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Ethnographic Representation, Statistics and Modern Power

BY TALAL ASAD

MUCH HAS BEEN WRITTEN recently on how the anthropologist's experience in the field comes to be inscribed as authoritative ethnography. Although in this effort the rhetorical structures of ethnographic representation have been usefully explored, my main interest in this essay is different. I ask how the problem of representation is addressed when the mode of inquiry is fieldwork and how this compares with representation in social statistics. I then reflect on some political implications of this contrast. My answer to the question is inevitably sketchy. It is a result of preliminary attempts to explore the idea of Western hegemony from an anthropological standpoint. I stress at the outset that I am not concerned with the merits and demerits of qualitative versus quantitative methods in reaching the truth about social reality. Rather, I want to examine the role statistical representation has played in creating the world of modern power that anthropologists inhabit.

I begin by stating baldly some of the contrasts I have in mind when I juxtapose ethnography and statistics. First, unlike the "real cultural wholes" of ethnography, the statistical universe, as well as the categories of which that universe is made up, are the products not of experience but of enumerative practices.

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Second, statistical universes can be expanded or contracted, segmented or merged, depending entirely on pragmatic rather than veridical considerations. Third, sampling techniques in statistical practice allow one to move from representing social types (which is a characteristic of ethnographic representation) towards representing the variational patterns of a population.

In making these contrasts, I am not saying that anthropologists never employ statistics. They do, and many anthropologists have long advocated that they should do so more systematically. Statistical inference in the service of evolutionary theory, as exemplified in the attempts by Tylor (1889) and Murdock (1949), may have virtually disappeared in social and cultural anthropology, but social surveys and other forms of quantification have not. Nearly sixty years ago, Malinowski regretted that he had not collected quantitative data more systematically: "Were I able to embark once more on fieldwork, I would certainly take much greater care to measure, weigh and count everything that can be legitimately measured, weighed and counted" (Malinowski, 1935, Vol. II, p. 459). Many anthropologists since then have proposed that statistics are indispensable in determining social norms. Clyde Mitchell, for example, suggested that statistical methods should be seen as a complement to qualitative information collected on the basis of field experience:

> Quantitative methods are essentially aids to description. They help to bring out in detail the regularities in the data the fieldworker has collected. Means, ratios, and percentages are ways of summarizing the features and relationships in data. Statistical measures based on the theory of probability go beyond the mere quantitative data and use devices to bring out the association between the various social facts the observer has collected[1] (Mitchell, 1967, p. 20).

But, in general, statistical reasoning has remained relatively marginal to the discipline.

I stress that my purpose here is not to press for a greater use of quantification in anthropology. Still less is it my intention to

attack the value of interpretive understanding (although I think it is necessary to remind some anthropologists that such understanding is not inextricably linked to ethnographic fieldwork). My aim is to explore the idea that ethnographic fieldwork characteristically invokes a conception of knowledge modeled on subjective vision but that statistics does not. (I use the term "statistics"—following historians of the subject—to refer both to social surveys and to probability theory because the two are connected.[2]) I argue that statistical concepts and practices are essential to the systematic manipulation of complex social formations and time series.

Doing ethnography has not always been a central concern of anthropologists. In the nineteenth century, anthropologists addressed themselves to the history of institutions and ideas without themselves ever doing ethnography. Even in the twentieth century some anthropologists have attempted to compare structures of behavior or systems of belief—but usually only after having established their credentials by doing original ethnography.

The definition of anthropology in terms of ethnographic fieldwork has had two interesting consequences which have often been noted: (1) A heavy emphasis on the present—and on the past as a symbolic construction in the present; and (2) A preoccupation with local conditions as an experiential whole. In considering what is relevant, the fieldworker is encouraged not only to direct her attention at small-scale events and structures, but also to identify events that are *typical* within the field under investigation. This last point is especially important for my theme.

Personal field research—in which the anthropologist observes and participates in the activities of the people being studied[3]—was originally justified as the basic method in ethnography on the grounds that the societies anthropologists studied were "simple" and "small." But it was as a consequence of fieldwork that the anthropologist's object of study was

limited in particular ways. In other words, many spatio-temporal complexities and variations were excluded from the object of study because they were not directly observable in the field. For example, the systematic force of European economic, military, and ideological powers in non-European regions was, and still is, often conceptualized as being external to locally observable discourse and behavior or as being an abstract system having little to do with the belief and conduct of people "on the ground."[4]

Anthropological investigation into the contrasts between historically diverse institutional practices and modes of reasoning also tends to get marginalized. In the late fifties, Franz Steiner complained that comparative work in social anthropology had all but disappeared, owing partly to the emphasis on intensive fieldwork (given the time and energy that that demanded) and partly to the functionalist doctrine of social integration (which discouraged the separation of beliefs and practices from their "full" context).[5] Since Steiner spoke, the great wave of Levi-Straussian structuralism has come and gone, and one consequence is that comparative work is not quite as exceptional as it once was. And partly due to the growth of Marxist anthropology, studies based on historical texts have become more common.[6]

Anthropological research, novices are told, is based on fieldwork, which means that ethnographers must live with the people being studied. Of course, while they are in the field, ethnographers observe, ask questions, conduct surveys, and read local documents. It would be a mistake to suppose that anthropologists depend on a single method. However, the primary foundation of anthropological research as a distinctive form of enquiry is *a particular kind of experience*. In the words of G. Condominas,

> the most important moment of our professional life remains fieldwork: at the same time our laboratory and our *rite de passage*, the field transforms each of us into true anthropologists.

> Of course, before undertaking one's fieldwork, one needs a solid intellectual background, but intelligence and training alone are insufficient. A minimum of human warmth and a certain openness are necessary in order to establish contact with others, and to maintain it (Condominas, 1973, p. 2).

E. Evans-Pritchard put the matter of ethnographic experience thus: "What comes out of a study of a primitive people derives not merely from intellectual impressions of native life but from its impact on the entire personality, on the observer as a total human being. . . . The work of the anthropologist is not photographic. He has to decide what is significant in what he observes and by his subsequent relation of his experiences to bring what is significant into relief" (Evans-Pritchard, 1951, p. 82).

How did experience come to be so central to the definition of anthropology as an academic discipline?

