

Notes for a Study of Fertility

There is a play in the idea of fertility that is of tremendous importance. First it is a capability to produce: children, ideas, crops, life of all kinds. But it is also performance: actual numbers of babies born, a slogan for the work of an author, a known attribute of the soil. Fertility as it extends over time is a process in which all living beings participate. But it is also a subject of assessment; the value of fertility is not everywhere conventionalized in the same way.

We have only the most haphazard idea of why this is, even though questions of this sort have been of considerable and indeed polemical interest almost perpetually. The demographic aspect was separated off very early, and put in its modern form by Malthus (1798) as a relation between social ideas and practices and material constraints. Malthus was also prescient in the elaborate efforts he made to get numerical information about population. Neither of these contributions were original, nor was the dubious class interpretation he built upon them. Nonetheless, the Malthusian model in which social mores decide the numbers of people, and in which these numbers, every increasing, approach a point at which the exhaustion of resources intervenes, is still the most widely accepted description. Productivity carries its dangers; fertility wants control. As a statement of general possibility this is trivially true, but the power of the idea is evident in its direct contribution to two defining features of our era. The first, which does not directly concern us, is Darwin's theory of natural selection; the second which includes some influence of Darwin, is the conceptualization of human populations in numerical terms in which social influences are included solely for their material, in this case, biological consequences.

The use of numerical methods in studying populations has a very long history; it cannot be said that Malthus contributed much to this, he was mostly just awake to its possibilities. By the time these methods had truly become statistical at the turn of this century the metaphors of evolution had pervaded the study of society, so that the writings we recognize today as the first formulation of fertility in the demographic sense were made as mathematical contributions to biology. The gradual sociologizing of these metaphors took place, as it did in anthropology, in the period up to about 1940. Sociologists of fertility since that time have chosen to concentrate on a statistical method parallel to demography; the categories of these statistics are a thoroughly ad hoc mixture consisting of remnants of the biological glosses, stock categories of academic sociology, and those items required by the statistical method itself. The assessments of prior periods are included in these categories in some scattered part; but there is to the demographic and sociological study of fertility little of the vital force of the idea of fertility itself.

It is well known that, aside from the occasional statistical advocacy, anthropologists applied their socio-biological metaphors to aspects of society in which the advantages of enumeration and statistics were not immediately apparent. The censuses taken by ethnographers are more in the way of initial reconnaissance than a major influence upon subsequent description. Mere survival is not an issue for most societies anthropologists have studied unless this was a matter of the encroachment of neighbouring or colonial groups. Malthus and even later writers who included primitive peoples in their population studies, such as Carr-Saunders and Krzwicki, have never had an anthropological following. This did not leave anthropologists free to take up their own approach. As it turned out the attachment of anthropology to colonialism, the pseudo-biological idea of functional intergration, and the correlate inattention to language and native representation

united to remove the importance of fertility in the self-definition of groups from the ethnographers' attention. Perhaps the study of 'kinship' and 'marriage', had it been left a greater element of self-definition, might have given the 'play' of fertility explicit attention. As it stands, it is an open question whether these institutions and their terminological and ritual expression embody anything like the range of ideas in English surrounding 'fertility', 'conception', 'creation', 'germination', and the like. The same is true if we ask what the influence of the range of activities so described has over changes in the numerical composition of groups. That is, if we ask the inevitable question of the relation of ideas and infrastructure, of classification and action.

The particular importance of fertility is the 'play' between the fact of the process in time and the conventional assessments which are made an object of study as if they were outside of time. The 'play' encapsulates a current problematic, that is, the definitions that we ordinarily go by and the ranges of experience we thereby shut out. We would like to reinstate time, not knowing altogether what is meant by such a grandiose phrase. And we would wish, thereby, to do away with the painful hyperstasis of phrases such as 'ideas and infra-structures' and 'classification and action'.

The centrality of fertility is not just its evocativeness, as tends to be the case with a similar term, viz 'generative'. Rather, it provides us with something of a course to follow, at least in the initial stages. The 'play' is equally inaccessible to demography and anthropology: to show that the situation of these two subjects is essentially the same is at least of polemical value; and insofar as this refers the major method of study in this century (statistics and formalisms generally) to a subject which considers itself a defender of the informal and semantic, we would be tackling a case of general importance. Inevitably this would say something of the capabilities of the methods of each for the problem at hand. The two subjects seem particularly suited for such a critique: demography, of all the social studies, is remarkably conscious of the artificiality of its method; the anthropology with the greatest implication for fertility, the study of prescription, marriage, and related symbolism, is among the most highly developed in the subject.

A critique does not offer a way out. One is inclined to agree with those who argue that the next steps await an ethnography we do not as yet have. At times this seems particularly damning, as if those who could go into the field if they merely wished do not, and those who would like to find they cannot get the most simple help. The history of these two subjects, which makes up a kind of ethnography of a certain scientific problem of our period, at least permits us to show the extent to which the current problematic may be stretched.

It is a remarkable impasse that we are unable to account for the influence of collective representations upon changes in population size and composition. A glance at history does tell us something about the demographic situation. At present we possess a remarkable calculus for expressing changes in relative numbers of people considered in the abstract; but there is no comparable analytical framework which conceptualizes these changes as they follow from native representations, considered for their own abstract structures. The sociological study of fertility, which has attempted to account for these changes statistically, without attending to the structure of native representations, has yet to produce anything like a theory. All of this can be said to follow from the historical situation at the beginning of this century: basically, that a certain conception of the use of formal methods was widely accepted, and that anthropologists while also accepting it generally chose to study situations in which such methods seemed pointless or impracticable.

