CHAPTER 1
INTRODUCTION AND BACKGROUND

Social Anthropology is normally thought of as the science of observation, classification and understanding of human cultures. To many it is an inherently literary subject, "humanistic" in that the typical practitioner thinks of "understanding a culture" on the lines of "appreciating a work of art", not on the lines of studying the mathematics of Hilbert spaces in order to grasp physical theory. However, the reader who accepts the above analogy has fundamentally misunderstood culture. Human cultures are neither more nor less tangible than the objects of physical theory. Both realms are understood only through our abstractions of them, and our ability to derive inferences from these abstractions on the nature, existence and distributional properties of the things observed. From this perspective, there is no reason to believe that an effective theory is not possible for the nature, existence and distributions of properties of cultures just as for physical systems. This book describes such a theory.

To understand this better, consider a redefinition: Social Anthropology is the study of cultural rules and their consequences. While my first definition above conjured the image of studying art works, this new definition invokes thoughts of a body of law, or the grammar of a language. To realize that an abstract theory of such objects is possible, consider the notational systems and analytical methods that exist around the various forms of music. While the notation of a Beethoven concerto is obviously not the same as a performance of the same concerto, there are nonetheless correspondences between notes on the music page, the physical motions of the performer, the sensations of a listener, and the analysis that distinguishes a concerto from a sonata, names the key and its harmonic properties, describes the melody, perhaps finds the origin of that particular melodic form in other forms, and so on.

Consider a less esoteric example: a system of exchange in the absence of a notion of contract. Does the recipient of an object of exchange assume all liability for the performance of the object? Does the physical transfer of objects terminate all aspects of the transaction? Or, are all transactions seen as of indefinite time duration, forming an unending bond between the parties caused not so much by the nature of objects exchanged as by the fact of a transaction? One may imagine their own list of similar questions, but the implication is clear: once an answer is provided to any one of them, then this answer forms a kind of "rule" that binds the possible actions of the parties to the exchange, and thereby has a material effect on the flow of objects through the system of exchanges.

It should now be clear how mathematical theory enters the study of culture, defined as the study of rules and their consequences. Such a theory must be capable not only of describing the rules, but of correctly predicting their consequences on material objects subject to their operations. The existence of such predictions implies a wide range of consequences. For example, it should also be possible to predict the properties of systems using the rules, and the conditions under which such systems may exist and would be observed. If this seems too general, consider the formulation quoted by Samuels (1966) from works of the early economist Adam Smith,

> "Our continual observations upon the conduct of others, insensibly leads us to form to ourselves certain general rules concerning what is fit and proper either to be done or to be avoided.
>
> "The general rule ... is formed, by finding from experience, that all actions of a certain kind, or circumstanced in a certain manner, are approved or disapproved of.
>
> "... The general rules which determine what actions are, and what are not, the objects of these sentiments, can be formed in no other way than by observing what actions actually and in fact excite them."

Thus it is not without precedent to take the observed existence of "rules" as a starting point for analysis of cultural systems.

Background of This Work:

Minimal structures are the smallest, self-reproducing diagrams which may describe the operation of a particular marriage rule on a population following that rule. The existence of such structures has been known and exploited since the earliest ethnographic studies, largely because they are a convenient means of ethnographic description. The demographic theory implied by the existence of minimal structures has found limited development elsewhere; this work is such a theory.

There are several other aspects of the present work. First, it provides a rigorous foundation for study of a large class of marriage rules. it does this by describing the rules in an efficient way which corresponds to ethnographic description, but it also tells how to study the consequences of existence of the rules. Second, it claims that the distribution of marriage rules known empirically may be predicted completely a-priori, knowing only certain structural numbers characteristic of the minimal representations of a rule. Third, the purely mathematical development of the theory of minimal structures is new; thus, the present work may be an elementary mathematical contribution, most probably to combinatorial geometry but possibly elsewhere.