In 1951, Evans-Pritchard recounted a neat little story which is probably still accepted in its essentials by most anthropologists today.[7] I reproduce it here not to invoke its authority but to examine its reasoning. He writes:

> Between the heyday of the moral philosophers and the earliest anthropological writings in the strict sense, between, that is, the middle of the eighteenth century and the middle of the nineteenth century, knowledge of primitive peoples and of the peoples of the Far East was generally increased. European colonization of America had been widely expanded, British rule had been established in India, and Australia, New Zealand, and South Africa had been settled by European emigrants. The character of ethnographic description of the peoples of these regions began to change from travellers' tales to detailed studies by missionaries and administrators who not only had better opportunities to observe, but were also men of greater culture than the gentlemen of fortune of earlier times (Evans-Pritchard (1951, p. 67).

The accumulation of this ethnographic information based on extended stays by sophisticated missionaries and colonial administrators allowed for speculation and new hypotheses by men such as Morgan, McLennan, and Tylor, who devoted themselves to the study of "primitive societies." However,

Evans-Pritchard tells us that "it became apparent that if the study of social anthropology was to advance, anthropologists would have to make their own observations" (Evans-Pritchard, 1951, p. 71). How did this become apparent?

In the nineteenth century, Evans-Pritchard notes, anthropologists were generally lawyers, biblical scholars, or classicists, and, consequently, they were accustomed to dealing with texts that had been composed by someone else. But the next generation was recruited largely from the natural sciences. Thus, Boas was a physicist and geographer, Haddon a marine zoologist, Rivers a physiologist, Seligman a pathologist, Elliot Smith an anatomist, Balfour a zoologist, Malinowski a physicist, and Radcliffe-Brown an experimental psychologist. "These men," Evans-Pritchard explains, "had been taught that in science one tests hypotheses by one's own observations. One does not rely on laymen to do it for one" (Evans-Pritchard, 1951, p. 72). The significance of experience, at this point in the story, lies in its being close to the idea of laboratory experimentation.[8] That is to say, the experience that is invoked here is conceptual (involving classifications, hypotheses, explanations) and active (involving the systematic manipulation of data and manufacture of events). It has nothing to do with empathy.

So it was—the story goes—that anthropological fieldwork came into being, and anthropological analysis and explanation were thus provided with a sounder basis than they could have had in the nineteenth century. However, we are given to understand that although the scientific attitude to research was introduced into anthropology by trained natural scientists, its full benefits were not obtained until much later, because most of the early scientific fieldworkers were largely ignorant of the languages of the peoples they studied and did not stay long enough among them. This deficiency, Evans-Pritchard relates, was eventually put right by the pioneers of anthropological fieldwork, Boas and Malinowski.

"We have now," concludes Evans-Pritchard, "reached the

final, and natural, stage of development, in which observations and the evaluation of them are made by the same person and the scholar is brought into direct contact with the subject of his study. Formerly the anthropologist, like the historian, regarded documents as the raw material of his study. *Now the raw material was social life itself*" (Evans-Pritchard, 1951, p. 74; emphasis added). Evans-Pritchard stresses that this raw material can be collected only through an appropriate command of the relevant language and long periods of stay in the society studied, so that the ethnographer "lives, thinks, and feels in their culture" (p. 79). Here the anthropologist's experience has become something he undergoes; it is by exposing himself to it that the ethnographer can spontaneously reproduce the thoughts and feelings of his subjects, understanding and interpreting things as they do.

According to this conception of fieldwork, direct access to "social life itself" does not preclude interpretation and analysis—hence the phrase "raw material." But it does presuppose the representation of social life as a *real, experiential whole*, which is one reason why anthropologists even today accept that fieldwork calls for extended periods of residence and sound knowledge of the language. "It is impossible," says Evans-Pritchard, "to understand clearly and comprehensively any part of a people's social life except in the full context of their social life as a whole" (Evans-Pritchard, 1951, p. 80). In other words, social life as a whole is not only real and representable, it is representable *because* it is accessible as a totality to the ethnographer's living experience.

Evans-Pritchard's notion of social life itself as raw material is an interesting fusion of two quite different metaphors. On the one hand, it suggests that the anthropologist's account is taken from the experience of social life; so the emphasis is on the contrast between experience and its representation. On the other hand, it refers to the process by which experience is transformed from one form (raw data) into another (finished text). Here, experience is thought of as itself capable of

modification and directly open to public inspection.[9] It is the former, however, that seems to give the dominant sense to the idea of "fieldwork." In this sense, "social life itself" is experienced and represented through the device of *typification*. The "typical" is what the investigator comes to recognize in the field and writes about in the ethnography. But to the extent that her personal experience is offered as the foundation for that knowledge, others with a different experience may reasonably receive it with skepticism. This fact becomes especially critical when, as in our day, ethnography is consciously planted in the domain of identity politics. It is then that the experiential basis of ethnographic representation is directly challenged—not by analyzing the latter but by confronting one experience with another.

In her essay on anthropological fieldwork published in 1939, A. Richards explained and justified ethnographic method in what had become a standard formula:

> The student of primitive societies enjoys certain advantages in observation. The communities he observes are for the most part so small that they can be investigated as functioning wholes, and not merely as subdivisions of a larger society. He is therefore able to collect data to show the working of all the fundamental institutions of a particular tribe, and is not limited, as in the case of a complex civilization, to a study of one particular aspect, such as the economic or the educational (Richards, 1939, p. 293).

The object of study as a functioning whole is thus defined in terms of the activities and relationships that are seen and heard by the anthropologist in the field. More precisely, that object is constituted by what her personal experience yields. But equally important, the ethnographer of a small community (or locality) generalizes on the basis of case studies in which she has learnt to identify *social types*. When she writes up her ethnography, these social types often appear as *representative samples* of the community.

Richards' earlier article on the village census dealt with the use of "case-histories" for comparing social types classified

according to generation and area. In it, she explained that the information she had collected about a large number of individuals could be tabulated to yield systematic information about historical change and local differences. Richards declared:

> It will be clear that any set of village case-histories can be used in two ways—to give information about individual types, if the entries are read horizontally; and to throw light on the structure of the group by comparing the entries in any one column vertically. For instance, the marriage and divorce rates of one village could be compared with those of another in a different part of the area, or the customs of two different generations in a village could be compared by examining the histories of the old men with those of the middle-aged men and boys (Richards, 1935, p. 28).