While there has always been a certain disdain for statistics or formalism, anthropologists have never bothered to produce a thorough-going critique. Some obvious problems, such as the inappropriateness of standard demographic categories and schedules to particular ethnographic situations have been noted many times; but these have become rather pat criticisms which are merely a folk-lore within anthropology. Nor have anthropologists applied themselves to semantical analyses of the representations that might be responsible for changes in population structure in particular societies; this in spite of the fact that most of the societies they have been studying have been going through the most radical displacements imaginable.

There is a good scattering of ethnographic information in the vicinity of the topic, some of it very interesting: these range over anecdotal information on sexual practices, historical and demographical accounts, physical and cosmological representations as they enter into systems of exchange, recent discussion of ethnic definition, and simple passing references. It would be an interesting if quaint exercise to assemble these materials, for the similarity in native manners of expression of fertility might well make up a kind of natural resemblance. However, previous experience in assembling these tangential writings in accord with the interests of other academic periods, has shown them to be very suggestive but inconclusive.¹ This is likely to be all that can be said.

A history of the separation of anthropology and demography, of the missed critique on the one hand and the missed ethnography on the other, would not be without interest. Needless to say, demographers are doing something quite different in their study than are anthropologists; the point of such a history would not be to suggest that they fail to take up the problem of the influence of native representations, for they never intended to; rather, it would show some of the consequences of setting such questions aside. These are of some interest as they are part and parcel of the statistical method generally. Demographic analyses, because they are in this way incomplete, have been susceptible to the wildest interpretations and, accordingly, have been used unintentionally to misrepresent the very relations they are intended to show. Such an account would not startle demographers at all, for they are accustomed to the mistakes their method engenders; but it also would not help them with this problem, nor give us a hold on the semiotics of fertility. However, a historical stretch of the successive interpretations of demographic statistics does provide us with a good set of examples of the semiotics.

A collation of anthropological part-references to fertility would only remind us of certain familiar limitations in the methods of interpretation of different periods of anthropology. It is not possible to consider these as part of a semiotics of fertility since anthropologists have never really conceptualized them in anything like that way. There is no tradition of study to be ferretted out here. But the recent experience of anthropologists in 'rethinking' the short-comings of earlier accounts has led them to regard questions of idea and infra-structure such as posed by fertility as left outstanding by traditional descriptive methods. Fertility is the kind of problem whose current fragmented state of formulation can be recognized as more than a consequence of preferred methods at the inception of these subjects and a subsequent division of labour. We can, instead, invoke that heavy word 'epistemological' to describe certain features of the thinking at that time which continue on into the present.

Obviously one such feature was introduced by the conception of formal methods: the requirements of a notational system, notably the total unambiguity of its characters and their relations, means that the manner in which it specifies events is remarkably different than that of ordinary language. The consequences of this difference are very far reaching. They

include not only the tendency to recast ethnographic situations in an alien form, but something of the rationale behind the division of academic subjects such as anthropology and demography. When we speak in passing of 'levels' of analyses, we are invoking an ideal in which the clarity and precision of mathematical and geometrical analyses is never far away. When anthropologists bicker about the status of formal methods in their subject, as I will go on to do in this paper, they are, for whatever their disagreements, basically just reasserting these familiar divisions.

A further epistemological issue is the way in which ethnographic situations, of which that of the analyst can only be another example, seem to present themselves. This is really a matter of our own inarticulateness. Fertility may serve as the case in point, considered 'just' with reference to its central aspect of human procreation. We might take this, as is often done, as a question asked by some hypothetical couple as to whether and when they should have a child. Of course familiar collective sentiments weigh in very rapidly. These may be on quite a different scale, such as the state of the economy in a particular sector, a totalitarian character of government, or a tightly-knit ethnic or religious community. All of these may be rendered locally as, for example, the social pressures on working mothers, the number of children one can expect to get into the Party, or the threat of assimilation to a small community.

The definitions over-ride even the unpredictable physiology of conception. Take, for example, the experience of those women 'on the pill'. Quite a number of births and abortions seem to follow from misgivings about its physiological effects - misgivings which lead to sporadic use. There is good cause for agonizing here, whether it is really unknown possibilities of clotting or cancer, or the daily physical discomforts. Some women put up with all of these and some women finally refuse, but the incidence of all of the symptoms is scattered through the full range of users. Who would say that their problems and self-diagnoses are merely either physiological or 'psychosomatic'?

To take just the pregnancies which seem related to this; the availability of abortion marks some change in the view of women and men and pregnancy; this seems to have lessened the reality of lumbered marriages and self-induced or clandestine abortions, if only by adding possibilities. One may note that this owed to social redefinition as well as technology; the technology has not removed the physiological indeterminacy, nor made contraception and abortion popular, although it has in some way participated in the changing ideas people have about what to do when unexpectedly pregnant. Plainly this is a part of a much larger and continuing change. Although we may consider the control of fertility as an axis along which the relative positions of men and women are conventionalized, there is much more to these situations than any simple linguistic statement can convey.

The epistemological puzzle posed by situations such as these is that, on the one hand, they expand to take in very large ranges of society; on the other, they reduce to a tenuous interpretation of infra-structure. No one characterization seems adequate. Nonetheless, when we sometimes refer to 'the pressures' on people who happened to be procreating (as well as all sorts of other activities similarly influenced) we are acknowledging the relatedness of all of this, and people in these situations do see themselves as 'pressurized'. The problem is not unlike the one, in an overlapping area, which led Edmund Leach to argue that there could be no simple definition of marriage; marriage is at best 'a bundle of rights'. All we have are these awkward, short-shrift phrases. No one will think, then, that I am trying to substitute 'fertility' for 'marriage', 'kinship', and the rest.