The reader will frequently encounter the term "marriage theory" in this text. In general this means "theory of minimal structures" in the spirit of Appendix I. However I also find it very natural to refer to the entire subject matter of the present work as "marriage theory" since "marriage rules" generate all of the structures and statistics discussed. From my perspective, "kinship theory" is a related topic which studies geneologies independently of rules of marriage (kinship theory). For example, most past English language work (and particularly that on kinship algebras) has been more nearly "kinship theory" while the more prominent French theorists seem more interested in topics akin to "marriage theory". The notion of seeking minimal structures has been "in the air" for some time, such as in the works of Ruhemann (1945 and 1967), and certainly was in the style of genetic work as far back as Wright (1921). On the other hand, Arensbergs' (1972) application and "mathematical" usages are much closer to the use of Haldane and Jayakar (1962), Murkerjee (1972), Atkins (1974) or Lorrain (1973) then they are to my own. The paper of Hajnal (1963) has a unique existence, falling more to the tradition of classical demography. Martin's (1981) paper ties the Hajnal work more closely to the "kinship theory" tradition but does not cross the boundary to minimal structure theory.

Anthropologists have studied marriage patterns, principally by genealogical methods, but also more recently by group theory and relational algebras (Weil, 1963; White, 1963; Livingstone, 1969; Liu, 1969; articles in Kay, 1971; Read, 1984; Tjon Sie Fat, 1981; and in Ballonoff, 1974a,b,c and others). Computer simulations of theoretical marriage statistics from kinship patterns have been studied for example in Dyke and MacCluer (1973) and Dyke and Morrill (1980). All of these works unfortunately missed the essential points on relation of population measures to ethnographic description, developed in this book. The last several decades has also seen the encroachment of mathematical forms into many other discussions, as in mathematical economics. As a final stronghold of essentially nonmathematical social science, anthropology thus finds itself in a unique position.

My own first approach to this problem was a logical representation of the minimal stability notion (smallest structurally stable self-reproducing representation on discrete generations) for the "American" system. Next I discovered that Harrison White (1963) and others has already worked on a mathematical representation of clan systems. Invention of my graphic notation (used in this text) as a simplification of a logical notation (which I have not used here), provided a means of linking the minimal stability idea to graph theory notations. This raised the possibility that the logic of minimal stability would apply to any marriage system.

The above notions were formulated by the end of 1968. There followed a period of extensions of the basic ideas by application of graph theory, binary logics, and explorations in control theory. During this period, several rather independent currents of work began to fit together. First, work on a separate problem lead to awareness of White (1971). White develops the notion of the flow of particles and holes as used in electronic theory, into a descriptive technique for analyzing organizational career and promotion patterns. General readings in science also lead me to a belief that quantum mechanical formulations might be of value to social theory. In the final writing of Ballonoff (1970), I speculated that a representation in quantum mechanical formalism would lead to a better understanding of stability properties of marriage systems. The difficulty became the proper representation. I began to speculate on the utility of the Dirac Notation for the appropriate transition probabilities. This resulted in two papers; Ballonoff (1976, Chapter 3); Duchamp and Ballonoff (1974). Since on reflection these papers are essentially graph theory and logics dressed in a "Dirac" notation, I subsequently decided that literal analogies to quantum mechanics was not operational science, considering it instead as a semi-operational philosophy. Similarly I draw several inferences in this work in direct contradiction to the claims of many "systems theorists," especially where they deal with complexity, hierarchy, and evolution of social systems. Such studies often fall into a trap studied by Maciejowski (1978), in which a modeler uses all the known data to model a system; a better theory uses part of the data, and predicts the rest. My resulting theory does this.

I was thus guided principally by three factors. First, the philosophy of quantum theory; second, by thorough rejection of universal answers to all problems; third, by the concrete examples provided by social anthropologists. A reader in social anthropology might therefore wonder on certain omissions in the above intellectual history. While I constructed this theory in knowledge of Levi-Strauss's (1961 and 1969) works, I did not intend to "mathematize" those insights. Nonetheless, on most notions of structural description, Levi-Strauss was a predecessor. His most important omission is explicit statement of structural number or minimal stability requirements. (He was, therefore, unable to draw inferences from these.) Andre Weil's appendix to part I of Levi-Strauss (1969) uses matrix operators in very nearly the formulation of Duchamp and Ballonoff (1974), which however we only realized in retrospect.