Richards thus advocates that anthropologists carry out social surveys of the locality being studied, but the purpose of the survey seems to be to construct *social types*.

Although Richards' method of deploying case histories may not be very common among ethnographers today, the recording of case studies is still regarded as central to anthropological fieldwork. In his Introduction to *The Interpretation of Cultures*, Geertz explains the value of case studies for ethnography as follows:

> the essential task of theory building . . . is not to codify abstract regularities but to make thick description possible, not to generalize across cases but to generalize within them.
>
> To generalize within cases is usually called, at least in medicine and depth psychology, clinical inference. . . . In the study of culture the signifiers are not symptoms or clusters of symptoms, but symbolic acts or clusters of symbolic acts, and the aim is not therapy but the analysis of social discourse. But the way in which theory is used—to ferret out the unapparent import of things—is the same (Geertz, 1973, p. 26).

The drawing of parallels between ethnography and psychoanalysis is a favorite theme among anthropologists.[10] Whatever

we may think of Geertz's resort to that parallel here, there is, it will be noted, an implicit acknowledgement that "generalization within cases" depends on and helps to construct *social types*—medical, psychiatric, or ethnographic. The recording of case histories can merge into the reportage of life histories. But whether it is constructed through "case history" or through "life history," the *type* stands for a universe of *the same*.

In ethnographic sociology—as distinct from social anthropology—there is a tendency to construct representative types directly in the form of individualized *characters*, thus, self-consciously writing ethnography as a form of literature.

> The 'heroes' of life-histories and ethnographic monographs," Atkinson claims of this genre, "provide the reader with two parallel sets of potentially satisfying experiences. On the one hand they furnish the sense of intimate acquaintance with characters that most readers themselves would not encounter at first hand in their everyday lives. On the other hand, these characters illuminate a range of settings in which, again, the respectable reader may have no direct involvement. The individual character may thus embody opportunities for social exploration and discovery . . .: [Thus] Matza's deviant, Paul Cressey's taxi-dancer, Donald Cressey's embezzler, Sutherland's professional thief, Marvin Scott's jockey and Becker's marihuana user are assembled and then set free to bring back intelligence about the nature of social life. . . . The detailed portrayal of individuals—usually through a mixture of their own words and the observations of the ethnographer—thus helps to establish the warrant for credibility and authority in the text (Atkinson, 1990, p. 133).

According to Atkinson, character is richer, more "real" than type; its depiction in ethnography is not only based on direct experience but can (re)create that experience for the reader.

Etymologically, *character*—like *type*—is an inscription ("in Arabic characters," "in bold-face type") which is made to be read. As such, character and type are visible differences that are infinitely reproducible and also part of a set by means of which a continuous reading and writing is made possible. A creative selection of readable signs, in which a continuous

exchange takes place between reading and being read, character is often taken to represent both itself and the essence expressed by it. It is at once Atkinson's "sense of intimate [personal] acquaintance" and the "range of [social] settings" illuminated by that acquaintance; both Geertz's visible "symbolic acts" and their "unapparent import." For Geertz, in particular, character expresses at once the individual human being and the individualized collectivity to which he belongs.[11] Of course, the concept of character (or type) is necessary to an understanding of social life. But it becomes problematic when it is seen as expressing an irreducible essence that is apprehended by experience.

In the empiricist tradition, the ethnographer may encounter "characters" in the field but she typifies them by way of a system in her ethnography. They become brother and sister, husband and wife, affines, clan members, and so on with specifiable rights and obligations. I quote from Richards yet again:

> The individual characters, with all their temperamental and physical peculiarities and the dramatic incidents of everyday life, seem to stand out in bold relief, while the formal patterns of kinship, which we have just described, fade from view. We are watching a number of people who like or hate to share their food, or to prepare it in common, and not plotting a system of relationships on a kinship chart. But this is, of course, how the scene appears in the context of everyday life (Richards, 1939, p. 160).

Here typification consists not in an inevitable simplification of experiential complexity but in the imposition by the anthropologist of an idealized cultural system on remembered and recorded data.

It should be stressed that in social anthropology the emphasis on observation in the field did not necessarily commit the ethnographer to a naive empiricism. Certainly, despite Radcliffe-Brown's methodological pronouncements, many of his followers insisted that the local social structures

which they attempted to study and describe were not accessible to direct observation. As Fortes put it in an essay published in 1949, "Structure is not immediately visible in 'concrete reality'. . . . When we describe structure we are already dealing with general principles far removed from the complicated skein of behaviour, feelings, beliefs, &c. that constitute the tissue of *actual social life*. We are, as it were, in the realm of grammar and syntax, not of the spoken word" (Fortes, 1949, p. 56). According to Fortes, the ethnographer's consciousness of "actual social life" needs to be distanced, sifted, and analyzed, a process that reveals local appearances to be the varying expressions of an enduring essence. He, therefore, regarded the problem of ethnographic representation as the explanation of social types (that is, recurring *events*) in terms of the statistical variants of a single underlying form—the cultural whole.[12] Five years after Fortes's essay, Leach developed a more complex version of this argument in non-quantitative terms.[13] Yet in spite of their sophisticated approach to ethnographic representation, anthropologists like Fortes and Leach retain the empiricist distinction between "observation" and "theorization" as two linked but separate moments in the ethnographic enterprise. There is supposed to be actual social life one observes in the field, and then there is theory one is expected to apply when one returns home with the data. And yet if pressed, such anthropologists would concede that "observation" is informed by theoretical concepts (of which typification is one) and that "theorization" is scarcely more than the organization of observational (*and* textual) "data" in ethnographic form.

There is an awareness of this complexity in a recent collection of essays by John and Jean Comaroff (1992). For although they begin by reaffirming the centrality of "the observer's eye" in the ethnographic enterprise, they go on immediately to register the urgent "need of a methodological apparatus to extend its range" (p. x). The extension they advocate is clearly metaphoric:

> Indeed, we would argue that no humanist account of the past or present can (or does) go very far without the kind of understanding that the ethnographic gaze presupposes. To the extent that historiography is concerned with the recovery of meaningful worlds, with the interplay of the collective and the subjective, it cannot but rely on the tools of the ethnographer (p. xi).

