The Utility of the Equilibrium Model in the Study of Social Change

William E. Carter

Secular Reinforcement in Aymara Death Ritual

Henry Orenstein

The Ethnological Theories of Henry Sumner Maine

Keith F. Otterbein

Internal War: A Cross-Cultural Study

Brent Berlin, Dennis E. Breedlove, and Peter H. Raven

Evolution of Primate Vocal-Auditory Communication Systems

Peter Carlton Reynolds

Origin of Large Households and Duolocal Residence in Central Japan

Harumi Befu

OBITUARIES

Alfred Vincent Kidder 1885–1963

Robert E. Greengo

Verne Dusenberry 1906–1966

Carling Malouf

BRIEF COMMUNICATIONS

Tribes and Traits: A Smallest Space Analysis of Cross-Cultural Data

Milton Bloombaum

Intergenerational Value Differentials and Family Structure among the Wind River Shoshone

Stanton K. Tefft

Bwaidongan Descent Groups

Michael W. Young

An Extension of Naroll's Linked Pair Solution to Galton's Problem

William D. Crano

The Position of Women in Anthropology

Ann Fischer and Peggy Golde

A Note on Unconscious Structure in the Anthropology of Edward Sapir

C. N. Modjeska

A Possible Benefit from Tooth-Blackening

Howard L. Bailit

Shamanizing on an Empty Stomach

Don Handelman

Shamanism, Schizophrenia, and Scientific Unity

John H. Weakland

LETTERS TO THE EDITOR

Personality and Religion in Burma

Everett E. Hagen

Religion, Personality and Behavior in Burma

Melford E. Spiro

The "Hole" Community

Norman E. Whitten, Jr.

Additional Clarification about a "Disadvantaged" Community

Miles Richardson

On Sather's Review of Sopher

Thomas R. Williams

Concerning Lamb's Review of Chomsky

Karl V. Teeter

Lamb's Reply to Teeter

Sydney M. Lamb

BOOK REVIEWS

General and Ethnology

Hoebel: Anthropology: The Study of Man (Whiteford)

Hymes (ed.): The Use of Computers in Anthropology (Coyeill)

Nash: Primitive and Peasant Economic Systems (Dalton)

Polanyi: Dahomey and the Slave Trade: An Analysis of an Archaic Economy (Belshaw)

Lowie: Culture and Ethnology (Stocking)

Radnitz: The Method and Theory of Ethnology: An Essay in Criticism (Stocking)

Kramer: Breaking Ground: Notes on the Distributions of Some Simple Tillage Tools (Soper)

Lewis: La Vida: A Puerto Rican Family in the Culture of Poverty—San Juan and New York (Manners)

Slobodin: Metis of the Mackenzie District (Berry)

Helm and Lurie: The Dogrib Hand Game (Roberts)


Euler: Southern Paiute Ethnobiography (Jorgensen)

Lister and Lister: Chihuahuas: Storehouse of Storms (Riley)

Vogt (ed.): Los Zinacantecos: un pueblo Taotil de los Altos de Chiapas (Reina)

Reina: The Law of the Saints: A Pocham Pueblo and Its Community Culture (Nash)

Spencer: Shifting Cultivation in Southeastern Asia (Yenoyan)

Van Baal: Dama: Description and Analysis of Marind-Anim Culture (South New Guinea) (Mead)

Finney: Polynesian Peasants and Protestants: Socio-Economic Change among the Tahitians of French Polynesia (Dewey)

Dounyenge: L'Homme dans le Pacifique Sud: Etude geographique (Finney)

Scott: On the Cordillera: A Look at the Peoples and Cultures of the Mountain Province (Douglas)

Benedict: People of the Seychelles (Colson)

Bacon: Central Asians under Russian Rule: A Study in Culture Change (Adelman)

Allworth (ed.): Central Asia: A Century of Russian Rule (Adelman)

Philips: Unknown Oman (Swez)

Goody (ed.): Succession to High Office (Swartz)

Sagree: Age, Prayer, and Politics in Tiri, Kenya (Winans)

Vansina: Kingdoms of the Savanna (Richards)

Basden: Among the Ibo of Nigeria: An account of the Curious & Interesting Habits, Customs & Beliefs of a little known African People by one who has for many years lived amongst them (Ottenberg)

Basden: Niger Delta: A Description of the Primitive Life, Customs and Animistic Beliefs, etc., of the Ibo People of Nigeria by One Who, for Thirty-five Years, Enjoyed the Privilege of Their Intimate Confidence and Friendship (Ottenberg)

Arjyle: The Folk of Dahomey: A History and Ethnography of the Old Kingdom (Lloyd)

Religion and Myth

Douglas: Purity and Danger: An Analysis of Concepts of Pollution and Taboo (Spiro)

Banton (ed.): Anthropological Approaches to the Study of Religion (Wallace)

Wallace: Religion: An Anthropological View (Geertz)

Edsman (ed.): Studies in Shamanism (Spencer)

Ferreira: Totemism in India (Orama)

Welbourn and Ogutu: A Place to Feel at Home: A Study of Two Independent Churches in Western Kenya (Simpson)

Ladurie: Piques Africaines: de la communauté clanique à la communauté chrétienne (Sundkler)

Jacobs (compiler) and Greenway (ed.): The Anthropologist Looks at Myth (Spencer)

Littleton: The New Comparative Mythology: An Anthropological Assessment of the Theories of Georges Dumézil (Swez)

Rappaport: Myth and Legend of Ancient Israel (Walsh)

Arts, Crafts, Music, and Literature

Schuster: The Art of Two Worlds: Studies in Pre-Columbian and European Cultures (Chapman)

ii
The Utility of the Equilibrium Model in the Study of Social Change*

MAX GLUCKMAN
University of Manchester

There has been considerable criticism of the use of the equilibrium model by social anthropologists. The argument of this paper is that much of this criticism is due to a misunderstanding arising because though all social life exists in time, and all processes in time involve changes, we have to define several different kinds of "time," as well as distinctive types of "change." It is contended that each type of social institution, or cultural pattern, has a particular kind of time scale in its very structure. Thus the structure of a family system can only be analyzed in four generations, and in subsistence systems perhaps only in five to six generations. Other institutions have other built-in time scales. It is proposed to call the time scale of an institution its structural duration. One important way of studying an institution is to analyze its structural duration. In an analysis of this kind, the emphasis is on the manner in which the institution would operate through time if internal contradictions or external intruding events did not interfere with its passage through its structural duration. Therefore an analysis of the structural duration of an institution is necessary in an equilibrium model. Many studies have taken this form, but have been misused as if they were dealing with what happened in actual historical time. It is argued that since changes are of several kinds in a suggested range, from repetitive or recurrent changes of personal through limited structural changes to radical structural changes, it is possible to assess what kinds of changes are occurring only by examining them within the structural durations of institutions, held steady as a first step in analysis as if in actual equilibrium. But this provides only a heuristic scheme in which to handle the observations; it is not a theory giving us a set of interdependent propositions. More and more actual changes may be fed in as the analysis tries to cope with greater ranges of reality. The argument is illustrated with examples from studies of several social spheres. There is also a discussion of how the method enables the analyst to analyze observations collected over a limited period of time in terms of a much longer period in order to assess the types of change that are occurring.