There is the graceful option to consider only those sets of definitions which cluster around recurrent events. Bundles of rights, kin terminologies, colour terms, are all examples. Particular rituals or myths also suggest themselves as encapsulations of basic social themes. One can imagine an attempt to take some situation in which the 'play' of fertility enters, such as the situation of young unmarried pregnant women in 'family planning' clinics in our own society, and try to trace the themes expressed in these regularly occurring situations through to the wider ranges of social representations that are of influence. Perhaps such events can provide a kind of text in the manner, for example, of Gregory Bateson's Naven.

There are many problems here, even setting to one side that we have no such accounts, and whatever might be the problems of the midst of such an ethnography. Taken as an idea of how to go about studying such situations, we might criticize the 'ritual' or 'terminological' approach in two ways. First, while such an ethnography would tremendously improve our understanding, there is nothing in the formulation which would allow us to monitor shifts, e.g. in attitudes toward abortion, or in control exercised by men and women, or in the very difficult questions of diagnoses. Such a description gives us valuable information about the current state of conventions, not of continuing process. Second, the status of such terminologies and rituals seems rather idealized. It is presumptuous to proceed as if important terms and routines will everywhere take up coherent sets of terms and actions; if approached as sets anyway, we should expect such sets to be loosely structured, full of 'hollow' categories, and impossible to interpret without a diachronic sequence of changes. The idealization is both a fixation into forms whose distinctiveness may be endlessly debatable, and a fixation of time.

We began this section by remarking on our inability to connect collective representations and population changes in a convincing way. Somewhere between the two we have insinuated young unmarried pregnant women and their men in situations somewhat like those in which the control of their fertility evolves. Anthropological descriptions, which might be very welcome additions to our knowledge about these people, do not seem suited to showing how the major changes in social definition of their situations occur, nor the consequences for demographic structures. Our description of these has been quite summary; however, the static quality of anthropological descriptions, and the monographic method in which the no doubt very plausible relations are filled out by illustration and anecdote seem to be sufficiently long-standing subjects of criticism within anthropology as to not require restatement. There is no question that recent work on classification marks a major improvement; the replacement of pseudo-biological analogies by pseudo-grammatical ones has not proceeded without an awareness that such changes are of the same kind as the ones anthropologists study; but insofar as these improvements are addressed to understanding ostensibly 'new' sets of classifications rather than attending to their modes of derivation or production - and the tendency to stereotype changes in time as 'evolutionist', 'functionalist', 'structuralist', 'post-structuralist' is one obvious example - all of these developments serve to obscure the very sort of problem we are trying to get at.

We have also begun to give some idea of the background of the particular forms, anthropological and demographic, through which the 'play' of fertility has been fixed. We identified two epistemological aspects of this, without however, relating them; the separation of formality from language; and the range of implications of particular instances of 'play' which resist formulation either in an englobing way or cluster by cluster. The potential of their linkage seems obvious enough: the 'play' which is both meaning and action, is in essential aspects non-linguistic, and our frustration in formulating the range and movement of these situations comes no doubt from our attempt to force them into language anyway; formal notations are non-

linguistic expressions of connectedness and suggest themselves, therefore, as ways of reaching beyond the language. However, insofar as notational systems have their own rules of specification, which have nothing to do with ranges of social events, there is at first glance no reason to believe that they can be any more attentive to non-linguistic specifications than language. What does the use of formal ideas entail?

The adoption of formal methods, whether in analogy to various schools of mathematics or linguistics, generally resolves upon a form which allows a tremendous multiplicity of events to be expressed through a few, concise relations. Even the use of general formal ideas such as opposition, homology, and symmetry on a piecemeal basis retains a form which insists upon the crisp connectedness of theoretical formulations, in contrast to the informal and vacillating character of the experience of reading, writing, conversing, and so on. Formal methods generally resolve upon notational systems or schemes which insure the unambiguity of the items and relations. A formal method thus involves a set of relations in which the connecting operations are quite different from those which order social events. The correspondence of formal schemes to the conventional assessments of the events is thus far from immediately apparent.

The question 'to what do the elements of formal systems refer?' is resolved by the institution of 'data'. That is, a substitute reality is constituted which purports to be an accurate selection of information from a local setting. The implications of this in the statistical case are well known: the categories of the data follow the interest of the collecting agent and not of the local setting, although there is often a great deal in common. Statisticians such as demographers generally consider the gathering and condensing of information as a separate problem from the theoretical manipulations of their notation; the inferences and assumptions that make up a statistician's handling of materials, before or after they are accorded the status of data, usually remain unanalysed; and insofar as writers tend to refer to 'collection of data' rather than of information - i.e. the data is reality - the solution to the question of reference can amount simply to banishing both the processes and assessments of the peoples studied.

Anthropology counts a partial improvement on this. There is a tendency, particularly in formal analyses, to consider the written ethnography as data, that is, as an adequate account of a particular people. This in spite of the fact that the formal analyst is almost invariably asking a different set of questions than did the ethnographer; the situation would seem to be improved only when the analyst and ethnographer are the same person, and the account includes a description of how the formal rendering of native representations was decided upon. The work on terminological sets (with its incumbent limitations) alluded to earlier is a case in point.