In this conception, it is not "the observer's eye" that is actually extended in range (as in "observing" with interference microscopes and radar telescopes)[14] but something within the subject—"the mind's eye"—that simulates its function. "The eye," now transposed onto an imaginary plane, is able to inhabit freely the categories of time and space (like any good story teller and listener). In other words, the "ethnographic gaze" is taken to be the source of a knowledge because it is rooted in the researcher's ability to observe, then to imagine a meaningful world around what is witnessed, and finally to present a verbal image[15] *corresponding* to that partly-imagined, partly-witnessed world. Existing texts are admitted to be important for the ethnographic researcher, but they play a supplementary role; it is the directly visible and locatable field that remains the privileged foundation. However, in the Comaroffs' presentation, the precise connection of that empiricist foundation to the extended world of the ethnographer's imagination is obscure because the historian—who can have no such privileged foundation—is also said to depend on "the ethnographic gaze." Yet the historian's "field" is not, like the ethnographer's, a visible ground on which people live but a conceptual space within which she interacts with texts. This obscurity may be resolved if by "the ethnographic gaze" we take the Comaroffs to mean the construction of a discursive universe inhabited by representative types.

So the ethnographer's attempt to represent a real cultural universe is rooted, in the final analysis, in "the experience of fieldwork" and mediated by an imaginative act of consciousness. The ethnographer "knows" what he has experienced once he has learnt to interpret "a culture from within;" his task is to communicate that knowledge in textual form.[16] He may concede that

his success has been only partial, because the qualitative character of his personal experience is difficult to translate into public language. Yet that concession does not disprivilege his experience. Thus, the gap between "experience" and "representation" parallels the notorious gap between *incommensurable cultures*. In what follows, I want to take up some political implications of that double gap by contrasting statistical representation to what I have said about ethnographic representation.

In the interviews with Raymond Williams published under the title *Politics and Letters*, there is an arresting passage in which Williams attempts to reformulate his views on the question of experience. The interviewer urges him to reflect on the limited character of experience as a foundation of social knowledge. Williams agrees and responds as follows:

> It is very striking that the classic technique devised in response to the impossibility of understanding contemporary society from experience, the statistical mode of analysis, had its precise origins within the [1840s]. For without the combination of statistical theory, which in a sense was already mathematically present, and arrangements for collection of statistical data, symbolized by the foundation of the Manchester Statistical Society, the society that was emerging out of the industrial revolution was literally unknowable. . . . After the industrial revolution the possibility of understanding an experience in terms of the available articulation of concepts and language was qualitatively altered. There were many responses to that. New forms had to be devised to penetrate what was rightly perceived to be to a large extent obscure. Dickens is a wonderful example of this, because he is continually trying to find fictional forms for seeing what is not seeable. . . .
>
> From the industrial revolution onwards, qualitatively altering a permanent problem, there has developed a type of society which is less and less interpretable from experience—meaning by experience a lived contact with the available articulations, including their comparison. The result is that we have become increasingly conscious of the positive power of [statistical] techniques of analysis, which at their maximum are capable of interpreting, let us say, the movements of an integrated world

> economy, and of the negative qualities of a naive observation which can never gain knowledge of realities like these (Williams, 1982, pp. 170–71).

Williams's remarks in this passage parallel the distinction I indicated at the beginning of this essay when I referred to ethnographic and statistical modes of representation. His primary concern is to defend "experience" as a legitimate basis of knowledge against Althusserians who had sought to dismiss it. For Williams as a democratic socialist the experience of living in a particular society is a necessary basis of responsible politics. But it is only one basis, because an understanding of historical structures and movements that are inaccessible to experience is also essential to an informed politics. It is this political commitment that sets Williams's interest in "experience" apart from the ethnographer's dedication to fieldwork experience as a source of disinterested knowledge—and also (more latterly) from experimental ethnographers in the service of identity politics.

However, in an earlier work, Williams (1983) had made a summary distinction which seems partly to overlap the contrast he notes in the passage I have quoted: between "experience past" (in the sense of lessons learnt) and "experience present" (meaning a full and active awareness). Although it is apparently more carefully constructed, this distinction is conceptually and politically less clear than the one employed casually in the interview because the two kinds of "experience" are conceptualized at the same time partly as types of authorized knowledge-material ("personal"/"unsystematic" or "impersonal"/"systematic"), partly as knowledge-purviews (more inclusive or less so), and partly as knowledge-stances (reflective or unreflective). The *Keywords* distinction is also politically more troubling than the formulation in *Politics and Letters* because of its conclusion: "in the deepest sense of *experience*, all kinds of evidence and its consideration should be tried." This recommendation might be open to the charge that it is either vacuous (of course, one should search for all evidence *relevant* to the problem in hand) or a recipe for chaos (the obligation to try *everything* makes a sustained and responsible politics

highly problematic). However, one could defend Williams by reformulating this sense of "experience" as a continuously self-correcting process, guided by specific standards, through which a particular practical learning takes place. In such a process there is no epistemological foundation—neither "subjective" experience nor "objective" social life itself.[17] Both ethnographic and statistical representations are part of the material for learning and teaching certain kinds of practice, including the practices of a nonfoundational anthropology.

Something else must be said about Williams's comments. When Evans-Pritchard gave his account of the fieldwork revolution in anthropology, he spoke approvingly of the shift from reliance on reading reports (compiled by others with experience of the object of study) to reliance on personal experience of "social life itself." This view of experience makes two assumptions, both questionable: First, that publicly accessible writing does not connect directly with social life, but that the memory of personal encounters does do so; and, second, that inscription is always a representation (either true or false) of social life but can never constitute social life itself. Williams would almost certainly have disputed both these assumptions. It is, therefore, all the more surprising that he fails to make the point that since the nineteenth century, statistics has been not merely a mode of *representing* a new kind of social life but also of *constructing* it.

