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Introduction: Norms for Ethnographic Reporting

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sily collected and reported, and hierarchical—so that the minima fit ›ad questions of theoretical importance to the comparative study of socies and the study of the human impact on the physical environment.

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I also wish to thank the authors. They quickly recognized the need for s volume when the idea was proposed to them, and they recognized that : standards would not please everyone. It will be hard for many readers to :ept some of these minima—but no harder than it was for the authors to t aside their less-than-minimal list of topics that *they* would have liked to ve seen listed as minima. They had the fortitude to recognize in most ses that personal research interests often intrude upon efforts at compari-ı. In the end, we tried to balance our specialized interests with our obligon to generate data that others can use and build on.

We hope this volume will be useful in training a new generation of :ial scientists and as a resource to colleagues in other disciplines. This is t a cookbook or primer; we hope it helps guide the tough choices that ıst be made in the field—and that it encourages social scientists to opt for ›se measures that can best lead to comparison and data sharing. We seek promote a dialogue among the various disciplines that are concerned th human activities in a physical landscape. Will you join us in this ort?

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Introduction: Norms for Ethnographic Reporting

EMILIO F. MORAN

Any effort at suggesting a set of standards—whether for language, sports, ecology, or anthropology—is likely to meet with the resistance of practitioners. Standards evoke images of rigidity, lack of creativity, and loss of individuality. Although we can live without standards, we also need them to ensure consistency in quality (for example, acceptable percentage of natural juice in fruit drinks), to ensure fairness in competition (weight categories in boxing, boat classes in racing), and to allow comparisons over time (consistent size in the playing field or in size or weight of the ball). Standards do not prevent innovation. Gifted individuals have challenged the canons of the most rigid literary academies—and won. In sports, as in music or science, standards exist—and they are changed over time. In fact, it can be argued that standards *promote* change by setting up a clear canon against which to match the gifts of individuals, who are challenged to best the current standard.

A standard is a measure, or set of measures and procedures, that practitioners of a given art, language, sport, or scientific discipline agree to follow to facilitate communication, competition, or comparison. Without a willingness to abide by those standards, there is no capacity to effectively share results or means by which to be judged fairly; there is no "evenness in the playing field." Judgments of quality are intrinsic to human affairs. Human beings always use their experience, and values, to make such judgments.[1] Standards, when they are put into place and followed, bring some order to "unmapped territory"—that is, situations otherwise lacking in agreed-upon referents. Classical ballet, for example, has its obligatory leaps and pirouettes (Royce 1977) that establish competency—but that perform-

ers routinely exceed both physically and emotionally in order to be judged brilliant or superb. The obligatory movements, though not the best moments in a ballet, tell us what to expect: a pedestrian performance, or one that promises to excite.

Standards in Anthropology

Anthropology has had both implicit and explicit standards in the past.[2] The monumentally thorough ethnographic practice of Bronislaw Malinowski served as a normative model—another term for a "standard"—that aspiring anthropologists tried to emulate, down to the categories or chapters into which an ethnographic report fell. Not everyone abided by this implicit model, but most practitioners tried to and in so doing contributed to the development of a science of humankind. If there had been no imitation of Malinowski's model of ethnographic writing and research, it is doubtful that anthropology could have contributed the ethnographic method of participant-observation to the repertoire of the human sciences. This procedure has been, in the opinion of most, anthropology's most lasting and important methodological contribution to date (cf. Ellen 1984; Kuper 1983). Educators, demographers, sociologists, agricultural economists, and urban planners today do ethnographic studies to gain the kind of "insiders' view" that has been associated with Malinowski and anthropology for almost a century. Has this been one of the rare cases where canons of ethnographic practice were followed?

A more explicit effort at developing a "canon" is represented by the field manual known as *Notes and Queries on Anthropology* (Royal Anthropological Institute [RAI] 1874, 1892, 1899, 1912, 1929, 1951). It was written to assist British social anthropologists in the field.[3] This fine little book, often derided or ignored today in European and U.S. academic departments, provided thoughtful reminders of things to ask—some obvious,[4] others counterintuitive and profound. Such is the nature of a primer or field manual. After all, there is very little that can be said unambiguously to be common sense. Things become common sense when they are shared by a community that has encoded in everyday language and behavior a given set of standards. Much of the standard of Malinowski became implicit in the socialization of that small community of British practitioners. It is with diversity in language and practice that we begin to feel the need for explicit canons to ensure evenness and fair assessment of quality.

Notes and Queries in Anthropology advised social anthropologists to distinguish clearly between observation and interpretation. It recognized that theory and working hypotheses guided fieldwork and influenced observation, wisely noting that "the observer who wishes to give a theoretical

construction to his material should consider this separately *after* recording the facts" (1951:27). This manual noted the need to be scientifically trained in observation and the danger of overlooking the familiar or judging some kinds of data as unworthy of being recorded (see beginning of Chapter 4, this volume): "It is in order to overcome these obstacles that the notes and questions in this volume have been framed, as well as to indicate lines of inquiry worthy of investigation and the method of obtaining and recording relevant facts" (1951:27–28). *Notes and Queries* also advised that "no sociological study of a community can be undertaken without an understanding of the natural environment within which it exists. . . . The investigator should make himself familiar, therefore, with the geographical, geological and meteorological features of the district . . . some knowledge of the flora and fauna . . . demography . . . technology . . . economic life . . . and material culture . . . must seek information from those who are [competent on all these subjects]" (1951:35).