Readers in anthropology may find this work ambiguous. Those who see cultural studies as "science" may claim this work, while the "humanists" may for the same reasons accuse it. My position is that anthropology is a "physical science," because of the predictive nature of marriage theory. Yet in no way does this work divide the "scientific" from the "human" in anthropology. Rather, it emphasizes the mutual dependence of the one upon the other. The very subject of the text is, after all, the existence of certain individual and highly personal acts. In the end, therefore, I merely characterize the same human history which concerns the "humanists".

Programmatic Review

Some the newest developments in the study of anthropology have been in its mathematical elaboration. Works by Paul Kay (1971), Dyke and MacCluer (1974), Selby (1968), Ballonoff (1974a,b,c), Jaulin and Richard (1971), Burton (1973), Hoffman (1969) and White (1963) provide a summary of both preliminary and more developed efforts in a number of areas.

The problem of mathematization has several aspects. An anthropologist attempting to use mathematics is faced with the difficulty of learning, and often inventing, a system of mathematics adequate to represent understanding of an anthropological problem. This requires knowing when a mathematical system is adequate, a frequently ambiguous problem. In some cases, adequacy implies no more than the existence of a quantitative measure of the likely difference from "reality" resulting from inadequate representation. Similarly, an anthropologist may be aware of differences in the structure of the mathematical system being used from the "real" system being represented. since such differences usually enter by the anthropologist's choice, as through simplification, they may often be handled by explicit recognition of choices and procedures. Discussion of these choices raises the question "adequacy for what."

Fortunately, this selection does not have to be dogmatic. For example, one may use an "ecological" mode of thought, with the central idea of a "niche" compared to the possible ranges of occupation of a particular kind of cultural configuration. One might then look at Andrewartha's (1961) work on Animal Populations and MacArthur and Wilson's (1967) Island Biogeography and discover that inferences can be made of the type: "If a population does not do something to alleviate or modify certain conditions, it will not be able to survive longer than certain time limits, or beyond a given maximum level of population while retaining the same organization." Such statements can often be expressed as numerical limits or constraints, but may also be stated as probabilistic arguments. Similar approaches are exemplified in Birdsell (1969), Yengoyan (1969), or Hoffman (1959, 1969a,b). Gregory Bateson seems also to have been aware of similar possibilities (Bateson, 1958: Appendix).

But anthropologists have not been unanimous in the use of biological models, as is exemplified in the literature on "formal" properties of social systems. These "formal" (often "mathematical") methods generally apply concepts from set theory and relation theory to kinship descriptions, and write statements about such systems in mathematical symbolism. This work has been summarized elsewhere, including Tyler (1969) or Mukherjee (1972) and with kinship mathematics references cited earlier. Another, similar approach is to write kinship systems as series of "rewrite" rules, and grammatical-like statements, with close similarities to the work of Chomsky (1965) and Bach (1964) or Chomsky and Miller (1963). Examples of these approaches can be found in Hammel (1965), Hoffman (1959), and Lounsbury (1956). This approach was strengthened by the work of Francois Lorrain, who was more interested in formal tools than the just noted authors (Lorrain, 1969, 1973; Lorrain and White, 1971); Guindi and Read (1979) apparently reinvented Lorrain's formalism.