In the newly constructed formations of the nineteenth century, administrative techniques had to be devised that would deal effectively with highly differentiated and continuously changing classes of population. The way in which such populations constituted a social problem (poverty, disease, education, racial imbalance) was identified, represented, and addressed in statistical terms.[18] Statistics was ideally suited to modern administration. More precisely, social surveys and probability theory have together become integral to modern life, and increasingly to life in societies becoming modernized. I shall say something about the significance of statistics for

modernization in the non-European world after a brief discussion of how the problem of representation was dealt with in the development of statistical thinking from roughly the last two decades of the nineteenth century through the first four of the twentieth century.[19]

The problem of representation in the history of statistical theory concerned the question of how one could grasp a complex and changing "whole" from knowledge of a "part," and the answer to that question evolved out of changing political and conceptual practices in the nineteenth and twentieth centuries.

There were, in effect, two methods of generalization depending on whether the part was selected purposively or randomly. The first, associated successively with Le Play and his followers (1830–1900) and with Halbwachs (1900–1940) is similar to the kind that ethnographers offer on the basis of fieldwork experience. What is common to all these inquiries, otherwise so different, is that informants were deliberately chosen from among people known to the investigator, informants who were considered to be in a crucial sense at once trustworthy and representative. This required the classification of actors as "typical" on the basis of intuition and experience. Although ideas of probability were not entirely absent in the "typification" approach, they depended on the concept of the average, a concept that subsumed dispersion and variability in the "typical" figure who stood for the social whole.

These earlier studies were directed at particular sections of society who were the objects of reforming zeal: the working classes and the out-of-work in particular cities or trades (factory-hands, prostitutes, immigrants), that is to say, all those whose conditions of life were the source of moral and material danger to themselves and to others.

It was clearly recognized that older stabilities were being radically undermined by political and economic developments,

and that this was leading to serious social problems. But in order to understand and defend these older arrangements, LePlay and his school argued, investigators would need to spend extended periods of time with their objects of study. For only through such intimate contact could the investigator hope to observe and understand social activities. Desrosieres notes:

> That this observation should take account of the entire significance of actions which the investigator could not isolate and code in *a priori* terms, is found in other modes of knowledge-creation which developed subsequently, and which embody other ways of generalizing than that of representative sample: ethnological descriptions of non-Western societies on the basis of long and patient periods of field work in the community by the investigator, psychoanalysis building a model of the structure of the unconscious on the basis of completely individual data, gathered in course of personal exchanges of very long duration (Desrosieres, 1991, p. 223).

Thus, already in the nineteenth century we have in LePlay an emphasis on the importance not of the social survey but of participant observation as a precondition for representing significant social types. Trained as a mineralogist before he became a social researcher and reformer, he conceived of the study of society as a kind of mineralogy, a science which depended on the collection and arrangement of *specimens* that represented a total system of classification.[20]

Although Halbwachs' study of working-class budgets is explicitly concerned with variations (indeed, he is critical of Le Play for not taking these into account), the variations are, nevertheless, attributed to macrosocial causes, like those identified by Durkheim in *Suicide*, and thus subordinated to unvarying essences. I quote again from Desrosieres:

> The objective of [Halbwach's study], however, is quite as much to identify the traits of a common 'working class consciousness', whose relatively homogeneous character stems not from a divine essence as with Quetelet (Halbwachs was a materialist) but from common material conditions of existence, which a quasi Darwinian adaptation leads to similar requirements both in

> practical life and in consciousness. It was because he was interested in this working-class consciousness that the problems of sampling did not present themselves in at all the same terms which they would do for those who, half a century later, would use such studies to promote national accounts (Desrosieres, 1991, p. 226).

Incidentally, Kruskal and Mosteller point out that Lenin was already aware of the different implications of statistical representation in 1899. Thus, in Chapter 2 of *The Development of Capitalism in Russia*, Lenin criticized statisticians of the rural administrative districts for relying on averages in their representation of the agricultural population instead of analyzing the variability of economic circumstances that was so basic to the Marxist understanding of peasant class relations (Kruskal and Mosteller, 1980, p. 174, n4). But then Lenin's primary problem in *The Development of Capitalism* was not that of Halbwachs'; his concern with measuring the distribution of discontinuous variables issued from the Marxist concept of class as a contradictory, historical phenomenon.

It was, ironically, not the work of socialists like Maurice Halbwachs who supported working-class movements, but the preoccupations of eugenicists like Francis Galton who advocated "race improvement" that enabled modern statistical theory to make a breakthrough.[21] This involved more than a mere awareness that averages could be misleading. Desrosieres describes this advance as follows: "by bringing attention to focus upon the *variability of individual cases*, with the notions of variance, of correlation, and of regression, the English eugenists moved statistics from the ground of the study of wholes summarized by a single average (holism) to analysis of the *distribution* of individual values *to be compared*" (Desrosieres, 1991, p. 235; emphasis in original).

The final phase in this development turned on a debate about the link between "random selection" and "what is known already." At first, even statisticians received the notion of the "representative sample" with skepticism, insisting that no

sample could replace a complete survey. As one statistician put it at the turn of the century, "no calculations when observations can be made."[22] This skepticism was eventually overcome when the imprecise notion of "representative sample" gave way to the technique of stratified sampling according to *a priori* divisions of the (national) population. This did not happen until the idea of the representative sample having to be a microcosm of the total (national) territory was abandoned.[23] The eventual realization that given developments in probability theory, sampling by random selection was more reliable led to a conceptual break with territoriality. This did not mean, of course, that prior knowledge of a field of inquiry, a global knowledge of a situation or group, would be irrelevant to the statistician. It meant only that the solution to the problem of representativeness did not depend on complete and certain knowledge of the entire geographical area under investigation. It was precisely uncertainty with which probability theory was designed to deal.

The modern concept of representativeness emerged in close connection with the construction of the welfare state (a process that began at the end of the nineteenth century) and the centralization of national statistics. Three developments occurring within and outside the domain of state practices were especially important in the history of statistical representations: social security legislation, markets for consumer goods, and market research and national election polls. All of these developments produced social knowledge that is continuously and profoundly interventionist. They constitute social wholes that do not depend logically either on the intimate experience of a given region or on the assumption of typical social actors. They encourage and respond to individualized agents making individual choices in a variety of social situations. Ways of statistical calculation, representation, and intervention have become so pervasive that capitalist social economies and liberal democratic politics are inconceivable without them.