IN THIS address I hope to clarify some of the implications in the use of the so-called equilibrium model in the study of allegedly stationary societies. I believe that clarification is necessary because of present-day criticisms of the model, some of which, I feel, arise from a misunderstanding of what its exponents were doing, although undoubtedly those exponents did not always themselves make their intentions clear. Obscurity and ambiguity have accumulated around the problem. If we can remove these so the problem becomes clear, I believe discussion will then show that the equilibrium model has what Leach has called great power (1954: 4), not only for the analysis of seemingly stationary social systems but also as one method for studying changing social systems, though I shall disagree with Leach about the reasons for this power. I also believe that if we can clarify the situation we may be some steps nearer to an attempt at bringing the results of studies of the structure of institutional systems together with those of the structure of fields of interaction between persons; this seems to me to be one crucial problem facing social anthropologists in the coming years.

I have defined the "equilibrium model" as one method for studying changing social systems. I emphasize that it is only one method. First, our field of study is so complex that there are necessarily many different approaches to analysis, each fruitful in its own way; if I argue the merits of one method, this is not to deny that others have advantages. Second, it is a method, a manner of approaching the study of social systems, because it illuminates understanding of the structure of those systems in reality. It might be described as a heuristic scheme; or we might follow Merton in calling it an orientation, and perhaps even class it as one of those orientations that he said are neglected at the peril of the investigator (1957: 87-89). Third, the equilib-
rium model is a method, and not in itself a theory, for it is not a body of interdependent propositions about the structure of social systems. But it can form a framework for a set of such propositions (see Homans 1950:1–11).

In his subsequent Highland Burma (1954, 1964) Leach worries about the difficulties of the equilibrium model, though (I repeat) he says it has “great power” in sociological analysis. These worries he sees as emerging from the fact already frequently stressed that “real societies exist in time and space... Every real society is a process in time...” (1954:5). The problem of time is critical for all studies of social and cultural systems.

Out of our common tradition, I believe we derive a common fascination with customs, and I believe that it is through this fascination with customs that we make our distinctive contribution to the social and behavioral sciences (Devons and Gluckman 1964:234 f.). But it is a long time since anthropologists operated with single, isolated customs. Cultural (sci. culturological) anthropologists seek patterns in customs; and psychological anthropologists seek their functions, and to study the antecedent relations between patterns of customs and personality structures. We sociological anthropologists see customs as linked together into what some call institutions, which are in some way external to, independent of, and constraining on, the individuals within them (to use Durkheim's criteria for what he called "social facts"). I do not want to enter into controversies about the different ways in which the term "institution" has been used in anthropology and sociology. Perhaps you will allow me to use it as a shorthand expression to cover the facts that men act in standardized roles, through specific modes of behavior, with reference to other men, as they aim to achieve certain purposes in an ecological, biological, and social environment; and that these purposes and the means of achieving them are defined by a set of controlling rules with guiding beliefs and values. Finally, in their actions men use types of material goods (cf. Malinowski, 1922).

When we deal with customs not as isolates, but as constituting patterns, or as influencing the development of types of personality, or as dominating social relationships within institutions, we come up at once against the problem of time—development in time, and change through time. It is about the significance of time and changes in time that much of the obscurity has accumulated; for "time" exists in social life, as many studies have shown, in different contexts and with different parameters. We can assess, a "real" history of events that have constituted the past of any personality or culture or society, events that in a phylogenetic sense account for its present form. Secondly, as many historians and social scientists have pointed out, there is an incubation of past events within the present pattern of society; thus Fortes wrote, "Among the Tallensi the lineage system enables us to see the operation of the time factor in social structure in a very concrete way. We see how the lineage structure at a given time incubates [with acknowledgements to R. G. Collingwood all that is structurally relevant of its past phases and at the same time continuously thrusts its growing-points forward" (1945:224). There is a similar incubation of the past within the present structure of a particular personality. Thirdly, this process of incubation produces certain standardized ways of inhibiting, in varying social contexts, held by membership in a particular society, on which anthropologists and other scholars have written at length, as we shall see later; while similarly individuals have varied conceptions of their own history and of time arising from the accumulation of experience and reaction to experience. Fourthly—and it is with this problem that I am mainly concerned—a cultural pattern, or a personality, or an institution, has its own time-scale "built into" it, if you will permit me a metaphor. The speed at which a personality operates is extremely high, and has no reference to the ends by which we normally measure time; all students of personality are aware of this. Similarly, I would argue that each institution has a specific time-scale built into its structure, and we cannot understand an institution unless we do so in that scale. Fortes has put the problem thus: "social life is a pattern of processes, and the time dimension is a significant one in it" (1945:1); and "the dynamic equilibrium of a lineage is an equilibrium in time" (1945:224). To avoid confusion, I shall follow a suggestion made to me by John A. Barnes, with whom I have had fruitful discussions of the general problem, and call the time-scale built into an institution, that institution's "structural duration."

The "structural duration" of an institution is that period of time required to work out the interdependence of its elements with the biological, ecological, and social environment (see Homans 1950:81 f. on external environment). This period of time, the duration, is contained in the structure of the institution, and it is only in terms of expounding the institution through that duration that we can work out the interdependence, the systematic structure, between the elements that comprise the institution—what Fortes has called the contemporaneous interdependence of variables (with no period attached to "contemporaneous"). Since we are here concerned with establishing constancies, it follows that this type of exposition is in terms of an equilibrium model, with the institution working through a cyclical, or oscillatory, or other form of process at the end of which it presents the same form as it started from.

Three caveats are essential in approaching the model. First, an analysis of this kind does not show that the institution has operated in that way in the past, or will continue to operate thus in the future; the analysis is not dealing with the institution in real, historical time, nor affirming that the institution is in a state of actual equilibrium—a state that we can describe as static. The idea of an equilibrium through time, with the elements of the institution in a "state of balance," and the fiction of stable equilibrium, "tending to recover equilibrium [stable balance] after disturbance" (Oxford English Dictionary), are devices to enable us to handle the time element involved in the assumed systematic interdependence of elements of the institution. We assume that this interdependence exists because that makes possible scientific analysis; and the assumption is in reality warranted insofar as we observe that the events relevant to the institution occur in a regular and not a haphazard manner.

Time here is handled in an as if manner, as Veblen put it; indeed Leach speaks of "as if systems" (1964:Introduction to reprint of 1954) though he sees the balance as being, for purposes of analysis, one of facts forced "within the constraining mould of an as if system of ideas, composed of concepts treated as if they were part of an equilibrium system." I shall argue later that Leach underestimates the dependence of the idea of equilibrium on reality (as Vaihinger underestimated the connection of scientific laws with reality). I myself feel that this model has its greatest power because it "maps reality" in the mathematicians' phrasing. But it is useful to speak of an "as if" equilibrium in order to emphasize that it is abstracted from reality, since it may not deal with the whole complexity of reality. I see it as a framework, which is based on observations of reality and then tested against other observations. We employ it in order to handle the time element, the duration, involved in the structure of the institution, since only thus can we analyze the real interdependence that exists within the institution as an organized structure.

In reality, many external events, and internal "distortions," prevent the institution working thus perfectly, and these have to be fed into the analysis, a most important step for a fuller study. The equilibrium model is used to provide a framework within which we can formulate propositions about the interdependence of elements; it is these propositions which determine the future. The model too must contain a time element, since all reality is a process in time. To formulate such propositions we need for the time being to exclude interference.

