlying structure). Ross, in his -ing, -ing
paper, formulates an exception to the global constraint (149, p. 77). Part of
the validity of this new device hinges on the notion “essentially the same,”
and therein lies much research and much restriction on the vast power avail-
able. We could too easily wind up with a problem of hopelessly regressive
grammatical constraints.
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LINGUISTIC MODELS IN ANTHROPOLOGY

Mridula Adenwala Durbin

Department of Anthropology, Washington University, St. Louis, Missouri

INTRODUCTION

The basis of analogy between disciplines leads to epistemological and other philosophical discussions. Likewise, the basis of analogy between anthropology and linguistics directs discussion into the similarity of their subject matter, goals, and problems of analysis. Anthropological literature abounds with assertions of an overwhelming congruency (substantive, functional, historical, ontogenetic) between language and culture, but because most statements are inexplicitly formulated, it is difficult to assess the common problems and properties of these two fields.

When discussing a problem of this nature, one could attempt to deal with several topics: (a) the basis of analogy between two disciplines in general and between linguistics and anthropology in particular; (b) the similarity between the goals and the subject matter of both anthropology and linguistics; (c) the nature of the linguistic model and its core concepts; (d) the comparison of various concepts used in both fields in order to expose differences that have developed by the very process of analogy; (e) the extent to which various concepts of linguistics are applied to cultural data, and the discussion of why certain concepts are not applied; (f) the various degrees of success in the use of these models; and finally (g) the overall impact of the use of the linguistic model in anthropology.

Because any attempt to incorporate discussions of all these topics would require a much larger work than this, I am forced to exclude discussion of the factors that have led to an analogy in the two fields. Instead, I aim to assess how effectively the linguistic model has been applied to anthropology. It ought to be pointed out first that because several formal models are available in linguistics one of the prominent dangers in analogy is the confusion of concepts originally restricted to different models. Anthropology has not been spared such mistaken uses of some linguistic concepts, e.g. contrast vs complementation, component vs distinctive feature. Furthermore, in the course of the discussion, it will also be pointed out that the application of the linguistic model to anthropology has only been partly successful (when compared to linguistics) because several aspects of the model were not utilized.
LINGUISTIC MODELS

The following models have been used in cultural anthropology.

1. Structural linguistics: (a) Emic-etic model  
   (b) Componential model
2. Transformational-generative model
3. Distinctive feature model of phonology
4. Comparative-historical model

STRUCTURAL LINGUISTICS: EMIC-ETIC MODEL

The concept of *etic* vs *emic*, first developed in the context of phonology (phonetics, phonemics) and subsequently extended to morphology (morphemics) and syntax (tagmemics) was later used by Pike (101) as a model of analysis for nonverbal human behavior. Pike's proposal was to develop two conceptually separate strategies, analogous to phonetics and phonemics in linguistics, for analyzing other cultural behavior. Thus, the *emic-etic* model used in anthropology is developed from structural linguistics. Proponents of an *emic-etic* model in anthropology applied linguistic concepts to large sectors of human behavior involving the intersection of various cultural subsystems as evidenced, for example, in the *The Nature of Cultural Things* (67) and *The Silent Language* (59). These attempts were programmatic (8, 60, 111) and rarely dealt with cultural analysis per se. The nonproductive results of these attempts can be attributed to two factors. On one hand, cultural anthropologists heavily underestimated the intricacies of the linguistic model, while on the other hand, linguists underestimated the complex nature of the cultural data. In addition, even those parts of the model used were not properly understood by some anthropologists, as manifested in Harris' *The Rise of Anthropological Theory* (68, pp. 568–604). Harris confounded the nature of the model by interpreting the *emic-etic* approach solely as classificatory vs non-classificatory, and by ignoring other characteristics. A close look at phonetics and phonemics reveals that it is not correct to consider phonetics solely as a non-classificatory frame since sounds (phones-*etics*) are classified according to their natural (articulatory, acoustic) inherent properties. Furthermore, in phonemics, even though sounds are classified as structurally same (i.e. same phoneme) vs structurally different (i.e. different phonemes) on the basis of their role in distinguishing meanings of utterances (i.e. discriminatory functions), the significant feature of the phonemic model is that the functional criterion of classification is operationally shifted to distributional criteria. This is a crucial shift in phonological methodology in particular and in the linguistic model in general. The distributional criteria of contrast and complementation used in linguistics exploits interconnections between sounds of a language as follows:

1. Two sounds occurring in identical contexts in two different words (i.e. with different meanings) as *p* and *b* in *pit* and *bit* are in contrast or contrastive distribution.

2. Two sounds occurring in exclusively different contexts, as [*p*] and [*pʰ*] in Table 1, are in complementary distribution.
In phonemic analysis, different sounds in contrast are analyzed as different phonemes, and different sounds in complementary distribution are candidates to be grouped as predictable variants (i.e. allophones) of a phoneme provided they are substantively similar (i.e. phonetically similar). As noted above, phonetic categories are developed with no reference to the linguistic system they constitute and are projected onto the data by a researcher prior to an analysis of the system as a mere convenient and consistent device for segmenting a complex verbal phenomenon (i.e. utterance), while phonemic classification is a product of the intersection of substantive and functional (shifted to distributional) dimensions of sounds. Complementary distribution is a hypothetical statement of a lack of contrast in the absence of contradictory data. Thus, in Table 1 it is a hypothesis that initial \$- and s- are necessary conditions for \$[pb]$ and \$[p]$ respectively. Consequently the nondistinctive difference between \$[pb]$ and \$[p]$ can be predicted by the knowledge of these contexts. Therefore, phonemic analysis can be considered as based on contrast or lack of contrast.

Harris' (68) propositions that (a) kinship systems are mixed domains of etic and emic (pp. 578–79), and (b) emic cultural categories cannot be transmuted into etic simply because they are found emic cross-culturally (pp. 577–78), are anomalous in the model described above. It must be noted that the elements of kinship (or of any cultural system) viewed from their inherent properties form etic categories, and when classified from the standpoint of contrastive relationships, form emic categories. Furthermore, emic categories are based on the configurations of relationships between an element (e.g. sound) and the rest of the elements of the system, and since no two systems share exactly identical configurations, it is meaningless to consider any element cross-culturally emic. Even if two systems are identical, the emic status of a category in both systems does not prevent its conception as an etic element when viewed as a preanalyzed element.

In brief, in the first attempts to apply the structural linguistic model to cultural analysis, the data—human behavior—consisted of a mosaic of cultural and psychological parameters. The goal was to discover the structure of culture in terms of its significant categories, but this encompassed too vast a territory and culminated in diffuse and vague analyses. Not only was the nature of the linguistic model inadequately understood but certain features were ignored. A complex set of criteria for determining phonemes (pattern congruity, morpheme boundary), and the tactics of phonemes (phonotactics, morphophonemics) were not brought into proper relevance. These criteria and tactics were formulated in the phonological model to bring out different types of relationships between sounds. When
some parts of the formal aspects of the linguistic model were left out in the application of the model to cultural data, poor results were bound to appear.

**Structural Linguistics: Componential Model**

*Semantic analysis.*—The next endeavor to use the structural model of linguistics in cultural anthropology is associated with a major shift in the goal of anthropology, namely, an emphasis on a mentalistic approach and a repudiation of an empirical, behavioral, and materialistic approach (54, 91). With this new stance (variously called cognitive anthropology, componential analysis, ethno-science, ethnosemantics, formal semantic analysis, the new ethnography), the conceptual frame of culture becomes the object of study in anthropology. Therefore, the semantics of natural languages gains paramount importance. In this new light, semantics is viewed as a bridge between linguistics and anthropology (50) since cultural categories are represented in linguistic terms. Lounsbury views the classification of natural phenomena as being determined by linguistic terms rather than by some objective order inherent in the natural phenomena, since linguistic terms become a major medium by which human learning of the natural environment takes place (91, p. 190). This is evidenced in ontogenetic relations between the learning of culture and a system of meanings (54, p. 39). Semantics, a new focus of cultural analysis, can readily avail itself of linguistic theory and method as noted by Goodenough (54). The same rationale leads Lounsbury (91) to analyze problems of semantics by techniques analogous to those already developed in linguistics. The extension of structural linguistic methods and concepts to cultural semantic analysis was also motivated by the desire to raise standards of rigor in ethnographic descriptions since linguistic procedures allow ready replication of others’ data and analysis (56). Thus, in the second attempt at using the linguistic model, the goal is well specified: to discern semantic structures represented by the linguistic terms of a given semantic domain. Contrary to the *emic* approach, the data of semantic analysis consists of the linguistic terms of a restricted semantic domain. Furthermore, the concept of contrast, crucial to structural linguistics, has been better understood in this second approach.

In semantic analysis, the semantic differences between terms are attributed to different semantic features. The search for principles underlying semantic structures led to two separate studies: *(a)* principles that link different terms within the same semantic domain (5, 6, 21, 23–25, 31, 32, 39–41, 82, 84, 112); and *(b)* principles that link different denotata of a single term (51, 53, 55, 91–94).

Thus, the two kinds of studies deal with different dimensions of folk classification. In folk classification, Frake (39) defines *segregates* as terminologically distinguished arrays of objects (p. 31). He separates conceptually *segregates* from *morphemes* (in linguistics) on one hand, and from *semantic category* on the other hand. *Blackbird,* for example, has two morphemes but represents one segregate, while *man* is one morpheme but represents two semantic categories [homo sapiens], [male homosapiens] and two segregates. Moreover, some possible combinations of semantic features may not be found coded in distinct linguistic
Linguistic Models in Anthropology

terms and hence do not have a corresponding segregate. These are called covert categories (6) or zero lexemes (51). A covert category is comparable to a zero allomorph of a morpheme in linguistics as it is postulated on the basis of its contrastive functions in a larger context. For example, the zero allomorph of a plural morpheme in English contrasts with the singular in the paradigm (the sheep is: the sheep-∅ are). Similarly, covert categories in plant classification contrast with the named categories of plants in regard to some external context such as importance of plant as food, firewood, etc (6).

A concern with hierarchical relations among terms in folk taxonomies has a focus on segregates that are characteristically organized by simultaneous arrangements of semantic features. A sequential relation among features within a segregate is absent. The display of features in the form of a tree represents sequential relations among features, and a paradigm (82) represents simultaneous relations among different features. The nature of tree, paradigm, and taxonomy is discussed at length by Durbin (31), Kay (82), and Tyler (112). Recently, Durbin (32) has proposed that taxonomy need not be considered as a formal model for semantic analysis since it represents an incomplete analysis of terms in which the underlying structure is arranged either in the form of a tree or paradigm. Kay (84) also suggests that levels of contrast is an ambiguous concept in many respects and hence ought to be replaced by the notions of five types of contrast: (a) direct contrast, (b) indirect contrast, (c) contrast by inclusion, (d) terminal contrast, (e) generic contrast. The first three notions are comparable, respectively, to phonemic contrast, contrast between allophones of different phonemes, and the relation between phoneme and morphophoneme. The last two terms do not have any correlation in the linguistic model. The term contrast is defined distributionally as in linguistics: "Two terms which can occur in the same cultural contexts as distinctive alternatives signifying different cultural phenomena are said to be in contrast semantically" (39, p. 33). In order to elicit proper contrasting terms in the same semantic domain, Frake and others propose to search for appropriate eliciting frames (9, 10, 38, 39, 41, 96). However, the notion of eliciting frame points to a more serious theoretical question in regard to semantic analysis. Some semantic features of the frame and some of the contrast-set are in an interdependent relationship with each other. In that case, can a semantic analysis of terms be considered sufficient if it does not incorporate the information about this interdependence? Just as an eliciting frame represents one kind of context for a term, there is also another kind of relevant context, namely, the semantic features forming the sequential context of a term. The semantic features of a segregate then are the products of the interaction between a segregate and its various types of contexts. The problem raised here is comparable to some of the problems encountered in linguistics, such as the role of grammar in phonemic analysis (98, 99); co-occurrence restrictions in phonology and syntax (70, 71, 73–75); discontinuous constituents (74); discourse analysis (72), and the role of semantics (44) in transformational grammar (2). This suggests that the tactics of semantics will require a model that accounts for the interdependence of terms and frames, and for sequential arrangements of semantic features.
Componential approach in anthropology.—Anthropologists seeking principles underlying the semantic structure of a set of segregates postulate contrasting values of a semantic dimension as attributes of the segregates. The values of a semantic dimension are designated as components. It ought to be noted that analysts such as Frake, Conklin, and others have assumed that components have binary contrast (i.e. the features are present or absent), while others such as Goodenough and Lounsbury have characterized the component as opposed values (e.g. male vs female) of a semantic dimension (i.e. sex). The decomposition of a segregate into a bundle of such components is called componential analysis, and it is the most widespread linguistic model used among anthropologists. Though kinship is the most exercised semantic domain (16, 17, 25, 33, 47, 51, 53, 55, 56, 58, 63, 91, 92, 105, 106, 113, 116), this method is also applied to other semantic domains such as trait psychology (28), disease and medicine (38, 95), religion (40), law (102, 103), verbs (3), numeral classifiers (7), color (22), firewood (97), weddings (96), food and eating (4, 46), archaeology (29), plants (5, 6, 104).

The explicit analogy is based on the componential model of phonology (51, 57, 91). However, the analysts delimit the data by selecting a specific semantic domain. This is comparable to morphemic analysis wherein the data are delimited by paradigmatic inclusion. The following discussion of componential analysis is divided into two parts: data and methodologies of analysis.

Data: In semantic analysis, all terms comprising the data are assumed to be in contrast with each other as phonemes contrast with each other in a language. Furthermore, different denotata of a term are assumed to be noncontrastive in the same way as allophones of a phoneme are. I hasten to point out that the data in semantic analysis (i.e. terms) are viewed as roughly equivalent to the products of the phonological analysis (i.e. phonemes). The contrast between different terms, and the noncontrastive relation between different denotata of a term are granted ipso facto. This is unlike the contrast between phonemes and the non-contrastive relation between allophones which are established operationally on the basis of distribution. Therefore, the relation between a term and its denotata on one hand, and that of a phoneme and its allophones on the other hand, are neither analytically nor epistemologically comparable. Substantively, they manifest superficial similarity in that a term represents a bundle of features (semantic) just as a phoneme represents a bundle of features (phonetic).

Methodology: The components of terms in a semantic analysis are derived by contrasting one term with the rest of the terms and by postulating a set of minimal differential semantic features (51, pp. 201–5; 92, pp. 196–202) as follows.


\[ 'father' \quad ha'nih \quad \sigma L^-,G^1.K \]

\[ 'mother' \quad no'yeh \quad \sigma L^-,G^1.K \quad (adapted \ from \ 92, \ p. \ 198) \]

Male and female components are derived by contrasting these two terms. This procedure is quite different from the methodology of componential analysis as developed by Z. Harris in linguistics (69–71). His concept of component is based on the co-occurrence restrictions of phonemes. The fact that there is less contrast
of phonemes in one position than in another (e.g. in English, /p b t d k g/ contrast after #- but only /p t k/ contrast after s-) is stated by phonotactic statements in the phonemic model. However, the restrictions on the number of contrasts in different contexts is stated in the componental model by redefining phonemes in such a way as to imply contrasts of phonemes only in those contexts where they are found. Thus, a phonological feature shared by phonemes in an ordered relation is abstracted as a component. For example, in English /sp/ and /zb/ can occur but */sb/ and */zp/ cannot, therefore, the voiceless feature of sp and the voiced feature of zb are components. Both being in binary opposition, the presence or absence of one of them (e.g. voiceless feature) can represent both sequences as in /zb/ = /sp/, and /zb/. Since components in the linguistic model are designed to capture distributional restrictions on phonemes, they replace not only phonemes but also the limitations on phonemic distribution. The procedure used in semantic analysis is comparable to the distinctive feature model of linguistics developed by Jakobson and others (81). For further discussion of the distinctive feature model, see below.

Componental analysis in anthropology thus differs from componental analysis in linguistics. In anthropological analysis the domain of components is the same for all components, but in linguistics, components have different domains in different contexts. Secondly, the semantic value of a component is the same in all contexts unlike the different phonetic values of a component in different contexts. Moreover, in linguistics, two or more components may be grouped into one phonemic component on the basis of complementary distribution. For example, if glottalization is found in a language only with stop consonants, e.g. /p' t' k'/, and voicing only with fricatives, e.g. /v z y/, the components glottalization and voicing are in complementary distribution with each other and therefore are analyzed as allophones of one phoneme component, say /+-/. Note that in the phonemic representations /p+ t+ k+ f+ s+ x+/ the phonetic value of the component /+-/ is predictable on the basis of the consonants in context: stop+ = stop'; and fricative+ = fricativevoicing. But the concept of complementary distribution is either completely absent in most of the semantic analysis or is grossly misunderstood except in Lounsbury’s, Berlin’s, and Romney’s analyses. Goodenough’s (41) description of complementary distribution is comparable to contrastive distribution in linguistics when he states that English phonemes p, t, and k form a complementary set with respect to the place of articulation. This use of the term “complementary” is that of colloquial usage rather than the technical usage developed in linguistics. In the frame of linguistics these phonemes form a contrastive set (not a complementary set). This misinterpretation is one of the key points for a less elegant application of the componental model in many analyses. Goodenough’s definition “Any other expression whose denotata suggest that it complements the first in some way, must by virtue of complementation, relate to another partition of the same universe of which the first is also a partition” (51, p. 198; italics added) is vague from a general standpoint and misleading from a linguistic standpoint. The definition of Wallace & Atkins (117) on this notion is also incorrect from the standpoint of the linguistic model, since their
definition of complementation describes the concept of contrast: “A term may be said to complement one or more other terms if it signifies some value which the other terms definitely deny in favor of another value. For instance, English mother complements father with respect to the two values, male and female” (p. 356). Male and female semantic features are found in the identical contexts of other features in father (lineal.ascending generation.once removed.male) and mother (lineal.descending generation.once removed.female); therefore, they contrast with each other according to the componential model of linguistic analysis.

Due to such mistaken interpretations of the concept of complementary distribution, Goodenough groups two values—male and female—into one dimension (i.e. sex), but his analysis must still retain both types of semantic features (values and dimension) as significant units in the analysis unlike the linguistic units in complementary distribution (e.g. allophones, allomorphs) which are submerged into phonemes and morphemes. Thus, relationally, dimensions and values in cultural analysis are not comparable to phonemes and allophones or morphemes and allomorphs, respectively, in linguistic analysis.

The true essence of the principle of complementary distribution as a technique of reducing redundancy in the system is brought out by Lounsbury (92) in his analysis of Seneca kinship terms when he groups three semantic features—sex of the designated kin the same as that of the first link (L"), sex of the last link same as that of propositus (γ"), and sex of the last link same as that of the first link (\(\wedge\"\))—as conditioned variants of one semantic features called parallel on the basis of complementary distribution (cf Table 2) since these features occur in mutually exclusive contexts. Berlin & Romney (7) group numeral classifiers into one sememe as they occur with different nouns. Berlin (4) uses this notion in classifying different verbs of eating by using the different categories of food that occur in their contexts.

From the discussion given above, it follows that semantic analysis based on the criterion of contrast only is an incomplete analysis since the criterion of contrast does not completely eliminate redundancy in order to arrive at the minimal distinctive features necessary to define the components of the system.

A characteristic feature of the componential model used in semantic analysis then is its monolithic adherence to the concept of contrast and to a single level analysis.¹ Though the criterion of contrast and complementation are central in exploring the organization of sounds in a language, they do not provide a conclusive analysis in all cases as regards the phonemic status of different sounds in the structure. In such problematic cases, the pattern of organization of sounds other than the ones under examination (i.e. pattern congruity) and the context of higher order units (i.e. morphemes) are used as relevant criteria to bear upon the solutions as follows: Pattern congruity:² If a sound [\(\sigma\)] is in complementary dis-

¹ This is not to be confused with the term “levels of contrast” or “dimensions of contrast” which refer to the levels of relations between terms and to the levels of analytical concepts.
² In the T-model of phonology, this problem would be analyzed quite differently.
### TABLE 2. Complementary Distribution (Semantic Analysis)

<table>
<thead>
<tr>
<th>Semantic feature</th>
<th>Occurs with G⁻¹</th>
<th>Occurs with G⁻¹</th>
<th>Occurs with G⁻¹</th>
<th>Examples</th>
</tr>
</thead>
<tbody>
<tr>
<td>L=</td>
<td>yes</td>
<td>no</td>
<td>no</td>
<td>σ'L = G⁺K ‘father’</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>φ L = G⁺K ‘mother’</td>
</tr>
<tr>
<td>λ=</td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>σ'λ = G⁻¹K ‘son’</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>φ λ = G⁻¹K ‘daughter’</td>
</tr>
<tr>
<td>Λ=</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>σ'Λ = G⁰K ‘brother’</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>φ Λ = G⁰K ‘sister’</td>
</tr>
</tbody>
</table>

Distribution with two phonemes /t/ and /s/, then [θ] can be analyzed as an allophone of either /t/ or /s/ by the criterion of complementary distribution. However, if the language under analysis already has [p, f] as allophones of one phoneme and [k, x] as allophones of one phoneme, then [t, θ] are analyzed as allophones since all the three pairs would manifest grouping of stop and fricative consonants as allophones. Morpheme boundary: If [p] and [f] are found in complementation (initially #p-, medially -p-, and finally -f) in all cases except in a set of few words where -f- is found medially, and if that set of words consists of polymorphic words, the segmentation of words into two morphemes is used to establish the medial -f- as final -f of the preceding morpheme (e.g. abcfdg(word) =abcf(morpheme₁)+-dg(morpheme₂) in order to conform with the more widespread complementary distribution. Note that morphemic analysis, though not a part of phonological analysis, intervenes into phonological analysis as a relevant context.

In addition, the classification of sounds into phonemes and allophones does not reveal many different types of patterned relations among sounds. Even two phonemes may exhibit a patterned restriction on their distribution in a language which results in a dependency relationship. For example, in English, out of phonemes /p/ and /b/, /p/ is predictable after s-. In order to distinguish the predictability of allophones [p pʰ] that encompasses all contexts from the predictability of phonemes /p b/ that covers a limited context (i.e. after s-), allophonic statements are separated from phonotactic statements within the linguistic model.

Similarly, the interchange of phonemes in different morphemic contexts is described as morphophonemic statements. The complex interaction of phonemes and morphemes reveals sound patterns based neither on allophonic relationship nor solely on phonotactic structure. For example, in English, the different phonemes /f/ and /v/ neutralize their contrast in /wáyf, wáyyv/ ‘wife, wives’. Phonotactic statements of English allow the sequence /fs/ (e.g. fifes), therefore the explanation for this alternation of /f/ and /v/ is sought in a morphemic context as follows:

1. wáyf-+plural⇒wáyyv-+plural
2. voiced consonants (except j, z)+ plural ⇒ voiced consonant (except j, z)+ -z
⇒ wayv-+ plural ⇒ wayv-+ -z

The discussion of the model of phonology is intended here to reveal that the internal organization of sounds in a language is not simply a matter of contrast on a single plane. It involves a complex structure in which (a) distinctive phonological components are grouped sequentially and simultaneously; (b) the principle of complementary distribution plays as significant a role as that of contrast; (c) the additional criteria of phonetic similarity, pattern congruity, and morphemic boundary are developed to resolve problematic cases; (d) statements of phonotactics are constructed to capture predictability of phonemes in a limited context, thus reducing redundancy further; and (e) the influence of higher order units (morphemes) on sound patterns is brought out in the use of morpheme boundary and in the morphophonemic statements.

