once asking me, "How come we treat population as an effect, and archeologists treat it as a cause?") More important, Blanton hints that what passes for "ecology" in archeology is often monolithic and environmentally deterministic by comparison with the ecology done by modern zoologists. If an animal ecologist studies wolves, for example, he does not limit himself to their subsistence practices; he studies everything from their adult social behavior to the play of their cubs. Since human social and intellectual behavior is so much more complex, why should "human ecologists" limit themselves to the way people hunt or farm?

Blanton's comments remind us that there is a middle ground between oversimplification and awe. One can start with population growth, economic symbiosis, and hydraulic agriculture but observe that these variables explain less than 100 percent of the phenomenon under examination. Then one can begin to add the variables Millon feels have been left out, gradually increasing the complexity of the model until a more convincing approach to explanation has been made. However, this cannot be done by settlement-pattern survey alone; it requires extensive excavation, for almost none of the variables Millon feels Sanders has neglected can be studied through survey.

A few years ago I suggested that religion, ideology, and other forms of sociocultural communication would have to be added to Sanders's model before it would resemble a "human" ecology. On p. 247, however, Millon rejects this suggestion as well, arguing that such "all-encompassing" models are "untestable." This will come as a surprise to the present generation of animal ecologists, who test even complex models, one variable at a time, through rigorous sampling, measures of association, and mathematical simulation; it will also come as a surprise to archeologists such as Henry Wright, who have been following a similar approach in the Near East. But this is perhaps not a point worth pushing; it could be that Millon prefers his ecology reductionistic because it makes a good whipping boy and because Sanders, his oldest adversary, is also his favorite adversary. Millon's arguments would also be more convincing if he did not show, through his frequent use of "ecological" when he means "environmental," and through the restricted area that he considers to be the "environmental setting" of Teotihuacán, that his concept of ecology does not extend to the ecosystem. Blanton's rank-size graphs have already suggested that Teotihuacán's effective "environment" was nearly the whole of Mesoamerica.

Finally, I must comment on the new chronology proposed in this book. Annoyed by the "value-laden" nature of Mesoamerica's traditional terms for major segments of time—"Classic," "Formative," "Post-classic"—some conference members propose adoption of the chronology used by John Rowe and his associates in Peru. This chronology features the supposedly value-neutral terms "Initial Period," "Early Horizon," "First Intermediate," "Late Horizon," and so on (to which they add the North American abortion "Lithic"). I don't know whose idea this was, but it's the worst one he (or she) ever had.

I agree that we must divorce chronological periods from developmental stages, but the fact is that very few people are losing sleep over this problem in Mesoamerica; over the years, terms such as "Formative" have long since lost whatever developmental significance they might originally have had, and are being used primarily to refer to large blocks of time. And if one wants value-neutral terms, we already have them—the individual ceramic-style phase names for the Valley of Mexico sequence, such as Tzacualli, Tlamimilolpa, Xolalpan, and Tezoyuca-Patlachique. Most of these are so unpronounceable they could never take on developmental overtones. Seemingly, only Millon sensed that the wholesale transfer of a Peruvian chronology to Mesoamerica might not be much of a solution: "I argued that while [Rowe's terminology] was a value-neutral classification, it probably had little chance of acceptance in Middle America, given the deeply-rooted nature of the existing classification" (p. 24). The authors should have listened to Millon. I don't know how my colleagues will react, but I would have to be smeared with honey and buried up to my neck in a red ant hill before they could get me to accept yet another chronological scheme on top of the profusion we already have.

This volume captures much of the heat, and a great deal of the light, that must have been generated by the symposium. It represents the best in academic give-and-take: impassioned scholars, often strongly in disagreement, each supporting his case with reams of skillfully collected and carefully analyzed data. It is the reader who benefits from this clash of heavyweights, and who is left to search for the more holistic framework which might resolve our profound disagreements on the rise of pre-Columbian civilization.

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Investigations of a Little-Known Way of Life

Kalahari Hunter-Gatherers. Studies of the !Kung San and Their Neighbors. RICHARD B. LEE and IRVEN DEVORE, Eds. Harvard University Press, Cambridge, Mass., 1976. xx, 408 pp., illus. $18.50.