The second caveat is that the very definition of equilibrium contains the idea of disturbance, with return to the previous state after that disturbance. In short, we assume in advance that the working of an institution will contain, in the very structure of the institution, processes occurring without any purposeful action on the part of its personnel, as well as from strife between its personnel. If the institution is functioning, balancing forces and repressive mechanisms come into play to restrain the effects of the disturbance in order to preserve, or restore as far as possible, the earlier form.

Third, the model of the system in no way states that the same persons will occupy the same positions relative to one another throughout the duration of the institution's working. Insofar as there is continuity, it is in the roles, not in the people.

Finally, I repeat caveats entered earlier. The use of the equilibrium model is to provide...
family structure. I shall argue later that in some societies the relevant number of generations, in part so-to-speak in each moment of a family's existence, may be five generations, and occasionally more. This structure is not new, but does not deny that some individuals have been as it is for any period in the past or will continue as it is into the future. It is a mode of analysis, not a description of reality, though it is derived from reality. Whether the institution is in actual stable equilibrium, in a state of stasis, is a matter of historical record over a denoted period, or of prediction about the future.

The nature of structural duration is well shown in all studies of family organization. The "stability" of the institution, i.e., the tendency to maintain its form—is only partially determined by its internal structure. Much depends on the constraining effects of the external environment, or of internal factors. In family organization the biological processes involved in mating, reproduction, maturation, and death constrain developments, and tend to maintain a particular form of structure. Within the biological framework, and at the same time incorporating it, there are numerous varied forms of family, but analysis of all of these forms is usually done in terms of much longer periods of generations than the actual period during which observations are made. That is, to understand even a day in the life of a family, it has to be analyzed as if its systematic interdependencies were worked out through a "cycle" (in this case) of at least three generations; for each family contains within itself, at any one moment, parents, children or the expectation of children marrying, those children marrying, and having children. It may be better to work with a structural duration of four generations, since the parents were themselves derived from an earlier pair of parents who produced siblings for their children, people who may be relevant for the actual Parliamentary history is affected by other systems of events, some intrinsically involved in the legislature itself. Thus problems of selection of party leaders may be influenced by the age distribution of members in the party electorate, and the career of slightly older than himself may be ruling out leadership for himself, and so on. Other events, in the external system of both Britain and the international world, are "haphazard" in respect to the system of the legislature, though systematic in relation to other events. Seen in this way, we can examine the effect of the House of Commons with its own duration on the history of political life in Britain and vice versa; the institution in its general cultural form has persisted for centuries during which radical changes have occurred both in the external environment and in the internal structure (as in the types of persons who sit in the House of Commons). We can then assess the relation between continuity of form of the legislature and actual political relationships, and we might conclude that the relation is relatively tenuous, though I think it is important. Again, as with the family, the structural duration of the House of Commons is constrained in large part by customs which organize it, but there is not a constraining framework akin to the biological framework which constrains the structural duration of the family.

If this is correct, it follows that whenever we attempt to analyze an institution, we have to "throw it" (forgive the metaphor) into its structural duration, since all social life is a "process in time" and rules and customs contain a time-element. This involves analyzing the institution as if it were operating through a far longer period than the actual period during which we observe it or its parts. This may be trueistic; but it is fundamental. The failure to appreciate this when reading an analysis of institutions may give the reader a false impression that the writer believes that the institutional form he analyzes has endured in exactly the same form from far in the past, and will thus endure into the future. This is not always the reader's fault, since often the analyst has not made clear that the time element in analysis is a structural duration, and not actual historical time. There is a tendency, however, to overlook the difference between these kinds of "time" or "durations," perhaps because in more recent times anthropologists have been faced with more rapidly changing societies. They then insist, against the analysis of structural durations, that all real societies in real time are always changing and have always been changing—without specifying what is changing, what the changes are, and for what purposes the changes go. In seeking structural forms it seems obvious that there are very different kinds of change, and we shall have to develop a vocabulary to differentiate between them.

II

I have written above in terms of the structure of a single institution. The problem of analyzing a set of institutions, or a whole social field, is obviously much more complicated. Various institutions have different structural durations, and their "intermesh" has to be analyzed. Events emerging from the operation of one institution may intervene in the operation of another institution in a manner that is haphazard as far as the systematic interdependencies of the recipient institution are concerned. External events from quite different areas of the world may intrude into the field under analysis, again in an haphazard manner, so the point of view of an analyst of systems, is a haphazard one. But for analysis of institutions we have to carry out our process of mental abstraction, and when we do this I consider that we find that institutions and wider social fields have a marked tendency to endure, that they and/or their parts are resistant to both unintended and deliberately attempted changes, though radical changes will, after some period of time, occur. We might say therefore that an institutional field or a field of institutional systems, will tend to develop, and even hypertrophy, along the main facets of its organization, until conditions make it quite impossible for the system to continue to work (e.g., major demographic shifts, invention of new tools and machines, and so forth). This excludes the effect of attacks on the system from outside, as by invasion from elsewhere. Even after radical change has occurred, moreover, institutions are so tough that they often survive into, or revitalize, the new conditions that eventuate, and operate again within a new overall system of institutions.

If this view of the history of institutions is validated by our knowledge of the actual history of events, it would mean that, as the
diachronists argue, periods of revolution and other forms of rapid and radical change subverting the structural system completely appear as sudden crises or are the steady accumulation of smaller and more limited structural changes, constrained within the major pattern of the institutions concerned (save where there is external invasion). I consider that here the dialecticians are right, and this implies that to understand even revolutionary change we have to work from the structural duration of institutions (or social fields) in an equilibrium model.

I find support for this view in the methods by which the best analyses of major social developments have been written. It is clear in the Marxists treatment of the movement from primitive communism, to classical slave societies, to feudalism, to bourgeois mercantilism, to early capitalism, and on to monopoly capitalism. In this Marxist model, centuries of relative stability of institutions are assumed and analyzed in a relatively equilibrated framework, before the conflicts and contradictions inherent within each system lead to revolutions that have largely been in the quite different form of narrative (as against institutional analysis) when the movement to a new institutional system occurs. Durkheim handles similarly the movement from the status of mechanical solidarity to the institutional structures emerging as the division of labor increases, as does Max Weber the developments from traditional society to rational bureaucratic society; the theories of Sir Henry Maine, Tonnies, Von Voss, and many others are similarly constructed. I see a similar mode of analysis in the work of all the cultural evolutionists, with quite different theories, who have enriched the history of anthropology. I repeat, they are using a mode of approaching history when they use this framework of long periods of relative stasis, to be understood in as if equilibriums of structural durations that to a large extent neglect actual historical time in a manner-inflating to many historians; therein they formulate theories of interdependence between social elements. The theories of each of these analysts or types of analysts may differ; the model of approach, the method, is very similar. If so many thinkers, holding different political views and elaborating diverse theories, all work in this framework, it shows that they are constrained in at least two ways. First, there has been the sheer incapacity of the human mind to think about, more than a limited amount of facts. Second, there is in reality itself something that is reflected in this framework. I consider the second the more important; and therefore I feel that the model of an equilibrium, if used in analyzing many parts of the institutional set-up within a complicated social field, does illuminate what has happened in actuality. It also illuminates the structure within institutions and the structure linking these together in more complicated framework.