A close comparison of the constructs that are used in semantic analysis and in linguistics leads one to raise some pertinent questions in semantic analysis such as: Are there distributional restrictions on semantic features in a semantic domain? As the answer must be affirmative, can semantic components be formed on the basis of distributional restriction patterns in the same way as phonemic components are developed? What kinds of units emerge by the combination of semantic components from different semantic domains? Role and status (56, 76) and land tenure (103) seem to form such units. Distributional restrictions on the combinations of semantic features from different semantic domains would form statements of tactics comparable to phonotactics and morphotactics. Such statements seem essential in any cultural analysis since no culture allows all the permutations of semantic components in different semantic domains. This is evidenced in the data of status (56) where five avoidance duties and three types of sanctions develop into only seven statuses, whereas all theoretically possible permutations of these components would develop hundreds of statuses if each permutation were correlated with a distinct status. The important point to be noted is that the restrictions on the tactics of semantic features may be the result of some general underlying rules. Also, one may ask whether semantic features contrasting in one semantic domain become noncontrastive in the context of the semantic categories of another semantic domain. If so, the neutralization of contrast in a specified context would be comparable to morphphonemic rules.

One analysis of land tenure (103) consists of correlations among external contexts of land tenure (type of rights, subjects of rights) and terrain classes and form the statements of tactics. Nevertheless, units entering into tactics, unlike the units of phonotactics and morphotactics, are unanalyzed gross complex units. Interaction of the semantic features of different semantic domains is brought out by Tyler (113) and Bricker (13) in the usage of kinship terms in Koya and Mayan languages respectively. Tyler reveals the distributional restrictions on semantic features of kin terms and components of external contexts such as audience composition, codes, social setting, and religion of participants.

Gardin (48) presents some theoretical and practical difficulties involved in the nature of archaeological data for componential analysis. The data in archaeology
consist of an incomplete range of vast arrays of technological products with fragments of information about each material object. This situation contrasts with the nature of ethno-semantic data, i.e. the complete range of limited segregates and full information about each segregate. Such difficulties are the result of granting the componential model of semantic analysis (as opposed to the linguistic model) as the ideal model for archaeology. As pointed out earlier, the componential model of semantic analysis is generally an overly simplified and misinterpreted version of the original componential model developed in linguistics. In the light of the original linguistic model, componential analysis in archaeology is feasible and fruitful. Incompleteness is inherent in any social data treated in their proper complexity. This model has been fruitfully applied to a vast and incomplete range of linguistic data. Furthermore, the basis of componential analysis—distributional restrictions—are present in material culture as well as linguistically represented cultural aspects. Distributional criteria indicating different types of relations—co-occurrence, mutual dependence, complete dependence—are used by archaeologists informally in their initial sorting of technological products. Formalizing such criteria explicates inherent correlations within the data that replace typological correlations of attributes of artifacts.

Deetz (29) conceives of the structural similarity between sounds and artifacts as a significant perspective for archaeology since it allows for the formal analysis of material culture. An analysis of material culture in turn develops the basis for reconstructions of nonmaterial cultural patterns such as exogamy, patrilineality, migration, trading, etc (29). He takes into account not only significant features (attributes) of artifacts, but also the way the attributes are combined (tactics) as an important level of analysis. According to him, words and artifacts are comparable because both are the products of human motor activities and are composed of smaller structural units, sounds, and attributes respectively. The analysis of attributes as distinctive (facteme) or nondistinctive (allofact) can be carried out by noting whether two attributes replace each other to produce different artifacts. Distributional restrictions on factemes are comparable to phonotactics in a language. Furthermore, factemes are arranged into minimal classes of objects (formemes, e.g. shaft, stonehead) as phonemes enter into the composition of morphemes. This analogy points out that formemes also follow rules of arrangement just as the morphemes of a language follow rules of morphotactics. The tactics of formemes result in artifacts on the analogy of morphemes resulting in wrods. Just as a morpheme can be found in different words, a formeme may be found in more than one kind of artifact. Finally, the structuring of words into sentences is equivalent to that of artifacts into subassemblages. He points out that the rules of tactics or units of facteme and formeme may be as unknown to natives as rules of grammar or phoneme and morpheme. Finally, he emphasizes the historical implications of these units and their levels of analysis, suggesting that the similarity of artifacts in two assemblages is comparable to the similarity of words in two languages, both indicating superficial connections between the cultures involved. The similarity between patternning of attributes in two assemblages, like that of grammatical rules, ascertains deeper connections between two cul-
tures. One could perhaps test the Deetz model by carrying the analogy further to correspondences of attributes similar to phonological correspondences and cognates used in the historical model of linguistics (cf comparative-historical model).

**Transformational-Generative Model**

Recently, the transformational-generative model (henceforth T-model) has gained some attention in the semantic analysis of kinship terminology (1, 11, 14, 33, 34, 64, 77, 79, 85, 89, 93, 94, 108). Two analyses have been concerned with the structure of music (12) and propositions of religion (30).

*Reduction rules.*—The term transformation in the analysis of segregates usually has a specific reference to reduction rules introduced by Lounsbury (93, 94) for analyzing kinship terms. The goal in formulating this rule is to establish an equivalence among multiple denotata of one term by selecting one denotatum as a primary reference (e.g. Si so) and by converting the rest of the denotata (i.e. secondary ones such as Fa Si so) into the primary denotatum through a set of rules. The rules are formulated by exploiting the structural equivalence between different kin categories (e.g. Fa Si, and Si) in a well-specified common context. The format of these rules is similar to that of morphophonemic rules in linguistics. Morphophonemic rule: Let a morphophoneme $N$ whenever it occurs between a vowel and a labial consonant be phonemically /m/ in that context, and be /n/ in the rest of the contexts. Consequently, $VN+p \Rightarrow Vmp$; but $VN+t \Rightarrow Vnt$.

Skewing rule: Let the kin category father's sister, whenever it occurs as a link between ego and another relative, be regarded as structurally equivalent to the kin category sister in that context (93, p. 220). Hence $Ego+Fa Si+son \Rightarrow Ego+Si+Son$; but $Ego+Fa Si \Rightarrow Ego+Fa Si$; and $Ego+Si \Rightarrow Ego+Si$. Just as morphophonemic rules are statements of neutralization of contrasts between two contrastive phonemes in a well-specified context, skewing rules are statements of neutralization of contrast between two kin categories, i.e. $Fa Si$, and $Si$, in a well-specified context. This type of rule exhibits a considerable advance in the use of the linguistic model. Just as transformational phonologists have disclaimed the *phoneme* as a valid unit of analysis, and have moved to the level of morphophonemics with its accompanying *distinctive features*, we can see a casting aside of the *emic-etic* influence and the movement into an area where distinctive features are considered in the light of their context. The ingenuity of this approach can be appreciated in its capacity to explain how 82 different kin types from four generations are systematically grouped into 26 kin categories by the operation of three rules and their corollaries (93). Such reduction rules are employed by Bright & Minnick (14), Elkin (34), Henderson (77), Hopkins (79), Keesing (85). Bright & Minnick suggest an ordered operation as a necessary property of the rules in Fox kinship terms and further suggest that the order of the rules may correspond to an historical order of development in some way. Hopkins (79) asserts that this order is determined by the order of the symbol in the string (from left to
right), and therefore implications regarding the historical significance of the order of the rules cannot be sustained.

Hammel’s (64) analysis of Comanche kinship terms is oriented towards demonstrating that transformational rules (hereafter tr. rules) can be formulated inductively “beginning with the empirical data, and working into successively more general statements” (p. 66). He uses the procedures of factoring, substitution, and determining common core types so that all kin types of a term can be generated from a core kin type in the same way as in Lounsbury’s expansion rules. Coult (26, 27) ascribed superiority to the T-model over the componential model since the former shows how complex states are merely the results of the repetitive application of certain rules to simple core states (p. 46). However, he was concerned about assigning the proper term to a given kin type in the way a native speaker does. Therefore, a transformational analysis that works is not necessarily acceptable to him. Furthermore, he considers the componential analysis of core kin types irrelevant since it is not the form explicitly used by natives (26). He proposes that tr. rules of equivalence cannot be accepted as underlying principles of kin terminologies but that both kinship terminology and tr. rules need to be explained by sociological (e.g. lineage solidarity) and logical (e.g. uniform succession, uniform reciprocals) dimensions. However, according to Lounsbury, such explanations require underlying configurations of kinship terms that are systematically expressed in a language. Thus, kin-term structures would be logically prior to sociological and logical explanations.

Hammel’s (64) view of the T-model as an inductive model is misleading as regards its conception in linguistics. One of the major properties of this model is to allow the formulation of strictly hypothetical structures (that may or may not have inductive justification) as deep structures that are subject to formal operations. Deep structures are the axioms of linguistic analysis, and their validity is ascertained only to the extent that the surface structures derived from them (through the operation of tr. rules) match empirical data (i.e. surface structures). The only formal contingency of deep structures in a T-model, in its most abstract form, is logical consistency, i.e. noncontradiction and freedom from ambiguity. Such a model is considered descriptively adequate, and as Coult (27) has recognized, there can be more than one descriptively adequate model.

The relative value of different descriptively adequate models can be measured only by their success in achieving goals of analysis which are set independently of the model. Therefore, further requirements of the model are determined by the goals of linguistic analysis, namely, to develop formal principles to account for the native speaker’s linguistic competence. Competence is defined as the explicit and implicit abilities and intuitions of the native speaker manifested as follows: The ability to (a) produce novel meaningful sentences; (b) to impose semantic interpretation on a sentence appropriately; (c) to identify ambiguities and anomalies; (d) to detect paraphrases; and (e) to judge relations between sentences. Since the concern is with competence, differential performance of individuals is not the subject of explanation.

More specifically, a set of transformations relating a surface structure to a
deep structure in a linguistic model is not an arbitrary list of operations decomposing one sentence into another, but they are characterized by certain formal properties, and the set of transformations has a structure of its own. They are instructions to introduce certain changes (by addition, deletion, or substitution of symbols) over specified parts of a string of symbols in specified contexts (i.e. they are context-sensitive). Other formal conditions are: 1. Each tr. rule operates upon one symbol at a time, so that one specific change in a complex structure can be identified independently and appropriately to allow generalizations concerning that change in various syntactic structures. Consequences of this condition can be seen in the formulation of multiple tr. rules that derive one syntactic structure from a deep structure by successive operations of rules. 2. The transformations must be ordered in an unarbitrary way so as to develop the widest possible range of syntactic structures as a domain for each tr. rule (19).

The most obvious transformations in a language are the lowest order tr. rules (See Tr. X, Figure 1) that introduce redundant elements in surface structures. Their immediate deep structures (Set B, Figure 1) can be postulated by zeroing the redundancy of the surface structures of sentences, and this procedure can be carried out inductively as noted by Hammel (64).

In this model, Tr. X operates upon the nonredundant structures (set B) to derive surface structures (set C) by introducing redundant elements. Though necessary, Tr. X are trivial in the model. It is the set Y transformations that are most unobvious and insightful in the structure of a language. Its function is to derive different subsets (p, q, r, . . . ) of set B, each subset consisting of diverse types of nonredundant syntactic structures and forming a natural group by the fact that the same transformation—say $x_i$—is applicable later to that subset in the language. The set Y transformations operate upon still deeper structures (set A), consisting of higher order hypothetical structures. They are necessarily hypothetical because each of the subsets of set B—irrespective of the degree of diversity of its syntactic structures—must be connected to a single element in the deep structure. Thus a deep structure containing a required element for a given subset must
have its internal structure sufficiently complex to derive all the diverse types of syntactic structures of a subset (of set B) under consideration.

The deep structures (Set A, Figure 1) represent elementary structures (not sentences) of a language formulated in terms of hierarchic relations of the primitives of a language. In order to insure that the hierarchic relations between primitives are unambiguously assigned to a string of primitives, the deep structures are derived by successive expansion rules—called rewrite rules—with primitive beginning symbols, e.g. A→B+C. The formal properties of these rules are: (a) they are context-free, hypothetical, and are written to disallow (b) the development of null symbols (A→0); (c) change in order (a+b→b+a); (d) more than one symbol on the left hand side (a+b→c); and (e) simple replacement (a→b). For a detailed discussion of the T-model see (19).²

It should be added that the T-model of phonology is similar to the syntactic model described above. The differences between the two models in syntax and phonology is due to the nature of their data. The T-model of phonology is more significant in semantic analysis because segregates (in semantic analysis) exhibit a closer inherent similarity with sounds—both are composed of simultaneous features, and both occur in sequential arrangement.⁴

**Distinctive Feature Model in Phonology**

In the T-model of phonology, each segment is represented by a column of distinctive features (henceforth DF) given in rows,

```
+ diffuse
+ consonant
− strident
```

DFs are chosen from a universal set in a way so as to distinguish each segment from all the others by at least one feature specified for its presence | + |, or for its absence | − |. A morpheme composed of several segments would be represented by several such columns forming a matrix in which columns represent segments and rows represent DFs of the language. As natural languages are characterized by redundancy, two types of redundancies are eliminated from the phonetic matrix of the morpheme in order to represent it in the deep structure by nonredundant (i.e. minimum) distinctive features: (a) features predictable in a column by the presence of other features (see i, below); and (b) those that are predictable in one column by the presence of some feature in other columns (see ii, below). The connection between the deep structure and the surface structure phonetic matrices is made by a set of morpheme structure rules (MS rules) such as,

² The reasons for these formal conditions, as well as many specific constraints on rewrite rules and tr. rules, are not discussed here.

⁴ Note that simultaneous arrangements are found in syntax (e.g. syntactic elements composed of simultaneous semantic features), thus the distinction between sounds and syntax as simultaneous vs sequential is spurious. But I have presented it as it is usually conceived in the T-model.
Morpheme structure rules are primarily redundancy rules of additions and they apply to individual morphemes (61).*

The nature of reduction rules.—In the light of the T-model of syntax and phonology as described above, the nature of reduction rules can be investigated. Buchler & Selby (15) consider them different from tr. rules by restricting tr. rules to the process of substitution only. But they do not give any reason for such restrictions. A rule of substitution is not substantively different from the rules of addition and deletion since a substitution rule can be converted into a rule of addition or deletion. Though reduction rules can be described as consisting of structural descriptions that specify how input strings are to be rewritten, they cannot be considered solely as rewrite rules because this also describes tr. rules. Similarly, even though reduction rules can be converted into expansion rules, and rewrite rules are rules of expansion in the T-model, it must be pointed out that the expansion of a string is also found in tr. rules when a tr. rule introduces a redundant category into the string. The most crucial distinction between rewrite rules and tr. rules in the T-model is that the former are context-free rules while the later are context-sensitive rules. By this criterion, the context-sensitivity of reduction rules does not leave any option but to consider them as tr. rules. The fact that reduction rules are reflexive, unlike tr. rules, may be relevant, though it ought to be noted that Gates (49) points out that reduction rules may operate independent of their reflexive corollaries (p. 57). Two important distinctions between linguistic tr. rules and reduction rules in semantic analysis seem to be as follows: 1. Since reduction rules convert one kin type (secondary denotatum) into another (primary denotatum), their domain of operation is characteristically the surface structures of kin types unlike the operational domain of tr. rules in linguistic model which is deep structures; in this sense, reduction rules are similar to a rule that may convert one sentence into another.* 2. As there are no formal conditions established in regard to the interrelationship between different reduction rules, they tend to be ad hoc, arbitrary, and too specific (in many cases applicable to only one kin type) unlike the tr. rules of grammar that form a structure in themselves and that unfold crucial generalizations about the structure of sentences.

* In the T-model of phonology, cyclically operating P-rules are designed to operate upon the sequences of phonological matrices of morphemes (20, 110), but they are not directly relevant for the present semantic analyses in anthropology and therefore are excluded from discussion.

* Rules converting one sentence (kernel type) into complex sentences were the first type of tr. rules in the T-model, and they have been replaced by much more abstract rules in the current model (19).
Langacker suggests that reduction rules are not equivalent to syntactic transformations (89, p. 847). This opinion is rooted in his concern for incorporating the semantic information of a lexicon into a linguistic analysis so as to derive grammatical sentences, and not for discovering the semantic structures of kin terms in order to incorporate them into larger cultural complexes. Subsequently, he mentions that only the semantic information directly relevant to syntactic rules has a place in the T-model of grammar. He recognizes the necessity of establishing a one-to-one correspondence between a term and its denotatum in order to incorporate the semantic information of a lexical item into the underlying structure of a sentence in a straightforward way. Consequently, he considers reducing multiple denotata of a term representing various genealogical configurations into one denotatum as a necessary process but to be viewed correctly as prelinguistic, thus having no defined status in the syntactic structure.

Components vs distinctive features in semantic analysis.—A term is represented in componential analysis by a number of components and each term is distinguished from all others by at least one component. This process is similar to representing a segment by a column of DFs in the T-model of phonology. However, unlike the DFs of phonology, components are not chosen from a set of strictly binarily opposed features. Consequently, components are neither minimal nor do they represent a minimum necessary number of features; e.g. Romney’s (105) components of \(|\text{ascending generation } + '1'\), and \(|\text{descending generation } - '1'\), requires a third component for \(|\text{same generation } 0'\). If one uses DFs specified positively or negatively as in Aoki’s (1) analysis, two features—\(|\text{generational removal (g.r)}\), and \(|\text{seniority (sen)}\)—would be sufficient to represent the above mentioned three components as follows:

\[
\begin{align*}
  +\text{g.r} & \quad \text{for ascending generation,} \\
  +\text{sen} & \\
  +\text{g.r} & \quad \text{for descending generation, and} \\
  -\text{sen} & \\
  -\text{g.r} & \quad \text{for same generation} \\
  -\text{sen} & 
\end{align*}
\]

Also, this model permits one to formulate the age distinctions between siblings

\[
\left( \begin{align*}
  -\text{g.r} & \quad \text{for older siblings, and} \\
  +\text{sen} & \\
  -\text{g.r} & \quad \text{for younger siblings} \\
  -\text{sen} & 
\end{align*} \right)
\]

without adding new symbols. Eleven components with complex denotations (105) can be reduced to four DFs \(|\text{masculinity}|, |\text{consanguineality}|, |\text{seniority}|, and \(|\text{generational removal}| with simpler denotations (see Table 3). These features are also sufficient to represent sex merging categories (parent, child, spouse, sibling) by unspecified features of masculinity as in Table 3:
TABLE 3. DISTINCTIVE FEATURES OF KINTYPES OF NUCLEAR FAMILY

<table>
<thead>
<tr>
<th>Semantic feature</th>
<th>F</th>
<th>M</th>
<th>s</th>
<th>d</th>
<th>B</th>
<th>Z</th>
<th>H</th>
<th>W</th>
<th>P</th>
<th>C</th>
<th>Sib</th>
<th>Sp</th>
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<tbody>
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<td>Masculinity</td>
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<td>-</td>
<td>+</td>
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<td>+</td>
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<td>+</td>
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<td>Generational</td>
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<td>+</td>
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<td>removal</td>
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</tr>
<tr>
<td>Seniority</td>
<td>+</td>
<td>+</td>
<td>-</td>
<td>-</td>
<td>(-) (-) (-) (-) (+) - (-) (-)</td>
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<tr>
<td>Consanguineality</td>
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</tbody>
</table>

* ( ) indicates redundant feature

Apart from numerical economy, there are far-reaching implications of using minimal elements as primitives in the analysis. Rules in terms of kin categories (Fa, Mo, so, d) produce the narrowest generalizations possible because such categories incorporate semantic information all of which may not be pertinent for the operation of a rule. For example, in rules ds→ss, dd→sd the information about the sex in the second symbol in each string is not necessary. This extra information forces us to design multiple rules where fewer more general rules would be sufficient. A rule formulated in terms of components produces somewhat broader generalization than the rules based on the kin categories. But even components are complexes of minimal features as shown above, and therefore they also incorporate some semantic information that may be unnecessary for the operation of a given rule. In that case, why formulate rules in terms of complex units that prevent generalizations? Distinctive features allow a formulation of rules by stripping off all the semantic features (of a term) that do not play a direct role in the functioning of a rule. Finally, such a formulation permits us to define the widest range of kin classes to which a specific rule (and hence semantic feature) is applicable.

Some advantages can be envisioned in Aoki’s (1) analysis of Nez Perce kin terms. In his analysis, he presents reduction rules using kin categories and then the same rules using DFs specified positively or negatively. By using DFs he collapses four collateral merging rules into one, and two secondary merging rules into one, and furthermore he collapses his collateral merging rule and his secondary merging rule into one rule. Similarly, four half-sibling rules and four step-kin rules are reformulated into one half-sibling rule and two step-kin rules by using the DF model.

Another important difference between componential analysis in semantics and the DF analysis of phonology can be viewed in the absence of redundancy rules in componential analysis. As a result, once segregates are cast into concatenations of components, these concatenations form composites of redundant and nonredundant components.

Aoki’s (1) rule formulated in terms of DFs can be further simplified by eliminating the redundant features from the matrix and giving redundancy rules in the T-model of phonology. In Table 3, |+consang| is predictable in the context of
| +g.r | and | −sen | is predictable in the context of | −g.r |. The following rules would eliminate 12 specifications in the semantic matrices of Table 3:

1. | +g.r | ⇒ | +g.r |
    | consan | 2. | −g.r | ⇒ | −g.r |

Few analyses of segregates using rewrite rules and tr. rules of the T-model are carried out in kinship (11, 33, 89, 108). Boiles (12) uses the T-model for analyzing music and his rewrite rules are based on the structural primitives of language, e.g. NP, adjective. Durbin (30) analyzes the propositions of religion in the T-model. Bock (11) attempts to account for ambiguity of kin terms (e.g. Brother-in-law=Si's H, W's Br) by deriving the same term from different structures |(+sp) Sb+Sp |; and |Sp+Sb |. The internal coherence of the rules is not achieved in his analysis since some rules operate upon the same string to derive different outputs. Furthermore, no attempt has been made to bring out formally the intuitive groupings of terminology made by natives. Langacker’s (89) excellent article accounts for such intuitive groupings by deriving each grouping from a common deep structure. Subsequently, his underlying structures are more abstract than those found in other analyses. For example, eight terms that have -in-law in them are derived from one deep structure, and these terms are in turn connected with two sets of deep structures that are formally related by a device called mirror image rule as given below.

| X spouse poss | sibling | +lineal | Y ⇒ X | sibling | +lineal | -in-law Y |
| 1 | 2 | 3 | 4 | 5 | 1 | 4-in-law 5 |

The mirror image rule indicated with * refers to the condition that when the constituents of the input are in exact reverse order (i.e. 5 4 3 2 1), the output will still be the same.

| Y | sibling | +lineal | poss spouse X ⇒ X | sibling | +lineal | -in-law Y |
| 5 | 4 | 3 | 2 | 1 | 1 | 4-in-law 5 |

This mirror image rule collapses the rule of spouse’s sibling and that of sibling’s spouse. Likewise, he provides one rule to derive terms uncle, aunt, nephew, niece; and one rule for all terms taking the morpheme grand- initially.7

**Comparative-Historical Model**

While linguistic models dealing with synchronic structures have been adopted in synchronic semantic analyses, there is little work relating the concepts of the historical linguistic model to diachronic problems of culture. Frisch & Schultz

7 For other attempts at analyzing kinship terms by using a different linguistic model, see Lamb (86, 88). For a description of Lamb’s stratificational model, see (87).
(47) have reconstructed a Proto-Central-Yuman kinship terminology by reconstructing morphemes and semantic features. They use the concept of semantic correspondence. Aoki (1) reconstructs morphemes of Proto-Sahaptian, and Hockett (78) reconstructs Proto-Central-Algonquian morphemes of kin terms. He reconstructs the meanings of these terms. The peculiar attitude of linguists as regards "lapsus linguae" as a major cause of phonemic change in language influences Hockett's comment regarding "lapse" as a cause of semantic change, rather than some sociological change as the underlying factor. Gates (49) reconstructs the forms of Greek kinship terms by using the comparative linguistic method. Three extensive studies by Friedrich (42, 43, 45) have set an example for reconstructing semantic information. First, he reconstructs morphemes by using the comparative method. Then he uses ethnological, social anthropological studies of the meanings of the terms and archaeological evidences (suggesting social and behavioral patterning of a community) in order to reconstruct the semantic structure of Proto-Indo-European kinship terms. Similarly, in reconstructing the terms for trees utilized by Proto-Indo-European, not only does he explore evidence from ethnology, archaeology, cultural history, and linguistics, but also brings in the relevant evidence from geology, paleontology, arborescent ecosystems, paleobotany, palynology, and finally literary and mythological references. Such rich and exhaustive information reduces the indeterminancy and nonuniqueness of semantic reconstructions based on narrowly conceived analyses.