This is a long-awaited book. The research reported in it represents the first, and what perhaps will be the only, long-term study of contemporary hunter-gatherers by a number of specialists from different disciplines. As has been said so often by so many, the ways of life of hunters, which characterized most of our species' prehistory, are little known. Therefore detailed studies of the few groups who remain as hunter-gatherers are important for the insights they can provide into the conditions under which the biological and cultural evolution of our species occurred.

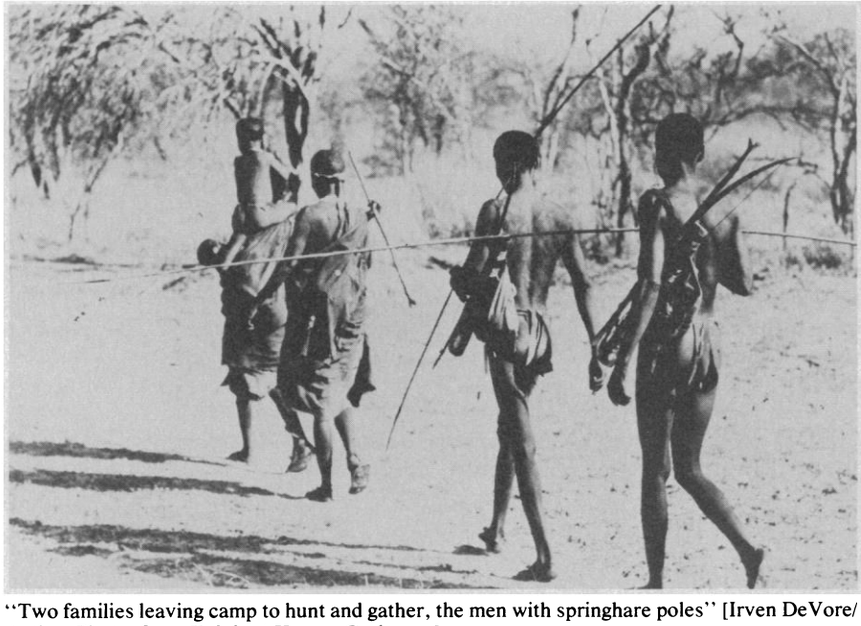
The book consists of 15 chapters representing the work of 17 specialists on subjects ranging from medicine to archeology to folklore. There is no overall synthesis and perhaps none is possible. Richard Lee does give an introduction that provides a background for the separate studies of the Kalahari Bushman (or "San," which the authors suggest as the preferred name).

The most important contributions to our knowledge of hunter-gatherers are to be found in section 2, Population and Health, and section 3, Childhood. In section 2, the chapters by Nancy Howell and by Henry Harpending provide the best data available to date on the demography of hunter-gatherers. Demography is, as statistics was once defined as, a science of large numbers. Hunter-gatherers living in small groups simply do not provide samples of the sizes that are necessary for conventional demographic analysis. But both Howell and Harpending, using different approaches, extract more information than one would expect, and they present their methods and results clearly. San children are nursed for a long time, until the mother's next pregnancy. The interval between pregnancies is at least three years. Births are

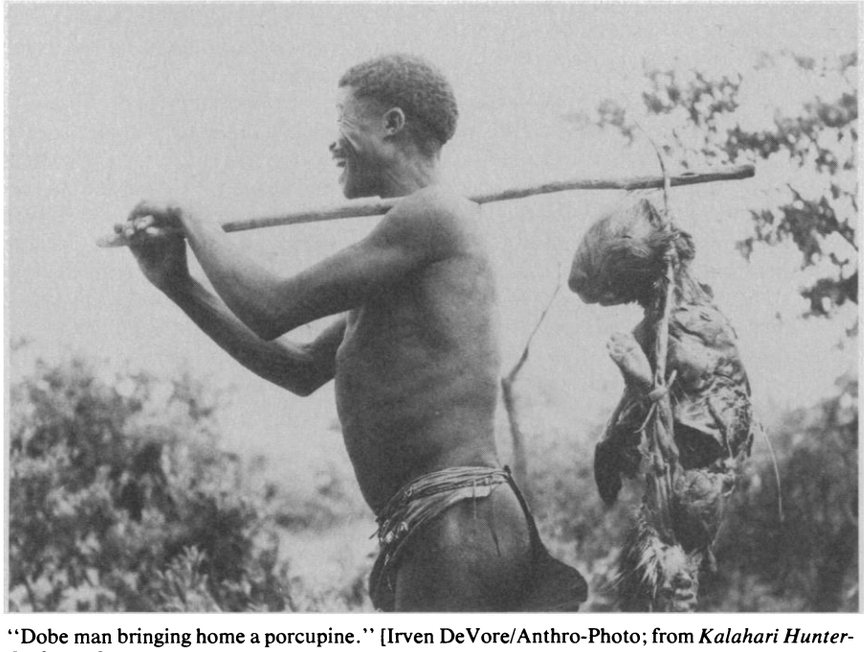
spaced in such a way that the mean number is only 5.1 per woman (and less among women who report they have had gonorrhea). Life expectancy at birth is estimated at 32.5 years for women and slightly less for men. This is not particularly short in comparison to life expectancies in many agricultural populations having no access to modern medicine.