Analysis of published ethnography has depended upon the generality of certain aspects of communication which lend themselves to formal expression. These ideas owe their entry into anthropology to Levi-Strauss's fitful explorations of linguistics and mathematics between 1945 and 1955, and their clarification to Needham's studies of prescription and lateral symbolism between 1958 and 1969. The basic distinction is that between prescription and preference, i.e. between self-defining categories and those for which there is a considerable element of choice. At a very general level there seems to be a close fit between the idea of a prescriptive rule and the categorical practice of native peoples. Thus, when Needham joins Leach in stating that 'prescriptive marriage is not merely (an) ideal type but actual'² he is asserting a one-to-one correspondence between theoretical relations formally expressed and the relations carried in certain native classifications. However, this applies only to the few categories that may be considered prescriptive: thus, while knowledge of a rule of pre-

scriptive marriage indicates what categories of people are allowed to marry, it does not say which individuals in these categories will marry, whether and how the categories may change, and it does not preclude that prescribed individuals may be reclassified as marriageable. For a working out of the practice, an intimate knowledge of personality, etiquette, tastes, manners of speech, local background - in short, of preferences - is necessary. Prescriptions arise out of preferences, both in the course of the investigator's understanding, and in the course of events generally. As Levi-Strauss notes, all prescriptions are preferences from a certain point of view:³ it is the assumption of a system of classification which in both cases turns the definition of certain preferences into the definition of a situation. Hence the quality of self-definition.

Thus, anthropology improves upon the use of formal methods insofar as the analyst first has some familiarity with native classification; and even then, the formal renderings are restricted to a few general conventions. The distinction between prescription and preference makes a slight but significant realignment in the usual attitude of anthropology which keeps formality separate from semantic interests. Formal ideas map selected ranges of representation rather well, and are an important aid in their exploration; but because this range is so limited, the direct applicability of formal systems - group theory, statistics, matrices and networks, etc. - as systems seems to imply an inevitable forcing of native classifications into some wholly alien mode.

We may class this clarification 'slight' in the sense that its main effect is to better articulate a long standing anthropological view. For example, although passing positive reference to statistical formalisms has been a part of anthropology practically from the beginning, there have been few attempts to give these methods a more than secondary role. These now tend to be identified with a certain period of the subject:

Certain members of the Central African/Manchester school of anthropologists did set out to improve the observational methods of fieldwork. Barnes, Mitchell and others made it possible to apply advanced statistical methods where they had been previously regarded as impracticable. The result was unexpected: such studies were not much welcomed even by avowed empiricists. The more 'statistically rigorous' seemed to mean, in some way, the less 'anthropological'. We may not necessarily deny the soundness of this instinct.⁴

No doubt the same instinct has participated in the misinterpretations of Needham's prescriptive studies; and these have, in turn, stimulated on his part several recent statements of method regarding the proper place of formal analyses.⁵ He plainly wishes to differentiate his work from the increasing mass of formal studies of all sorts; and where the question is of the nature of reference between the formal and some social reality, his comments turn specifically on prescription. The use of formal methods may be extended to cover prescriptive categories provided that the formal method is not a full-blown system but an opposite selection of formal relations. The situation for preferences, however, remains unchanged from Levi-Strauss's distinction between mechanical and statistical models: the formal approach to prescription is not suitable for preferences due to their multiplicity and changeability; formal methods such as statistics, while applicable, still do not follow the preferences as they are implied by native classification.

The problems posed by preferences are further confounded, as Leach noted several years ago, in that there is no necessary connection between collective and individual representations.⁶ These vagaries of preference no doubt account for the turn in some of Needham's current writings from the publication of formal analyses to an advocacy of conceptual analysis ac-

according to 'a combination of the approaches of Benveniste and Wittgenstein'.⁷ For this programme he states the limits of formal methods quite succinctly:

...the mode of analysis necessarily remains subject to two main critical qualifications. First, that the formal constructs themselves call ultimately for a validation that is independent of the ideological tradition in which they are framed; and this cannot be done either by meta-formal analysis or by reliance on the traditional concepts that the abstractions are supposed to rectify. Second, that however abstract or purely logical the formal notions may be, they are useful only to the extent that they mediate between the concepts of natural languages; and as soon as these are brought into any connection there rearise all of the stock hazards, of grammar and social circumstance, that attend any attempt to convey meaning from one form of life into the categories proper to another.⁸

This is, I think, an elegant clarification of the long-standing view that formal analyses and anthropological attention to the native point of view don't mix. In this conception, as in the definition of anthropology in contra-distinction to statistics, it is the relation which gives the respective sides much of their significance. Together they make up a common view, a seemingly inevitable division in the understanding of society. With the aid of the clarity Needham has brought to this relation we can make two points.

The first is that the relation as phrased is solely between formality and language. All of the argument above regarding our inability to formulate extensive ranges of social relations in language as well as the movement of these relations over time, weighs-in here. Insofar as these ranges influence our use of language, we can expect any accounting of concepts confined to their linguistic aspects to be frustrated. This is equally true for any accounting of the use of formal ideas without reference to the constant interdigitation of formal abstractions and their semantic counterparts. The application of formal ideas will have to be taken not merely in terms of their notational relations, but according to their use in the midst of reading, writing, arguing and other ways of understanding. This will vary considerably according to the situation of the notation. Needham rightly considers this as an inter-relation with language where mathematical notation and his own use of general formal ideas are concerned; but we cannot expect this to be the case for musical notation, for notations of human movement such as the Laban system, and for whatever schemes might be of use for ritual and other events in multi-dimensions.

The second point is that the distinction between prescriptive and preferential rules, insofar as it marks an overlap of formal and collective representations, does make the first step toward a consideration of the formal as used in combination with other semiotics. However, insofar as the distinction leaves unchanged our inability to model preferences in any other than statistical way, it makes no real advance. Examination of this inability allows us to elaborate upon the commonality of certain anthropological and statistical methods.