CONCLUSION

First, it must be pointed out that in this review I have not attempted to evaluate linguistic models for obvious reasons. Rather I have tried to assess the degree of success in the application of several linguistic models to other cultural data. In this assessment the following major problems are seen to emerge: (a) analytic concepts when isolated from their linguistic frame have at many times lost their rigor when applied to anthropological data; (b) the mistaken interpretation of some linguistic concepts (e.g. complementary distribution) has prevented fruitful results; and (c) the strategies of linguistic analysis are not adequately used to develop analogous strategies of analysis in anthropology.

As a result of the above problems, some anthropologists have accused formal semantic analyses of being a "sterile mathematical game" (26) dealing with triviality (68) or as being arid in implications. It must be remembered that the process of analogy, especially in the first stages, is never completely free from the problem of which new data are relevant to the various aspects of the borrowed model. The discussion of new data in strategies that fit into the borrowed model requires the insight of a philosopher who can exploit epistemological properties of the data for this purpose. Since anthropologists are not trained philosophers, they are forced to a trial and error approach. For example, context is a key con-

8 If my aim were to evaluate linguistic models per se, the T-model would be the more highly preferred because of the richness of its conceptual frame.
cept in all linguistic models. In order to utilize it adequately in semantic analysis, one must seek the cultural data that form a relevant context in the analysis of a given semantic domain. Furthermore, one must also seek the relevance of different strategies of cultural context at different levels. The importance of simultaneous units, sequential units, higher order units, and the units from different semantic domains as contexts seem to emerge from a direct analogy with the linguistic model. However, these contexts are generally not identified in semantic analysis as to their form or function. The introduction of context into analysis seems to lead us to an ecological-adaptational model. This type of model probably would not be identical with the traditional model used in ecological-adaptational studies at present, but perhaps may be more similar to what Friedrich (45) and Tyler (113) have hinted at. Since the linguistic model was developed to handle explicitly only a part of culture (language), some aspects of the model may not in fact be applicable to all aspects of culture. Hence two questions remain to be answered: (a) which aspects of the model are inapplicable to nonlinguistic data; and (b) which aspects of nonlinguistic data have no adequate representation in this model. These questions can only be answered epistemologically or on a trial and error basis.

One may also note that the concepts of linguistics that have been applied with appropriate understanding have been productive in revealing some important aspects of conceptual systems, e.g. the notion of contrast has thrown light on the way a semantic domain is dissected to formulate different sorts of structures such as tree and paradigm. The psychological implications of the degree of redundancy involved in these structures seems far reaching. Also, how tree and paradigm intersect in actual folk taxonomies may indicate the level of redundancy necessary in folk taxonomic structures (32). This can be evaluated in the light of future research. Similarly, the notion of reduction rules in kinship suggests that nonlinguistic cultural aspects may determine one type of reduction rule rather than another (27), thus pointing to the territory where linguistic and nonlinguistic aspects articulate with each other. A formulation of the way they articulate may result in a formal model in which linguistic and nonlinguistic aspects of kinship integrate.

**Overall Impact**

Even though applications of the linguistic model have been fragmentary in anthropological analysis, they have introduced new perspectives as regards the goals, the subject matter, the nature of analysis, and methodologies (83, 109, 112) in anthropology. The significant value of these applications at present lie not in the actual analyses as much as in the emerging new stances that lead anthropology to nonbehavioral and nonempirical formal approaches.

*Goals and subject matter.*—There has been an increasing awareness of the futility of analyzing multidimensional (functional, formal, historical, psychological) human behavior simultaneously in all its aspects (35, 112, 114). Similarly, it is recognized that the analysis of cultural systems in one dimension at a time is a
feasible and fruitful approach (54, 62, 83, 91–94). This recognition is due to the fact that formal analysis in linguistics ultimately has led to better functional (sociolinguistics), psychological (psycholinguistics), and historical analyses of language.

The subject matter of the study in this new perspective is the conceptual systems of a culture, rather than human behavior. However, this shift from empiricism to rationalism is obscure in anthropology since many analysts still insist that formal analyses simultaneously "capture ontogenetic process" (18), "match with the natives' verbalized statements of a system" (18), and produce "cognitively real" analyses (115). In linguistics, the subject matter has been gradually identified and defined in the last decade as competence as opposed to performance which is determined by competence and various other factors such as memory, individual abilities, sociocultural and psychological factors. In anthropology, no such distinction has yet evolved.

*Nature of analysis.*—The concepts in the linguistic model relate to abstract, directly unobservable aspects of language (e.g. morphophoneme, DF, transformation) and not to empirical segments of language. Therefore, the resultant analysis is in nonisomorphic relation with the empirical data. On the other hand, anthropology has been characterized by inductive generalizations that are isomorphic to the data and are formulated in terms of taxonomic categories. The value of an abstract, nonisomorphic, deductive model is self-evident in linguistics by its results. Though such a model is not currently used in anthropology, its influence can be seen in the emerging distinctions of structural reality vs empirical reality.

*Nature of explanation.*—The aim of discovering some systematic order in observable things, and formulating laws that allow explanation and prediction, has failed in anthropology since the relations of dependence between phenomena are not formulated as a logical strategy in terms of universally valid "ideal" laws. Rather they take the form of correlations of empirical data which remain at the level of inductive generalizations. A formal explanation possessing the status of "ideal" laws is a part of linguistic analysis (e.g. explanation of ambiguity). Gradually, by the process of analogy from linguistics, such explanations are being accepted as valid in anthropology (56).

*Methodologies.*—As a result of the use of linguistic models, new methodological requirements in anthropology are set such as (a) an explicit formulation of each and every structural detail relevant in the analysis; (b) a tight organization of structural statements to prevent the entrance of unrequired structural information and inconsistencies; and (c) an elimination of ad hoc explanations (62, 65, 66, 92–94).

This analogy points to many productive avenues of research such as the relation between segregates in propositions. The meaning of a proposition is a result of three types of meaning: (a) logical meaning; (b) grammatical meaning; (c) cultural meaning. Logical meaning can be conceived as a set of universal
implications dependent upon the semantic features of a lexical item, e.g. a semantic feature of *give* implies that there is a recipient. Grammatical meaning is dependent upon the grammatical categories of lexical items in propositions, e.g. in *I gave Mary a pencil*, indirect object (*Mary*) received the direct object (*a pencil*), and not vice versa. Cultural meaning is dependent upon a set of cultural assumptions, e.g. *John married Mary* implies that *John* and *Mary* acquired different sets of specific duties, rights, and obligations for each other. The boundary between grammatical and logical meaning is not clear. While the logicians and linguists are interested in logical and grammatical meaning respectively, the anthropologist’s aim is to fathom the cultural meanings of a given semantic domain by examining segregates in the context of propositions. One of the manifestations of cultural meanings can be seen in the replacement of one segregate by another in the context of certain semantic features in order to derive one culturally acceptable proposition from another. Thus a native speaker can derive *John is Mary’s husband* from *John married Mary*, but cannot derive *John is Mary’s secretary*. Note that the replacement of segregates between derivationally related propositions cannot be solely explained by the logical or grammatical relations between them.

An analytical model that reveals cultural meanings of segregates and propositions of a semantic domain (e.g. religion) by defining formal relations between them may best be sought in a hypothetico-deductive model. Such a model allows the postulation of hypothetical propositions that can function as deeper cultural assumptions which are mostly implicit and hardly verbalized. The formal conditions that connect the set of hypothetical propositions with a set of verbalized propositions about a semantic domain may involve a set of formal specified operations. The nature of formal properties of such hypothetical propositions and those of operations point to a new horizon in semantic analysis.

Finally, just as I have not attempted to evaluate linguistic models per se, I have also not attempted to judge which linguistic models are best applied to anthropology. In the first place it is impossible to make such a judgement since, as pointed out continually throughout the paper, no model has been applied in its entirety to anthropological data. Secondly, no model is adequate for all aspects of any given data. No single linguistic model can cover all the aspects of the data of a natural language (e.g. as conceived by a psycholinguist, an ethnographer of communication, etc). Similarly, no linguistic model can cover all nonverbal aspects of a culture. Each different model uncovers a portion of structural facts from the data; one must decide what kinds of information about the data one wants to unfold. In anthropology, there is no reasonable agreement as

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9 Keesing (85a) has recently criticized my application of the T-model to religion, believing I have assumed that the linguistic model is applicable to all cultural data. My stated goal in the article (33) was to extract cultural propositions formally interconnected within a semantic domain. The fact that the linguistic models cannot be applied to all cultural data is recognized by everyone who is engaged in utilizing linguistic models in anthropology (e.g. 94).
to the kind of information which should be revealed from the data. Just as the structural linguistics model yielded considerable understanding about sound patterns and word structuring, but failed in the domain of syntax, one further notices that the T-model, much richer than the structural model in its conceptual frame, has given many insights into the operation of syntax but has failed to explain many crucial facts of semantics, psycholinguistics, and the ethnography of communication.

Keeping in mind the incomplete application of linguistic models in anthropology, that no model can suffice for all cultural data, and that anthropologists are not yet sure as to their central goals, it becomes even more difficult to evaluate the use of these models when we consider that neither in linguistics nor in anthropology are they in direct competition. Though the field of linguistics has been changed radically by the introduction of the T-model since the inception of anthropological semantic analysis, it must be emphasized that the T-model is an evolutionary outcome of the structural model, evidenced by the fact that many crucial concepts of the T-model are rooted in the conceptual distinctions developed in structural linguistics. While on one hand the concept of the phoneme has been rightfully rejected in the T-model, many of the concepts underlying the phoneme such as contrast, complementary distribution (context-sensitivity), distributional restrictions, and distinctive features are crucial for transformational analysis up to the present day. Furthermore, complementary distribution (context-sensitivity) has been not only retained but extended to syntax (tr. rules) and into the latest writings of interpretive semantics in the T-model. The point to be made is that as the well-defined goals of linguistic analysis have been expanded, the original model has evolved to encompass ever-widening domains, carrying with it the relevant conceptual material acquired by the previous model. We can only hope that as anthropologists continue to employ various linguistic models, they recognize that all these models stem from a continuous tradition based on common epistemological properties and that one linguistic model cannot be totally rejected in favor of another. Only by this recognition can we continue to seek conceptual material relevant to cultural analysis.

ACKNOWLEDGMENTS

I am heavily indebted to Marshall Durbin for his many suggestions and comments, though all shortcomings of this article are my responsibility. It was impossible to cover the entire literature in this field in the space allotted. Consequently, many significant references could not be discussed here, such as Durbin (32), Fischer (36, 37), Lévi-Strauss (90), Walker (115), Werner (118); unpublished works by Henry Selby, Frank Wardick, Michelle Zimbalist Rosaldo, and many others.
LINGUISTIC MODELS IN ANTHROPOLOGY

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Anthropologists tend to look to American Indian linguistics for a classificatory philosopher's stone, one which will provide large-scale schemes to correspond with those derived from study of cultural traits and natural areas. Alas, the facts are yet recalcitrant. There is thus a constant temptation to exceed actual knowledge, but here the neophyte has the advantage over the scholar. The true key to American Indian languages is a willingness to accept diversity. The well-known Indo-European language family had the geographical extent its name suggests prior to New World colonization, but even the wildest scholarly speculation finds no less than four American Indian language families of Indo-European internal diversity within the confines of North America.

As a basic outline in this paper I shall use a conservative classification that is not original with me, but rather is based directly on a little-known aspect of a famous article by Sapir (78), as clarified and somewhat simplified by later work, particularly that of Haas. Within this framework I shall summarize the current state of description and comparative work for each family. Languages not securely classifiable in an internally differentiated family, either for lack of evidence or distance from obvious relatives—language isolates in the term of Haas—will be treated separately, and possible wider relationships then briefly discussed.

Space limitations have dictated a narrowly linguistic concern in this paper. Such related topics as ethnographic semantics are omitted, and the emphasis here is on the status and details of recent and current work in descriptive and historical linguistics. The same consideration plus the writer's own limitations additionally circumscribe the America of the title to America north of Mexico, although a more reasonable division in terms of relationship would include both Mexico and Central America.

In writing this paper I have incurred two debts. The first is to direct correspondents. In 1971, Kinkade (49) published a roster of linguists and anthropologists currently working on American Indian languages, containing the names of over 300 scholars. I have circularized those on the list, inquiring as to their current and recent work, and received a generous response from more than half this number. I regret the necessity for severe selection in actual literature cited, but hereby acknowledge gratefully the education received from all those kind enough to reply. Secondly, a major volume
edited by Sebeok (81), summarizing work in various areas of American Indian linguistics, is scheduled to appear in 1972. Through the kind cooperation of Sebeok, his associate Alexandra Ramsay, and of the authors involved, I have read most of the articles to appear there in proof or in manuscript. For this I am deeply obliged, and urge readers desiring more detailed information on areas and families to refer to the volume in question.

Classification

A reasonable starting point for North American language classification is still the well-known paper by Edward Sapir (78) which sets forth a threefold classification. Sapir begins by listing the results of an earlier taxonomy based on word lists conservatively interpreted by Powell (74), and ends with a guess as to the future. Unfortunately, this last scheme was adopted thereafter in textbooks despite Sapir's warning that it was "far from demonstrable in all its features at the present time." This is still the case. Nearly unnoticed, except by Hymes (e.g. 41, pp. 73–74) and possibly by others who presented similar schemes without citing Sapir, was an intermediate classification uniting Powell's families on the basis of published evidence and consensus among field linguists [Darnell (18) describes its development]. It is this intermediate scheme of Sapir's on which this paper is based, with a further separation of "language isolates," as previously noted. This gives the following classification:

1. Eskimo-Aleut
2. Na-Dené
3. Algic
4. Wakashan
5. Salishan
6. Penutian
7. Hokin
8. Gulf
9. Siouan
10. Iroquoian
11. Caddoan
12. Kiowa-Tanoan
13. Uto-Aztecan
14. Language isolates:
   a. Beothuk
   b. Kutenai
   c. Chemakuan
   d. Yukian
   e. Karankawa
   f. Keresan
   g. Yuchi
   h. Timucua
   i. Zuni
The number of separate languages would have been in excess of 300 in pre-Columbian times. Chafe (11) counts about 200 languages as still spoken 10 or more years ago, and more recently Bright (8) gives about 150 languages, with more than a third now obsolescent or no longer spoken.

Considerable readjustment in the methodology of linguistic description has taken place over the last 15 years in connection with the theoretical advances summarized under the name of generative transformational grammar, and most recent work has automatically been accommodated to this newly recognized importance of abstract syntax. However, those who begin with this (or another) theoretical persuasion and nothing else have achieved little in basic description, for they seek out only those facts which illuminate rather narrow areas of concern. Despite recent advances in linguistic theory, the basic difficulty in American Indian linguistics still remains a paucity of serious linguists to describe a large number of diverse languages which are, moreover, disappearing.

FAMILIES

Work within the various language families differs enormously in tempo. Activity is incomparably greater and the number of workers many more in four families: Na-Dené, Algic, Salishan, and Hokan. (Well over half of the replies I received concerned these four.) This does not by any means imply adequate or available descriptions for all of the languages of these families, and conspicuous cases of languages which especially need research are mentioned in the paragraphs which follow.

1. Eskimo-Aleut. The languages of this family are few, though widely diffused from Greenland to Siberia, and it has long been recognized as a family. There is much work on the family by Soviet and Danish scholars, and a program at the University of Alaska has worked towards training native Eskimos in linguistics. For details, see the article by Krauss (52) in the Sebeok volume, which includes a bibliography of over 500 items. Least work to date has been done on Alaskan Eskimo and on Aleut, spoken now by a rapidly diminishing population. Krauss also reports on a conference on Eskimo linguistics held in Chicago in 1970. Among results of interest beyond the family is the conclusion based on work by Robert Underhill that Eskimo, long considered a paradigm of an "ergative" language (one which grammatically equates the subject of an intransitive verb to the object of a transitive verb, as shown in Eskimo in the absolutive vs relative cases of nouns), in fact requires postulation in deep structure of subject vs object in the conventional sense. In this family, where relationships are close enough that no basic questions arise, history is simply a matter of getting the details right. This is a fortunate position, and we may hope that those in the area continue on the road mapped by Krauss and do not lose their patience.

2. Na-Dené. Here we have a much different situation from that of Eskimo-Aleut, and one not dissimilar in aspect to that of Algic. There is a widely diffused family, Athapaskan, extending from interior Alaska and Canada,
through enclaves down to the coast of California near Cape Mendocino, and
from there to the compact and relatively close Apachean area of the American
Southwest (including Navaho). On the other hand, there are three individual
languages—Eyak of the Copper River Delta in Alaska, to be further men-
tioned shortly, and Tlingit and Haida, located on the lower Pacific Coast area
of Alaska and Canada and the Queen Charlotte Islands. All scholars agree
today on a common history between Tlingit and Haida on the one hand and
Athapaskan, but differences of opinion remain as to whether this may originate
in diffusion. The problem is joined but not solved in a series of papers pub-
lished from the mid-1960s by Krauss and Pinnow [see Krauss (53) for de-
tailed references and discussion]. The controversy is complicated by too much
weight laid on purely lexical comparisons and by implied separability of
language systems such that one piece can be genetically related and not another,
an unwarranted extension of the nineteenth century “wave-theory” of language
relationship. In any family both vertical (Stammbaum) and lateral (Welle-
thetorie) connections exist, and the problem is to sort these out.

Na-Dené has attracted many serious scholars of late, though Haida must
still be considered a deprived language of first priority; speakers remaining
are in the low hundreds, and we lack a full description. Tlingit has been
studied in detail recently by Naish and Story, in master’s theses from the Uni-
versity of London, soon to be published. Citations for these and many other
pertinent items are to be found in Krauss (53), a primary as well as second-
ary source since Krauss has become the current leader in the race to provide
adequate scientific data on Na-Dené. He has single-handedly performed a
definitive task of description on Eyak, along with a demonstration of its
affinity to Athapaskan [see particularly (51) for a concise but compendious
report of results; much of the descriptive work is yet to appear]. Krauss has
also directed what he modestly styles a “phonological survey” of interior
Alaskan languages, previously the darkest mystery of Athapaskan and a key
to understanding the history of the family since it is here that the point of
greatest linguistic diversity is to be found. Members of the missionary Sum-
mer Institute of Linguistics have also been instrumental in recent work on
these Athapaskan languages, with greatest effort devoted to direct applica-
tion, but there have been short noun-stem dictionaries on several languages,
and at least one more purely linguistic study by Henry (35).

Canadian Athapaskan has also been the subject of continuing practically
oriented study by missionaries, above all those of the Northern Canada Evan-
gelical Mission based in Saskatchewan. Here and in Pacific Coast Atha-
paskan, Fang-Kuei Li has done basic work. More recently there have been
important studies by Howren (40) on Dogrib and closely related dialects
and on Sarcee by Cook (e.g. 14, 15). Pacific Coast Athapaskan as well as
Apachean have long been a concern of Hoijer, and Hupa, the principal re-
mainling language of the Pacific group, has been the subject of extensive study
by Golla, though to date only one item has appeared in print (27).

Apachean and particularly Navaho have been the subject of much work
over the years, again summarized in detail by Krauss (53). Hoijer is at work 
on a dictionary of Navaho which will add to the many important items he 
has contributed over the years, most recently a paper in 1971 (39) which 
goes well beyond Apachen to study the morphology of Athapaskan. An 
interesting paper by Akmajian & Anderson (1) deals with an aspect of 
Navaho syntax. Much other important recent work on Navaho is still 
unpublished, including meticulous and detailed lexicographical and ethnosem-
antic studies by Oswald Werner and collaborators, and descriptive studies 
of Navaho phonology and morphophonemics by Richard Stanley and Muriel 
Saville. The several other Apachen languages put together have not received 
the attention given to Navaho, and at least two of them (Lipan and Kiowa 
Apache) are rapidly passing out of currency.

Comparative and historical Na-Dené studies are again instructively sum-
marized by Krauss. An additional recent paper is by Tharp (92).

3. Algon. Like Na-Dené, this family consists of one group of extremely 
wide distribution, this time from east to west, namely Algonquian, together 
with two isolated languages of northern California, Wiyot and Yurok, the 
former no longer spoken and the latter obsolescent and in line to receive atten-
tion Alonquian is treated in the Sebeok volume by Teeter (91), and as in 
other sections I shall use the opportunities afforded by the imminent appear-
ance of this volume to omit some references.

Several scholars have recently been engaged in descriptive work, and 
their activity is reflected in doctoral dissertations (at least seven from 1968–
1971) concerned broadly with the grammar of a particular language. Among 
those Algonquian languages which are still extant (16 of an original 26), 
Potawatomi and Shawnee are the only ones which have not received fairly 
recent attention. In the wider family, Yurok is obsolescent and in need of 
more detailed description. Recent papers include a grammatical sketch of 
Malecite-Passaquaddy by Teeter (90), discussions of Cree by Wolfart 
(105) and Ellis (20) and of Plains languages by Salzmann on Atsina (77) 
and Frantz on Blackfoot (23) and Cheyenne (24). There is a particularly 
active project centering on Ottawa at the University of Toronto (where the 
language is referred to as Odawa); its current work is suggested by the 
mimeographed volume of papers by Kaye and others (47). Goddard (26) 
depicts how linguistic study can throw light on ethnohistory. Comparative and 
historical studies are in an advanced state in the field thanks to the ground 
work laid some years ago by Leonard Bloomfield, but recent publications are 
merely mentioned in this might suggest. Representative are two discussions of Proto-
Algonquian phonology by Cowan (16) and Goddard (25). There has also 
been a series of Algonquian conferences including other anthropologists as 
well as linguists.

4. Wakashan. This is a compact and closely related family of two divi-
sions, Kwakiutlan and Nootlan, located on Vancouver Island and in contin-
ental British Columbia and Washington. Internal relationship is in fact so 
close as to make the family next to the point in the continuum at which it
would be styled a "language isolate." One of the languages, Kwakiutl, is known to us as the main focus of Boas' field work. He left us thousands of printed pages which, however, are badly in need of more structured reinter- pretation, a task for which new fieldwork would provide a shortcut. The only recent publication I know of is by Haas (29), based on fieldwork of many years ago. Jacobsen, a Haas student, has studied Nootka (44) and done fieldwork on Makah. Here is a group on which more modern information is badly needed. This family is one of those discussed in Thompson's wide- ranging paper on the Northwest (93).