Weber's conception of “ideal types” is clearly a model similar to the one that anthropologists have developed and that I am delineating here. But there are differences between them. Weber's “ideal types” are composed of consistent principles of organization, with one dominant. The “ideal types” are drawn from reality, but they go beyond reality in emphasizing the dominant principle, against which real institutions can be assessed. The other theories I choose to emphasize dominant principles of association and economic structure, at each stage or relative stasis. Marxists theories bring in the emergence of a contradicting “submerged” principle that will become dominant; here the relative stasis involves that the groups whose interests are dominant suppress the increasing resistance and revolt of those whose interests are founded in the new potentiality. The models with which anthropologists have worked usually have reference to institutions that are much shorter in their structural durations. And most anthropologists (like many sociologists) have stressed that, though there may be dominant ideological principles, and constraining variables, the equilibrium results from the working out of processes arising from principles of social organization that are independent of one another, while many principles are mutually discrepant, discordant, conflicting, and, eventually, when radical structural change is going to develop, contradictory. These principles of organization are partly contained in social relations operating on the physical environment through material equipment, partly in the interests and demands of the varied social relationships in which individuals severally participate, and partly in the setting of social relationships in a culture (the “shreds and patches” [Lowie] of values, allegiances, goals, and so forth).

III

A certain ambiguity, and lack of clear exposition that we were analyzing in terms of structural durations and not of real historical time, has contributed to misunderstanding of what was attempted to do. This ambiguity arises from the multivocality of words like equilibrium and stability; Leach (1954:7) therefore alleged that there has been “confusion between the concepts of equilibrium and stability . . . so deep-rooted in anthropological literature [that] any use of either of these terms is liable to lead to ambiguity.” I consider that the ambiguity arises because not everyone, writing or above all reading, distinguishes continually between the equilibrium of the relative stability of a system of interdependent relations, and the stability of actual social life. Some readers have regarded a statement about equilibrium as implying that no disturbance occurs, or they have, as is much more easily done, failed to see that there can be stability at one level of organization and not at another. As a chair maintains a constant stable form (stasis) despite the continuous movement of particles within the molecules of the elements that compose it, so a macroscopic structure might remain stable despite disturbances in the structures of families; conversely, structures of family patterns may remain in stasis while the macroscopic political structure is radically altered (Homans 1950). In the analysis of systems we have constantly to be shifting our viewpoint with the problems we pose.

This confusion seems to me to be inherent in Leach's argument in Political Systems of Highland Burma when he says that [all? most? many? some? a few?] anthropologists present a “stable equilibrium.” He adds that Malinowski, Firth, and Evans-Pritchard “write as if the Trobrianders, and Tikopia, the Nuer are as they are, now and for ever” (1954:7). This does not seem to be correct for any of them, once we see that they were analyzing structural durations, but Leach's statement is presumably explicable on the grounds that they did not make this clear. Evans-Pritchard did however put forward a differentiation between various kinds of time in order to indicate that he did not believe that his analysis showed the “Nuer as they are, now and for ever.” I therefore review the implications of his analysis, and particularly of his concept of “structural time” (Evans-Pritchard 1940, and articles from 1934 onwards cited therein). So, after a monstrous deal of sack, I come to the loaf of bread.

Evans-Pritchard seems to me to have made clear that his analysis of the Nuer was not set in actual historical time, when he differentiated for the Nuer what he called “structural time” as against ecological time, social time, and historical time. In the Nuer system of segmentary lineages, rights and duties between male agnates (patrilineally related men) and the fluctuations of amity and hostility between groups, theoretically depend on how recently they share a common agnatic ancestor. Where the lineage system provides the framework for territorial groups, the closer men are on the ground, the less the “structural space” (as Evans-Pritchard called it) between them, and the more recent their linking ancestor on the agnatic genealogy which provides the ideology for political combinations and feuds. Thus depth of structural time is a means of coordinating the community, rather than a means of coordinating events in the past. Therefore Evans-Pritchard proposed the theorem that depth in structural time is directly proportional to spread in structural space. The depth to the original ancestor of a maximal lineage in the Nuer system is about eleven generations, presumably because this covers the widest spread of groups that can be coordinated in this type of system. We can say this because there is something constant about this depth of eleven generations, since we find it amongst other peoples, some with otherwise very different systems, such as the agnatic Bedouin, Nyakuya, Zulu, Tallensi and Tikopia, and the matrilineal Ashanti, though there are deeper and shallower systems. Evans-Pritchard argued therefore that the lineage system never grows; its largest structural time is fixed in depth. This shows clearly that he did not believe the system had started eleven generations ago, and that he could not decide how long it had been as he found it.

That eleven generations is not a report of real descent is clearly demonstrated by genealogies among the Bedouin of Cyrenaica. We know they invaded the region in 1056, i.e., 900
years ago (Peters 1960, 1967). But the gene-
alogies of their nine tribes show eleven genera-
tions to the nine sons of their alleged founding
ancestors. Some ancestors may be fictitious;
but clearly many actual progenitors have been
eliminated from memory.

These eleven generations show a definite
break at the fifth or sixth generation level
going backwards in "structural time." Up to
this point, the genealogies serve to coordinate
relationships of everyday cooperation, inheri-
tance, and so on, between living people, re-
lated through the recently dead. The upper
half of the genealogies relates larger groups
together—that is, these genealogical links
define political or group relations, rather than
interpersonal relations. This is demonstrated
by the fact that in shorter lineage-type gene-
alogies we still find five-generation bases for
groupings of living people, interconnected at
higher levels by a band of two (Cuninios 1951,
1956, 1957, 1959) or three (Watson 1958)
generations. This band consists of politically
significant relationships of "kinship" (Cuninios
1959) between groups or political positions; and the names in this band do not alter or shift their relative positions
when there are major changes in the rela-
tionships of larger groups. The key positions
are maintained in perpetuity by the groups themselves and in relation to one another by
what Richards (1933) called "positional suc-
cession"; they stand now for political positions,
not for men or women. I have not space to
consider the systems where the genealogies are
deeper than eleven generations.

Evans-Pritchard's concept of structural
time is thus a means of describing people's
modes of thinking about their past, and ideas
of structural time may cover institutions of
duration. The theorem that
structural time is directly proportional to
structural space leads to further analysis. For
the lower five-generation base is presumably
connected not only with descent as such, but
also with external factors controlling reproduc-
tion and education of people and with modes
of organizing production, distribution, and
consumption of goods in domestic organiza-
tions at this technological level. The wide
correlation of genealogical and ethnic organiza-
tion probably is related to external factors that
determine the extent to which cooperation of
political groups, and the distribution of power,
can be organized by a kinship ideology. Salt-
ins (1961) has suggested that a developed
lineage system of this type emerges from pre-
sory expansion at the cost of other peoples, but
he does not discuss, as it is not relevant to his
argument, the problem of why this depth.
Nuer ideas about time are thus not merely
the ideas of an isolated Sudan people. Nor do
they provide us with an analysis of social
structure, any more than do Kachin ideas
about aristocratic authority (gumsa) and
democratic organization (gumlae) (Leach
1954, 1964) provide us with an analysis of pol-
itical relations (see 1958, 1966:72). Such
ideas are clues to the structure, and the struc-
tural durations, of various institutions.