The demographic and health data bring into question the idea that hunter-gatherers today are existing in "marginal" areas. In the lusher north the San suffer greater infant mortality and the population seems, throughout the past, to have been sparser than in the more arid south. San culture seems well adapted to the drier parts of their range. The implication is that land that is marginal for the horticulturalist or the pastoralist may not be so for the hunter-gatherer.

A recent controversy about the nature of hunting-and-gathering society seems close to resolution. Approximately 12 years ago Lee advanced the hypothesis that hunter-gatherer populations equilibrated, by undescribed mechanisms, in such a way that hunger was not a factor in the process. Lee depicted the San of the Dobe area as having "superabundant" food in the form of mongongo nuts ad libitum. It appears that the disillusionment with Western society of the 1960's led to a renascence of the "noble savage" view of the hunter-gatherer. The picture of a society with the simplest technology spending little time in the quest for food and yet having superabundant food struck a responsive chord. One prominent anthropologist termed the San the original affluent society.



"Two families leaving camp to hunt and gather, the men with springhare poles" [Irven DeVore/Anthro-Photo; from *Kalahari Hunter-Gatherers*]



"Dobe man bringing home a porcupine." [Irven DeVore/Anthro-Photo; from *Kalahari Hunter-Gatherers*]

This bit of hopeful romanticism is still prominent in anthropology. It is a position still advocated by Lee in the introductory chapter of this book, although he notes that the medical data seem to be a problem. Indeed they are. Stewart Truswell and John Hansen's description of the health status of the San portrays a generally healthy people, although tuberculosis, rheumatic fever, and venereal diseases are important problems. The San are notable for lack of hypertension and show no rise in blood pressure with age. But all the data are consistent with chronic undernutrition beginning with weaning and persisting into adulthood. Other health studies of the San are cited that are consistent with this hypothesis. The authors conclude that the nutritional weakness of the San diet is strictly a caloric deficiency. This does not sound like the product of superabundant food.

Other chapters document the great concern the San have for food and for its equitable distribution. Cultural elaborations of these themes also bring into question the superabundant-food hypothesis. But the real clincher, as it appears to this reviewer, was there from the beginning. The information does not appear in this book and has not, to my knowledge, been published elsewhere, but Lee noted in his Ph.D. dissertation of 1965 that two-thirds of the San population in the Dobe region had been removed from there in a resettlement program only 2 to 3 years prior to his fieldwork. That there were superabundant gathered foods after two-thirds of the population had been removed is not surprising, nor is the superabundance relevant to general hypotheses concerning hunter-gatherer adaptations. It speaks for the quality of the contributions in this volume that they provide evidence such that, even without the historical information, the superabundant-food hypothesis can be brought into question if not rejected.

Section 3 also presents data of a sort almost totally lacking for other hunting societies. The data are too detailed to review here but will be a valuable resource in social science.

The last section of the book, Behavior

and Belief, is the most variable in quality. I single out two chapters to illustrate this.

The chapter by Richard Katz, "Education for transcendence," deals with altered states of consciousness induced in an important form of San healing dance. This chapter raises old but still serious questions of what constitutes data in the natural history approach that cultural anthropology depends on. Katz writes in an idiom appropriate to a devotee. It is impossible to distinguish the views of the San from the views of the observer. No line is drawn between observation and ideology. The chapter is a display, rendered excellent by exaggeration, of the problems of maintaining objectivity in anthropological reporting.