5. Salishan. This family of around 15 languages in British Columbia and the northwestern United States is currently benefiting from more research activity than any other, both descriptive and historical, with a particularly large number of recent doctoral dissertations and several more in process. Fortunately, there is a large and bibliographically detailed paper in the Sebeok book, by Thompson (93), who may fairly be said to be responsible for the recent surge of work in the field. This paper is on the northwest in general, and covers a wider area than Salishan proper, including Wakashan, Chemap- kuan, Kutenaï, as well as contiguous branches of Penutian and Na-Dené. Salishan is a particularly fertile field for studying linguistic history in process, for the family consists of several more or less continuous dialect chains, with relative locations of the languages apparently little altered over a long period of time (somewhat similar to the situation of northern Athapaskan). Salishanists have recognized the general interest of this situation, and work on areal distributions and their structure has also been going on at a rapid and increasing tempo. A series of conferences on Salish languages have been held from 1966 on Thompson's initiative, and papers from the fifth conference held in 1970 have been reproduced from typescript in a volume by Hoard & Hess (38) which well conveys the spirit of interesting work in progress. Another center for such work has been the British Columbia Provincial Museum in Victoria, which under the supervision of Randy Bouchard and the direction of David McC. Grubb has sponsored literacy projects and scholarly work concerning British Columbian Indians (extending beyond Salishan, of course).

One hopes that the many detailed descriptive studies reported by correspondents will be published. Already a number of papers based on such data have appeared in print but, as usual, primary sources are hard to come by. Among recent descriptive papers of importance, Newman's analyses of Bella Coola (71–73) based on former fieldwork may be mentioned first. Kuipers has produced a large study of Squamish (54; see also 48). Straits Salish has been the subject of work by Thompson & Thompson (94, 96). The former paper, on the process of metathesis, has implications of interest beyond Salishan. Snyder (86, 87) has published on Puget Sound Salish, and Thomas M. Hess has more than usually extensive unpublished materials on these languages, principally Snohomish. Drachman (19) has written a generative phonology of Twana, and he has collected much more material not yet in
print. There is a recent grammatical sketch of Kalispel by Vogt (103), based on fieldwork from more than 30 years ago.

Excellent and thorough work towards the history of Salishan is also extensive. Recent papers include a study of subgrouping by Elmendorf (22), work on vowel reconstruction by Kinkade & Sloat (50), and Kuipers (55) begins a project for an etymological dictionary. On typology, the Thompsons have an interesting paper (95) on the violation of supposedly universal constraints on nasal consonants in certain Salishan languages. Several Salishan languages are obsolescent, but efforts toward the fullest possible description are under way and well organized under the direction of Thompson, Kinkade, and others.

6. Penutian. Until recently considerable skepticism was warranted on the historical validity of Sapir's Penutian hypothesis, which linked five groups of languages centering on the present state of Oregon, and included Tsimshian in British Columbia and a major, closely-related family in California as well. The family is indeed a diverse one and many details remain to be worked out, but a rash of new descriptive work in the 1960s and continuing comparative studies of this and earlier materials have led to a growing consensus among Penutianists that this is a real entity. Much less certain is Sapir's suggestion that certain Mexican languages are also included in Penutian. This wider relationship will not be discussed here. Two articles in the Sebeok volume treat recent work in Penutian: that by Thompson on the Northwest (93) and a paper by Shipley (81a) on California.

Outside of California there is a particularly large amount of unpublished material, especially that of the late Melville Jacobs, who called attention to its extent in a paper (42) published shortly before his death. Work in progress includes that of Rigsby on Tsimshian (Nass-Gitksan) and Silverstein on Chinookan (Wishram-Wasco). Hymes also has extensive data on Chinookan, and Aoki (2) has recently published a grammar of Nez Perce. Inside California the flow of descriptive materials seems to have ebbed over the last few years, but one may cite studies which reflect much larger amounts of material not yet in print: Ulan (100), a dictionary by Callaghan (10), reviewed by Silverstein (84), Beeler's work on Yokuts (5), as well as a semantic study by Aoki (3).

Historical Penutian is finally beginning to get its due, with such recent papers as those by Shipley (82, 83), and more in store including extensive unpublished studies by Silverstein. A paper by Beeler (6) reflects work with earlier written records, a concern too often ignored by scholars. There has been a very interesting series of papers on vowel harmony in Sahaptian (taken by Sapir as a typological mark of Penutian), summarized by Rigsby & Silverstein (76). Not strictly Penutian but connected is work on Chinook jargon, a lingua franca of the Northwest, by Silverstein (85).

7. Hokan. Even more so than Penutian, Hokan is a series of single languages or groups with unusually great distance between them. Although workers in the field seem generally to accept the family, this situation makes
it more than usually difficult to sort out genetic and diffusional elements. There are further difficulties because some extinct branches are very meagerly recorded. As Bright (7) has noted, Hokan thus raises general problems in historical linguistics.

The background of work on California Hokan languages is sketched in the Shipley paper on California (81a) already mentioned. Voegelin & Voegelin (101) discuss other Hokan work in their paper for the Sebeok volume on Southwestern and Great Basin languages. Although Hokan is one of the most active fields of research, recent published work is slight. The paradox is resolved by the prediction that a renaissance is under way in Hokan studies. Unpublished work is reported to be greatest in Pomo and Yuman, the most diversified subgroups. Because Yuman languages are those most actively spoken, it is not surprising that a Yumanist, Margaret Langdon, is a leader in the current revival. Her grammar of Diegueño (59) is the only major recent published descriptive work. Langdon was also primarily responsible for a First Hokan Conference held at San Diego in 1970, whose proceedings are reported by Shipley (81a). Beeler (4) discusses a process in Chumash, on which he has much more material yet to appear. Other major work still unpublished is that of Robert Oswalt and of Sally McLendon on Pomo languages.

Comparative studies including those by Wares (104; reviewed by Langdon 60) and Langdon (58), are a foretaste of more to come on Proto-Yuman. Wider Hokan considerations are treated by Jacobsen (43), who also discusses syntactic devices.

8. Gulf. In the early 1950s, Haas proposed the unification of Natchez-Muskogean and Tunica (Tunica-Atakapa-Chitimacha) into a family to be styled the Gulf languages, though from the beginning she has also suggested wider comparisons. This is discussed in Haas’ paper on the Southeast (30), but here and elsewhere she has also implied that wider connections (e.g. with Siouan) may make Gulf not a unified family after all. Nevertheless, one’s impression is that her evidence for Gulf is much more direct than that for putative lateral connections, and hence I list it as a family, noting the need for further work.

All of the languages but those of the Muskogean family are extinct or moribund, and it is to Haas that we are primarily indebted for modern work on these languages, as well as on Tunica and Natchez; she has substantial unpublished material still in her possession. Recent fieldwork is reported by Yolanda Raffo on Mikasuki and by Muriel Saville, of the University of Texas at Austin, on Alabama. Choctaw has been studied by T. Dale Nicklas, of the University of Kansas, and Chickasaw by William Pulte. I can find no publications on the family, descriptive or comparative, since Rand’s in 1968 (75), and would conclude that here is a place where new work would be more than welcome.

9. Siouan. Here is another linguistic stepchild, a surprising one in view of Sioux fame and the fact that five of seven languages of this family of the Plains and Southeast are still actively spoken, one (Dakota/Lakota) with
over 15,000 speakers. Yet since Matthews' pioneering generative study of Hidatsa in 1965 (64), I am able to find no published items beyond those on Crow by Kaschube (45, 46) and by Haas (28) on the last-remembered words of now extinct Biloxi, a Southern language. Robert C. Hollow Jr., of the University of North Carolina at Chapel Hill, has a manuscript dictionary of Mandan, and Matthews (65) seems to have written the only recent comparative paper. There is a large amount of older published data and a rich field of important research here. A paper by Chafe (13) in the Sebeok volume deals with Siouan, Iroquoian, Caddoan, and possible interrelationships.

10. *Iroquoian*. The family divides into a number of closely related dialects centered in northern New York and the more divergent Cherokee spoken originally in the area of North Carolina. The only recent paper known to me on the northern languages, extensively described by Lounsbury and his students, is Chafe's (12) on Onondaga. Cherokee, surprisingly, has not been described in detail, and comparative work is scarce. Within northern Iroquoian, relationships are particularly close, and it may be that the fine-grained work thus required to study true history has discouraged scholars, though there are details in earlier work by Lounsbury (62). Here again is a family needing attention.

11. *Caddoan*. The four Caddoan languages are now spoken in Oklahoma (Caddo, Wichita, and Pawnee) and in North Dakota (Arikara). The only publication on the languages since Taylor's (88, 89), in which the classification and history of the languages was discussed, is that by Bucca & Lesser (9), based on notes some 40 years old. A recent Berkeley dissertation by David Rood on Wichita is reported by Chafe (13), as well as work in progress on Pawnee by Douglas Parks. Chafe also reports fieldwork in process, but nonincidental publication is still lacking.

12. *Kiowa-Tanoan*. This is a closely related family, on the borderline of what one would call a language isolate, located in New Mexico pueblos and Oklahoma (Kiowa). It has often been proposed that it is related to Uto-Aztecan (13). Details are lacking, however, and in their absence it seems best for the present to treat the families as separate. Recent work has been done on Tiwa by F. H. Trager, partially reflected in a recent paper (97), and by Leap (61). The major descriptive and comparative work in the family is due to G. L. Trager, from whose papers we may cite two (98, 99) on chronology of the languages. Hale (32) has studied the protophonology of Kiowa-Tanoan (Kiowa, incidentally, seems to have received no recent attention). Voegelin & Voegelin (101) discuss the family in their paper in the Sebeok volume.

13. *Uto-Aztecan*. This family extends through the western desert area from as far north as Idaho down through the Great Basin and southwestern United States into Mexico, with perhaps ten languages in the United States. There is some work on every language, and the history of the family has been worked out in outline for the phonology at least. A leader in research has been Wick R. Miller of the University of Utah. Important descriptive
studies of the past few years include two by Hale, one of which (33) is part of a much more detailed study of Papago syntax, and the other (34) on Papago laryngeals, which utilizes Papago data for reconstruction. Papago is thrice blessed, with a large dictionary due to appear, including an extensive grammatical introduction by Mathiot (63), and another dictionary of Papago and Pima published by Saxton and Saxton (80).

A fundamental study of Uto-Aztecan phonology appeared in 1962 (Voegelin, Voegelin & Hale 102). More recently phonological questions have been pursued by Miller (66), who gives cognate sets, and by Langacker (57), whose work on the vowel system is summarized and new suggestions made. Crapo (17) reports a comparative study which goes beyond phonology, and Miller (67) discusses western Shoshoni dialects. Clearly both close-grained and longer range comparison as in these various papers are needed, and their integration will lead to true history. An unpublished step towards this end is a massive undergraduate thesis done at Harvard in 1971 by Jeffrey Heath, in which phonological class-markers of verbs are carefully followed through all branches of the language. Other work is summarized in the paper by Voegelin & Voegelin (101) on Southwestern and Great Basin languages.

14. Language isolates. A word of caution is in order in this discussion. Beyond doubt there are languages which have become extinct without ever having been recorded, and there are many language names which cannot be placed. Therefore it is in part an accident that extinct languages such as Beothuk, Karankawa, and Timucua are mentioned in this list and not others.

(a) Beothuk was once spoken in Newfoundland. Records are sparse, but a link with Algonquian has been repeatedly discussed. Recently Hewson (36, 37) has collated all available data and reopened the question of this link. Systematic evidence is still lacking, however, and the question may never be solved.

(b) Kutenai is still spoken in the region of present northern Idaho. It received modern study in the 1940s and 1950s, and affiliations with Algonquian as well as Wakashan and Salishan have been suggested.

(c) Chemakuan consists of a closely related pair of languages, one extinct, in northwest Washington. Several scholars have worked on the surviving member, Quileute, but there are no recent publications. Thompson (93) discusses work both on Chemakuan and Kutenai.

(d) Yukian consists of two obsolescent languages of northern California, and has been a truly deep genetic mystery, with relationships proposed to Penutian, Hokan, and Siouan, most recently to Siouan by Elmendorf (21). Published description to test these hypotheses is lacking, and besides much deeper historical work on the proposed congener would be required. The only up-to-date descriptive source is Sawyer (79), and we hope for more.

(e) Karankawa is an extinct and little known language of the Texas Gulf coast. There has been no recent work, and its affiliation is uncertain, though relationship with Gulf, among others, has been suggested. A 1968 paper by
Landar (56) reports discovery of a short vocabulary of the language and compares it, inconclusively, to Cariban.

(f) Keresan is a closely-knit group spoken in New Mexico pueblos. The languages are extant and have been described in the 1960s, but no clear suggestion of a genetic affiliation has arisen.

(g) Yuchi is still spoken, now in Oklahoma, and a relationship to Siouan has been repeatedly proposed, but with little evidence. New work is being done on the language by James M. Crawford of the University of Georgia, and we may therefore hope for data which would render possible a rational comparative study.

(h) Timucua is an extinct language of Florida. According to Haas (31) there is sufficient material for restudy. Affiliations to Muskogean, Siouan, and Arawakan have been suggested.

(i) Zuni is the language of an important New Mexico pueblo. It has been studied by Newman, who published a dictionary (68) and a grammar (70) and in 1964 a set of comparisons with Penutian (69). Intriguing as this and other hypotheses are, all must await comparison, study, and historical reconstruction of the putative links.

**Wider Relationships**

The future will undoubtedly turn up genetic as well as diffusional interrelationships among these postulated families, but the task will necessarily be more difficult than that of detailing the history of various languages and language families, for it will necessitate reconstructed languages and historical postulates. To explain resemblances, history may call for original unity (meaning that this is as far as we can go at a given time), or for a demonstration of later diffusion, so our comparisons cannot be dignified as historical except at the point from which we may show how what we have came to be. Whatever we do not know then becomes prehistory, but much remains to be done in American Indian linguistics before such a confession is necessary. For a treatment of current sensible hypotheses as to wider relationships I refer the reader once again to Haas’ (31) definitive study of the state of that art entitled American Indian Linguistic Prehistory.


13. Chafe, W. L. Siouan, Iroquoian, and Caddoan. See Ref. 81


16. Cowan, W. 1969. PA *a*, *k* and *t in Narragansett. *Int. J. Am. Ling.* 35:28–33


30. Haas, M. R. The Southeast. See Ref. 81

31. Haas, M. R. American Indian linguistic prehistory. See Ref. 81


52. Krauss, M. E. Eskimo-Aleut. See Ref. 81
53. Krauss, M. E. Na-Dené. See Ref. 81
paradigms. *Int. J. Am. Ling.* 35:299–306


81a. Shipley, W. California. See Ref. 81


91. Teeter, K. V. Algonquian. See Ref. 81


93. Thompson, L. C. The Northwest. See Ref. 81


101. Voegelin, C. F., Voegelin, F. M. Southwestern and Great Basin languages. See Ref. 81


AUTHOR INDEX

Blake, R. L., 62
Blanc, M., 65
Blanton, R. E., 161
Blau, P., 254, 258, 259
Bloom, M., 222, 235, 317, 318, 320, 321
Blochova, L., 67
Bloomfield, L., 365
Bluhm, E., 130, 161
Blumer, H., 263
Boas, F., 227, 236, 240, 333
Bock, E. W., 56
Bock, P., 394, 401
Bodmer, J. G., 66
Bodmer, W. F., 66, 169
Bodzsaar, E., 73
Boev, P., 77
Bogonova, V., 74
Bohannan, L., 237, 241
Bohatova, J., 65
Boflen, J. G., 81
Boles, C. L., 394, 401
Boissevain, J., 259
Bojactitjev, E., 75
Bokarins, L. V., 63
Bolinger, D. L., 374
Bolzs, H., 76
Bonosz-Raskan, S., 68
Bonne, B., 60-63, 65
Bonte, M., 57
Booth, P. B., 62, 63
Borah, W., 157-59, 161
Borrie, W. D., 56
Boserup, E., 163, 165, 211
Boston, J. S., 241
Bott, E., 259
Bottini, E., 60, 64
Boulding, K. E., 183
Bowen, P., 59, 64
Bowler, J. M., 38, 45
Bowman, J. E., 60, 61
Boyce, A. J., 82, 169, 172
Boyd, J. P., 325
Boyle, E. Jr., 63
Boynton, J. W., 56
Brace, C. L., 28
Braidwood, R. J., 128
Brain, C. K., 33, 153
Brane, M. K., 377
Braroe, N. W., 239
Breedlove, D. E., 274, 277, 386-88
Brejcha, M., 75
Bresler, J. B., 57
Bresnan, J. W., 372, 375
Breyan, G., 74
Brichard, M., 70
Bricker, V. R., 392
Bright, W., 394, 413, 418
Brill, R. H., 115, 121
Brinkmann, B., 60-62
Brodar, V., 67
Brunniesstam, R., 64
Brookfield, H. C., 221
Broom, R., 29, 33
Brose, D. S., 137
Brothwell, D. R., 77, 113, 118, 160
Brown, F. H., 34
Brown, J. A., 133
Brown, K. S., 62, 64, 65
Brown, R., 232
Brown, W. H., 82, 155
Brunner, E., 59
Bryan, A. L., 38
Bucca, S., 419
Buch, V., 118
Buchler, I. R., 248, 249, 251, 256, 261, 313, 343, 344, 398
Buck, A. A., 60, 70
Buckley, W., 265
Buettner-Janusch, J., 64, 66
Buettner-Janusch, V., 64, 66
Bugyi, B., 76
Buist, A., 134
Bullard, W. R., 131, 141
Bulmer, R., 331
Bunak, V. V., 63
Buragohain, S. N., 67
Burger, J. F., 285
Burling, R., 259, 276, 321, 324, 325, 326, 404
Burr, W. A., 75
Barridge, K. O. L., 340
Buskirk, E. R., 79
Büttler, R., 59, 60
Butzer, K. W., 38, 115
Bůžková, P., 76
Bytheway, W. R., 75
C
Cadete Lette, A., 77
Cahn, A. J., 70
Callaghan, C. A., 417
Camoens, H., 60, 64
CAMPBELL, B. G., 27-54;
28-30, 37, 42, 43, 46, 47
Campbell, J. M., 146
Campbell, R. D., 208
Campusano, C., 58
Canclan, F., 231
Caplow, T., 260
Cardenas, G., 69
Cardoso de Oliveira, R.,
155
Caries-Trochan, E., 60
CARMAK, R. M., 227-46;
239
Carneiro, R., 132, 248
Carneiro, R. L., 163, 165
Carney, J., 35
Carrel, R. E., 60
Carr-Loch, D. L., 67
Carter, J. E. L., 77
Casado, A., 61
Casagrande, J. B., 271, 275, 292, 303
Caso, A., 241, 242
Castle, B. L., 60
Caschi, M. N., 66
Cavalli-Sforza, L. L., 169
Cedergren, B., 64
Chafe, W. L., 333, 343, 359, 360, 361, 413, 419
Chagnon, N., 146
Chagnon, N. A., 58, 59, 156, 169
Chai, C. K., 67, 70
Chakravartti, M. R., 63, 66
Chambers, M. A., 70
Chan, K. L., 60
Chandhuri, S. K. R., 62, 65
Chandra, R. K., 68
Chang, K. C., 129, 131, 146
Chao, T. K., 36
Chapin, P. G., 357
Chapman, A. M., 58
Chapman, G. R., 32
Chapple, E. D., 6, 9, 17
Chariton, T., 141
Charzewski, J., 77
Chattopadhyay, P. K., 67, 77
Chaudhury, R. H., 56
Chaves, J., 67
Chen, S.-H., 66
Chia, L. P., 36
Chien, R., 268
Childe, V. G., 136, 141
Cholnoky, P., 67
Chomasky, N., 279, 351, 352,
354-56, 258, 363-65, 367,
372, 374
Chopra, S. R. K., 62
Choudhury, M. A., 56
Chruszter-Spruch, H., 71
Chung, C. S., 71
Clark, D. L., 113
Clark, H. K., 356
Clark, J., 67
Clark, J. D., 40
Clark, J. G. D., 136, 137
Clark, W. E. le G., 28, 29
Clarke, D., 184
Clarke, K. W., 240
Clarke, L. M., 69
Clarke, M. W., 240
Clarke, R. J., 33, 35
Clarke, W. C., 219, 221, 222
Clegg, E. J., 78
Clegg, J. B., 66
Clements, V. R., 60-62
Cliquea, L. R., 57
Coe, M. D., 131, 141, 161
Coe, W. R., 160
Cohen, A., 259
Cohen, B. H., 63
AUTHOR INDEX

Cohen, D. W., 241
Cohen, T., 65
Cohen, Y., 248
Cohn, B. S., 235, 236
Colby, B. N., 272, 386
Collins, G., 40
Collins, J., 8
Collins, O., 8
Colton, H. S., 161
Conger, P. R., 78
Conklin, H. C., 386, 388
Conrad, R. J., 350
Conti, C., 59
Cook, E. D., 414
Cook, G. C., 69
Cook, P. J. L., 82
Cook, S. F., 132, 157-59, 161
Cook, H. B. S., 33
Coombs, L., 57
Coon, C. S., 9, 17
Coope, E., 67, 68
 Coppens, Y., 34
Corney, G., 62, 68
Cornoni, J., 63
Correnti, V., 72
Coser, L., 262
Cossman, J., 57
Cotes, J. E., 77, 78
Coult, A. D., 395, 402, 403
Cowen, W., 415
Cowgill, G., 133
Cowgill, G. L., 114
Crapo, R. H., 420
Crawfoito, J., 70
Crawford, M. A., 70
Crhák, L., 73
Crhákóva, E., 73
Cristescu, M., 73, 75
Crockford, G. W., 80
Crogner, E., 70, 72
Cross, H. E., 55, 155
Crow, J. F., 171, 172
Cruz-Coke, R., 169, 172
Csikszentmihalyi, M., 250
Cunnison, I., 237, 241
Curr, E. M., 313
Curran, C. C., 65
Curtis, G. H., 30, 36, 120
Curtis, L., 344
D

Dahlberg, A. A., 71
Dalrymple, G., 120
Dalton, G., 260
Damas, D., 207, 210, 214
Damjanovski, J., 72
Damon, A., 75, 76
D'Andrade, R. G., 277, 303, 318, 324, 341, 388
Dander, F., 360
Daniel, G., 179
Danilkovich, N. M., 73
Danson, E. B., 161
Darlu, P., 74, 81
Darnell, R., 412
da Rocha, F. J., 67
Dart, R. A., 29
Dart, S. J., 59
Das, B. M., 66, 67, 69, 75, 77
Das, B. S., 62
Das, M. S., 75
Das, P. B., 67, 77
Das, S. K., 65
Das, S. R., 62
Das-Choudhury, A., 68
Datta, P. K., 67
Davenport, W., 248, 251, 262
David, T. J., 68
Davies, C. T. M., 77, 80
Davila, G. H., 74
Davis, M., 250, 261, 262
Davivos, V., 67
Day, M. H., 36, 38, 39
De, B. 7
de Almeida, A., 62
de Azevedo-Fialho, M. G. N., 59
de Bartolo, M., 62
DeCamp, D., 369-71
De Castro and Almeida, M. E., 62
Deetz, J., 133, 388, 393
de Filippis, A., 61, 74
de Graff, A. C. Jr., 79
de Heinzelin, J., 34
Debruck, H., 66
De Licardie, E. R., 70
Delithesos, A. K., 61
Dell'Acqua, G., 61, 74
Delorme, E., 375
de Luca, L., 65
de Lumley, H., 37, 42
de Lumley, M. A., 37
Denevan, W. M., 220
Denich, B., 254
Denisova, R. Ya., 67
De Oliveira, A. E., 171
de Oliveira, R. C., 58
Des, M. G., 70
Desai, P., 69, 72
Despres, L., 254
Devereux, E., 250
Dev, B., 69, 75
de Villiers, H., 87
DeVore, I., 153, 155-57
Dezsos, G., 75
Diaz, J. M., 62
Dick, W., 63
di Prampero, P., 81
Dissing, J., 62, 63
Dittert, A. E., 130
Dixon, M., 68
Dixon, R. M. W., 373
Dobyns, H. F., 158
Dodinval, P. A., 63
Doran, J., 184, 185
Dorson, R. M., 240
Dougherty, R. C., 358, 372, 375, 376
Douglas, M., 336
Doujol, A., 81
Drachman, G., 416
Dräghicescu, T., 62
Drdová, A., 61, 65
Dreyfus, H. L., 287
Drobná, M., 73
Drozdowski, Z., 76
Dubois, E., 36
Dubois, J., 358
Ducros, J., 67
Dumond, D. C., 163
Duncan, G., 136
Dun, O. D., 172
Dun, F. L., 157, 170
Dunn, L. C., 170
Dunn, S. R., 170
Dunnell, R. C., 115
Durbin, M., 272
DURBIN, M. A., 383-410; 394, 401
Durbin, M. E., 386-88, 394, 401, 403, 405, 406
Dutt, J. S., 171
Dutta, P. C., 67
Dyke, B., 156
E