It is of interest that most of the mathematics used by the authors just mentioned may be discussed under the topic "automata theory." As represented in Arbib's (1964) work, this is a collection of mathematical and logical ideas which have found application in problems from design of computer circuits and programming languages, to neural networks and perceptual systems. Automata theory has also been applied to problems of adaptation and self-organization, e.g., Bellman (1961) and Von Neumann (1966). One anthropological use has been in discussing properties of "cognitive maps" as used by Wallace (1961) (leaving aside the question of the "validity" of such representations in the first place). It can be shown (as in Marcus 1967, Chapter VI) that statements in automata theory are equivalent to statements in other branches of mathematics, notably graph theory, and to theories of branching processes (trees). A recent attempt to take advantage of properties of kinship terminologies to get at mathematical properties of language using Suppes' (1967) logic, with at least supposed cultural significance attached to the results, was by Geoghegan (1971). A related paper is Kay and Romney (1967).

Structural Theory

In discussing the structural view, I must confess ignorance of much work particularly in Dutch but also in French. Therefore my comments are methodological, not historical. The first authors I consider are E. E. Evans-Pritchard and Fred Eggan (1952), who argue:

> When we use the term structure we are referring to some set of ordered arrangement of parts or components. (These are) persons . . . considered not as an organism but as occupying position in a social structure. (9-10)

Since the authors have not said what sort of ordered arrangements are "social structures" and which are not, one may presume that any arrangement will do. But this is clearly wrong, or at least in need of qualification, as there are not merely "positions" in a social structure, there are also past, present, and potential "positions", unactualized present "positions", conceivable "positions", and inconceivable ones. Clearly to intelligibly talk of "positions in a structure," it is necessary to first talk of sets of rules for "structure" creation, sets of rules for "position" creation within the "structure," and adequate sets of rules for "position" creation within the "structure," and adequate sets of descriptors of both the "positions" and the characteristics of components of the "positions" themselves (apart from their occupants). The choice of how to apply the constructs of such a theory are up to the ethnographer, and choice of the theoretical constructs themselves must be justified anthropologically.

Comments similar to those listed above for "structure" are also necessary in a meaningful discussion of "continuity of structure" which Eggan and Pritchard claim as the object of studies (1952:10). One will not simply be interested in "continuity of structures," but in continuity of "positions" in them, of the adequacy of descriptors in different conditions, and the continuity of the rules for construction. May the rules evolve, but continue to produce the "same structure?" This is not only a semantic problem: it is observational of rules, and observational of the "positions" themselves. It is as well a considerable problem in the formal logic of axiom systems, since one must potentially allow for a continuously changing set of axioms producing the same analytic results. In addition, the conditions under which variants of the rules may occur must be studied.

Possibly because of an awareness of such problems, Eggan and Pritchard slightly lighten the descriptive load by adding:

> ". . . social structure (is used) as an arrangement of persons in institutionally controlled or defined relationships, such as the relationship of king and subject . . . and to use organization as referring to an arrangement of activities" (1952:10)

But this is not the same as "persons occupying positions in a structured arrangement." The first definition is much broader. This leads quite directly to a consideration of this concept of Radcliffe-Brown (1952):

> . . . direct observations does reveal to us that these human beings are connected by a complex network of social relations. I use the term "social structure" to denote this network of actually existing social relations of the particular primitive society under study (1952:190).

> . . . the social phenomena which we observe in any human society are not the immediate result of the nature of individual human beings, but are the result of the social structure by which they are united (1952:192).

This is quite in line with the concept being presented here: to explain the hen-scratchings called "data" created as a result of social phenomena by studying rules of interaction and their operation on particular occasions.

Radcliffe-Brown, however, is much more explicit on what he means to include in his studies. In particular, Brown specifies that he wishes to study

> all relations of person to person, the differentiation of individuals . . . by social role, and that existing social relations are the concrete reality which should interest the researcher (1952:191-192).

My approach differs principally with the last of these. Even other descriptive sciences do not claim to deal with only existing reality. What of the unspoken as yet, but potential and grammatical sentences that worry the linguists? In any such case, "concrete realities" are dubiously described at best, even where they "really exist." For example, sources of data for many ethnographic studies typically are as follows: (1) Personal observation, including both participation and non-participation in events, individual questioning, attendance at various meetings, following subjects in their day's work, and other activities. Any of these may have been reported in either of the following ways: simultaneously on note paper, and typed with no modifications; or recorded on tape or paper afterward with no notes because of the nature of the investigating conditions, with various time lags from time of observation to time of writing; (2) Similar reporting of other researchers; (3) Voluntary offering of information by subject; (4) Formulation of views and interpretations of the researcher and others in meetings and in working papers; (5) Responses by many individuals to questionnaires constructed by few individuals. Where in all of this can one speak of "reality," and even if there is such, how can one expect to find an undistorted version of it?