Levi-Strauss's distinction between mechanical and statistical models was drawn from Wiener: there are models expressible in the language of classical mechanics - ordinary language - and there are models in which the components are so many and various that they can only be considered in the aggregate. That is, the individuals of statistical mechanics are classes of individuals. However, both the classical and statistical models are mechanical explanations, and the logic of analysis of the individuals of one is true for the classes of individuals of the other. A statistical model is merely a mechanical model in which the operations which follow-out the assumptions of the system are probabilistic. We can carry the analogy on to refer to prescription and preference in the following way. The logic of

both statistical and mechanical explanations consists of prescriptions which rule the elements of the notation. Where the latter is concerned, the prescriptions state the relations of individuals - in the case of marriage, of groups in alliance and even of particular marriages. It so happens that prescriptive rules are characteristic of societies of moderate size so there would be little point to their statistical specification. However, in mass societies the definition of groups and the significance of particular marriages is much less clear, statements about the marriage practices of mass societies will still be based on a logic consisting of prescriptions; but insofar as particular marriages and groups could only be identified tediously, a statistician usually defines his own classes which, in the analogy to individual intermarrying groups, stand as collections of very large number of alliance groups.

The point I wish to make is that statistical analyses are not different from formal prescriptive analyses in their logic; their difference, as Levi-Strauss noted,⁹ is a matter of scale. A statistical analysis could be carried out within the logic of a prescriptive marriage system, although there would be little point to this other than confirmation in certain cases. . . statistical analyses becomes suitable for preferences due to the considerable scale of possibilities they admit; but what such an analysis does, in effect, is to reproduce a prescriptive analysis, a mechanical model, in which the details are settled in the aggregate. There is no analysis of preferences as preferences; there are only prescriptive formal analyses, some of which are statistical; any of these may attend to the nature of reference between the analyst's prescriptions and the data, or the data and the ongoing events.

The similar consequences of formal analyses of preferences, whether carried out statistically or verbally, can be seen by a brief consideration of the results of the papers on lateral symbolism. Fortunately, the status of these results have recently received explicit statement.¹⁰ The elements drawn from the ethnography in these analyses are taken from reports of particular situations which show clear evidence of dual classification; the elements are then listed in columns, but the placing of an element in one or the other column is not indicative of any common property among the elements: the only common factor is that they enter into the same kind of relation, and that their distribution seems to accord to some very widely applied distinctions, such as right and left. Needham takes up two questions regarding the theoretical status of this scheme and its components: first, the question of the relation of the analysis to the peoples concerned; and, second, the extent to which such analyses may be objectively validated or refuted. The notational scheme is not, of course, in the minds of the natives. However, having accorded the ethnography the status of data, the elements in relation are regarded as one-to-one with collective representations as used in native situations. The listing of these relations together does not indicate that either the situations or the dual symbolizations are in any way connected. That is, analysis says nothing further about a context than that it exhibits dualism; and nothing is said of the relation of contexts. Finally, presence or absence of dual classification says nothing necessary about the presence, absence, or relative importance of other principles of classification for these isolated situations.

Plainly, any similarity of such an analysis to one performed by someone using a statistical method, such as a demographer, must be at the level of the underlying logic, the prescriptions of method, rather than in the statistical elaboration. We have already noted the similar attitude toward information which accords its written presentation the status of reality. A demographer expects certain general principles to be operating in the data; we could say that the counterpart to dualism in a statistical analysis would be the regular characteristics of aggregates, such as the tendency of elements to cluster around a mean, or their asymptotic properties. The demographer would, of course, choose a principle to which the data seemed suited; and

the notational scheme is, of course, in his mind and not in those of the peoples studied. The situations in the data in which the principle is exhibited are organized according to the usual practices of the discipline: the settings in which the data are constituted, both in collection and writing-up, are no more apparent in demographic tables than they are for the situations described verbally in anthropological monographs. In both cases the reader must try to decide the general plausibility of the relations for himself. In our analogy then, each demographic table is the equivalent of each situation for which a dual relation is listed in a table of dual classification. A demographic table is, after all, a collective representation. And, as in the case of the table of oppositions, there is no necessary connection between the contexts or the symbolizations represented by a table; similarly, a table is subject to analysis according to many principles, without specifying their relation.

In sum, the careful limitations Needham places upon the analysis of lateral symbolism, particularly the way relations are shown to operate in the data, and the extreme generality and yet isolated specificity of these relations, are very much in the character of ordinary demographic analysis. We would expect this to be the case insofar as both utilize a mechanical model consisting of a few descriptive injunctions which do seem appropriate to the data; and both models convey the impression of producing relations which go past the data and have some hold upon actual situations. The methods differ only in that, once the model is in place, a demographer will confirm its assumptions statistically; the additional difference, that demographers tend to draw their prescriptions in line with what they regard as infra-structure rather than with the structure of native representation, is simply an academic convention and is not a necessary or essential characteristic of this kind of analysis. However, we can now readily understand why the demographer is inclined to do this: insofar as formal anthropological analyses are subject to the same restrictions with regard to preference as demographic analyses, there is no readily available structure to native representations for the demographer to accommodate his analyses to. It is not so much that demographers fail to attend to differences in classification, as that the information and analyses of those who specialize in such classification have never been suited to the assessment of changes in population structure.

It is perhaps not surprising that Needham concludes his paper with the nagging question of the validity of such analyses:

...it is still an unavoidable concern to ask how, or in what degree, oppositional analysis can ever be said to be right."