In a memorable passage in his history of probability in the nineteenth century, Hacking writes:

> Probability and statistics crowd in upon us. The statistics of our pleasures and our vices are relentlessly tabulated. Sports, sex, drink, drugs, travel, sleep, friends—nothing escapes. There are more explicit statements of probabilities presented on American prime time television than explicit acts of violence (I'm counting the ads). Our public fears are endlessly debated in terms of probabilities: chances of meltdowns, cancers, muggings, earthquakes, nuclear winters, AIDS, global greenhouses, what next? There is nothing to fear (it may seem) but the probabilities themselves. This obsession with the chances of danger, and with treatments for changing the odds, descends directly from the forgotten annals of nineteenth-century information and control (Hacking, 1990, pp. 4–5).

Statistical knowledge and statistical reasoning, Hacking reminds us, have become central to the way we conceptualize and respond to our modern hopes and fears. But they are central also to the grounds we now adduce for regarding some beliefs as more reasonable than others in a social world of uncertainty, grounds we inherit from classical probability theory's alliance with the Enlightenment.[24]

What is the relevance of what I have said so far for anthropology? I want to suggest tentatively that a mode of knowledge which grew out of counting large numbers of human beings living in variable conditions, bodies of knowledge that were continuously directed through economic and political practices at those variations, had to be marginal in a discipline that regarded itself primarily as interpretative rather than practical, that defined its object of investigation in terms of spatial and temporal events accessible to the fieldworker's personal experience, and that sought to represent what is locally typical.

There is now increasing awareness among many anthropologists of the limitations of fieldwork and the need to pay greater attention to heterogeneities within wider spaces and

over longer periods of time than personal observation allows. In a brief section entitled "Limitations of the Fieldwork Perspective" at the conclusion of his textbook on cultural anthropology, Roger Keesing notes:

> The problem of sampling has become much more acute in larger-scale, more complex societies. Cultural diversity, large populations, social stratification, and rapid change have made fieldwork in large-scale modern societies, whether in the Third World or the West, a complicated business in which more concern with sampling, statistics, and methodological precision is needed.[25]

This is an acute comment, but as I remarked earlier in my discussion of Raymond Williams, statistics is much more than a matter of representation; it is a tool of political intervention. As a political tool, it is infinitely more powerful than ethnographic representation—for good or for ill.

As a tool of social intervention, statistical knowledge has been important not only to European societies. Especially from the latter part of the nineteenth century on, statistics became increasingly important in the European empires: Asia, the Middle East, and Africa. Its importance, however, was reflected primarily not in social anthropology as an academic discipline but in colonial administration as a political discipline. Of course, academic ethnographers describing social change in particular localities of the non-Western world often employed statistics. But with some minor exceptions, colonial administrations did not depend on them. All administrators had their own resources for carrying out surveys, compiling tabulations, and keeping records. The point is that the practice of assembling and classifying figures periodically on births, deaths, diseases, literacy, crimes, occupations, natural resources, and so on was, from a governmental standpoint, not merely a mode of understanding and representing populations but an instrument for regulating and transforming them.

This applies also, and even more strongly, to the "modern-

izing" nation-states that have succeeded European colonies. It is true that the statistics were (and are) often unreliable and based on questionable categories,[26] that administration-inspired transformations were (are) often very uneven, producing unintended results, but that is not the point for the moment. What matters is that the figures and the categories in terms of which they were (are) collected, manipulated, and presented belong to projects aimed at determining the values and practices—the souls and bodies—of entire populations. Central to these projects has been the liberal conception of modern society as an aggregate of individual agents choosing freely and yet—in aggregate—predictably.[27] The construction of modern society in this sense is also, of course, the construction of radically new conditions of experience.

Conventionally, anthropologists have dealt with societies that are not—yet—liberal in this sense, which makes typification a more plausible device for ethnography. It may be objected that statistical categories also typify, and that is certainly true. But they are more readily challengeable and alterable because they can be subjected to procedures of disaggregation. Industrial working norms, for example, are based on statistical calculations of a typifying sort, but the norms are open to recalculation and negotiation in the familiar conflict between unions and employers. It is less easy to question the ethnographer's *types* or *characters*, partly because they are presented as indissoluble forms (to break up a character is to undermine its "human integrity"), and partly because they are guaranteed by the ethnographer's personal experience (she witnessed the character, the reader did not).

Today, each of the so-called "developing countries" has its own tabulations in which its relative successes and failures (from GNP and international debt to health, family planning, and education to the record on women's liberation and human rights) are measured, probabilized, and compared with the figures of other countries. Each nation-state compiles, manipulates, and acts upon its statistics and calculates the risks of its

policies failing. So, too, do multinational corporations, the United Nations, and the World Bank. Within each nation-state and between them, almost all statistics are contestable; but in the domain of social power they have become indispensable. Groups opposing the policies of particular business enterprises, a local administration, a national government, or international agents contest the figures and the categories on which the figures are based, but in doing so they employ statistical arguments. In brief, moral and material "progress" presupposes the continuous use of comparative statistics. Put more strongly, the very concept of such progress is in great measure the product of statistical practices. The politics of *progress* differs precisely in this from the politics of *reform*—it is inconceivable without the concepts and practices of statistics. "Progress"—not mere "reform"—is the political aspiration that the non-European world has acquired from Europe.[28]

Statistics is a vital part of what I have elsewhere called "strong languages" (Asad, 1986), discursive interventions by means of which the modes of life of non-European peoples have come to be radically transformed by Western power. I want to say that modern statistics is the strongest language of all. Of course, in saying this I do not refer to essentialized or biassed Western representations of non-European peoples.[29] On the contrary, through statistics it is Western representations of modern (that is, Western) society that are offered, adopted, adapted, and employed. What makes statistics a strong language is that statistical figures and statistical reasoning are employed in the attempt to reconstruct the moral and material conditions of target populations. The implications of this for the old problem of cultural relativism are worth noting. Statistics converts the question of *incommensurable* cultures into one of *commensurable* social arrangements without rendering them homogeneous: the ranking of every country differs and changes in complex ways even when the several variables remain constant; flexible marketing caters to consumers seeking a variety of different (and incompatible)

experiences. I do not say statistical *thinking* solves the philosophical problem of incommensurability; I say that statistical *practices* can afford to ignore it. And they can afford to ignore it because they are part of the great process of conversion we know as "modernization."

When social power is exercised through statistics, experience is no longer a moment of awareness but an experimental practice, that is, a test of the precise degree to which a given social objective has succeeded.