The point of this is not to criticize the conduct of such research; similar problems arise in any field work. What is questioned is that the subject of study is existing reality. Rather, study is itself a process, a search for regularities of pattern from observations recorded or remembered; these regularities should then be tested on the consistency of these observations with the observations and beliefs of other observers, and/or by subsequent events which may both confirm or deny conclusions made, as well as provide data for further investigation. Brown seems to recognize at least part of the problem, when he notes that consistency is found in "the kinds of relations that I can observe" and that "structural" form does not change if these do not change (1952:193). But my argument is a stronger one: the true subject is one's beliefs about what has been observed. No subject of study or formal "scientific" knowledge exists outside of these beliefs and the process of changing or maintaining them.

This however does not prevent my agreement with Brown's intent in the following:

> We cannot study persons except in terms of social structure, not can we use social structure except in terms of the persons who are the units of which it is composed (1952:194).

It is the recognition of the molding of human life and ambition by the very relations through which they may be expressed, which gives anthropology such a distinctive place as a field of science, and justifies its application to society.

Rather than following Brown in details other than the above, the concept of this study is closer to that of E. R. Leach, who expresses himself as follows:

> How can a modern social anthropologist . . . (with so much empirical work in the history of this field) . . . embark upon generalization with any hope of arriving at a satisfying conclusion? My answer is quite simple too; it is this: by thinking of the organizational ideas that are present in any society as constituting a mathematical pattern (1961:2).

The important point of Leach's emphasis on generalization, as opposed to "comparison" proposed by Brown, is that when one starts "guessing" one needs to know how, and "this is what I am getting at when I say the form of thinking should be mathematical" (1961:6). Leach also provides one caution: ". . . while the consideration of mathematical models may help the anthropologist to order theoretical arguments in an intelligent way, his actual procedure should be non-mathematical" (1961:8). While a mathematical literature many come to exist, the reasons for selecting or rejecting the use of any particular mathematical idea must be anthropological reasons, arguments must be relevant to anthropological theory, and theories, however mathematical, must be anthropological theory. I also agree with Evans-Pritchard when he says

> . . . the social anthropologist is not content merely to observe and to describe . . . but seeks to reveal the patterns which, once established, enable him to see it as a whole, as a set of interrelated abstractions (1962:61-62).

The final aim, even for mathematical approaches, is to present such coherent descriptions.

This observation raises a question asked by Claude Levi-Strauss: "What sort of model deserves the name 'structure'?" (1961:271), and which he answered as follows: a) the model exhibits the characteristics of a system, i.e., it has interrelated parts; b) "for any given model, there should be a possibility of ordering a series of transformations resulting in a group of models of the same type," a property which says that a good model is more than a description of a particular case; c) the first two properties make it possible to predict how the model will react if one or more of its elements are submitted to certain modifications; d) the model should be so constituted as to make immediately intelligible all observed facts (1961:271-272). Criteria d) gives the acceptance criteria for one model over another. If one theory or model explains something another does not, the better explanation is to be the accepted theory. So defined, a description of a particular society must be a particular application of a model suitable for more than one possible society.

Next, Levi-Strauss defines two types of models: mechanical models, or "models the elements of which are on the same scale as the phenomena to be modeled" (1961:275-276), and statistical models, where the elements are on a different scale (1961:276). Outside of social theory, such differences seem to abound. For example in population genetics, the Hardy-Weinberg representation of Mendellian inheritance is a mechanical model in Levi-Strauss' sense. Such laws may involve "probabilistic" behavior of individual genes, but are not statistical as Levi-Strauss uses that term. However, R. A. Fisher (1958) took the entire population of genes as the basic unit and examines the entire population with a few statistics, being therefore a statistical model in Levi-Strauss' sense. In the context of the cultural mathematics examples discussed earlier, those approaches classed under "automata theory" would probably have to be called mechanical, while those biologically inspired models are generally statistically formulated. This distinction however is not directly applied in my own work.