Echeverry, L. E., 69
Eckland, B. K., 170
Eddy, F. W., 130
Edgerton, R. B., 208, 217
Edholm, O. G., 70
Edwards, Y. H., 66
Eggan, F., 228
Ehnholm, C., 65
Eiben, O., 75
Eiben, O. G., 72, 73
Eliafe, M., 16
Elkins, R., 351, 394
Ellis, C. D., 415
Elmendorf, W. W., 417, 420
El-Sallghal, M. O., 69
Elston, R. C., 63, 82
Enächescu, T., 82
Engel, M., 75
Erasmus, C., 251, 260
Erickson, R. P., 58
Eriksson, A., 59
Eriksson, A. W., 59, 60, 63, 97, 98, 83
Espinal, F., 69
Eto, M., 74
Evans-Pritchard, E. E., 216, 229, 231-33, 239-41, 255
Eveleth, P. B., 71
Evernden, J. F., 30, 120

F

Facchin, F., 64
Fagan, R. E., 184
AUTHOR INDEX

H
Haas, M. R., 416, 418, 419, 421
Hacham, N., 67, 80
Hagedorn, W., 272, 274, 276, 277
Haight, J. S. J., 80
Haisman, M. F., 76
Hajniš, K., 74, 75
Halberg, F., 81
Halder, A., 57
Hale, K., 419, 420
Hale, K. L., 271, 275, 292, 303, 420
Hall, A. D., 184
Hall, E. T., 384
Hall, K. R. L., 153
Halle, M., 389, 398
Halm, N., 74
Halpern, C., 63
Hamberg, D. A., 154
Hamilton, M. C., 71
Hamley, E. J., 78
Hammel, E. A., 324, 325, 386, 394–96, 404, 604
Hammond, A. H., 182
Hammond, B., 318, 321
Hanifara, K., 71
Hanna, J. M., 80, 81
Hansen, I. L., 70
Harada, S., 64
Harms, R. T., 387
Harner, M., 250
Harner, M. J., 163, 165, 211
Harpending, H., 171
Harris, D. E., 58, 169
Harris, D. R., 201, 220
Harris, H., 62, 66
Harris, M., 207, 209, 210, 227, 231, 250, 384, 385, 402
Harris, Z. S., 387, 388
Harrison, G. A., 68, 78, 80, 83, 169, 172
Harrison, J. M., 76
Harrison, T., 38
Harvey, R. G., 60–62, 77
Harayashida, Y., 55, 63
Haskell, E. H., 11
Hastings, D. W., 56
Hatayama, A., 72
Hauge, M., 64
Hauser, P. M., 172
Haviland, W., 137
Havlova, Z., 75
Hay, R. L., 34
Heer, D. M., 56
Heeren, H. J., 56
Heilberg, A., 63
HEIDER, K. G., 207–26; 208, 221, 222, 257
Heidolph, K. E., 376
Heiken, A., 64
Heinreich, A., 392
Heinz, H. J., 75
Heizer, R. F., 132

Hellström, B., 81
Heltne, P. G., 60
Henderson, R. N., 394
Henin, R. A., 56
Hennig, W., 60–62
Hennigh, L., 212
Henningsen, K., 60
Henriques, F. P., 75
Herbout, J. C., 81
Henry, D., 414
Henry, J., 265
Herman, Y. F., 70
Hermann, R. F., 74
Herald, J., 130
Heron, C. E., 69
Herskovitz, M. J., 229
Hertz, R., 333
Herzog, M. I., 362, 370
Herzog, P., 61, 65
Hess, T. M., 416
Hester, J. J., 130, 141
Hewson, J., 420
Hey, E. N., 80
Hernaux, J., 82
Higgs, E., 113, 118
Hill, A., 35
Hill, J. N., 133, 137, 182, 183
Hillman, R. W., 72
Hine, V., 254
Hinko, E., 73
Hiorns, R. W., 82, 169
Hirschhorn, H. H., 67, 77
Ho, P. – T., 137
Hoard, J. E., 416
Hobler, P. M., 141
Hocart, A. M., 314, 316
Hockett, C. F., 362, 402
Hodson, F. R., 114
Hoffstetter, R., 36
Hogg, A. H., 136
Hogland, C., 64
Holjer, H., 415
Hole, F., 162
Holland, P. V., 64
Holloway, R. L., 33, 35
Holmgren, G., 64, 65
Holmans, G., 258
Hopkins, N. A., 394
Hopkinson, D. A., 62, 66
Hoppe, H. H., 60–62
Hornbrook, R. W., 83
Horowitz, M., 251
Horvath, S. M., 81
Householder, F. W., 362
Howell, F. C., 29, 33–36
Howells, W. W., 35, 82
Howitt, A. W., 312
Hoven, R., 414
Hsu, J., 36
Huang, W., 121
Hubert, H., 333
Hughes, A. R., 32
Hulzinga, J., 250
Hulse, F. S., 75, 171
Hunt, R., 255

Hunt, V. V., 5
Huntsman, J. W., 321
Huq, F., 77
Husain, I. Z., 56
Hussein, F. H., 38
Hussel, L., 58
Hutchinson, W. J., 358
Hymes, D., 271, 272, 288, 331
Hymes, D. H., 412

I
Iba, B. Y., 62
Igbuzurike, M. U., 220
Ikemoto, S., 62
Ikin, E. W., 63, 66
Imai, M., 58, 68, 82, 169
Ingram, D., 375
Inamani, S., 70
Irigaray, L., 358
Isaac, G. L., 34, 40, 41
Ishimoto, G., 61, 64
Ito, A., 56
Ivanka, K., 73

J
Jackendoff, R. S., 351, 356, 376
Jackson, C. E., 63
Jackson, R. T., 221
Jacob, T., 36, 77
Jacobs, M., 417
Jacobs, P. A., 59
Jacobson, W. H., Jr., 416, 418
Jacquard, A. M., 58
Jaeger, G., 81
Jain, A. K., 56, 57
Jakobi, L., 62
Jakobson, R., 389
Jakobsson, A., 60
James, A. G., 131
James, G. T., 30
Jamison, P. L., 70, 74
Jantz, R. L., 67, 76
Jasicki, B., 72
Jaulin, R., 338
Jaura, R., 67
Jenkins, T., 65, 171
Jeurissen, A., 73
Jhala, H. L., 63, 66
Jilek, W., 344
Jilek-Aall, L., 344
Jimenez-Moreno, W., 237
Johannessen, C. L., 220
Johnson, R. S., 64, 65
Johnston, F. E., 58, 67, 74, 76, 155, 172
Jolly, C., 45
Jones, G. R. J., 136, 141
Jones, P. R. M., 76
Jones, R., 38, 45
Jones, R. T., 59
Joo-Szabados, T., 62
AUTHOR INDEX

Jordanov, J., 77
Juberg, R. C., 63
Jürgens, H. W., 67, 73-75

K

Kadanoff, D., 72
Kageyama, S., 65
Kahan, A., 136, 141
Kaliszewska-Drozdowska, M. D., 72
Kalla, A. K., 68
Kanashiro, V. K., 67
Kanda, S., 72
Kang, B. S., 81
Kanerano, R. L., 74
Kaplan, B. A., 169
Kaplan, D., 230
Kapria, Z., 75
Karkal, M., 57
Karlsson, J., 81
Karsten, P., 39, 45
Karttunen, L., 367, 368, 374, 375
Kaschube, D. V., 419
Katz, G., 80
Katz, J. J., 274
Katz, J., 365
Kay, P., 277, 293, 303, 375, 386, 387, 403, 404
Kaye, J., 415
Keatinge, W. R., 80
Keenan, E. L., 365, 375, 376
Keesing, R., 261, 316
Keesing, R. M., 271, 394, 405
Kelley, H., 248, 258, 264, 265
Kelly, R., 219
Kemp, W. B., 222
Kennedy, J., 134, 141
Kennedy, K. A. R., 38
Kenessinger, K. M., 67, 76, 115, 172
Kersan, M. M., 61
Kerley, E. R., 160
Khalifa, S., 67, 80
Khayanova, A. B., 82
Khit, G. L., 67
Kiefer, F., 360
Kimura, K., 74, 80
Kinkade, M. D., 411, 416, 417
Kins, H., 65
Kiparsky, C., 366, 374, 376
Kiparsky, P., 366, 374, 376
Kirjarinta, M., 63-65, 67
Kir, R. L., 60-62, 64, 65
Kirby, A. V. T., 167
Kirkby, M. J., 187
Kirsch, A. T., 240
Kissmeyer-Nielsen, F., 59, 60
Kissmeyer-Nielsen, K., 66
Kitchin, F. D., 65
Klissouras, V., 68
Klotz, K., 59, 61
Kluckhohn, C., 248

Kluger, R., 74
Klujber, L., 67
Knepper, F., 64
Knibbs, A. V., 78
Knip, A. S., 80
Knudsen, J. B., 62
Knussmann, R., 77
Knussmann, V., 75
Kobylansky, Ye. D., 77
Kochman, T., 257
Koeher, H., 76
Koford, C. B., 154
Koller, R. D., 59
Komatsu, I., 59
Konaszewska-Hymarkiewicz, K., 72
Kongas Maranda, E. K., 331, 333, 334, 336-38, 340-44
Kooke, E., 60-62
Kopeč, A. C., 60-62
Korfel, A., 76
Korn, F., 312, 315, 318, 325
Kotsakis, P., 61
Kozelka, R., 251
Kralovicová, A., 73
Krause, V. M., 77
Krauss, M. E., 413-15
Kreitner, P. C., 56
Kretzol, M., 42, 116
Krieger, H., 59, 77, 82
Kroebel, A. L., 157, 227-29
Krog, J., 80
Krogman, W. M., 159
Krompova, Z., 74
Krotki, K. J., 55
Kruger, J., 63
Kryzwicki, L., 181
Krzészko, M., 82
Kuhn, E. S., 179
Kulpera, A., 416, 417
Kumar, N., 62
Kummer, H., 153
Kuno, S., 375
Kurchenow, C. F., 82
Kurnik, G., 75
Kuroda, S.-Y., 352, 354, 366, 375
Kurth, G., 75
Kusner, G., 185

L

Labov, W., 362-64, 370, 371
Lakoff, G., 272, 278-80, 282, 283, 302, 365-67, 374, 375, 377
Lakoff, R., 357
Läm, T. H., 75
Lamb, S. M., 325, 387, 401
Lamm, L. U., 59, 60, 62
Landar, H., 421
Lane, M., 338, 342

Lang, A., 313, 314, 316
Langacker, R. W., 361, 394, 399, 401, 420
Langdon, M., 418
Langendoen, D. T., 376
Lanning, E. P., 161
Lapitisky, F. G., 72, 73
Lasik-Mierzewska, T., 73, 80, 82
Laskier, G. W., 81, 169
Laughlin, W. S., 170
Lauridsen, U. B., 65, 70
Lavelle, C. L. B., 71
Layton, M., 344
Lázár, A., 67
Lazo, B., 58
Lea, H., 250
Leach, E., 247, 252, 255, 256
Leach, E. R., 321, 323, 331, 335, 338, 341
Leach, H. M., 134
Leacock, E., 229, 230
Leacock, E. B., 236, 238
Leal, M. J., 317, 318, 321, 322
Leakey, L. S. B., 28, 30, 32, 33, 35, 41
Leakey, M. D., 33, 35, 41
Leakey, R. E. F., 34, 35, 38, 41
Leap, W. L., 419
LeBlanc, S. A., 182
LeClair, E., 260
Lee, C. S. N., 59, 64
Lee, D. R., 208
Lee, G. T. R., 71
Lee, M., 61
Lee, M. M. C., 74
Lee, N. H., 168
Lehn, R. B., 211, 222
Lee, R. D., 156, 167
Lees, R. B., 358
Lehiste, I., 364
Lehmann, H., 63, 66
Lehmann, W., 67
Lehrer, A., 375
Lema, O., 69
Lenfant, C., 79
León Portilla, M., 241
Lesser, A., 419
Levene, C., 65
Lew, R., 58
Lewin, T., 68, 73, 75, 83
Lewis, A. B., 63, 71
Lewis, G. E., 32
Lewis, H. E., 70, 73, 75
Lewis, I. M., 230, 235, 237, 241
Lewis, O., 228
Lewis, W. H. P., 62
Lewthwaite, G. R., 210
Mandler, G., 298
Mandler, S. B., 67
Manners, R., 248, 254
Manners, R. A., 230
Manso, C., 61
Mantel, N., 57
MARANDA, P., 329-48; 330, 331, 333-36, 338-44
Maranigala, M. N., 56
Maratosa, M., 368
March, J., 260, 261
Marchocka, M., 71
Marciukiewicz, S., 68
Marshall, W. A., 70, 73-76
Martensson, L., 65
Martin, A. O., 59
Martin, P. S., 116, 130, 182
Marzdorff, J., 74
Marzuki, A. B., 56
Mason, J. A., 161
Massé, J., 72
Masterson, J. G., 58
Mathiow, M., 420
Matthews, G. H., 419
Mätzner, T., 62, 64, 65
Mauny, R., 236
Maurette, M., 121
Mauss, M., 249, 257, 333
Maybury-Lewis, D., 170, 253, 315
Mayer, A., 259
Mayer, B., 72
Mayerová, A., 65
Mayhall, J., 70, 71
Mayhall, M. F., 71
Mayo, O., 69
Mayr, W., 66
Mazzes, R. B., 78-81
Mazur, A., 261
Mazur, J., 76
McArthur, N., 55, 155
McBurney, C. B. M., 37
McCance, R. A., 80
McCawley, J. D., 280-82, 300, 354, 366, 372, 376
McCranek, R. D., 69
McCreeery, L. D., 74
McCullough, J. M., 69
McDougall, D. J., 122
McHenry, H., 82
McIntire, W. G., 131
McKern, T. W., 160
McKusick, V. A., 55, 155
McNaughton, J. W., 70
Meggitt, M. F., 156
Mehra, B., 64
Meighan, C. W., 115, 123
Melartin, L., 64
Menini, C., 61, 74
Meredith, H. V., 71, 75
Merhauwe, J., 76
Merino, C., 97
Merton, R. K., 232
Messeri, E., 77
Metzger, D., 387, 388
Miall, W. E., 69, 72
Miaškiewicz, C., 74
Michael, B. A., 133, 118
MICHEL, J. W., 133-26; 113, 115, 116, 118, 122, 123, 137, 161
Mickelsen, O., 70
Mickey, M. R., 66
Mills, P. A., 55, 58, 70, 168
Miles, S. W., 131
Milledge, J. S., 79
Miller, J., 357
Miller, J. A., 35, 120
Miller, P. S., 73
Miller, W. R., 420
Millon, R., 133, 137, 161
Mills, C. W., 230, 232
Milton, G. B., 337
Mindeleff, C., 128
Minnick, J., 394
Minsky, M., 283, 286
Mintz, S., 248, 254
Miraglia, L., 75
Misawa, S., 55, 63
Misra, S., 62
Mitchell, J., 249, 259
Mitchell-Kernan, E., 257
Mittra, A. K., 75
Modiano, G., 61, 67, 77, 79
Moehr, M., 76
Mollison, T. L., 30
Mönckeberg, F., 69
Monge, C., 79
Morr, E., 59, 61, 62, 64-67
Morn, J., 63
Montal, E., 59
Moodie, P. M., 60-62, 65
Moody, D. L., 76
Moore, O. K., 16, 248
Moors, H. G., 56
Morath, D., 59, 61
Moravcik, J. M. E., 366
Morena Azorero, L., 63
Moreno, R., 58
Morgan, L. H., 127, 309
Morgan, R. J., 61, 70
Morgenstern, O., 249, 251, 261
Morin, V., 337
Morpurgo, G., 67, 79
Morris, C., 309, 310, 317, 322
Morton, N. E., 58, 63, 65, 82, 169, 171
Moseley, M. E., 161
Motulsky, A. G., 61
Mourant, A. E., 60-63, 65, 66
Mukherjee, B. N., 65
Mukherjee, D. P., 62
Mungall, J. M., 54
Murdoch, G., 256
Murdoch, G. P., 231, 232
Murphy, R., 250
Murra, J. V., 235
AUTHOR INDEX

A

Ornas, M., 261
Orzalesi, M., 60, 64
Otterbein, K., 250
Owen, D. G., 71
Ozimić-Gerber, M., 76

P

Padilla Seda, E., 248, 254
Paglia, D. E., 60
Pakrasi, K., 57
Pai, A., 77
Pai, L., 75
Palatnik, M., 58
Palmarino, R., 60, 64
Palmer, F. R., 362
Palmere, J. A., 56
Paloušová, Z., 60
Palsson, J. O. P., 60, 62, 64
Paolucci, A. M., 67, 79
Pardee, W. H., 221
Parikh, N. P., 63, 66
Pařízková, J., 76
PARSONS, J. R., 127-50; 133, 141, 161
Parsons, P. A., 67
Partee, B. H., 352, 356, 372, 376
Pascal, R., 42
Pateria, H. N., 67
Patterson, B., 35
Patterson, T. C., 161
Pattnak, B., 73
Peacock, J. L., 240
Pearson, H. W., 12, 14, 18
Peel, J., 56
Pellicer, A., 61
Peng, R. C., 37
Penrose, L. S., 67
Perchonock, N., 276, 298
Peritz, E., 63
Perlmutter, D. M., 375, 376
Persson, I., 64, 64, 70
Peters, P. S. Jr., 371
Petrakis, N. L., 61, 67
Pflugshaupt, R., 60
Phenice, T. W., 159
Phillips, P., 128, 234
Piaget, J., 334, 335
Pichan, G., 80
Pickles, H. G., 67
Picon-Reategui, E., 79
Piggot, G. L., 415
Pigram, P., 60, 64
Pike, K. L., 384, 387
Pilibine, D. R., 32, 43
Pilgrim, G. E., 32
Pincherle, G., 76
Pineau, M., 71
Pinto-Cisternas, J., 58
Pittkin, H., 364
Piveteau, J., 37
Planas, J., 62
Plato, C. C., 68
Plog, F., 182

Q

Quilici, J. C., 66
Quillian, M. R., 283-86
Quine, W. V. O., 368

R

Radin, G., 61, 62
Radcliffe-Brown, A., 248, 257, 316
Radu, E., 77
Ragaci-Pilato, V., 76
Rai, K. M., 80
Raffa, H., 249, 264
Rajkai, T., 72
Ralph, E. K., 113, 118-20
Rand, E., 418
Rao, D. C., 77
Rao, M. B., 67
Rappaport, R. A., 166, 208, 222
Rastogi, S., 67
Ratananbolk, K., 62
Ratanastirvanich, P., 62
Raven, P. H., 274, 277, 386-68
Rea, J. N., 72
Read, A. E., 68
Read, C., 251, 252
Read, D., 251, 252
Reddy, S., 66
Redman, C., 182
Reich, P. A., 350
Reichenbach, H., 289
AUTHOR INDEX

Reid, S., 344
Reel, L., 72
Reinskow, T., 64, 66
Rempe, U., 75
Renfrew, C., 123
Rennie, D., 79
Rennie, D. W., 81
Renwick, J. H., 59
Revelle, R., 72
Rex-Kiss, B., 65
Reynafarje, C., 79
Reynolds, R., 34, 38
Reys, L., 61
Richard, P., 338
Richardson, F. L. W., 12
Richardson, J. F., 76
Richardson, M. A., 73, 75
Riederer, J., 122
Riggsby, B., 417
Ritchie, R. W., 371
Ritchie, W. A., 131
Ritsuer, M. S., 67
Ritter, H., 59, 60, 62, 63
Rittner, C., 59, 61
Rivat, C., 65
Rivat, L., 65, 70
Rivers, W. H. R., 316
Robbins, B. L., 351
Robbins, M. C., 137
ROBERTS, D. F., 55-112; 59, 61, 67, 68, 70-73, 79, 80, 155, 170
Roberts, J., 318, 322
Robinson, J. J., 358
Robinson, J. T., 29, 35, 45, 46
Robinson, M. G., 63
Robson, E. B., 59, 62
Roche, A. F., 74
Rochelle, R. H., 81
Rodrigues Grande, N., 77
Roep, T., 354
Rogers, E. S., 214
Roginsky, Ya. Ya., 68
Rohl, F. J., 169
Rohr, V. J., 70
Roisenberg, L., 58, 63
Romano, A., 38
Romney, A. K., 277, 325, 388, 390, 399
Rood, D. S., 358, 359
Ropartz, C., 65, 70
Rose, A., 263
Rosenthal, F., 239
Ross, J. R., 372, 376, 377
Roth, G., 272, 274, 276, 277
Rothhammer, F., 68
Rothhauwe, H. W., 69
Rouse, I. B., 115
Rousseau, P. Y., 65, 70
Rowe, J. H., 158
Rowell, L. B., 60
Roybillis-Berrut, L., 76
Roys, R. L., 161, 239
Rozaire, R., 34, 38
Rozentsweig, V., 336
Rozner, L. M., 72
Rudescu, A., 77
Ruesh, J., 288
Ruffie, J., 65
Runc, D. W., 71
Ruoslahi, E., 59, 65
Ružičkova, J., 75
S
Sackett, J. R., 34, 38
Saengudom, C., 69
Saffilios-Rothschild, C., 56
Saha, N., 61, 78
Sahlin, M., 217, 222, 247, 253, 254, 257, 258
Sahlin, M. D., 159, 217, 227
Salák, J., 60
Saldana, P. H., 68
Salinas, C., 58
Saltarelli, M., 388, 394, 401, 405
Salli, F., 59
Salzano, F. M., 58, 67, 80, 155, 169-71
Salzmann, Z., 415
Samposon, G., 350
Sanchez, C., 97
Sandy, P. R., 277
SANDERS, W. T., 151-78; 116, 130, 131, 133, 137, 141, 158, 160, 161, 163
Sandison, A. T., 160
Sanger, R., 62
Sanghvi, L. D., 170
Sankalia, H. D., 37
Santachiara Benerecetti, S. A., 62
Sapir, E., 236, 411, 412
Saramonio, A., 75
Sarich, V. M., 32, 43
Sarkar, D., 57, 67
Sarkar, S. S., 67
Sartono, S., 36
Sasmal, B., 57
Sauerlich, H. E., 70
Saur, C., 158
Savage, D. E., 30
Savard, R., 337, 343
Sawyer, J. O. Jr., 420
Saxton, L., 420
Saxton, O., 420
Scheael, R. P., 161
Schaller, G., 153
Schanfield, M. S., 65
Scheele, R., 248, 254
SCHFFLER, H. W., 309-28; 309, 312, 313, 315-19, 321, 322, 324, 325
Schelling, T., 251, 257, 263
Scheper, E., 272, 274, 276, 277
Scheper, G. W. H., 29
Scherz, R., 60
Schmidt, H., 77
Schneider, D. M., 309-11, 315, 317-23
Schneider, H., 260
Schnell, G. D., 169
Scholes, F. V., 239
Schull, W. J., 59, 63, 156, 159, 171
Schultz, J., 64, 65
Schultz, N. W., 388, 402
Schulze, L., 310
Schwartz, R. M., 285
Scott, E. M., 60, 61
Scozzari, R., 64
Scragg, R. F. R., 68
Sears, W. H., 131
Seboek, T. A., 240, 412
Segal, D., 344
Selby, H. A., 2, 3, 313, 343, 344, 398, 403
Sendral, A., 66
Seppälä, L., 59
Seppälä, M., 65
Service, E., 247
Service, E. R., 163, 210, 231
Seth, P. K., 67
Seth, S., 63, 67
Setty, L. R., 57, 77
Shah, F. K., 57
Shamylan, N. P., 82
Sharma, J. C., 68
Sharma, P. D., 77
Schekochikhina, L. K., 81
Shephard, R. J., 78
Shephardson, M., 318, 321
Shimkin, D., 248
Shiplay, W., 417, 418
Shook, E. M., 161
Shreffler, D. C., 63
Shukla, B. R., 67
Sidhu, L. S., 62
Sikora, P., 72
Sill, W. D., 35
Silverman, M. G., 316-19, 321, 322
SILVERSTEIN, M., 349-82; 417
Sime, F., 79
Simmons, R. F., 285, 286
Simmons, R. T., 62, 63
Simon, H., 260, 261
Simons, E. L., 30, 32, 43
Simpson, G. G., 39
Simpson, H. W., 81
Simpson, L. B., 157
Simpson, W. G., 136
Sinclair, L., 81
Sing, C. F., 63
Singel, D. P., 66
Singer, R., 60
Singer, N. R., 77
Singh, R., 72
Singh, S., 64, 67
Singh Uberoi, J., 249
Sinnett, P., 60, 61, 63
Srivastava, R. P., 67
Skinner, G. W., 146, 221
Skude, G., 60, 67
AUTHOR INDEX