The chapter by Nicolas Blurton Jones and Melvin Konner, "!Kung knowledge of animal behavior," is objective and enlightening. The authors demonstrate not only the existence of a wealth of knowledge of animal behavior among the San but, more important, that San hunters use the same methods in evaluating data and in formulating and testing hypotheses that scientists use. The only 19th-century evolutionist impressed by such thought processes in "primitives" was Alfred Russel Wallace. And although all anthropologists of this century have subscribed to the intellective equality of different peoples few have bothered to give such satisfying evidence. This is a study to be emulated.

General descriptions of San (Bushman) culture and social organization are to be found in other publications, such as those of Lorna Marshall. But this book is a unique store of data on hunter-gatherers. As such it will be of value to anthropologists and to social scientists of many disciplines.

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Measuring the Effects of Education

Schooling and Achievement in American Society. Papers from seminars, Oct. 1971–May 1973. WILLIAM H. SEWELL, ROBERT M. HAUSER, and DAVID L. FEATHERMAN, Eds. Academic Press, New York, 1976. xxviii, 536 pp. $24. Studies in Population.

A decade has passed since the publication of Blau and Duncan's *The American Occupational Structure* and since *Equality of Educational Opportunity* ("the Coleman report") began to have an impact on research and policy in education. This collection of papers, the result of a series of seminars sponsored by the American College Testing Research Institute, reflects the impact of these two works on quantitative research in sociology and provides an overview of some of the best educational research conducted in the interim. The specialist will welcome the publication of so many new and exemplary additions to the technical literature, and the book offers the nonspecialist a glimpse of what quantitative sociologists have been up to during the last decade.

Substantively, the two major questions addressed are What measurable effects do schools have on their students? and How should we understand the process of achievement in American society? The issue of school effects centers on the degree to which a particular educational context can be viewed as uniquely and causally related to student achievement, aspirations, or ultimate attainments. This question continues to arouse controversy and debate. William Spady ("The impact of school resources on students") ably summarizes the conventional critiques of the Coleman report and argues that the methodological flaws of the various impact studies that have been done are so great as to raise doubts about their conclusions. He points to the crudity of the resource measures available and draws the distinction between "value climates" and the distribution or utilization of tangible assets. I agree that sophisticated techniques are often used injudiciously and that undue emphasis is placed on cognitive achievement. Nevertheless, the weight of evidence clearly indicates that it is being in school that matters, not which school one happens to attend.

David Wiley ("Another hour, another day") argues that it is the amount of exposure to instruction during schooling that influences achievement. Using cross-sectional data from the Coleman report and looking at average daily attendance, hours in the school day, and the number of school days in the year, he estimates the effect of the time spent in school on achievement and concludes that it is crucial. The substantive points Wiley raises are important; his research results, however, have been difficult to replicate (see N. Karweit's analysis based on more extensive data, *Sociol. Educ.* **49**, 236 [1976]).

The two papers by Henry Levin ("A new model of school effectiveness" and "Measuring efficiency in educational production") are the only contribution by an economist; they combine technical virtuosity with a considerable amount of skepticism about the usefulness of econometric models for research bearing on educational policy. As Levin points out, schools are not competing firms and cannot be expected to use inputs efficiently to maximize achievement. I share his concern that in the absence of any certain knowledge about how to augment achievement levels estimating production functions for education can lead to quite misleading results.

A paper by Hauser, Sewell, and Duane Alwin ("High school effects on achievement") and one by Alwin ("Socioeconomic background, colleges, and post-collegiate achievement") report findings from a longitudinal study of the educational aspirations and attainments of a cohort of 1957 Wisconsin high school graduates. The models presented extend earlier empirical work undertaken by Hauser on the effects of schools on achievement, with similar results. In brief, differences between schools have neither an additive nor an interactive effect on students that is sufficiently large or consistent to be considered important. Insofar as there were differences associated with schools along measured dimensions, the differences are attributable to the social composition of the schools. Alwin extends this line of inquiry to higher education. Once again, the effects of attending a particular college on later achievement are negligible. Alwin finds that gross differences between colleges account for 5 to 8 percent of the variance in postcollegiate attainment and that perhaps half of this is accounted for by patterns of selection and recruitment.

Kenneth Feldman and John Weiler ("Changes in initial differences among major-field groups") extend the search for school effects by assessing the role of particular academic fields in accentuating personality differences during the course of an undergraduate career. The authors provide a useful typology of the changes one might expect if exposure to a given course of study influenced the students who enrolled in it. There is some evidence that female students are