In the institutional analysis that Evans-
Pritchard made of the Nuer system, his first
step was in effect an analysis of the structural
duration of some institutions in as if equilib-
rium. He then showed that there were occurring
changes in systematic structure, and he
assessed these against the analysis of the other
prophet. After showing that the greatest structural
change in Nuer time-scale is eleven generations, Evans-Prit-
chard discusses the rise of the "prophets" who led ephemeral unions of Nuer groups both in
attacks on the Dinka and in resistance to
Arab and European attacks. He shows that
difficulties these prophets had in uniting Nuer
groups because of the instituted hostilities and
persistent feuds between them, and he is able
to assess what they achieved and how they achieved it. That is, the historical rise of the
prophets is made analytically significant only
because of the equilibrium analysis involved in
treating the Nuer lineage system, this equilib-
rium analysis being based both on historical
and data and on observations in the present.
Evans-Pritchard's later analysis (1940) of the rise
of the Sanusi holy order to religious prestige
among the Bedouin of Cyrenaica takes this
problem a step further. Again, the setting is
an equilibrium analysis of the Bedouin lineage
system; only by understanding that system in
this way can we understand how the Sanus
holy men placed their lodges at key points in
the interstices of the lineage system, so that
they were not aligned with any local section,
yet could exert influence. During the Italian
invasion of Cyrenaica, the main resistance by the Bedouin into the period of
World War II, the Sanusi could emerge as the
titular, if not the actual fighting, leaders of
the Bedouin until their head became king of
the new state of Libya.

It seems that his "structural time" is a
partly a description of Nuer ideas, partly a
conception to analyze their relationships. However, through this analysis, a sense of
time in terms of institutional and individual
changes into varied kinds of time, in order to work
out the structural duration of the institutions
involved, before we assess what kinds of change are occurring. We examine how far
observed differences between the groups, re-
lated to institutional patterns, and individuals can be explained in terms of situations or phases
involving in some institutions' structural dura-
tion, and how far the differences indicate that
there is more radical change occurring in the
basic structure of the institutions.

This is the technique which Evans-Pritchard
used in his study of the Nuer. It has been fre-
cently applied to the study of domestic groups whose structural duration, as stated,
is largely controlled in an almost cyclical pattern by the processes of birth, maturation,
and death. If we collect quantitative and other
data on, say, different families with parents
and children of different ages, and we study
family law, customs and, wherein we have to throw away
in terms of institutional
duration or durations, for various
families may be at different phases of a
pattern or systematic change of
family structure may be occurring, or both
processes may be present. If structural change
is occurring, how can we assess its incidence
except against an as if equilibrium analysis of
what was the family structural duration in
the past, possibly shown in some phases in
families observed in the present? Otherwise
we would be left with random observations
only. We use life histories and other similar
data to make this assessment.

Since structural duration of family and
domestic organization is largely set by bio-
 logical processes, this technique is under
desirable control. Fortes discussed the tech-
nique in his essay "Time and Social Structure" (1949b), where he dealt with Ashanti domestic
organization, thus: "An important aspect of
domestic organization is the normal duration of
the unit. It is an index of the process of
growth by which the physical and social re-
placement of one generation by the next is
assured and depends on the functions per-
formed by the domestic unit in the rearing and education of children.” Fortes is concerned to demonstrate for the Ashanti both the cyclic duration involved in family processes and the changes occurring in Ashanti families in new conditions, and certain conditions for an analysis on an argument for the use of quantitative and statistical data to elucidate structure, as against bare statements of principles like matrilineality and matrilocality. He presents quantitative data, collected from dispersed situations in two areas of Ashanti, on such matters as age distribution of household heads; age, residence, and marital status of women; and so forth. The structure of matrilateral mixed virilocal and unilocal, patrilocal and matrilocal, and avunculocal, societies are well known, so I give summarily Fortes’s conclusion:

Elementary statistical analysis is indispensable for the elucidation of certain problems of social structure that arise in a society which is in process of becoming socially diversified. The utility of blanket-terms like “patrilocality” and “matrilocality” in this context is obvious. The use of numerical data has enabled us to see that Ashanti domestic organization is in fact the interaction of a number of fairly precisely defined factors operating both at a given time and over a stretch of time. Granted the dominance of the rule of matrilocal residence in the recognition of patriarchy in Ashanti law and values, the sex of the household head is the factor of first importance. It determines the main possibilities of the arrangement of kindred in the domestic unit in relation to the polar values of “matrilocal” and “patrilocal” grouping. The other factors are the tendency to seek a compromise between the opposed tie of marriage and parenthood on the one hand and those of patrilineal kinship on the other; and the ideal that every mature person, especially a man, should have his own household. How these factors interrelate depends, among other things, on local social conditions and historical circumstances. The domestic arrangements I have described are possible only in the long-established stable capital towns of chieftaincy, where both spouses in every marriage are equally at home. In new villages the ordinary patrilocal household is more common [1953, p. 29].

These domestic arrangements include the movement of children as they mature between different homes, the movement of men, and the movement of women—as well as the movement of food between houses. Fortes’s quantitative data, backed by biographical data, here summarize a series of synchronic observations, which are thrown into two distinct diachronic models: the one is of varied types of domestic units moving through patterned durations, the other a process of limited structural change proceeding through the durations, and possibly leading to radical social change.

The same procedure was followed by Esther Goody (1962) in an analysis of the factors influencing the divorce rate of women at different ages among the Gongja. She recorded divorce rates of women of different ages through their lives up to that point and illuminated the varying pressures that move a woman at different ages to remain with her husband, or leave her husband for another husband, or return to her brother’s house. But when it came to assessing what factors influenced the divorce rate in the past of older women, she had to rely on observations on younger women in the present day. The method is full of dangers, and I suppose it is always vulnerable to tautology. The method has its flaws. But if we are to make progress we must face the dangers, and do what we can (see Barnes 1949, 1951, 1967). Always, in my opinion, we can seek support in the extent of our comparative evidence. Goody finds a high divorce rate among the Gongja, and it is striking to me that they are what we call cognatic; that they do not have levirate, sororate, or ghost marriage; that they disapprove of sororal polygyny; and that they let a child go to its father and not its mother’s husband. Their institutions resemble those of the Lozi (Gluckman 1950) who are also cognatic and whom we know, from historical records, have had a high divorce rate ever since they came to our knowledge. This makes it seem plausible that the Gongja have had a high divorce rate is unlikely, in this institutional concatenation, to be entirely a product of modern times, for in these respects the Gongja, like the Lozi, contrast sharply with the Zulu type of society (Gluckman 1950) with its agnostic emphasis and distribution of property through wives of a husband. Here we find levirate, sororate, ghost-marriage, and sororal polygyny approved, adulterine children going to the mother’s husband; and although there are very few, both in the past and in modern times, of a pattern distributed throughout South and (far away) Northeast Africa. Hence we have

several types of controls on this kind of investigation and it is so that it is not sheer guesswork when we try to assess what are the situational changes through a traditionally oriented life-cycle and what are radical changes related to new conditions.