This kind of problem is a long-standing one for demographic analyses. Indeed, demographers are continually reminded of the consequences of the removal of information in space and time from ongoing social settings, for they are concerned to project future population structures, and thus are regularly confronted with the possibility of factual refutation. The kind of formal analysis we are describing renders the multiplicity of native preferences according to prescriptive principles and academic conventions agreed upon beforehand; it does not attend to the flow of preferences, and it cannot connect the particular principles it identifies in the data with the wider range of conventions that may be current in a society. The example of demography shows that the problems faced by this method are of two kinds: a tendency to state the obvious, in part because all statements repeat the initial assumptions; and, a tendency for results to be wrong because they assume an absence of change. It is well known, for example, that the most highly regarded actuaries of the 1920s and 1930s believed that western Europe and North America faced a dire threat of depopulation; or, that the extraordinary decline in American fertility which began in the 1960s, due in large part to changing opinions about contraception, was not anticipated. The Knowledge, Attitudes, and Practices surveys of the 1960s,

which were intended to provide information on the realities of contraceptive use, but were conceived with little or no attention to native representation, indicated, for example, that people will say they do not want too many children.

This last example reminds us that anthropologists, insofar as they are able to make direct and sometimes remarkably continuous contact with native representations may not be as susceptible to a reading-in of their own cultural posits as are demographers. Indeed, the potential contribution of anthropology to population studies has always been for this reason tremendous. Anthropological study, nonetheless, is particularly vulnerable to changes in time owing to the short duration of field studies. Formal analysis accentuates this, and it is remarkable that the effects of the removal of events as data from time are not considered in Needham's introduction. In the case of the Nyoro, there is, as he notes, a century of published ethnography in several languages and of varying quality; there is no comment on the effects of this upon analysis, nor on how the effects might be accounted for. In his Nyoro article the various references are cited one after the other, as if the information of the periods was equivalent. Thus, in a consideration of colour symbolism¹² the sequence of published examples runs: 1964; 1922, 1911, 1953; 1879; 1938; 1911; 1920; 1960; 1911; 1867; 1911; 1895; 1911; 1922; 1920; 1911; 1938; 1867; 1893, and so on. The ethnography is variously English, French and German. Even though the 'colours' under examination are white, black and, in passing, red, which seem to have some general significance,¹³ it is presumptuous to assume that their ranges would remain identical for a century in four languages. At least the terminological approach could be brought to bear here.¹⁴

The definition of the Nyoro in space is also not considered. Although I do not have an extensive command of the literature, this may very well be because the written ethnography does not include an account of how the Nyoro define themselves, particularly with reference to neighbouring groups and dialects. There is also some variation in the locations to which the existing accounts refer. But again, as long as the point of analysis is to show the presence of certain general principles, and to illustrate their operation, the definition of the social units may be assumed and moreover, assumed to have no effect upon analysis. The situation is much the same for the demographer, who chooses the social units under consideration to suit his own convenience. It is as if the Nyoro exist in a pure space, much in the way they exist outside of time.

Thus, although anthropologists are not in the habit of trying to make practical use of their limited methods in the way demographers, rightly or wrongly, have, they thereby miss a certain critical edge which gives demographers a good idea of the applicability of their methods. We may surmise that, given the similarities in the situation of anthropology and demography and, indeed, in all of the social studies, anthropological analyses would be subject to a similar fate.

Needham notes three other paths to confirmation which are closed to formal analysis. There is no final recourse to the traditional concepts as expressed by participants; nor is the very general incidence of relations such as dualism, and the comparison this facilitates, a basis upon which formal expressions may be completely justified. Quite so, Finally, confirmation is precluded even if a particular formal analysis, based upon written materials collated by an author otherwise unfamiliar with the society, is reviewed affirmatively by the ethnographers of that society. Once such an analysis is a part of the written record it may set the terms in which the society is viewed, and thus influences whatever criticism it may receive; thus a negative review does not erase either the influence or the possible validity of such an analysis. There is not only no confirmation, there is no refutation.

This last argument acknowledges that the criteria of the validity are set by academic discourse; as academic discourse cannot be a subject of analysis without further recourse to itself, the problem of validity is intractable. This is not a very interesting situation, and it can be said to follow from the initial mistaken belief that the course of analyses or the course of events in society can and should be separated from the analysis of that society. We are in the habit of considering formal schemes as if they were wholly alien to language, while insisting at the same time that they are in important senses dependent upon language. And there is now a dangerous possibility that this specious separation will be extended to semiotics generally.

The separation of formality and language, of theory and observation, of observer and participant are all of the legacy of the separation of puted objective and subjective realities. While compelling and necessary to an idea of theory which involves a separation of levels of discourse, these distinctions are widely recognized as incomplete descriptions of analysis. Specifically, they exclude the possibility of understanding and following the influences of the analyst. This fixation is unintentionally extended into anthropology by the break which is posited between the mechanical, the prescriptive, the paradigmatic, and the myriad, the preferential, the syntagmatic. There is no rendering the preferential for itself, precisely because it is an ideal, created by the success, one might say by the naturally imperialistic tendency, of the paradigmatic tendency of thought.

Plainly this tendency will participate in any attempt we make to conceive of the flow of events. We need not fear, then, that we will lose hold of this faculty if we return, for example, to the position of Levi-Strauss, and say that all prescriptions are really preferences. We know they are of a special kind, but that is not all that interests us here. Equally we may return from the view that formal theory is essentially reductionist; there is no doubting that its use has been; but the applied use of formal ideas - here I have to bracket aside pure mathematics - is always embedded in linguistic practice, not to exclude semiotics generally.