Strauss, A. L., 180
Street, J. M., 218, 219
Strehlow, C., 310
Strehlow, T. G. H., 310
Srocki, Z., 73
Struver, S., 134, 143
Szalko, J., 82
Stukovsky, R., 71
Subramanian, T. A., 70
Sud, J., 73, 82
Sukharev, A. G., 72
Sukhatme, P. V., 70
Sunderland, E., 67
Sutter, J., 167, 169
Suyama, H., 64
Suzuki, M., 80
Svejgaard, A., 59
Swan, A. V., 72
Swanson, G., 263
Swartz, M., 251
Swartz, M. J., 229
Szabo, L., 65
Szalay, F. S., 35
Szecsi, L., 81
Szeinberg, A., 60
Szwaykoswska, I., 76

T

Tabani, E., 67
Tamada, M., 71
Tanaka, T., 71
Tanner, J. M., 71, 73, 75
Tarikian, M., 38, 46
Tattersall, L., 41
Tauber, L. B., 155
Taylor, A. R., 419
Taylor, P. S., 78
Taylor, W., 189
Taylor, W. W., 230, 234
TEETER, K. V., 411-24, 415
Teisberg, P., 64, 66
Teitelbaum, M. S., 57
Telesca, A., 64
Terasaki, P. L., 66
Thakur, H. N., 55
Tharp, G. W., 415
Thein, H., 63, 66
Thibault, J., 248, 258, 264, 265
Thieme, F. P., 159
Thoma, A., 37, 63, 75
Thomas, L. V., 236
Thomas, R. B., 80
Thompson, D. E., 161
Thompson, D. F., 132
Thompson, J. E. S., 242
Thompson, L. C., 416, 417, 420
Thompson, M. T., 416, 417
Thompson, S., 240
Thomson, D. F., 312
Thorpe, A. G., 38, 45
Thorley, E., 66

Threlplan, L. M., 136, 141
Tibbs, G. J., 60-62, 65
Tillikainen, A., 65
Tisala, R., 74
Tills, D., 60-62
Tiwari, S. C., 67, 68, 77
Tobias, P. V., 28, 32-36, 39, 45, 46
Tokaichi, K., 415
Tolchin, D., 63
Tomashevsky-Tamir, S., 60
Tooth, G., 12
Torrance, J. D., 79
Toth, A. T., 67
Toth, U., 70
Trager, F. H., 419
Trager, G. L., 384, 419
Tran-Ngoc-Toan, 169
Trigle, D. R., 61, 70
Trigger, B., 137, 141, 180
Tsehol, P., 161
Tucker, H., 122
Tudan, A., 251
Turnbull, C. M., 215
Turner, J. R. G., 63
Turner, V., 251, 334, 336, 337
Turney-High, H., 250
Turovtsiev, A. L., 77
Tyazi, D., 67, 77
Tye, C. Y., 73
Tyehako, L. I., 68
Tyler, S. A., 272, 273, 286, 311, 321, 386-388, 392, 403
Tzagd, D., 67
Tzatscheva, L., 77

U

Uchida, H., 64
Ulmann, S., 313
Ulan, R., 417
Underwood, B. A., 69
Urban, S., 73
Ursart, L., 272, 274, 276, 277
Uzzell, T., 43

V

Vacikova, A., 67
Valaoras, V. G., 63, 75
Valk, A., 58
Vallois, H. V., 29, 37
Valls, A., 77
Valisk, J., 73
Valisk, J. A., 77
Vamison, P. L., 75
van Balen, H., 57
van de Kaa, D. J., 217
van de Berghe, P., 254
van den Branden, J. L., 60-62
Vandermeersch, B., 37
van Gennep, A., 333
Van Lawick-Goodall, J., 154
van Loghem, E., 65
Author Index

W

Waddell, E., 219
Wagner, G. A., 121
Waldron, R. A., 313, 314
Walker, A., 35
Walker, G. F., 67, 76
Walker, R., 121
Walker, W., 404, 406
Wall, R. E., 372
Wallace, A. F. C., 325, 388, 389
Walsmley, J. B., 357
Walsh, R. J., 63, 68

Walter, H., 60, 62-66
Wang, C., 56
Wangspa, S., 67
Ward, R. H., 58, 169
Ward-Perkins, J., 136, 141
Wares, A. C., 418
Washburn, S. L., 43, 154
Washburn, W. E., 230, 236
Wasi, P., 59
Wasow, T., 354
Waterbolk, H. T., 38
Watson, P. J., 182
Watson, R., 78
Wauchope, R., 131
Weatherall, D. J., 66
Weaver, D. D., 60, 61
Weaver, K. F., 118
Weiner, J. S., 29, 37, 80
Weinreich, U., 303, 361, 362, 370, 373
Weitkamp, L., 169
Weitkamp, L. R., 58, 60, 61, 64, 65
Wendt, G. H., 59, 60, 62
Weninger, M., 68
WERNER, O., 271-308; 271, 272, 274-77, 287, 292, 298, 303, 406
West, M., 137, 161
Whallon, R., 133
Whitehead, A. N., 27
Whittaker, J., 62
Whittembury, J., 79
WHITTEN, D. S., 247-70
WHITTEN, N. E. Jr., 247-70; 248, 249, 253, 255, 257-59
Whorf, B. L., 239
Whyte, W. F., 12
Wika, M., 80
Willey, G. R., 128-31, 141, 146, 160, 161, 234
Williams, A. W. Jr., 167
Williams, G., 387, 388
Willmott, J. V., 70
Willson, J. O. C., 80
Wilmore, J. H., 76
Wilsen, E. N., 133
Wilson, A. C., 43
Wingred, J., 72
Winter, M. C., 182
Winters, H. D., 132, 141

Y

Yada, S., 61
Yamaguchi, M., 59
Yamamoto, M., 59
Yamashita, Y., 80
Yamapol’skaya, Yu. A., 73
Yanase, T., 59
Yasuda, N., 58
Yee, S., 58
Yen, D. E., 220
Yengoyan, A. A., 156, 168
Yoshida, K., 153
Yoshida, A., 66
Young, T. C. Jr., 162
Yu, P. N., 66

N

Zavala, C., 67
Zegura, S. L., 75
Zenkenich, P. I., 73
Zetuner, F. E., 188
Zimmerman, D. W., 121, 122
Zontendyk, A., 65
Zubov, V. A., 73
ZUBROW, E. B. W., 179-206; 161, 183
Zuckerman, S., 29
Zuckerman, Sir Solly, 154
Zwicky, A. M., 274, 368
SUBJECT INDEX

A
Aborigines
see Australian aborigines,
American Indians, etc
Accountability
in linguistic theory, 362-71
Acid phosphatase
 genetic studies of, 60
Adaptation
to cold, 42, 80-81
to heat, 79-80
to high altitude, 78-79
Adaptational theory
definition of, 248
Adaptive strategies
in social relationships, 250-57
Adenosine deaminase
 genetic studies of, 62
Adenylate kinase
 genetic studies of, 60
Adolescence
onset of puberty and menarche, 72-73
Aerobic capacity
 components of, 77-78
Africa
anthropometry in, 75-76
demographic studies in, 164
distribution of cited literature on, 195-97
ethnography of, 233-41
folk tale structure in, 337
fossil man in, 37
catalog of, 30
gene tic studies in, 60-69
lactase deficiency in, 69
morphology in, 76-77
pastoralists of
cultural studies of, 216-17, 220-21
rebellions in
developmental cycles of, 256
settlement patterns in, 141
Aggression
in nonhuman primates, 153
Agricultural people
demographic studies of, 162, 185-86
Agriculture
development of, 201
implications of diversity in, 220-21
botanical, 220

cognitive, 221
cultural, 220-21
Aguila
 genetic studies in, 61
Ainu
cold adaptation by, 80
genetic studies of, 63, 65
morphology of, 77
Akan kingdoms
leadership in, 17
Alabama Indian
linguistic study of, 418
Alaskan Indian
linguistic studies of, 414
Aleutians
genetic studies in, 60
Algic language
classification and families of, 412, 415
Algonquian
linguistic classification of, 415
Proto-Central-Algonquian kin terms, 402
Algonquin Indians
 genetic studies of, 64
land tenure among, 238
Alkaline phosphatase
genetic studies of, 64
Altar de Sacrificio
demographic studies at, 161
Altitude
adaptations to, 78-79
Amazon
Dravidian kinship systems in, 21
Amazon basin
demographic studies in, 170
settlement patterns in, 132
America
fossil man in
catalog of, 30
dates of fossils, 38
genetic studies in, 61, 63-64
obsidian hydration dating in, 122
tree-ring dating in, 118
American Indians
demographic studies of, 157
lactase deficiency in, 69
linguistics, 411-24
classification, 412-13
families, 413-21
introduction, 411-12
wider relationships, 421
American language
kinship terms of, 318-20, 322, 325
Americans
word association tests of, 339-40
American Southwest
prehistoric social organization of, 183
Amhara
 genetic studies in, 61
beta-Aminoobutyric acid excretion patterns of
genetic aspects of, 67
Amish (Holmes County, Ohio)
demography of, 55
Amylase, salivary
genetic studies of, 67
Anaphora
transformational aspects, 375
Anatomy
of fossil man, 38-40
Andamanese
structural study of thought in, 335, 337
Andes
see Central Andes
Angola
anthropometry in, 75
morphology in, 77
Animals, domestic
history of, 188
Answering systems
automatic, 287
Anthropometry, 75-76
 applied, 75-76
body composition, 76
of elderly, 75
of head and face, 75
Apachean language
linguistic studies of, 414-15
Lipan and Kiowa, 415
Apache Indians
growth and development in, 73
Apes
demographic studies of, 153-54
Arabs
 genetic studies of, 61
lactase deficiency in, 69
Archaeology
citation matrix, 187
componental analysis of data in, 392-93
dating methods, 113-26
436
<table>
<thead>
<tr>
<th>Subject Index</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Asai</strong></td>
</tr>
<tr>
<td><strong>Asia</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
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</tr>
<tr>
<td><strong>Assam</strong></td>
</tr>
<tr>
<td><strong>Athabascan (Athapaskan)</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Athens</strong></td>
</tr>
<tr>
<td><strong>Athletes</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Australia</strong></td>
</tr>
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<td><strong>Australian aborigine</strong></td>
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<tr>
<td><strong>Australopithecus africanus</strong></td>
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<tr>
<td><strong>Australopithecus boisei</strong></td>
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<tr>
<td></td>
</tr>
<tr>
<td><strong>Australopithecus robustus</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Babinga pygmies</strong></td>
</tr>
<tr>
<td><strong>Baboons</strong></td>
</tr>
<tr>
<td><strong>Balearic Islands</strong></td>
</tr>
<tr>
<td><strong>Banan</strong></td>
</tr>
<tr>
<td><strong>Baniya</strong></td>
</tr>
<tr>
<td><strong>Bantu</strong></td>
</tr>
<tr>
<td><strong>Basques</strong></td>
</tr>
<tr>
<td><strong>Bavaria</strong></td>
</tr>
<tr>
<td><strong>Bedouins</strong></td>
</tr>
<tr>
<td><strong>Behavioral aspects of culture</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
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</tr>
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<td><strong>Beja</strong></td>
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<td><strong>Belgium</strong></td>
</tr>
<tr>
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</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Belize Valley, British Honduras</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Bellona Islanders (Solomon Islands)</strong></td>
</tr>
<tr>
<td><strong>Bengal</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Beothuk languages</strong></td>
</tr>
</tbody>
</table>
**SUBJECT INDEX**

<table>
<thead>
<tr>
<th>Burma</th>
<th>genetic studies in, 63, 66</th>
</tr>
</thead>
<tbody>
<tr>
<td>Burundi</td>
<td>ethnohistory of, 241</td>
</tr>
<tr>
<td>Bushmen of South Africa</td>
<td>anthropometry in, 75</td>
</tr>
<tr>
<td>demographic studies of, 157, 165-68</td>
<td></td>
</tr>
<tr>
<td>environment, subsistence, and society of, 210-11</td>
<td></td>
</tr>
<tr>
<td>C</td>
<td>Caddoan Indian languages classification and families of, 412, 419</td>
</tr>
<tr>
<td>Caingang Indians</td>
<td>genetic studies of, 67</td>
</tr>
<tr>
<td>sweat gland distribution in, 60</td>
<td></td>
</tr>
<tr>
<td>California</td>
<td>growth and development in, 72</td>
</tr>
<tr>
<td>Cameroons</td>
<td>Fang political enterprise sequences in, 255</td>
</tr>
<tr>
<td>Canada</td>
<td>demographic studies in, 56</td>
</tr>
<tr>
<td>ecological systems of Indians of, 214-15</td>
<td></td>
</tr>
<tr>
<td>genetic studies in, 62</td>
<td></td>
</tr>
<tr>
<td>Cantometrics</td>
<td>cultural aspects of, 7</td>
</tr>
<tr>
<td>Caribelli's cusp inheritance of, 71</td>
<td></td>
</tr>
<tr>
<td>Caribs</td>
<td>genetic studies of, 60-61</td>
</tr>
<tr>
<td>Carter Ranch, Arizona settlement patterns in, 133, 139</td>
<td></td>
</tr>
<tr>
<td>Cashinahua</td>
<td>anthropometry in, 76</td>
</tr>
<tr>
<td>demographic study of, 58</td>
<td></td>
</tr>
<tr>
<td>genetic studies of, 67</td>
<td></td>
</tr>
<tr>
<td>Cosma Valley</td>
<td>demographic studies of, 161</td>
</tr>
<tr>
<td>Catalase</td>
<td>genetic variants in, 62</td>
</tr>
<tr>
<td>Caparo of Para</td>
<td>demographic studies of, 58</td>
</tr>
<tr>
<td>Celts</td>
<td>settlement patterns of, 136</td>
</tr>
<tr>
<td>Central America growth and development in, 74</td>
<td></td>
</tr>
<tr>
<td>Central Andes</td>
<td>demographic studies of, 158, 161</td>
</tr>
<tr>
<td>Ceramics</td>
<td>in study of settlement patterns, 128, 133, 139, 143</td>
</tr>
<tr>
<td>Cerumen types</td>
<td>genetic distribution of, 67</td>
</tr>
<tr>
<td>Ceylon</td>
<td>cause of golter in, 70</td>
</tr>
<tr>
<td>Chad</td>
<td>growth adaptation in, 72</td>
</tr>
<tr>
<td>morphology in, 76</td>
<td></td>
</tr>
<tr>
<td>nutrition studies in, 70</td>
<td></td>
</tr>
<tr>
<td>Chan Chan</td>
<td>demographic studies at, 161</td>
</tr>
<tr>
<td>Chemakuan language classification and families of, 412, 416, 420</td>
<td></td>
</tr>
<tr>
<td>Chemeron</td>
<td>fossil man from, 35</td>
</tr>
<tr>
<td>Chenchiaou, Shenbei, China fossil man from, 36</td>
<td></td>
</tr>
<tr>
<td>Chesowanja, Kenya fossil man from, 35, 44</td>
<td></td>
</tr>
<tr>
<td>Cheyenne Indian linguistic classification of, 415</td>
<td></td>
</tr>
<tr>
<td>Chickasaw</td>
<td>linguistic study of, 418</td>
</tr>
<tr>
<td>Chilam</td>
<td>ethnohistorical studies of, 242</td>
</tr>
<tr>
<td>Chilocotin</td>
<td>genetic studies of, 61</td>
</tr>
<tr>
<td>Chile</td>
<td>demographic studies of, 169</td>
</tr>
<tr>
<td>genetic studies in, 68</td>
<td></td>
</tr>
<tr>
<td>nutrition in, 69</td>
<td></td>
</tr>
<tr>
<td>Chillon</td>
<td>demographic studies at, 161</td>
</tr>
<tr>
<td>Chimpanzee</td>
<td>demographic studies of, 153-55</td>
</tr>
<tr>
<td>China</td>
<td>cycles of dynastic health and decay in, 20</td>
</tr>
<tr>
<td>ethnography of, 255</td>
<td></td>
</tr>
<tr>
<td>fossil man from, 39-37, 39, 43</td>
<td></td>
</tr>
<tr>
<td>genetic studies in, 61-62, 65</td>
<td></td>
</tr>
<tr>
<td>market hierarchies and settlement distribution in, 146</td>
<td></td>
</tr>
<tr>
<td>Chinookan</td>
<td>linguistic classification of, 417</td>
</tr>
<tr>
<td>Choctaw</td>
<td>linguistic study of, 418</td>
</tr>
<tr>
<td>Choice</td>
<td>in problem solving, 260-61</td>
</tr>
<tr>
<td>Cholesterol, serum</td>
<td>genetic aspects of, 69</td>
</tr>
<tr>
<td>Cholula</td>
<td>settlement patterns at, 144</td>
</tr>
<tr>
<td>SUBJECT INDEX</td>
<td></td>
</tr>
<tr>
<td>---------------</td>
<td></td>
</tr>
<tr>
<td>Choukoutien, China</td>
<td>fossil man from, 36-37, 42</td>
</tr>
<tr>
<td>Circadian rhythms</td>
<td>comparative studies of, 81</td>
</tr>
<tr>
<td>Climate</td>
<td>literature sample on, 192-99</td>
</tr>
<tr>
<td>Coca leaf</td>
<td>and cold stress, 81</td>
</tr>
<tr>
<td>Nutritional effects of, 69</td>
<td></td>
</tr>
<tr>
<td>Cold</td>
<td>adaptation to, 80-81</td>
</tr>
<tr>
<td>Color</td>
<td>semantics of, 388</td>
</tr>
<tr>
<td>Color vision</td>
<td>genetic aspects of, 66-67</td>
</tr>
<tr>
<td>Colombia</td>
<td>Indian nutrition in, 69</td>
</tr>
<tr>
<td>Modeling of culture in, 19-20</td>
<td></td>
</tr>
<tr>
<td>Pacific Lowlands of economic strategies in, 256</td>
<td></td>
</tr>
<tr>
<td>Reciprocity studies in, 257</td>
<td></td>
</tr>
<tr>
<td>Comanche</td>
<td>kinship terms of, 395</td>
</tr>
<tr>
<td>Community</td>
<td>settlement pattern of, 137-40</td>
</tr>
<tr>
<td>Competence</td>
<td>linguistic, 363, 368-69, 371</td>
</tr>
<tr>
<td>Anthropological aspects of, 395, 404</td>
<td></td>
</tr>
<tr>
<td>Complement</td>
<td>genetic aspects of, 63</td>
</tr>
<tr>
<td>Complementary distribution</td>
<td>linguistic, 384-85, 390-92</td>
</tr>
<tr>
<td>Semantic, 390-92</td>
<td></td>
</tr>
<tr>
<td>Compliance</td>
<td>interactive behavior in, 16</td>
</tr>
<tr>
<td>Componential analysis</td>
<td>in anthropology, 388-94</td>
</tr>
<tr>
<td>Compound relations in ethno-</td>
<td>science, 291-93</td>
</tr>
<tr>
<td>Autonomy, 293</td>
<td></td>
</tr>
<tr>
<td>Comparison, 292</td>
<td></td>
</tr>
<tr>
<td>Exemplification, 292</td>
<td></td>
</tr>
<tr>
<td>Function and operation, 292</td>
<td></td>
</tr>
<tr>
<td>Grading (queuing), 292-93</td>
<td></td>
</tr>
<tr>
<td>Part-to-whole, 291-92</td>
<td></td>
</tr>
<tr>
<td>Provenience, 293</td>
<td></td>
</tr>
<tr>
<td>Synonym, 293</td>
<td></td>
</tr>
<tr>
<td>Computer</td>
<td>demographic use of, 155, 169</td>
</tr>
<tr>
<td>Question-answering systems on, 283-88, 303-5</td>
<td></td>
</tr>
<tr>
<td>Partitioning vocabulary for, 293-302</td>
<td></td>
</tr>
<tr>
<td>To study ethnosience, 272-73, 283-88, 303</td>
<td></td>
</tr>
<tr>
<td>Memory models, 283-86</td>
<td></td>
</tr>
<tr>
<td>Typology, 283-86</td>
<td></td>
</tr>
<tr>
<td>Use in dating of archaeological materials, 114-16</td>
<td></td>
</tr>
<tr>
<td>Use in studying interactive behavior, 7</td>
<td></td>
</tr>
<tr>
<td>Congolese Bantu genetic studies of, 64-65</td>
<td></td>
</tr>
<tr>
<td>Congo rain forest</td>
<td>ecological aspects of, 215-16</td>
</tr>
<tr>
<td>Connotation in kinship terminology, 316-18</td>
<td></td>
</tr>
<tr>
<td>Consensus interactions in, 15</td>
<td></td>
</tr>
<tr>
<td>Contraceptives, oral demographic studies of, 56</td>
<td></td>
</tr>
<tr>
<td>Contrast in linguistics, 384</td>
<td></td>
</tr>
<tr>
<td>Costa Rica</td>
<td>nutrition in, 69</td>
</tr>
<tr>
<td>Cracow</td>
<td>growth studies in, 72</td>
</tr>
<tr>
<td>Cree Indian</td>
<td>adaptation study of, 254</td>
</tr>
<tr>
<td>Ecological systems of, 214-15</td>
<td></td>
</tr>
<tr>
<td>Linguistic classification of, 415</td>
<td></td>
</tr>
<tr>
<td>Cross-dating, 116</td>
<td></td>
</tr>
<tr>
<td>By correlation of culture traits, 116</td>
<td></td>
</tr>
<tr>
<td>Crow Indians</td>
<td>kinship terms of, 325</td>
</tr>
<tr>
<td>Linguistic study of, 419</td>
<td></td>
</tr>
<tr>
<td>Cuba</td>
<td>growth and development in, 73</td>
</tr>
<tr>
<td>Cultural anthropology</td>
<td>culture as behavior: structure and emergence, 1-26</td>
</tr>
<tr>
<td>Comparison and generalization, 12-19</td>
<td></td>
</tr>
<tr>
<td>Forms, and search for structure, 1-6</td>
<td></td>
</tr>
<tr>
<td>Measuring and ordering interactions, 6-12</td>
<td></td>
</tr>
<tr>
<td>Minimal sequence models, 19-25</td>
<td></td>
</tr>
<tr>
<td>And demography, 152, 163-67</td>
<td></td>
</tr>
<tr>
<td>Ethnohistory, 227-46</td>
<td></td>
</tr>
<tr>
<td>Structuralism in, 329-48</td>
<td></td>
</tr>
<tr>
<td>Culture</td>
<td>evolution of demographic aspects of, 163-67</td>
</tr>
<tr>
<td>And folk song style, 7, 12</td>
<td></td>
</tr>
<tr>
<td>Male-female relationships, 12</td>
<td></td>
</tr>
<tr>
<td>Implications of diversity of, 220-21</td>
<td></td>
</tr>
<tr>
<td>Semantic aspects of, 404-6</td>
<td></td>
</tr>
<tr>
<td>Systems analysis of, 403-4</td>
<td></td>
</tr>
<tr>
<td>Culture traits</td>
<td>correlation of cross-dating by, 116</td>
</tr>
<tr>
<td>Cyprus</td>
<td>genetic studies in, 68</td>
</tr>
<tr>
<td>Cyrenaica</td>
<td>Samusi ethnology, 239</td>
</tr>
<tr>
<td>Czechoslovakia</td>
<td>anthropometry in, 75</td>
</tr>
<tr>
<td>Genetic studies in, 61, 67</td>
<td></td>
</tr>
<tr>
<td>Growth and development in, 71, 73-74</td>
<td></td>
</tr>
</tbody>
</table>