I would argue that here too, if we are to assess the extent and nature of institutional changes in marriage patterns, we have to set these against an equilibrium model. In studying the rate of divorce among the tribes of Zambia, Malawi, and Rhodesia, my colleagues have worked out divorce rates despite awareness of the difficulties involved in throwing synchronic observations into a diachronic model. Their studies show (Mitchell 1963) that among the patrilineal peoples the rates are lower than among the matrilineal and probably cognatic peoples. (There are some, but very few, exceptions.) Central African patrilineal peoples have adheplic succession and inheritance along a line of brothers; and this may be why they have a higher divorce rate. The Zulu type of patrilineal society where position and property are divided up among wives in a polygynous household and devolve through those wives on to their own sons. In this region (and most others) the generalization stands that divorce rates are higher among patrilineal peoples, the estranged patrilineal systems. However, in the urban areas the rates of divorce of patrilineal and patrilineal peoples move toward a common norm, as does the amount of the marriage payment—tribal areas marriage payments are low in patrilineal and cognatic tribes, much higher among patrilineal peoples who have property to give. The indications are therefore that the urban situation is beginning to establish a common institutional pattern in marriage, for the changes in the amount of marriage payment accompany changes in the rights transferred, and duties entered upon, in marriage. This most important set of changes in the institutions of marriage can be understood only in the setting of the equilibrium studies of tribal marriage.

IV

It is again not too difficult to work out structural durations, and to isolate continuity with regard to the historical changes in such singular institutions as kingdoms. It is most difficult to do so with groups like villages, which are multiple like families but not so tightly controlled by the biological framework; yet similar methods have been applied with similar gains.

When he studied the Yao of Malawi, Mitchell (1951, a, b; 1956) observed that their villages varied in internal composition. Some consisted of small patrilineal lineages, others of a range of several such lineages grouped in complex association both by more distant matrilineal or other matrilateral links and by patrilateral ties. He examined how some of these patrilineage-based sections within the villages left their parent villages and set themselves up as independent villages, either in search for new land or after quarrels. He related processes of growth in numbers through marriages and births to an ever-increasing complexity of all villages, realized in reality in some of the villages, and then the reduction of expanding villages through division. He thus established the structural duration of various types of Yao village, in denticulate processes of growth and proliferation of sections till rising to the status of a colonial named of variable prestige, of the custom by which in a dominantly uxorilocal society village headmen brought their wives to their own homes, of beliefs in witchcraft and sorcery and ancestors—underlying misfortune, of the rules of positional succession and perpetual kindship between positions, and of other customs and beliefs in these denticulate processes built into varied types of villages. If we judge from written and oral tradition, some villages remained large and complex for long periods, despite shedding some sections. Mitchell was able to specify the social factors associated with this durability. Other villages, denoted by different social factors, never became very complex, and sections breaking off found in large villages tended to fall into the second category. Marwick (1965) confirmed this analysis among the nearby Chewa, and was able to pursue more deeply how sorcery beliefs, and so on, operated to control, expel, and maintain the system and the structural durations of its parts.

The core of Mitchell’s analysis of Yao villages was investigation of a limited number of types of structural durations in a complex as if equilibrial. By applying his records he examined how far war, slavery, and trade to the coast from Malawi and beyond
had in the past contributed to the growth of big villages, in order to assess what were the effects of British conquest and rule. British rule abolished land tenure and made life more secure. It brought in specialized traders in cloth and other goods formerly fetched from the coast by Yao caravans. New economic opportunities (such as cash cropping and migration to labor for Europeans) affected social relations as well as the emergence of new types of land shortage in some areas. But through these radical changes there was considerable continuity; people still lived in villages and many men were ambitious to become village headmen. New wealth was partly expended in traditional social relationships. Even when a man, in this normally xorolocal society, was allowed to take his wife to live with him where he worked on a European plantation, their children did not form a group regarded as dominantly descended from the husband, but rather, in terms of Yao dogna, a group regarded as a matrilineage descended from his wife (Mitchell 1951b).

Mitchell worked out his analysis from two different kinds of data he could gather, both from published and unpublished written records and from histories of villages and individual Yao. Secondly, in the field he made a series of synchronous observations of different societies. The technique of analysis was to throw these synchronous observations, with the help of historical data, into different kinds of diachronic processes. The skill of the analyst is to assess when the differences he finds are repetitive situational changes and when they are changes in the structure of the system of institutions itself.

Mitchell had some historical records, written by Europeans, to facilitate his diachronic study and to enable him to assess different kinds of change. Similar more or less valid historical data have not been available to those anthropologists who have studied tribes in recently opened up areas, as in New Guinea (and indeed, this was largely Evans-Pritchard’s position among the Nuer). These anthropologists employed the method of collecting synchronous observations on the distribution of categories of groups and persons, as well as oral traditions and life histories, and examined the situation in a variety of cultural rules and customs. They have thrown their observations into diachronic “structural durations” (see e.g., Posiippil, 1958a, 19586, 1960; Reay, 1956; Salisbury, 1962; Brookfield and Brown, 1963; Meggitt, 1965), before they tried to derive possible changes to some extent, they have produced analyses of processes subject to repetitive changes (i.e., in stable equilibrium or stasis), but they have used this device to indicate processes of limited structural change (i.e., changes which do not go to the root of the system, but which alter its form), and processes of radical structural change (which alter the form of the system). For example, Meggitt shows that Mae-Enga agnostic lineages, at various levels, have had to deal with a series of conflicts of values. It is wrong to dissemble an agnatically related “brother lineage” of land, yet when numbers increase, this is the most suitable land to seize (cf. Peters 1967). Again, it is proper to allow kinsfolk related through women to settle on one’s land. If an agnostic group is falling in numbers, it needs these other kinsfolk to help it defend its land against more numerous and therefore more powerful “brother lineages.” Yet if you invite in other kinsfolk they may, like the cuckoo, see the nest, and seizing existing traditional dilemmas, he argues that there runs a thread of limited structural change, a tendency to emphasize, where land is short, the primary principle of filiation—kinship in the agnostic systems. He cites comparative evidence (which has been criticized, Brookfield and Brown 19639) that this tendency to enforce the dominant cultural rule appears in many tribes, and he specifically quotes the “longitudinal” analyses made by Firth in Tikopia twenty-four years apart as showing this shift (Firth 1936, 1959). This change is here hypothesized not only from throwing synchronous observations among the Mae-Enga into a diachronic process, but also by seeing a general process occurring over several societies analyzed similarly in equilibrium models by several anthropologists. In each of these studies, the process of structural change is seen significantly because of the equilibrium model of structural duration at the core of analysis. The generalization here put forward by Meggitt can be applied more widely. For instance (Gluckman 1943, 1965b) in a somewhat different situation of land shortage in South-Central Africa, it became short for tribemen acquiring their marriage income from labor migration, the basic rule emphasized is that of right of every subject to some arable land. This leads eventually to the chiefs restricting by legislation the amount of land each subject may cultivate. The system breaks down completely where land shortage is too acute for even this restriction to allow sufficient land to each subject. This demonstration was possible only in an analysis of the relation between land-holding and the structure of the politico-economic system in an equilibrium model. The nature and setting of radical changes and development again could be assessed only in an analysis of institutional duration.

These processes can be seen as part of a more general proposition already stated, viz., that an institutional system will tend to develop and hypertrophy along the main lines of its organization until external conditions make it quite impossible for the system to continue to work. This proposition in itself emphasizes my main contention, that institutional change is best understood in a setting of equilibrium analysis. It is theoretically possible that in the kind of situation in which Meggitt and his colleagues worked, without any vouched-for historical data, they might not have been able to separate from one another what were repetitive (Gluckman 1940b, Vogt 1960) situational (Mitchell 1966) changes, what were limited structural changes, and what were radical structural changes. In these circumstances, the same methods of analysis might have to be adopted; but two or more possible analyses of structural durations and of types of change might have to be advanced. No anthropologist has yet felt this to be necessary.