The interest of semiotics is that at least it gives us a way of talking about non-linguistic and para-linguistic phenomena. Ardener's papers¹⁵ show that the advantages of the distinction between prescription and preference may be subsumed in the Saussurian paradigm; and this gives us some idea of the way in which the congeries of events, such as in any 'play' of fertility, are determined. It does, however, leave the question of movement 'outstanding'. And while some place for formality has always been secure in the Saussurian tradition, the question of the manner in which formal methods are to be explored seems completely open.

The idea of semiotics originates, it could be said, in the hopeful anticipation that those aspects of experience for which linguistic description is inadequate may nonetheless be said to be 'related' or 'integrated' or 'systematic' or in some sense orderly. Semiotics are not completely articulated or articulable in language, and there is no reason to expect them to be. This poses the interesting possibility that insofar as these ranges of 'meaning' cannot be expressed in language without fundamentally changing them, anthropologists may need to develop other-than-linguistic modes for their interpretation. This is not to revert to some argument that, for example, to understand mimes and clowning anthropologists will have to become clowns - though that argument is not so silly. Rather, the theoretical rendering of semiotics cannot be entirely in language, though language inevitably participates; and it seems, through sheer want of other possibilities, that we are thrown back upon formal methods.

This is not to suggest that events that defy linguistic description are any more susceptible to, say, mathematical expression. There would be

little advantage to substituting the hyperstatis introduced by one for the other. The same is true for any idealized consideration of non-linguistic expression in itself, whether a notation is suggested for it or not. There is a possible danger of the assimilation of non-linguistic forms to linguistic ones, as an effect of semiotics. We may, to begin with, place inverted commas around statements that refer to say ritual or musical 'signs', 'languages', 'grammars', 'events' and so on: but we can expect these to fall away on all sides in a short time. It can only be hoped that the simply unsatisfactory quality of linguistic expressions of the non-linguistic will on the whole mitigate this. There is already a tendency to delegate the questions of use such as asked in this paper to rather unoccupied and, as we have shown, unoccupiable spaces. It seems important to insist upon the obvious fact that these ranges of experience to which semiotics are supposed to refer are not pure but composite: they are tangled mixtures of language (i.e. speech, writing, reading etc.), physical movement, machines and artifacts, of unstated and unstateable definitions of state.

If this is the case, then we are more or less in the position of the particle physicist: even if we can develop a formal notation to express events fundamentally different in kind to those of the language of classical mechanics i.e. ordinary language, we are still left the problem of needing some at least partial translation of these entities into language.¹⁶

This puts anthropologists in a fine quandary. They have for some years been aware that formal methods cannot hope and do not try to account for the subtlety and nuance of the images surrounding situations such as those of fertility. Anthropology, of all the sciences, has retained a hold on the fact that explanation is in language; formal analyses are satisfactory to the extent that they can be translated, for it is by their effects upon ordinary description that we usually judge their plausibility. Now there is this reminder of what was known all along: much (how much?) of what is experienced in thinking, believing, feeling, expecting and so on seems to resist depiction in language. Care and attention to language, essential as it is, is not merely futile but misleading insofar as it expects to be complete. The very questions anthropologists seek to answer, which concern the envelope of representation and physical action in which events are experienced, seem to fall very much at the edge of what can be said. Anthropology appears stuck between conceptual analysis it knows to be partially inappropriate and formal analysis in which it has no confidence.

We can already see that the walls of this predicament are paper-thin. Our tendency to speak of language as separate, as if linguistic expression were privileged and isolable, is really quite abstract and ideal. The metaphor of 'grammar' was apt because it helped to explore behaviour as if it were 'ruled'; we had the habit of speaking that way any way, even if it was not always grammar we had in mind and the social facts expressed in language seemed peculiarly accessible, at least when compared, e.g. to the expression of power by a charismatic leader or a dancer. A subtle change in our use of language is introduced as the more schematic and less immediately apposite aspects of the metaphor become acceptable: thus we have become accustomed to speak of 'behaviour' and even 'ruled behaviour'; we are not likely to be aware that we are wedding what we think to be a grammatical analogy to one that was chemical.¹⁷ Formal schematizations are no different, even if more indecently exposed; still, that American families average 2.53 children scarcely raises an eyebrow. In both cases, however, there are operations in the background of the analogy whose schematic import is not so plausible: the linear form of speech and writing is hardly suited as a model for events which occur simultaneously in several dimensions; and no one would expect American birth rates to be fixed at their present level for the next 50 years. This description will not be too far from the facts: specialists accept schematizations as limiting cases, but as they further develop the analogies, their use, for several reasons, becomes more lax; usage passes into a wider

public, and the specialist will only rarely deny the acclaim for his image and likeness. What is true with the elaborate metaphor of a 'grammar' is true for analogies or schematizations generally, whether formal or informal: they are imperialistic. They are capable of replacing and reducing other classifications in language, as well as those manners of expression which do not fit into language. The elements displaced and the early stages of displacement may be erased; and the implications of the analogy are inevitably traced partially.

It would seem more fruitful to examine formal methods in the context of their use, that is, in the midst of linguistic analogies and institutional incentives, rather than to consider them only for their alienating effects in particular analyses. If semiotics are composite, we can expect the effects of language and formal methods in composing some aspect of the unexpressed much in the way the physicist uses mathematics to circumscribe sub-atomic phenomena. There would be somewhat less of a problem of assimilating these experiences to language given the less familiar and even peculiar sense of mathematical expression. And to understand such a rendition of events would require, as in the reading of most any mathematical text, a careful, step by step working-through of the relations. That is, it involves a reconstruction of the relations by the reader, which no doubt would raise many of the options and preferences taken up or set aside by the analyst in his own particular presentation. The greater emphasis this would place on reading would be welcome, and could turn it more into a simulation. Such a reading could only be a part of method, and it is to these questions of the relation of formal schemes to semiotics generally that we should now turn our attention.