The political success of statistics in the modernizing world is a fact of considerable anthropological significance. From this latter standpoint, it is something to be analyzed and explained as a cultural feature of modern social life. If I suggest that such ana analysis be conducted, it does not follow that I think ethnographic fieldwork has no merit. Nor—I repeat—do I say that there is any essential superiority of statistical over ethnographic methods. The only general opinion I offer about ethnography is that the rich historical tradition of anthropology is unduly narrowed if it is defined simply in terms of fieldwork.

Notes

[1] See also Nadel (1951, p. 114).

[2] For example, the compilation of vital statistics in the eighteenth-century and "the avalanche of printed numbers" in the nineteenth-century were both crucial to developments in probability theory in different ways. (See Hacking [1987] and Daston [1987a].) In turn, probabilistic sampling theory has come to be indispensable to social surveys.

[3] Much later, anthropologists began to speak of "participant observation," a term that was popularized by American sociologists. In her interesting history of this concept, J. Platt writes:

> In the 1920s and 1930s, some of the key ideas now associated with 'participant observation' were associated with other meth-

> ods and categories. . . . Although it is obvious that participant observation studies are quite likely to be in the 'case study' tradition in at least some of its senses, 'case study' can certainly not be translated as 'participant observation.' First, it is evidently a category not defined in terms of the manner in which its data are collected; second, insofar as data collection is mentioned, it is written or oral statements, often solicited, that are emphasized. When participation is mentioned, which it seldom is, it is primarily as a means to the elicitation of such statements. . . . Initially, at any rate, the mere idea of recording solicited data in the subject's own words was novel enough to seem a significant step in the direction of natural reality. . . . The idea that the further step of observing the behavior in its normal context should be taken was not raised [by sociologists]. The long march away from the library had only recently started, and perhaps merely living among people was seen as too like journalistic work from the which the social scientists were anxious to distinguish themselves (Platt, 1983, p. 381).

For anthropologists, "living among people" could be articulated as a *method* only when professional social scientists could be distinguished from such non-professionals as missionaries and administrators. According to Platt, E.C. Lindeman invented the term "participant observation," although he meant something very different from its usage today. For Lindeman, the "participant observer" was not the investigator himself but a member of the group being studied whom the investigator recruited as an informant (Platt, 1983, p. 386).

[4] Thus: "It is our capacity, largely developed in fieldwork, to take the perspective of the [small communities we study] that allows us to learn anything at all—even in our own culture—beyond what we already know. . . . Further, it is our location 'on the ground' that puts us in a position to see people not simply as passive reactors to and enactors of some 'system,' but as active agents in their own history." See Ortner (1984, p. 143).

[5] See Chapter 1 of Steiner (1958). Steiner was not thinking of statistical comparisons such as Murdock's.

[6] J. Goody's materialistic studies (notably Goody, 1983) were influenced by this current, although they do not belong to it. Incidentally, the recent inauguration of a periodical entitled *History and Anthropology* is an indication of a growing interest in analyses based on historical sources.

[7] Compare Evans-Pritchard's (1951) in Chapter IV with the fuller scholarly one by Urry (1984).

[8] In the section on Method in the first chapter of *Argonauts*, Malinowski employs the term "experience" in exactly this sense:

> No one would dream of making an experimental contribution to physical or chemical science, without giving a detailed account of all the arrangement of the experiments. . . . In less exact sciences, as in biology or geology, this cannot be done as rigorously, but every student will do his best to bring home to the reader all the conditions in which the experiment or the observations were made. In Ethnography, where a candid account of such data is perhaps even more necessary, it has unfortunately in the past not always been supplied with sufficient generosity. . . . It would be easy to quote works of high repute, and with a scientific hallmark on them, in which wholesale generalizations are laid down before us, and we are not informed at all by what actual experiences the writers have reached their conclusion (Malinowski, 1922, pp. 2–3).

[9] P. Kaberry tells essentially the same story about the emergence of fieldwork but with a somewhat different focus: "Until the end of the nineteenth century, most anthropologists wrote from the armchair and relied for their raw data on material recorded by missionaries, explorers, travellers, government officials, and settlers." Her attention is directed at the contrast between kinds of people who record the "raw data"—the "professional" anthropologist on the one hand and non-professionals on the other—not at the contrast between kinds of "raw material"—written accounts versus "social life itself." But she shares with Evans-Pritchard the assumption that ideally the person who reworks the "raw data/material" should be the one who records/collects it. See Kaberry (1957, p. 73).

[10] S.F. Nadel drew a parallel between anthropology and psychiatry in the matter of interview techniques thus:

> If it is true that the psychiatrist concentrates almost wholly upon problems concerning the personality of his patient, perhaps for him no interview can ever be a complete 'failure;' even the most negative reactions of the patient have their diagnostic significance. In social work generally [including social surveys] . . . the failure of an interview may obstruct the main source of information. It might be expected that in anthropology, with its pursuit of 'objective facts,' the failure of the interview would be

> equally serious. In fact, the success or failure of an interview, irrespective of the information which it produces or fails to produce, may itself be of diagnostic value to the student of culture. For the 'objective reality' with which the anthropologist is dealing is a social reality, and the informant and his responses are themselves elements and factors in this social reality (Nadel, 1939, p. 318).

[11] "It is perhaps as true for civilizations as it is for men that, however much they may later change, the fundamental dimensions of their character, the structure of possibilities within which they will in some sense always move, are set in the plastic period when they first are forming" (Geertz, 1968, p. 11). The theme of this book is the way the individual character of representative person and representative civilization mirror each other and express an essence called "Islam" (typified by Morocco and Indonesia).

[12] Fortes wrote:

> We are dealing not with two 'types' or 'forms' of domestic organization but with variants of a single 'form' arising out of quantitative differences in the relations between the parts that make up the structure. We can imagine a scale varying from perfect 'patrilocality' at one end to perfect 'avunculo-locality' at the other. . . . Individual households are scattered all along the scale; and over a stretch of time a particular household may change its position through the loss of some kinsfolk and the accession of others (Fortes, 1949, p. 75).

But because this interest in variability deals in averages it remains preoccupied with representing "types" as statistical "norms."