Without the diachronic analysis of structural duration and continuity in these studies, it is impossible to assess the nature and extent of change in the institutions under investigation. I emphasize that I am here discussing institutional change and not all types of change. That is, I am arguing that even when we are interested in radical changes, institutional change is clearly one core of our subject. To study institutional change, we must therefore adopt a methodology that focuses on institutions, and this leads us back to an implicit conception of equilibrium. When we feed in actual historical changes as we move nearer to the reality we observe, we assess changes in relation to diversions of the processes of structural change, either to show repetitive change of personnel only, or to exhibit processes of sudden or gradual elimination and substitution or of addition and multiplication of components in the social field, or of several of these processes.

VI

Each of the above analyses does not depend entirely on itself; they are mutually supporting. Not only do they conform to a general pattern of orientation and theoretical statement, but they also produce substantial evidence of external facts to which the theoretical statement has to fit. It is for this reason that I cannot agree with Leach (1954:4; 1964:Introduction) when he defines an anthropological model thus:

We first devise for ourselves a set of verbal categories which are nicely arranged to form an ordered system, and we then fit the facts into the verbal categories and hey presto the facts are “seen” to be systematically ordered! But in that case the system is a matter of relations between concepts and not of relations “actually existing” within the raw factual data, as Radcliffe-Brown and some of his followers have maintained (1964: xii-xiii).

I gladly enroll myself among these followers of Radcliffe-Brown, even without being con-
scribed by Leach to serve thus, for he sees me as his “most vigorous opponent in matters theoretical” (1964:ix). For here he and I part company, and it is because this parting is of crucial importance for the whole of the approach I am outlining that I comment on Leach’s undoubtedly influential book. I do not consider that our analyses are the ordering only of “a set of verbal categories”; instead, I consider that the “ordered system” actually exists within the institutional realities that we study. The ordered systems are the structural durations which are built into social institutions, and they are, in Durkheim’s phrase, within the “entités spéci-ﬁes”, “things”. That is, we cannot learn their characteristic properties except by investigation, and we cannot change them merely by an act of will. Much of recent history also emphasizes that it is not so easy to alter institutions by legislative acts and maintain social pressures. Institutions are resistant to radical changes, and patterns of customs and customary beliefs persist and continue into new situations, even though in the end they may be altered. This is
not the resistance of isolated customs or ideas, but a resistance born out of the hard reality of the interdependence of elements within institutional patterns, an interdependence that we labor to discover. To some extent, it is true, a body of persons working within a discipline are likely at any period to see similar problems, but the constraint of evidence is always upon us, and not all of us are easily deceived against the evidence.

Before I pursue this point, I must separate off the apparent similarity sometimes present in our subjects' ideas and in our own analyses. We do not merely analyze the set of ideas of the people involved in an institution. These indigenous ideas, as I am wont to say, are the product of what social reality is believed by its participants to be, are some of the facts we take into account in our analyses. The actors' ideas, like their behavior, are part of, and influenced by, the total reality in which they live. This reality, as I have said, is something external and constraining. It is hard, and cannot be changed merely by changing the set of ideas. It is sufficiently hard to shape the future in unexpected ways. This makes it easier to say certain things about the hard reality in which they live, and in flashes these may be accurate perceptions of what exists and of what is happening or emerging. But actors are often self-deceived about events and motives, and their ideas may be rationalizations. Hence social and individual ideas cannot be taken as accurate perceptions of reality; and they are almost always incomplete (Peters 1967). Comparative study by specialists may be necessary to put these ideas in perspective. Analysis after analysis shows that often the actors do not fully understand what they are doing and why they do it.

I cite only anthropological analyses of how particular categories of persons are divided, in various tribes, as the witches responsible for misfortune, and of genealogies as ideologies rather than as ancestral pedigrees. Anyone can multiply examples.

The reality we study is equally external to, and constraining on, us, the anthropologists. It is hard, in the sense that we want to suit ourselves. Hence our technical as if models are not only an order between a set of verbal categories. They have to be logically constructed, and this construction may be fruitful or not. In addition, we have the idea that our ideas are self-referential. We constantly refer them to the evidence of our observations, and these are hard and constraining, not to be changed by an act of will or merely by altering a verbal category. The observations, as well as the analyses, of our colleagues working in quite different social fields are also external, constraining, and hard for us. For we take these into account in developing our own analyses. We cannot make up institutions and get away with it. No one has yet analyzed that Bantu tribe that emerged from the fertile imagination of Schapera—the Balio. And Miners' (1950) delightful satire on the Nacirema is too near the truth to be altogether fictitious. All of you have struggled to analyze your field data and know how difficult it is for a trained outside observer. I repeat: this is support of what my predecessors and colleagues have done, to get at the real interconnections within a social system, by taking account of all the evidence in order to erect a model. Most of you have changed your models in the light of new facts about the real world, whether found by yourselves or appearing in the criticisms and works of your fellows.

VII

I have spoken about as if equilibrium models to demonstrate how useful they are in studying social change. But particular sets of institutions may have been or still be in actual equilibrium, persisting in continuity, for long periods. This is not a matter of a prior judgment but of historical record, of present assessment, or of future prediction. Or institutional change theoretically could proceed with a constant readjustment of each part to changes in the other parts—in moving, actual equilibrium. This is more likely to occur in parts, rather than in the whole, of a social field. More often there will be a steady change of magnitudes within and between institutions, until there is a sudden and radical transformation of form. All experience indicates this.

I will describe how I applied the methods I have outlined to radical change in the history of Zulandia. From the evidence of shipwrecked mariners, and of oral traditions collected from the Zulu and their neighbors, who are now established many thousands and hundreds of miles apart, and from neighboring tribes, the political field from at least 1400 until about 1800 can be analyzed as being in an actual state of stable equilibrium (in status). It was composed of many tribes, varying in size with a maximum so that a tribe became too big, sections broke away either peacefully or after struggles between rivals for chieftainship. Through four centuries tribes split, new tribes appeared, others disappeared, and there were raids but no conquests. This was replication in time of duplication in spatial pattern and structure, despite many disturbances involving wars and divisions. There was no radical change, either in institutions or in structural durations. It was stable equilibrium through a long period.