Phil Kreager

Notes.

1. Lorimer, F. (ed.) Culture and Human Fertility 1954; Nag, M. Factors Affecting Human Fertility in Non-Industrial Societies: a cross cultural study 1962. The first of these includes writings by Fortes and others which attempt to relate ideas about the function of lineages and marriage practices such as polygamy to the categories of demography. All of the subsequent criticisms by Leach (Rethinking Anthropology 1961) regarding the irrelevance of matrilineal classes of societies and of universal definitions of marriage apply to these papers. Some problems of reconciling native expression with the narrow demographic view of fertility are described by Ardener, E. Divorce and Fertility, 1962. Nag's study was based upon the Human Relations Area Files; again the basic criticism of this kind of study is by Leach. The criticisms which might be levelled against yet another try with these materials would no doubt include the following: 1. the extreme variance in the reliability of these accounts, and of their location and the location of the societies to which they refer in history, precludes any point of reference for such a study other than some association with mankind in general; 2. the interest of this kind of general attribute of human expression for the understanding of particular situations would seem confined to some distant future, that is, to a time in which there will be more useful accounts directed to the specific purposes; 3. such an accounting, for its attention to representations, is very suited to the aspect of assessment, but as it considers process to be an inaccessible, internal aspect of individual mental operations, it removes essential aspects of fertility we wish to consider here. (cf. Needham, R. Belief, Language and Experience 1972: 156-9)
2. Needham, R. 'Introduction', Rethinking Kinship and Marriage 1971: lxviii.
3. Levi-Strauss, C. 'Preface to the Second Edition', The Elementary Structures of Kinship 1969: xxxi-xxxii.
4. Ardener, E. 'The New Anthropology and its Critics' Man 6:3 (1971): 450-1.
5. The misunderstandings of the various critics of his early papers on the Purum attempt to assimilate his use of formal ideas to more typical bases such as ideal types or statistical frequency; alternately, because these bases are what formal analysis is believed to imply, some critics seem to assume that if he is using formal methods at all he must be 'grossly manipulating' native representations. The main citations can be found in Needham, R. op. cit. 1971: lxvi-lxxxii. A similar argument is made by Beattie in his criticism of Needham's interpretation of his and related ethnography on the Nyoro ('Aspects of Nyoro Symbolism' Africa 38 (1968): 413-42). Unfortunately, because Beattie evidently misunderstood the lack of necessity implied by the dual preferences listed by Needham we still lack a critical assessment of this way of handling ethnography as data by an ethnographer of the people concerned. The assumptions of method, however, were not stated until 1973 ('Introduction', Right and Left). Indeed, the materials on lateral symbolism have thus far not received critical attention. Korn and Needham (Man 5 (1970): 393-420) critique recent mathematical analyses of kinship with specific reference to their inability to accord to native classification, particularly regarding prescription. This paper develops the opposition between the application of formal systems developed outside of anthropology to anthropological materials, and the use of a few particular formal ideas in the midst of analysis in which attention to representations has already begun. Some of the possible consequences of anthropological use of other

systems, with glancing reference to economics, are contained in his 'The Future of Anthropology: Disintegration or Metamorphosis?' in Anniversary Contributions to Anthropology 1970.

6. Leach, E. 'Magical Hair' JRAI LXXXVIII (1958):147-64.
7. Needham, R. op. cit. 1972:209.
8. Needham, R. op. cit. 1972:222.
9. Levi-Strauss, C. 'Social Structure', in Structural Anthropology 1963:283-9.
10. Needham, R. op. cit. 1973. I am concerned particularly with this most recent assessment; however, an example has also been drawn from his 'Right and Left in Nyoro Symbolic Classification', originally published in Africa 37 (1967): 425-51, but reprinted in Right and Left 1973: 299-341.
11. Needham, R. op.cit. 1973:xxxi.
12. Needham, R. op. cit. 1973:310-312.
13. Berlin, B. and Kay, P. Basic Color Terms, their Universality and Evolution 1969.
14. This is not to return to the mistaken attempt to make formal analyses of preferences refer to the character of events in a society as a whole. It is important, however, to realize the implications of the limits of this method which Needham so carefully articulates. Anthropologists appear to be merely reinventing the logic of demographic methods, without at least the benefits of validation which give such methods some value. If we are interested in the question of the rightness of such analyses, then it is necessary to consider the nature of their reference; insofar as such an analysis cannot be said to refer to anything, except a collection of isolated instances, we may wonder about the significance of 'Nyoro'. One may wonder, given the seeming shift from formal to conceptual analyses, why Needham did not take the opportunity of the republication of the papers on left and right to comment upon the nature of opposition which separates in the same way pairs such as these: normal, esteemed/hated; wealth/poverty; joy/sorrow; good/evil; good omen/bad omen; etc. (1973:328). Surely the conceptual ranges of each of these are very different; one wonders whether these brisk, formal glosses on the isolated situations he considers would stand up to the strictures of Belief, Language and Experience.
15. op. cit. 1971; also, 'Some Outstanding Problems in the Analysis of Events', 1973.
16. There is, of course, the additional option of not insisting upon some separation of levels of analysis and observation; this, as anthropologists have long been aware, is tantamount to doing something other than 'science'.
17. Ardener, E. "Behaviour": A Social Anthropological Criticism. JASO IV:3:152-4.