**D**

Dakar | growth and development in, 72 |

Dakota-Lakota | linguistic studies of, 418-19 |

Dama | anthropometry in, 75 |

Dating methods, 113-26 |

Chronometric dating, 116-23 |

Archaeomagnetic dating, 118-19 |

Dendrochronology, 117-18 |

Fission-track, 120-21 |

Obsidian hydration, 122-23 |

Potassium-argon, 120 |

Radiocarbon, 119-20 |

Thermoluminescence, 121-22 |

Concluding remarks, 123-24 |

Introduction, 113-14 |

Literature sample on, 193, 204 |

New developments in phasing, 114 |

Relative dating, 115-16 |

Cross-dating, 116 |

Seriation, 115 |

Decisions | social aspects of, 261 |

Deductive models | current status of, 286-88 |

Deep structure-surface structure | see Transformational grammar |

Demography | advances in descriptive techniques, 217-20 |

Area, 218 |

Carrying capacity, 218-20 |

Population, 217-18 |

Conclusions, 173 |
and cultural evolution, 163-67
developments in descriptive, 153-63
living human populations, 155-57
nonhuman primates, 153-55
reconstruction of prehistor-
ic and ethnohistoric popu-
lations, 157-63
human ecology and demo-
graphic variability, 172-73
introduction, 151-52
biological anthropology, 151-52
cultural anthropology, 152
literature sample on, 193, 201-2
modern studies of, 55-59
descriptive, 55
genetic, 57-59
variables in, 55-57
population genetics and evo-
lution, 167-72
assortative mating and in-
breeding, 170-71
gene flow, 166-69
genetic drift and the founder
principle, 169-70
genetic selection, 171-72
Dendrochronology
methodological aspects of, 117-18
Denmark
 genetic studies in, 60, 62-63, 65-66
Dentition
 culture change effects on, 71
 in fossil man, 35, 40
 quantitative studies of, 70
Dermatoglyphics
 genetic aspects of, 67-68
 Desert dwellers
 heat adaptation by, 79-80
 Determinism
 environmental, 209-10
 Harris' theories, 209
 Developmental cycle
 as approach to structural
time, 255
Dieri
 kinship terms of, 312
Digraph theory
 use of, 344
Discontinuous constituents
 linguistic, 387
Discourse analysis
 linguistic, 387
Disease
 semantic aspects of, 388
 Distinctive feature model
 in phonology, 397-401
components vs distinctive
 features in semantic
 analysis, 399-401
 nature of reduction rules,
 398-99
Divers
 cold adaptation by, 80
Diyala, Iraq
 settlement patterns in, 131
Dogrib
 linguistic classification, 414
Dominica
 genetic studies in, 60
Dravidian kinship system
 minimal sequence model
 of, 21-24
Dyribal language
 taboo lexicon, 373
Dzibilchaltun
 demographic studies at,
 160
E
East Africa
 culture and ecology project
 in, 216-17
 fossil man from, 35-36
Eating
 semantics of, 388
Ecology
 and demographic variability,
 172-73
 interrelated with archaeol-
 ogy, 180
 relationships with ritual,
 208
 relationships with values
 and personality, 208
 Economic behavior
 guaranteed income, 18
Economics
 distribution of cited litera-
ture on, 193, 200-1
Economic systems
 adaptive strategies in, 254,
 256, 258-60
 interpersonal aspects of,
 12, 14-15, 17
 Pacific Lowlands of Colom-
 bia and Ecuador
 economic strategies of,
 256
 reciprocity studies in,
 257
Egypt
 anthropometry in, 75
 genetic studies in, 67
 growth studies in, 72
 morphology in, 76-77
 Egyptian Nubians
 demographic study of,
 59
 Embedded sentence
 presuppositions of, 280-81
Emic-etic approach
 to linguistics, 384-86
Energy
 studies of use of, 221-23
England (Britain)
 anthropometry in, 76
 demographic studies of,
 169
 genetic studies in, 62, 64
 growth and development in,
 73-74
 settlement pattern study
 tradition in, 136-37, 145
English language
 kinship terms of, 317, 320-
 21
 linguistic theory in, 349-
 77
Environment
 as independent variable, 213-
 17
 African pastoralists, 216-
 17
 Cree and Ojibway, 214-
 15
 Eskimo, 214
 Mbuti pygmies, 215
 literature sample on, 192,
 198-99
 Environment, subsistence,
 and society, 179-226
 changing perspectives in,
 179-206
 changing categorical em-
 phasis, 190-205
 classics, 188
 conclusion and summary,
 205
 history of pre-paradigm,
 180-81
 introduction, 179-80
 new paradigm, 182-85
 recency, 188-90
 recent developments in, 207-
 26
 advances in descriptive
 techniques, 217-20
 conclusion, 223
 contrastive comparative
 explanations, 213-17
 energy studies, 221-23
 environmental determinism,
 209-10
 Harris and determinism, 209
 holistic approach to com-
 plex system, 208
 implications of diversity,
 220-21
 introduction, 207-8
 logic of unique event, 212-
 13
 logics of systematic ex-
 planations, 210
 search for regularities,
 211-12
 spatial studies, 221
<table>
<thead>
<tr>
<th>SUBJECT INDEX</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fiji</td>
</tr>
<tr>
<td>demographic study of, 55</td>
</tr>
<tr>
<td>kinship terms in, 314-15</td>
</tr>
<tr>
<td>Fingerprint</td>
</tr>
<tr>
<td>genetic studies of, 67-68</td>
</tr>
<tr>
<td>Finland</td>
</tr>
<tr>
<td>genetic studies in, 60-61, 64-65, 67</td>
</tr>
<tr>
<td>growth and development in, 74</td>
</tr>
<tr>
<td>Finnish</td>
</tr>
<tr>
<td>language structural studies of, 343</td>
</tr>
<tr>
<td>Firewood</td>
</tr>
<tr>
<td>semantics of, 388</td>
</tr>
<tr>
<td>Fishing behavior</td>
</tr>
<tr>
<td>adaptive strategies in, 251-52</td>
</tr>
<tr>
<td>Fission-track dating</td>
</tr>
<tr>
<td>current status of, 120-21</td>
</tr>
<tr>
<td>Flagstaff, Arizona</td>
</tr>
<tr>
<td>demographic studies of, 161</td>
</tr>
<tr>
<td>Florence, Italy</td>
</tr>
<tr>
<td>demographic study of, 59</td>
</tr>
<tr>
<td>Folk history</td>
</tr>
<tr>
<td>ethnohistorical importance of, 239-42</td>
</tr>
<tr>
<td>Folklore</td>
</tr>
<tr>
<td>structure aspects of, 334-37, 342, 344</td>
</tr>
<tr>
<td>Folk song style</td>
</tr>
<tr>
<td>and culture, 7, 12</td>
</tr>
<tr>
<td>male-female relationships, 12</td>
</tr>
<tr>
<td>Folktales</td>
</tr>
<tr>
<td>morphology of, 333</td>
</tr>
<tr>
<td>Folk taxonomies</td>
</tr>
<tr>
<td>componental analytical studies of, 386-87</td>
</tr>
<tr>
<td>ethnosience aspects of, 275-77</td>
</tr>
<tr>
<td>Food</td>
</tr>
<tr>
<td>semantics of, 388</td>
</tr>
<tr>
<td>Food ingestion</td>
</tr>
<tr>
<td>during cold stress, 80-81</td>
</tr>
<tr>
<td>Food supply</td>
</tr>
<tr>
<td>and population density, 165-66</td>
</tr>
<tr>
<td>and population size in nonhuman primates, 153</td>
</tr>
<tr>
<td>Foot</td>
</tr>
<tr>
<td>fossil, 39</td>
</tr>
<tr>
<td>Fort Ternan, Kenya</td>
</tr>
<tr>
<td>fossil man from, 30, 40</td>
</tr>
<tr>
<td>Fossil man, 27-54</td>
</tr>
<tr>
<td>cataloga of, by continent, 30</td>
</tr>
<tr>
<td>conceptual evaluation of data, 38-49</td>
</tr>
<tr>
<td>anatomy and variability, 38-40</td>
</tr>
<tr>
<td>chronology, 42-43</td>
</tr>
<tr>
<td>ecology and behavior, 40-42</td>
</tr>
</tbody>
</table>

| Enzymes         |
| genetic studies of, 60-62 |
| of red blood cells |

| Eskimo          |
| Copper          |
| subsistence and society, 214 |
| demographic studies of, 157, 168 |
| dentition in, 71 |
| environment, subsistence, and society in, 210, 212-14 |
| Greenland, 60-61, 63, 66 |
| genetic studies of, 60, 64-65, 67 |

| Igloolik        |
| nutrition in, 70 |
| morphology of, 77 |

| Netsilik        |
| subsistence and society of, 214 |
| wife trading and female infanticide, 212-13 |
| settlement patterns of, 146 |
| space and energy sources of systems analysis of, 222 |

| Wainwright      |
| anthropometry in, 75 |
| cold adaptation by, 80-81 |
| demographic studies of, 55, 58 |
| growth and development in, 74 |
| nutrition in, 70 |

| Eskimo-Aleut language |
| classification and families of, 412-13 |

| Ethiopia        |
| fossil man from, 35, 38 |

| Ethnography     |
| literature sample on, 193, 201-2 |
| and structuralism, 331 |

| Ethnohistory    |
| definition of, 230-42 |
| folk history, 239-42 |
| historical ethnographies, 238-39 |
| methodology, 232-36 |
| specific histories, 236-38 |
| theory, 231-32 |
| history and anthropology, 227-30 |
| definitions, 229-30 |
| historical trend, 228-29 |

| Ethnosience     |
| conclusion, 303-5 |
| convergences, 272-74 |
| generative semantics, 273 |
| semantic information processing, 273 |
| sociolinguistics, 273-74 |

| Introduction to |
| problems and comments, 269-302 |
| compound relations, 291-93 |
| partitioning vocabulary, 293-302 |
| predicate calculus, 289-91 |
| variables, 302 |
| summary, 303 |
| typology, 274-89 |
| convergence of typology and ethnoscience, 274-78 |
| deductive models, 286-88 |
| generative semantics, 278-83 |
| memory models, 283-86 |
| semantic information processing, 283 |
| sociolinguistics, 288-89 |

| Europe          |
| demographic studies in, 58, 160 |
| ethnohistory of, 235 |
| fossil man in |
| catalog of, 30 |
| literature sample, 192 |
| settlement patterns in, 141 |
| tree-ring dating in, 118 |

| Europeans       |
| adaptation studies of, 78-80 |
| genetic studies of, 60-69 |
| growth and development studies of, 72-74 |
| lactase deficiency in, 69 |
| morphology in, 76 |

| Evolution       |
| historical vs anthropological approach to, 227 |
| literature sample on, 193 |

| Exchange theory |
| interpersonal actions in, 15 |
| social aspects of, 258-59 |

| Eyak Indians    |
| linguistics studies of, 414 |

| F              |
| Facial cleft   |
| and growth rate, 74-75 |

| Family         |
| kinship semantics, 309-28 |

| Fayum Oasis, Egypt |
| growth of children in, 72 |

<p>| Fertility       |
| as demographic variable, 55-57 |</p>
<table>
<thead>
<tr>
<th>Subject Index</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>nomenclature</strong>, 46-48</td>
</tr>
<tr>
<td><strong>summary</strong>, 48-49</td>
</tr>
<tr>
<td><strong>taxonomy</strong>, 43-46</td>
</tr>
<tr>
<td><strong>new finds</strong>: <strong>new dates</strong>, 29-38</td>
</tr>
<tr>
<td>Australopithecus africanus, 32-33</td>
</tr>
<tr>
<td>Australopithecus boisei, 33-35</td>
</tr>
<tr>
<td>Australopithecus robustus, 33</td>
</tr>
<tr>
<td>Homo erectus, 36-37</td>
</tr>
<tr>
<td>Homo habilis, 35-36</td>
</tr>
<tr>
<td>Homo sapiens, 37-38</td>
</tr>
<tr>
<td>Ramapithecus punjabicus, 32</td>
</tr>
<tr>
<td>Ramapithecus wickeri, 30-32</td>
</tr>
<tr>
<td><strong>France</strong></td>
</tr>
<tr>
<td>anthropometry in, 75</td>
</tr>
<tr>
<td>genetic studies in, 63</td>
</tr>
<tr>
<td>growth and development studies in, 71</td>
</tr>
<tr>
<td>settlement patterns in, 133</td>
</tr>
<tr>
<td><strong>Fukuoka, Japan</strong></td>
</tr>
<tr>
<td>demographic study of, 59</td>
</tr>
<tr>
<td><strong>Functionalism and history</strong>, 228</td>
</tr>
<tr>
<td>and structuralism, 331</td>
</tr>
<tr>
<td><strong>G</strong></td>
</tr>
<tr>
<td>Game theory</td>
</tr>
<tr>
<td>anthropological use of, 248-49, 261-66</td>
</tr>
<tr>
<td>Gapping</td>
</tr>
<tr>
<td>linguistic, 372</td>
</tr>
<tr>
<td>Gene flow</td>
</tr>
<tr>
<td>demographic aspects of, 168-69</td>
</tr>
<tr>
<td>Generative semantics</td>
</tr>
<tr>
<td>see Semantics, generative</td>
</tr>
<tr>
<td>Genetics</td>
</tr>
<tr>
<td>demographic studies, 57-59, 167-72</td>
</tr>
<tr>
<td>assortative mating and inbreeding, 170-71</td>
</tr>
<tr>
<td>gene flow, 168-69</td>
</tr>
<tr>
<td>genetic drift and founder principle, 169-70</td>
</tr>
<tr>
<td>genetic selection, 171-72</td>
</tr>
<tr>
<td>literature sample on, 193, 203</td>
</tr>
<tr>
<td>in study of modern man, 59-69</td>
</tr>
<tr>
<td>blood group, 62-63</td>
</tr>
<tr>
<td>characters of complex inheritance, 67-69</td>
</tr>
<tr>
<td>miscellaneous, 66-67</td>
</tr>
<tr>
<td>other inherited blood characteristics, 65-66</td>
</tr>
<tr>
<td>red cell enzymes, 60-62</td>
</tr>
<tr>
<td>serum polymorphisms, 63-65</td>
</tr>
<tr>
<td>Geology</td>
</tr>
<tr>
<td>literature sample on, 192, 199</td>
</tr>
<tr>
<td>Germany</td>
</tr>
<tr>
<td>anthropometry in, 75-76</td>
</tr>
<tr>
<td>genetic studies in, 60-62</td>
</tr>
<tr>
<td>Gilbert Islands</td>
</tr>
<tr>
<td>nutrition in, 70</td>
</tr>
<tr>
<td>Glucose-6-phosphate dehydrogenase</td>
</tr>
<tr>
<td>genetic studies of, 61, 68</td>
</tr>
<tr>
<td>Gorillas</td>
</tr>
<tr>
<td>demographic studies of, 154-55</td>
</tr>
<tr>
<td>Goteborg</td>
</tr>
<tr>
<td>anthropometry in, 75</td>
</tr>
<tr>
<td>Grammar</td>
</tr>
<tr>
<td>generative, 357, 371-77</td>
</tr>
<tr>
<td>role in phonemic analysis, 387</td>
</tr>
<tr>
<td>rules of promotion of subject, 357</td>
</tr>
<tr>
<td>search for universals of, 349</td>
</tr>
<tr>
<td>see also Transformational Grammar</td>
</tr>
<tr>
<td>Grammaticality</td>
</tr>
<tr>
<td>definitions of, 364-65</td>
</tr>
<tr>
<td>Gran Canaria</td>
</tr>
<tr>
<td>genetic studies in, 62</td>
</tr>
<tr>
<td>Greece</td>
</tr>
<tr>
<td>anthropometry in, 75</td>
</tr>
<tr>
<td>ethnohistory of, 235</td>
</tr>
<tr>
<td>genetic studies in, 61, 63, 69</td>
</tr>
<tr>
<td>growth and development in, 72</td>
</tr>
<tr>
<td>settlement patterns in, 131</td>
</tr>
<tr>
<td>Greek</td>
</tr>
<tr>
<td>kinship terms of, 402</td>
</tr>
<tr>
<td>Growth and development, 71-75</td>
</tr>
<tr>
<td>adolescent, 72-73</td>
</tr>
<tr>
<td>birth weight variation, 71</td>
</tr>
<tr>
<td>and hettosis, 74</td>
</tr>
<tr>
<td>at high altitude, 78-79</td>
</tr>
<tr>
<td>infant, 71-72</td>
</tr>
<tr>
<td>secular trends, 63</td>
</tr>
<tr>
<td>of specific body parts, 74-75</td>
</tr>
<tr>
<td>Guarani</td>
</tr>
<tr>
<td>genetic studies of, 67</td>
</tr>
<tr>
<td>Guatemala</td>
</tr>
<tr>
<td>demographic studies in, 161</td>
</tr>
<tr>
<td>ethnohistory of, 237</td>
</tr>
<tr>
<td>nutrition in, 69</td>
</tr>
<tr>
<td>Guatemala City</td>
</tr>
<tr>
<td>demographic studies in, 56</td>
</tr>
<tr>
<td>Guayaki</td>
</tr>
<tr>
<td>anthropometry of, 75</td>
</tr>
<tr>
<td>Gujarat</td>
</tr>
<tr>
<td>anthropometric data on, 75</td>
</tr>
<tr>
<td><strong>genetic studies in</strong>, 66</td>
</tr>
<tr>
<td>Gulf Indian languages</td>
</tr>
<tr>
<td>classification and families of, 412, 418</td>
</tr>
<tr>
<td>Guyana</td>
</tr>
<tr>
<td>adaptive strategies in, 254</td>
</tr>
<tr>
<td><strong>Guyanese demographic study of</strong>, 55</td>
</tr>
<tr>
<td><strong>H</strong></td>
</tr>
<tr>
<td>Habbanites</td>
</tr>
<tr>
<td>genetic studies of, 63, 65</td>
</tr>
<tr>
<td>Haida Indians</td>
</tr>
<tr>
<td>language classification, 414</td>
</tr>
<tr>
<td>Hand</td>
</tr>
<tr>
<td>fossil, 40</td>
</tr>
<tr>
<td>Havasupai</td>
</tr>
<tr>
<td>demography of, 55</td>
</tr>
<tr>
<td><strong>Hawaii</strong></td>
</tr>
<tr>
<td>dentition studies in, 71</td>
</tr>
<tr>
<td>genetic studies in, 64</td>
</tr>
<tr>
<td>plasticity of physique of Japanese migrants, 73</td>
</tr>
<tr>
<td>Polynesian settlement patterns in, 134</td>
</tr>
<tr>
<td><strong>Hawaiian</strong></td>
</tr>
<tr>
<td>kinship terms in, 314</td>
</tr>
<tr>
<td>Hay Hollow Valley, Arizona</td>
</tr>
<tr>
<td>demographic studies of, 161</td>
</tr>
<tr>
<td><strong>Heat environment</strong></td>
</tr>
<tr>
<td>adaptation to, 79-80</td>
</tr>
<tr>
<td>Hemoglobin variants</td>
</tr>
<tr>
<td>genetic aspects of, 60, 65-66</td>
</tr>
<tr>
<td>Hessians</td>
</tr>
<tr>
<td>morphology of, 77</td>
</tr>
<tr>
<td><strong>Hidatsa</strong></td>
</tr>
<tr>
<td>linguistic study of, 419</td>
</tr>
<tr>
<td>History</td>
</tr>
<tr>
<td>ethnohistory, 227-46</td>
</tr>
<tr>
<td><strong>Hokan language</strong></td>
</tr>
<tr>
<td>classification and families of, 412, 417-18</td>
</tr>
<tr>
<td><strong>Hollis</strong></td>
</tr>
<tr>
<td>definition of, 208</td>
</tr>
<tr>
<td>Hominidae</td>
</tr>
<tr>
<td>classification of, 48-49</td>
</tr>
<tr>
<td><strong>Hominids</strong></td>
</tr>
<tr>
<td>see Fossil man</td>
</tr>
<tr>
<td><strong>Homo erectus</strong></td>
</tr>
<tr>
<td>chronology of, 42</td>
</tr>
<tr>
<td>evolution of, 28-30</td>
</tr>
<tr>
<td>fossils of, 36-37, 39</td>
</tr>
<tr>
<td>geographical range of, 37</td>
</tr>
<tr>
<td>taxonomy of, 44-48</td>
</tr>
<tr>
<td><strong>Homo habilis</strong></td>
</tr>
<tr>
<td>chronology of, 42</td>
</tr>
<tr>
<td>evolution of, 28, 30</td>
</tr>
<tr>
<td>fossils of, 35-36, 40</td>
</tr>
<tr>
<td>taxonomy of, 44, 46-47</td>
</tr>
<tr>
<td><strong>Homo modjokertensis</strong></td>
</tr>
</tbody>
</table>
SUBJECT INDEX

I

Iceland
genetic studies in, 60, 62, 64
Illinois Driver's Manual (Rules of the Road)
description of, 286-87
Illinois Valley
settlement patterns in, 135

Inbreeding
demographic aspects of, 59, 170-71

Incas
demographic studies of, 158

India
anthropometry in, 75
demographic studies in, 57, 170
caste system effects on, 170
ethnohistory of, 235
fossil man in, 31-32, 43
geneic studies in, 60-63, 65, 67-68
kinship system in, 21-22
morphology in, 77
society compared with Assam, 210

Indians
see American Indians; North American Indians; South American Indians; or India

Indo-European languages
kinship terms in, 313
Proto-Indo-European, 402

Indonesia
central Flores
nutrition in, 70
geneic studies in, 61
middle Flores
growth and development in, 71

Infanticide
demographic aspects of, 157, 167

Eskimo, 212-13

Inflection
semantic layer in, 360
information processing semantic, 283

Intelligence
artificial, 287

Interaction strategy
social aspects of, 257-65

Interactive behavior
measuring and ordering observations of, 6-12

International Biological Programme
human adaptability project of, 82-83

Iran
anthropometry of Bori Ahmedi, 75
demographic studies in, 162
genetic studies of, 62, 67

Ireland
demographic studies of, 58

Iroquoian language
classification and families of, 412, 419

Iroquois Indians
pottery styles of, 133
sociopolitical evolution warfare tactics, 250