[13] In Leach (1954). In this book, Leach argued that because the Kachin Hills Area was not culturally homogenous, he could not follow the "classical manner in ethnography" of choosing a locality "of any convenient size" for intensive study and then writing "a book about the organization of the society considered as a whole" on that basis (p. 60). Leach's procedure instead was to describe the Shan and Kachin "categories" common to the entire area which help to generate a range of unstable political structures in a variety of ecological contexts and over a cycle of historical time. His overall conclusion was that "while conceptual models of society are necessarily models of equilibrium systems, *real* societies can never be in equilibrium" (p. 4; emphasis added).

[14] See the chapter on Microscopes in Hacking (1983).

[15] In *Iconology*, W.J.T. Mitchell argues persuasively against the belief that there is a radical difference between verbal and visual images. This argument seems to me especially relevant for examining critically the notion of the "ethnographic gaze." Because if "seeing" merges into "reading," the epistemological priority of fieldwork experience over textual engagement can be questioned. The anthropologist who collects his own material in the field has no more grounds for certainty than the anthropologist who reads texts composed by someone else.

[16] " 'Writing up' is a transformation of data from the category 'what you know' into a new category: 'what you communicate.' The two are related: at least in an ideal world, what you know sets nearly all the limits of what you can write. And because you create both categories, they may be linked also by your intentions from a very early stage" (Davis, 1984, p. 295). The fact that "data" can be thought of as belonging to the category "what you know" and ceasing to belong to that category (which "you create") when it is "written up" makes sense only because what the anthropologist knows is indissolubly linked to ethnographic experience.

[17] Incidentally, when J. Scott, in her excellent article (1991, p. 783), contends that, "The concepts of experience described by Williams [in Keywords] preclude inquiry into processes of subject-construction; and they avoid examining the relationships between discourse, cognition, and reality, the relevance of the position or situatedness of subjects to the knowledge they produce, and the effects of difference on knowledge," she is, of course criticizing not Williams but the concepts he has analyzed. The major limitation of Williams' article, in my view, is its almost exclusive concern with cognition. It has nothing to say about experience in the sense of a socially-developed, embodied capability, as in "an experienced mountaineer," "an experienced actor," "an experienced teacher"—that is, in the Maussian sense of *habitus*. For anyone interested in the problem of subject-construction, the development of such capacities is of major importance. When J. Scott insists that the denial of the discursive character of experience is merely an attempt to defend their unquestioned authority ("it is precisely the discursive character of experience that is at issue for some historians because attributing experience to discourse seems somehow to deny its status as an unquestionable ground of explanation [Scott, 1991]), she moves too quickly in attributing obscurantist intentions to those she criticizes. Surely, the determination to prioritize cognition and to assign it to

discourse may be resisted for reasons other than a desire to defend the unquestionable status of particular categories. Thus, in themselves, embodied practices are not a *ground of explanation* at all; they are simply skills, abilities, virtues (in the Aristotelian sense) that are exercised with greater or lesser facility. It is only when they become objects of discourse that explanation—and criticism—can take place. And then, of course, any concept may be questioned—but not all concepts all of the time or practical life becomes impossible.

[18] Even in early modern Europe, "political arithmetic," as it was then called, had a close connection with government, religious sectarianism, and commercial society. See the two articles by Buck (1977 and 1982).

[19] I am indebted in the account that follows to Desrosieres (1991), Hacking (1990), Kruskal and Mosteller (1980), and Lazarsfeld (1961).

[20] Thus, on the first page of the Forward to Volume I of his *Les Ouvriers européens*, LePlay states: "In order to find the secrets of the governments which provide mankind with happiness based on peace, I have applied to the observation of human societies *rules analogous* to those which had directed my own mind in the *study of minerals and plants*. I *construct a scientific mechanism*." Cited in Lazarsfeld (1961, p. 314).

[21] For a detailed discussion of the connection between projects of racial progress and statistical theory, see MacKenzie (1981).

[22] Cited In Kruskal and Mosteller (1980, p. 175).

[23] But as Kruskal and Mosteller (1980, p. 191) note, "Quota sampling in public opinion polling and marketing went on for years, and still continues, without much interaction with the work of academic or otherwise organized statistics."

[24] In the second half of the seventeenth century, the fledgling calculus of probabilities was first applied to the task of providing a mathematical basis for rules of evidence then current in Roman and canon law. L. Daston writes: "In viewing their theory as the 'art of conjecture,' classical probabilists adopted the legal habit of thinking about probability epistemically, as a continuum of degrees of certainty. They also inherited a set of problems related to legal evidence—in particular, responsibility for establishing rational grounds for belief not only in the courtroom but in the world-at-large" (Daston, 1987b, p. 296). In the eighteenth century, the subjectivism of this notion of probability joined associationist psychology, and the two together helped to construct the Enlighten-

ment conception of *belief that was supported by reason*. The classical probabilists held that their mathematical theory was simply "good sense reduced to a calculus," says Daston, but their "good sense" was simply the good sense of an Enlightenment elite. In our day, calculations of probabilities are regarded as crucial for arriving at "rational decisions," although probability theory has developed considerably since then.

[25] See Keesing (1981, p. 7). However, this awareness of the limitations of a tradition that represents the part as "a microcosm of the whole" (p. 6) seems to come up against a contrary tendency, namely, to regard the contemporary world as relatively homogenous ("this stereotypic view, largely created by anthropologists, exaggerates the diversity of cultures" [p. 7]). The resolution of this conflict clearly resides in a more precise specification of what is to be regarded as "the same" and what as "different."

[26] This has long been recognized. See Thorner and Thorner (1962) for an impressive critique of the categories used in government statistics on the agricultural population of India. In her splendid essay on a statistical report on urban workers in mid-nineteenth-century Paris, Joan Scott demonstrates the political assumptions and concerns which motivated its classifications and inferences (Scott, 1986). Cullen (1975) argues that from its beginnings in the nineteenth century, quantitative social inquiry was inherently political.

[27] Cf. Metz (1987).

[28] Incidentally, Foucault, who wrote insightfully about the modern concept and practice of government (which he called "governmentality"), never theorized "progress" in that context.

[29] Essentialized or biassed representation in Western discourses of non-Western peoples is part of what critics have called "Orientalism;" "orientalists" typically construct *typifications*.

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