Evidence indicates that human, and probably more importantly, cattle populations increased beyond the point of critical density. The increase accumulated gradually, then suddenly led to change of structure. There was an apparently a short period of moving actual equilibrium in which some tribes emerged as dominant, conquering their neighbors. Structural durations, short in period, show tribes trying various means of conquest and defense, until the Zulus, through wars that solved the population problem, established hegemony that lasted sixty years, ending with their defeat by the British. During this period, there was a pattern of civil wars based on the same factors that determined an earlier equilibrium of small tribes: relatively poor food (suggested as consuming goods, slow communications, widespread of population, simple weapons held by every warrior giving each leader a private army. The result was autonomy of local sections not linked to the center by utilitarian organic independence and civil war around the kingdom. That is, here we have, within the equilibrium model, theoretical propositions arising from the relation between technology, dispersal of population, and authority systems continuing despite some increase in the power and spread of authority. One cannot understand an event in each reign unless one sees this equilibrium in struggles for power; and the analysis can be duplicated, and its variations correlated, from other states in the history of Africa, Europe, and Asia. The change from small tribes to Zulu kingdom would limit structural change, rather than radical structural change, because relations between parts were still determined by similar basic technological and other factors. Meanwhile, outside of the analytically isolated kingdom, relationships with whites developed slowly, until suddenly the pattern changed and British invaded Zulandia. After defeat, a temporary equilibrium, with short-term durations, was established between the remnants of the kingdom; then the British set up rule. And there was a new temporary equilibrium based almost entirely on force. One can disentangle archival records to find how this temporary equilibrium was extended into a complex set of interdependence ties between British and Zulu in which political institutions involving various white and Zulu officials, and people, had their own structural durations. By making analyses of several periods of relative stability, and of processes of change from one to the other, I could handle both the structural durations of institutions and also types of change. Barnes (1954) has handled in similar frameworks the history of a group of Ngoni from the time they were driven out of Natal up to their position in Nyasaland under British rule. In terms of the equilibrium found at different periods, changes showing both in continuity and innovation can be assessed. Without the conception of equilibrium, one is left with narratives of rapid radical change of structure it may be necessary to resort almost entirely to narrative, so far as this is possible, and to seek here for different kinds of generalization and propositions (Gluckman 1965b, p. 176).

I consider that it is profitable to apply a similar method of analysis to, for example, the history of the English king'ship. In the Middle Ages there was constant change and development; some magnates accumulated more land and power, cities grew at specific phases and trade increased, and so forth. But through several centuries it is possible to isolate the institutional relationships between king and magnates in order to understand the pattern of civil wars. These relationships can be assessed in relative terms, to constant material elements such as types of weapons, supply of goods and money, and modes of communication, then also to patterns of law, such as rules about treason (Gluckman 1965b, Chap. 11). The result is an analysis of a policy that has many systematic regularities similar to those I have described in the middle period of Zulu history. I would argue again that without some equilibrium model we cannot make sense of either our analysis of civil changes in the relationships between king and magnates, or in relationships between them and the bourgeoisie in the cities, the artisans, and the rural laborers. Moreover, without applying this
model to examine some institutional complex or the other, the whole becomes again recita-
tion of a narrative.

VIII
I maintain that the above analysis of the use of equilibrium models brings out the extent to which it was a dynamic method, trying to deal with various types of disturbance and different types of change. Moreover, its ex-
ponents were very much concerned to analyze social process, and very deeply aware of the problem of time. Why then the tendency in recent years for younger anthropologists to dismiss the pre-individual as static and not dynamic, concerned more with an idealized structure than with process? There is now almost an element of abuse in the epithet "a structural-functional study." The answer may in the widest terms be that each new genera-
tion wishes to outdo its predecessors, and wholesale condemnation is an easy way of
beginning. Specifically I suggest that as far as tribal systems were concerned, the preceding generation sought sense into the variety of cultural forms in which these systems are manifested. From my own experience in train-
ing a succession of research workers, I can vouch for the fact that analysis of the institu-
tional systems became easier, in the sense that each new study is a rapid observation and work out the structure of his system, in the light of preceding analyses. This liberated them from months or even years of work, and they were able to go on to observe in greater detail the interaction between persons in- volved in the institutional structures. Hence, particularly as improved methods of field re-
search brought in more and more complicated data, they became involved in study of the structure of interaction patterns. As in the other social sciences, there was development of extended case-studies, trying to handle variations and deviations from institutional norms. This led to greater interest in the com-
plexity of each unique period and parcel of history; in the life that men and groups of indi-
viduals; in the choices that individuals have available to manipulate to their advantage (Leach 1954; Bailey 1957; Van Velsen 1964).

When these complexities are reduced in a structural analysis, much of the uniqueness and richness of the material is lost. So the anthropologist has to wrestle with a dilemma: if he presents all the data, we cannot see the

GLUCKMAN

The Utility of the Equilibrium Model

tention from a whole field of problems. I have been talking above of institutional structures since this is the field in which I work, and I have argued that institutions have a tendency to continuity over time through the systematic interaction of positions, roles, material cap-
paratus, values, beliefs, etc. My analysis is developed for this field. But I recognize that
within the apparent continuity of form of an institution (e.g., House of Commons) there can be a slow accumulation of change: in types of personnel, through the fashion of style, through the operation of choices by individuals, and so forth. Lack of continuity seems marked in many patterns of interaction. Nevertheless, many studies have shown that there is a high degree of continuity in interaction patterns, which makes emphasis on structure equally es-
ential (see Goffman, various). But whether interaction patterns are continuous or un-
stable or changing, it is important that we try to see whether we can bring together these ap-
parently very diverse modes of analysis. I con-
sider this to be one of the challenges before us in the coming years, and hence I am against false disputation between persons dealing with different types of problems in which one person argues that the other's analysis is wrong in principle because it is not the kind of analysis that he (the first) is interested in.

I would rather draw attention to the fact that the separation of institutions from in-
teraction is to a large extent an analytical distinction. For it is partly from interaction and in-
teraction that we build up our abstract struc-
ture of institutions; and conversely, in studies of interaction, we are concerned with in-
capsulations from institutions. Somehow we must try to weld these different types of analyses together.

I believe firmly that anthropology is a science and therefore progressive and accumu-
I...

FORTES, M.
1930 The dynamics of clanship among the Tallensi. London, Oxford University Press.
1940b The web of kinship among the Tallensi. London, Oxford University Press.

FRANKENBERG, R. J.

GLUCKMAN, M.
1964 (ed.) Closed systems and open minds. Chicago, Aldine; Edinburgh, Oliver and Boyd.
1956 The ideas in Barotse jurisprudence. New Haven, Yale University Press.

GREGORY, E.
1936 We, the Tikopia. London, Allen and Unwin.

HOMANS, G. C.

LEACH, E. R.

MALINOWSKI, B.

MANDELBACH, M.
1967 A note on history as normative. History and Theory, VI, 3, 413-419.

MARWICK, M. G.
1965 Sorcery in its social setting. Manchester, Manchester University Press.

MORRIS, J.
1953 The lineage system of the Maasai of the New Guinea Highlands. Edinburgh, Oliver and Boyd.

MERTON, R.
1957 Social theory and social structure. 2nd ed. revised & enlarged. Glencoe, Ill., Free Press.

MIDDLETON, J., and E. WINTER (eds.)

MIDS, H.

MITCHELL, J. C.
1965 The Yao village. Manchester, Manchester University Press.

NADEL, S. F.

The Utility of the Equilibrium Model


PHILPOT, L.

POPPHOLZ, L.
1958s Kapauku Papuans and their law. Yale University Publications in Anthropology 54.

REAY, M.

RICHARDS, A. J.

SALMIR, M. D.

SALISBURY, R. F.

SHEPPERD, N. J.

TURKEE, W.

VAN VARK, J.
1960 The politics of kinship: a study in social manipulation among the Lakeside Tonga of Southern Rhodesia. Manchester, Manchester University Press.

WORTH, E. Z.

WATKINSON, W.
1938 Tribal cohesion in a money economy. London, Manchester University Press.

WILSON, M.