It is also useful to review a notion of model from Frederic Barth. Barth (1966) argues that models of social organization must be "designed so that they, by specified operations, can generate such regularities of forms." These models should have a limited number of well-defined parts, which may be varied in number and arrangement to produce a variety of different forms. In addition, "the logical operations whereby forms are generated should mirror actual, empirical processes." Constructs which have this generative property may also be called "axiomatic," according to a definition in Beth (1959:81).

One approach to developing a mathematical anthropology was by Hoffman (1967) who was concerned with time series. In using time sequenced numbers, one usually asks: Is the series to be considered stationary or not? Stationarity may be tested for in the construction of probabilistic mathematical models. The term has been defined as follows:

> Stationary time series are unchanging in respect to their general structure. The fluctuations up and down in such a series may seem random or show tendencies to regularity -- in any case, the character of the series is, on the whole, the same in different sections" (Wold, 1954:1).

It is important to notice that this definition deals only with the character of the numbers themselves. Even if one can show stationarity in a time series, one has said nothing about the process that produced them. If there is no theoretical interpretation available for which the property of stationarity is a predicted result, the search for stationarity is essentially meaningless. This conclusion holds for every "statistical property" which may be manipulated out of a lot of numbers.

However, consideration of ad hoc numerical series as dependent on the flow of time has found favor. For example, W. H. Holtzman commends the idea to psychologists as follows:

> Everyone is familiar with such time-series as annual rainfall, the gross national product, or the family income over a number of years. The study of change is commonplace in many dynamic systems, although forecasting future trends is still only approximate at best for most such series. Unlike the usual situation in psychology, the statistical population is the lifespan of the system and the statistical sample is a relatively short-time series within this total life span. The main purpose of the analysis is to gain enough insight into the internal structure of the time-series to permit valid generalization about the system's behavior (1967:200).

Anthropologists are using time series in exactly this way even when making simple comparison of, say, population size of a small town in 1930 and 1955. Such statements have taken a relatively long sampling interval, and used a simple comparison of the numbers to make an inference about things happening to or in the town -- inferences about system behavior. But however much this idea may aid in conceptualizing "data" available, it tells nothing about the purpose for which "the data" may be analyzed, and therefore tells nothing of itself, on "valid generalization about the system's behavior".

This conclusion also conflicts with the view of E. Leach (1954), who argued that any conceptual model must be an "equilibrium model", meaning that the society must be presumed stable to make a description at all. But the comparison of two conceptualizations of two societies, or of one society at two points in time, is not the same as the comparison of these societies themselves. One need not presume that because the description is "stable" that the society must be. Presuming other than this would violate Leach's own statement: "the structures which the anthropologist describes are models which exist only as logical constructs in his own mind" (1954:5).

J. Clyde Mitchell pointed out that use of quantitative methods encroached quickly into sociology, while anthropologists remained as biographers skeptical of formal methods. Mitchell says that this

> . . . is partly due also to a possible reluctance to use methods based on material that has been collected in a way that violates the theoretical assumption and the mathematical procedures derived from them; and partly from misgivings about the appropriateness of mathematical and statistical methods to the sort of material with which anthropologists are normally dealing (1967:19-20).

Mitchell's comment is quite relevant to the present discussion. As criticized also by Brush (1978:132-134), it is quite disruptive to consider "quantitative methods" (or "mathematical methods") a-contextually, and rejection by anthropologists of deified procedures is quite justified, and desirable. Mathematics <u>per se</u> has as much, and only as much, value as it has ability to provide modes of thought successful for particular problems. Statistics in the absence of theory have little value.