Islamic Africa
ethnohistory of, 234

Island populations
demographic studies of, 155

Israel
fossil man in, 37
geneic studies in, 60

Italy
demographic studies in, 169
genetic studies in, 60-61, 64-65
settlement patterns in, 156

J

Jamaica
fishermen in
game theory of, 251-52
growth and development in, 72, 74
nutrition in, 69-70
post-Creole variations in, 369

Japan
demographic studies of, 58-59
genetic studies in, 60-61, 64, 66, 68
growth and development in, 72-74
nutrition studies in, 70

Japanese
cold adaptation by, 80
demographic studies of, 171

Java
fossil man from, 36, 39
morphology in, 77

Javanese
structure of folk dramas of, 337

Jaw (mandible)
of hominids, 30-32, 39-40
photographs of R. wickeri, 31

Jewish ghettos
demographic studies of, 170

Jicaque Indians
demographic studies of, 58

Jivaros
genetic studies of, 67
warfare strategy of, 250

Judeo-Christian culture
development of, 236
<table>
<thead>
<tr>
<th>SUBJECT INDEX</th>
</tr>
</thead>
<tbody>
<tr>
<td>K</td>
</tr>
<tr>
<td>Kalahari Bushmen see Bushmen of South Africa</td>
</tr>
<tr>
<td>Kalispel Indian grammar of, 417</td>
</tr>
<tr>
<td>Kaminaljuyu project in cross-dating, 116-17 demographic studies at, 161</td>
</tr>
<tr>
<td>Kanapoi fossil man from, 35</td>
</tr>
<tr>
<td>Karankawa languages classification and families of, 412, 420-21</td>
</tr>
<tr>
<td>Karimajong ecological systems of, 216</td>
</tr>
<tr>
<td>Kashmir morphological studies in, 77</td>
</tr>
<tr>
<td>Kenya fossil man in, 30, 32-35, 40 genetic studies in, 64-66</td>
</tr>
<tr>
<td>Kenapithecus africanus evolution of, 28 fossils of, 32</td>
</tr>
<tr>
<td>Kenapithecus wickeri see Ramapithecus wickeri</td>
</tr>
<tr>
<td>Keresan languages classification and families of, 412, 421</td>
</tr>
<tr>
<td>Khasi genetic studies of, 62</td>
</tr>
<tr>
<td>Khmer settlement patterns of, 131</td>
</tr>
</tbody>
</table>
fossil man from, 35
Lucknow
demographic studies of,
56
Lumbreras
demographic studies at, 161
Lurin Valley
demographic studies at, 161

M

Macao
genetic studies in, 62
Macedonia
growth and development in,
73
Madhya Pradesh
demographic studies of,
58
Madras, India
kinship system in, 21
Magic
structural aspects of, 333
Makapan, South Africa
fossil man from, 32-33,
38
Makiritare
demographic studies of, 58
genetic studies of, 60-61,
63-65
Malaria
genetic aspects of, 61, 66
Malaysia
genetic studies in, 60, 64,
66
Malaysia, West
demographic studies of,
56
Malecite-Passamaquoddy
grammatical sketch of, 415
Male genitalia
measurement of, 75
Malta
genetic studies in, 66
Man
modern, 55-112
conclusion, 81-83
demography, 55-59
genetic studies, 59-69
growth and development,
71-75
morphological studies, 76-
77
nutrition, 69-70
physiology and adaptation,
77-81
see also Fossil man
Mandan
linguistic study of, 419
Mandible
see under Jaw
Manipur
morphology in, 77
Maori
settlement patterns of,
134
Market
distributive mechanisms of
minimal sequencing of
interactions in, 18
Marriage
consanguineous
demographic studies of,
58-59
Marriage classes
ecological considerations
of, 156
Mating
assortative, 170-71
Maya
demographic studies of,
58, 160-61
ethnohistory of, 237,
242
kinship terms of, 392
settlement patterns of,
131
Mayapan
demographic studies at,
161
Mbuti pygmies
ecological studies of, 215-
16
Medicine
semantics of, 388
Mediterranean basin
demographic studies of,
160
Meganthropus paleojavanicus
fossils of, 45
Melanesia
demographic studies of,
170
genetic studies in, 62,
65
settlement patterns in,
131
Melungeons of Tennessee
study of, 82
Memory
models of, 283-86
Menarche
onset of, 72-73
Mental retardation
and dermatoglyphics, 68
Merina
kinship terms of, 318
Mesoamerica
demographic studies of,
157-58
ethnohistory of, 232, 234-
35, 237-39, 241-42
settlement patterns in,
131, 133, 141-44
Mesolithic era
literature sample on, 192,
197-98
Mesopotamia
demographic studies in,
162
settlement patterns in, 133-
34, 141
Metaphor
in kinship semantics, 318
Mexicans
genetic studies of, 67
Mexico
demographic studies of, 158,
160-61
developmental cycle of co-
parent selection in, 255
ethnohistory of, 237
valley of
ethnohistory of, 237
settlement patterns in,
141-44
Middle East
demographic studies of, 161-
62
development of agriculture
in, 201
literature sample, 192
distribution, 195-97
Migration
and fertility, 56
Mikasuki
linguistic study of, 418
Mindel age
Acheulian industry of,
37
fossil sites of, 42
Minimal sequencing
of behavioral interactions,
12-19
Miocene era
fossil man in, 28
Miri
genetic studies of, 67
Mississippi Valley
settlement patterns in,
128
Mixtec
ethnohistory of, 239, 241-
42
Modeling
of cultural behavior, 19-25
Modern man
see Man, modern
Mohawk language
structure of, 373
Mokil atoll
demographic study of, 58
Monkeys
demographic studies of, 153-
55
Montagnais-Naskapi Indians
transformational analysis
of, 343
Monte Alban
demographic studies at,
161
Montmaurin, France
fossil man from, 37
Morphology
body type, 76-77
emic-etic approach to,
384
literature sample on, 193,
203
Morphophonemic statements
definition of, 391
Mortality causes
demographic studies of, 157
Moscow
geneic studies in, 62
Mozambique
geneic studies in, 61-62, 64, 68
Bantu, 62, 64, 68
Multilinear evolution
definition of, 248
Muskoegan
linguistic classification of, 418
Mythology
structural studies of, 334, 336, 340-41, 344
Myths
histories implications of, 240

N
Na-Dené language
classification and families of, 412-14
Namibikwara
leadership organization in, 17
Natchez-Muskogean
linguistic classification of, 418
Navajo (Navaho)
language of, 298
linguistic studies of, 414-15
Near East
ethnography of, 234, 237
Negentropic processes
in structuralism, 331, 339
Negro
lactase deficiency in, 69
Neolithic era
literature sample on, 192, 197-98
Nepal
demography of, 55
genetic studies of, 62
morphological studies of, 77
Nepa Valley
demographic studies at, 161
Network analysis
social, 249, 258-59
Newars
genetic studies of, 67
New Britain
genetic studies in, 68
New Guinea
genetic studies in, 60-63, 66, 68
Nadang, 62
growth and development in, 72
Bundu and Asai, 72
nutrition in, 70

New Guinea Highlands
ecological studies in, 218-19, 221-23
New World
development of agriculture in, 201
prehistoric settlement patterns in, 129-31, 144
New Zealand
demographic studies of, 56
literature sample, 192
distribution, 195-97
settlement patterns of
Polynesians in, 134
Nez Perce
kingship terms of, 400
linguistic classification of, 417
Ngorora, Kenya
fossil man from, 32
Nicarao-Mangue
ethnography of, 239
Nigeria
demographic studies of, 56
genetic studies in, 66
growth and development in, 72
Tiv ecological study, 219
Nilghiri Hills
Kota of
demographic studies of, 57-58
Nochixtlan Valley
demographic studies at, 161
Nominalization
alternative ways of expressing, 354
Nootkan Indian
linguistic classification of, 415
North America
literature sample, 192, 195-97
North American Indians
ethnography of, 234-36, 238-40
folktales of
structure of, 337
settlement patterns in, 127-37, 141
Northwest Coast Indian of
America
demographic studies of, 164
ethnography of, 236, 238
Northwest Pacific Coast
myths of, 337
Norway
genetic studies in, 60-61, 63, 65-66
morphology in, 77
Norwegians
cold responses of, 81
Nucleosidase phosphorylase
genetic aspects of, 66
Nuer
ecological studies of, 216-17
ethnography of, 238, 241
time reckoning, 241
Numeral classifiers
semantics of, 388
Nutrition
modern studies of, 69-70
lactase deficiency, 69
population studies, 69-70
protein calorie malnutrition, 70

O
Oaxaca
demographic studies at, 161
ethnography of, 237, 241
Obsidian hydration dating
description and current status of, 122-23
Oceania
genetic studies of, 63
kinship systems in, 342
settlement patterns in, 134
Ojibway Indians
ecological systems of, 214-15
Okanagan Indians
folklore of, 344
Olduvai Gorge, Tanzania
dating techniques used at, 120-21
fossil man from, 33-37, 40-41
Omaha Indians
kinship terms of, 325
Omo, Ethiopia
fossil man from, 36-37, 40
Onondaga
linguistic study of, 419
structural aspects of language of, 359
Oral traditions
structural analysis of, 336-37
Oriental
cold response of, 80
Orissa, India
genetic studies in, 62, 65
growth and development in, 73
Otomis
ethnography of, 239
Ottawa (Odawa) Indian
language of, 415

P
Pacific coast
SUBJECT INDEX

PROTOSYNTHEX III
description of, 285
Provenience
linguistic, 293
Psycholinguistics
developmental, 363
Puebla Plain, Mexico
demographic studies at, 161
Pueblos
settlement patterns of, 133,
138-39
Punjabis
genetic studies of, 67
growth and development of,
72
Pygmy, African
demographic studies of, 157

Q
Quaker meetings
leadership in, 17
Quechua
cold adaptation by, 80-
81
Quetzalcoatl
ethnohistorical aspects of,
241
Queuing relation
linguistic, 292-93
Quichean
ethnohistory of, 239
Quileute
linguistic study of, 420
Quintana Roo
demographic studies of, 160

R
Radiocarbon dating
current status of, 119-20
Ramapithecus punjabicus
chronology of, 42
evolution of, 28, 30
fossils of, 32
taxonomy of, 43, 48
Ramapithecus wickeri
chronology of, 42
evolution of, 28, 30
fossils of, 30-32
photographs of, 31
taxonomy of, 43, 48
Reciprocity
in social structure, 257-
66
Red blood cells
genetic studies of enzymes
of, 60-62
Religion
and fertility, 56
semantics of, 388
Resident patterns
and distribution correlates,
189
Riddles

generation of, 343
Romania
anthropometry in, 75
genetic studies in, 62, 67
growth and development
in, 73-74
morphology in, 76-77
Rome
ethnohistory of, 235
genetic studies in, 61
Ruanda
demographic studies of, 57
ethnohistory of, 241
Russia
ethnohistory of, 235
genetic studies of, 67
hemoglobin variant in, 81
Russian language
comitative case in, 357

S
Sacrifice
structural aspects of, 333
Sahaptian
Proto-Sahaptian, 402
Salishan languages
classification and families
of, 412, 416-17
Samoa
ethnohistory of, 239
Sangiran, Java
fossil man from, 36
Santal
demographic studies of, 57
Sanusi
ethnohistory of, 239
Sapir-Whorf hypothesis, 340,
345
Sarcee dialect
linguistic classification of,
414
Sardinia
genetic studies of, 62
Saskatchewan, Canada
adaptation studies in Jasper
region of, 254-55
Scientific
revolutionary progression
in development of, 179-
80
Secular variation curve
description of, 118
Segregates
definition of, 386
Semang
demographic studies of, 157
Semantics
generative, 366
and ethnoscientific, 273
schema of, 278
and typology, 278-83
interpretable vs generative,
354-56
of kinship, 309-28
relationship with syntax,
361, 374
semantic analysis, 386-
87
structural aspects of, 357-
61
three types of meaning, 404-
5
in transformational grammar,
387
Seminole Indians
genetic studies of, 67
Semiotics
approaches to, 336
Semolexemics
case relations in, 358
Seneca
kinship terms, 390
Sentence Association tests
structural use of, 343
Sentences
generation of, 369
structural aspects of, 350-
51
Seriation
computer-assisted, 115
validity of, 115
Serum polymorphisms
generative aspects of, 63-
65
Settlement patterns
community, 130, 137-40
definition of, 128-29
development of concept and
methodology, 127-37
American tradition, 127-
36
English tradition, 136-37
introduction, 127
selected case studies, 137-
44
individual structure, 137
region, 140-44
settlement, 137-40
systems analysis of, 133
use of ethnohistoric analogy
in interpreting, 131
zoological, 130, 140-44, 160-
62
Sexual behavior
of nonhuman primates, 154
Shamanism
behavioral interactions in
minimal sequencing of, 16
Shoshoni
linguistic study of, 420
Sinai
genetic studies in, 60-62
Singapore
growth and development in,
73
physiological studies in,
78
Siouan Indian languages

classification and families of, 412, 418-19
Sioux Indians genetic studies of, 64
Swatik hills, India fossil man from, 30, 32, 40
Skeletons determination of age and sex of, 159-60
literature sample on, 193, 203
Skin color assortative mating effects on, 171
genetic studies of, 68
Slovakian gypsies genetic studies of, 65, 67
Slovenia growth and development in, 72
Slum social order of, 19
Snohomish linguistic study of, 416
Social change models of, 249
Social strategies and social relationships, 247-70
adaptive strategies, 250-57
introduction, 247-50
reciprocity and interaction strategy, 257-65
summary, 265-66
Social structure minimal sequencing of, 12-19
search for, 1-6
Social theories major convergences among, 12
Society see Environment, subsistence, and society
Sociolinguistics ethnoscientific convergences with, 273-74, 288-69
macro- and micro-, 288-89
Somali morphology in, 76
South Africa Bushmen of Kalahari demographic studies of, 157
genetic studies in, 61, 67
Bushmen-nonBushman, 65
South America ecological studies of, 220
genetic studies in, 63, 65, 68
literature sample, 192, 195-96
South American Indians demographic studies of, 169
Southeast Asia development of agriculture in, 201
ethnohistory of, 235
settlement patterns in, 131
Southwest United States demographic studies in, 161
settlement patterns in, 130
Soviet Union (Russia) growth and development in, 72-73
see also Russia
Spain genetic studies in, 61
Speech traditional parts of structural aspects of, 282-83
Spinal curvature comparative study of, 74
Squamish linguistic study of, 416
Star Carr settlement patterns at, 136
Steinheim, Germany fossil man from, 37
Sterkfontein, South Africa fossil man from, 32-33
Strategic interaction analysis, 263-64
Strategy definition of, 248
Strategy-strategem definition of, 250
Structural anthropology social strategies and relationships, 247-70
Structuralism in cultural anthropology, 329-48
background, 332-34
diachronic view of, 334
introduction, 329-32
recent contributions, 334-38
summary and conclusion, 344-45
theory, 338-41
transformational analysis and testable hypotheses, 341-44
Structural linguistics, 384-94
componental model, 386-94
componental approach in anthropology, 388-94
semantic analysis, 386-87
emic-etic model, 384-86
Structure of language presuppositions in, 366-87
surface and deep, 351-53
of social behavior, 249
Subsistence see also Environment, subsistence, and society
Sudan demographic studies of, 56
morphology in, 76
Sudanese kinship system of, 24
Surinam anthropometry in, 75
Swanscombe, England fossil man from, 37
Swarthmannsomin fossil man from, 29, 33, 40
Swa Pathans genetic studies in, 64
strategy analysis of, 251-53
Sweden genetic studies in, 60, 64, 67
Sweet potato ecological studies of, 221-22
Switzerland demographic studies of, 58
genetic studies in, 60
Symmetry structural aspects of, 342
Syntax autonomous demise of, 274
co-occurrence restrictions in phonology and, 387
emic-etic approach to, 384
relationship to semantics, 361, 374
see also Grammar; Tagmemics; Transformational grammar
Systematic Index
in settlement patterning, 132
Systematics logic of, 210
Systems analysis in dating methodology, 113-14
in social anthropology, 265
T
Tabu structural aspects of, 335-36
Tagmemics theoretical aspects of, 350-51
Taiwan
demographic studies in, 56-57
genetic studies in, 66-67
morphology in, 76
Tarascan ethnohistory of, 239
Taxonomy (Ethnoscience)
block diagram and directed graph of, 275
hypothetical 3-level, 294
breath-first search, 296-97
3-dimensional matrix, 295
graph of attribution relations, 300
subtree search, 298-99
Taxonomy (Fossil man)
current status of, 43-46
Teachable Language Comprehender (TLC)
description of, 284-85
Telanthropus classification of, 29, 33, 47
see also Australopithecus robustus
Teotihuacan Valley, Mexico demographic studies of, 160
settlement patterns in, 131, 133, 141, 143
Tepepi el Viejo demographic studies at, 161
Terena demographic studies of, 58
Termitlne, Algeria fossil man from, 36
Texcoco settlement patterns in, 141-44
Thailand demographic studies of, 56
genetic studies of, 62
lactase deficiency in, 69
Thermoluminescence as dating technique, 121-22
Thermoregulation comparative studies of, 80-81
Tibet genetic studies in, 67-68
Tikal demographic studies of, 160
Timbira transformational analysis of, 343
Time strategic importance of, 255
Time reckoning Hopt, 239-40
Nuer, 241
Timucua languages classification and families of,
<table>
<thead>
<tr>
<th>Subject</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>growth and development in, 73-74</td>
<td></td>
</tr>
<tr>
<td>see also America</td>
<td></td>
</tr>
<tr>
<td>Uto-Aztecan languages classification and families of, 412, 419-20</td>
<td></td>
</tr>
<tr>
<td>Utrecht demography of, 56</td>
<td></td>
</tr>
<tr>
<td>Uzbek genetic studies of, 67</td>
<td></td>
</tr>
<tr>
<td>V</td>
<td></td>
</tr>
<tr>
<td>Valley of Mexico see Mexico, valley of</td>
<td></td>
</tr>
<tr>
<td>Vallonnet Cave, France artifacts from, 42</td>
<td></td>
</tr>
<tr>
<td>Valparaiso demographic studies of, 58</td>
<td></td>
</tr>
<tr>
<td>Venezuela genetic studies in, 64</td>
<td></td>
</tr>
<tr>
<td>Verb-first propositional form predicate calculus of, 290</td>
<td></td>
</tr>
<tr>
<td>Verona, Italy growth and development in, 72</td>
<td></td>
</tr>
<tr>
<td>Vertesszölös, Hungary fossil man from, 37, 42</td>
<td></td>
</tr>
<tr>
<td>Vietnam anthroscopy in, 75</td>
<td></td>
</tr>
<tr>
<td>genetic studies in, 60-61</td>
<td></td>
</tr>
<tr>
<td>Viru Valley, Peru demographic study of, 160</td>
<td></td>
</tr>
<tr>
<td>settlement patterns in, 128-29, 131, 141</td>
<td></td>
</tr>
<tr>
<td>Vocabulary partitioning of, 293-302</td>
<td></td>
</tr>
<tr>
<td>W</td>
<td></td>
</tr>
<tr>
<td>Wabash Valley settlement patterns in, 132</td>
<td></td>
</tr>
<tr>
<td>Wainwright Eskimos see Eskimos</td>
<td></td>
</tr>
<tr>
<td>Wajana Indians anthropometry of, 75</td>
<td></td>
</tr>
<tr>
<td>Wakaskan languages classification and families of, 412, 415-16</td>
<td></td>
</tr>
<tr>
<td>Warfare tactics to show sociopolitical evolution, 250</td>
<td></td>
</tr>
<tr>
<td>Wari demographic studies of, 161</td>
<td></td>
</tr>
<tr>
<td>Weatherhill Mesa dendrochronology of, 118</td>
<td></td>
</tr>
<tr>
<td>Weddings semantics of, 388</td>
<td></td>
</tr>
<tr>
<td>West Indies genetic studies in, 61-62</td>
<td></td>
</tr>
<tr>
<td>growth and development in, 73</td>
<td></td>
</tr>
<tr>
<td>West Irian Grand Valley Dani of ecological studies of, 221-22</td>
<td></td>
</tr>
<tr>
<td>West Virginia genetic studies in, 63</td>
<td></td>
</tr>
<tr>
<td>Wichita Indians linguistic study of, 419</td>
<td></td>
</tr>
<tr>
<td>Wife-trading Eskimo, 212</td>
<td></td>
</tr>
<tr>
<td>Wik Mankan kinship terms of, 312</td>
<td></td>
</tr>
<tr>
<td>Witchcraft environmental aspects of, 215</td>
<td></td>
</tr>
<tr>
<td>Wyot Indians linguistic classification of, 415</td>
<td></td>
</tr>
<tr>
<td>Word Association Tests cultural aspects of, 339-40, 343</td>
<td></td>
</tr>
<tr>
<td>Work capacity training effects on, 78</td>
<td></td>
</tr>
<tr>
<td>Work patterns interactive behavioral study of, 8</td>
<td></td>
</tr>
<tr>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Xochicalco demographic studies at, 161</td>
<td></td>
</tr>
<tr>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Yams ecological considerations of, 220-21</td>
<td></td>
</tr>
<tr>
<td>Yankee City interactions in, 19</td>
<td></td>
</tr>
<tr>
<td>Yanomama Indians demographic study of, 58, 170</td>
<td></td>
</tr>
<tr>
<td>settlement patterns and warfare patterns of, 146</td>
<td></td>
</tr>
<tr>
<td>Yemenite Jews genetic studies of, 61</td>
<td></td>
</tr>
<tr>
<td>Yokuts linguistic study of, 417</td>
<td></td>
</tr>
<tr>
<td>Yucatan ethnohistory of, 237</td>
<td></td>
</tr>
<tr>
<td>Yucatec ethnohistory of, 239, 241</td>
<td></td>
</tr>
<tr>
<td>Yuchi language classification and families of, 412, 421</td>
<td></td>
</tr>
<tr>
<td>Yugoslavia genetic studies of, 65, 67</td>
<td></td>
</tr>
<tr>
<td>growth and development in, 72-73</td>
<td></td>
</tr>
<tr>
<td>morphology in, 77</td>
<td></td>
</tr>
<tr>
<td>Yukon language classification and families of, 412, 420</td>
<td></td>
</tr>
<tr>
<td>Yuma Indian linguistic study of, 418</td>
<td></td>
</tr>
<tr>
<td>Proto-Central Yuman kinship terminology, 402</td>
<td></td>
</tr>
<tr>
<td>Yurok Indians linguistic classification of, 415</td>
<td></td>
</tr>
<tr>
<td>Yusufzai Pathans (Pakistan) land control strategies of, 252-53</td>
<td></td>
</tr>
<tr>
<td>Z</td>
<td></td>
</tr>
<tr>
<td>Zapotec ethnohistory of, 239, 242</td>
<td></td>
</tr>
<tr>
<td>Zinjanthropus boisei see Australopithecus boisei</td>
<td></td>
</tr>
<tr>
<td>Zonal settlement pattern as approach to demography, 160-61</td>
<td></td>
</tr>
<tr>
<td>Zooemesh American Indian, 343</td>
<td></td>
</tr>
<tr>
<td>Zulu ethnohistory of, 238</td>
<td></td>
</tr>
<tr>
<td>sociopolitical evolution in warfare tactics, 250</td>
<td></td>
</tr>
<tr>
<td>Zuni Indians genetic studies of, 64</td>
<td></td>
</tr>
<tr>
<td>kinship terms of, 318</td>
<td></td>
</tr>
<tr>
<td>languages of classification and families of, 412, 421</td>
<td></td>
</tr>
</tbody>
</table>
Noted in the bound periodical card index.

Anthropology