This volume has a different objective from that of *Notes and Queries:* to help readers reflect on alternatives to data collection and reporting so that research is more productive and comparable. Contributors to this book still believe in the possibility of objective, verifiable, replicable social science, unlike many contemporary cultural anthropologists. The range of comparison is left to the investigator. It can be comparison of systems of agrarian production across the world or the more workable comparison of farming systems within a given region. The latter have proven most satisfying over the years, as it is possible to know the historical context of communities within a region better than across regions, in which cases the role of historical contingency must perforce be reduced (Kuper 1983: 200).

By the time *Notes and Queries* began to be prepared for its sixth edition, in 1936,[5] another effort was taking place on this side of the Atlantic. *The Outline of Cultural Materials*[6] was designed for the organization of "available information on a large and representative sample of known cultures with the object of testing cross-cultural generalizations" (Murdock et al. 1945: v). It also sought by this method to discover deficiencies in the literature and direct corrective fieldwork. Its origins go back to the efforts of G. P. Murdock and the Human Relations Area Files project to code ethnographic information to facilitate searches and quantitative cross-cultural or holocultural studies (Lagace 1974; Narroll and Narroll 1976). In practice, the *Outline* was used in two ways—by cross-cultural analysts to arrive at theoretical generalizations and by others as a list of topics of potential ethnographic importance while in the field—and modified as needed to conform with one's ethnographic situation. Most of us who were put into contact with the *Outline* found it curious and somewhat archaic—but almost inevitably we used it in the field and appreciated it for reminding us of certain things to ask that were tangential to our focused topic. It made

available to others information that otherwise would have never been recorded and reported. Unfortunately, all too often the information collected was not reported in publications and ended up lost along with the researchers' raw data. Does it matter if we do not tell readers what the rainfall is in the area we studied? If we are engaged in the development of a science of humankind and want to be part of a community of scholars who share data, it is of great importance that we collect "baseline data" that establishes the biophysical characteristics within which people exist.

Standards come and go, but the challenge of teamwork and comparison of data has tended to galvanize the demand for and implementation of standards. The challenge of the International Biological Program (1964–1974) to study human adaptation across the world using common data measures led to the creation of a section concerned with human adaptability. This section, made up of physical anthropologists to a large extent, found it necessary to agree on a set of measurement standards (Weiner and Lourie 1969). Their work proved to be a major step in standardizing methods of data collection in physical anthropology and facilitated dialogue with human biologists in other disciplines. The results of this international effort were substantial (e.g., Baker and Little 1976; Jamison et al. 1978; Baker 1978, among others) and enhanced sharing of data not only among biological anthropologists but also with scientists in many other fields. Some of the standards have been improved on since that time, whereas others have stood (Frisancho 1990). Efforts in the early 1960s to include cultural anthropology in this international effort failed because of the apparent unwillingness or unreadiness of cultural anthropologists to select and follow clear and unambiguous data-collection procedures. The entire effort to understand the "ecology of mankind" was weakened by the absence of this important information (Worthington 1975). Anthropologists working in other disciplines on a daily basis experience regular pressure to abide by predictable canons of data collection and reporting (e.g., van Willigen and Finan 1991; Epstein 1988), resulting in several such efforts at application-specific standards.

It is striking that despite the return of a post-Boasian historical approach to anthropology (e.g., Wolf 1982; Smith 1984; Mintz 1985, among others), so little has been done to ensure the comparability of time-dependent data. Though quantitative and social historians have increasingly adopted the more rigorous canons of the social sciences, much of contemporary historical analysis in anthropology seems to follow a humanities model of history—idiosyncratic and literary rather than analytical and deductive.[7] Moreover, agreement on data sets facilitates collaboration on research, permitting an efficient and effective division of labor among collaborating colleagues. As early as 1967, Epstein noted that "as anthropological analysis becomes more refined, it becomes increasingly important

that students in the field should at least be aware of the need to collect certain basic kinds of data and know how to set about this" (vii). In this regard, this volume follows in the steps of that earlier effort, aimed at giving guidance rather than producing a "cookbook." However, it relies less than Epstein's did on a set of colleagues from a particular "school"—a "roll-call of the Manchester school," as Kuper has suggested (1983:129). This volume's contributors hope to suggest fruitful ways of making choices among alternatives.

Comparative research is important to anthropology, as to any field of science, because of its potential contribution to the development of robust theory. Much anthropological research and "theorizing" has fallen victim to myopia.[8] All too often, investigators have constructed theory based on a single case or upon a set of noncomparable cases and claimed broad significance for such "theory." The very fact of human biological, linguistic, social, and cultural variation makes comparative studies necessary, though no less difficult, to the development of theory. Reluctance to produce broadly useful and reproducible data sets gradually marginalizes anthropologists from social science and scientific discourse, ensuring the impoverishment of the discipline both academically and in its policy significance.

To advance our current state of knowledge and to enhance our capacity to participate in joint scientific research with colleagues in other disciplines, we must make some progress in defining fundamental variables, agreeing upon the form in which they should be collected and reported, and deciding how to share such data with other researchers. There is a growing movement across disciplines to share data. To do so, such data must be in standard formats and must be integrable in normative models based on accumulated and testable knowledge across disciplines. Though anthropology often highlights what is unique in a culture, not everything in it is idiosyncratic and noncomparable. As Edgerton (1970) noted, East African pastoralists were more alike across ethnic groups than were pastoral and agricultural members within a single ethnic group. This conclusion was defensible only because Edgerton and his colleagues applied standardized research procedures from ecology, economics, psychology, and other disciplines to a carefully sampled cross-section of the four ethnic groups.

The tradition of the lone researcher needs to be balanced with a greater acceptance of team research—just as the twelve- to eighteen-month-field-stay "standard" may not always be *the* appropriate time frame for research. There are various ways to organize field research from an operational standpoint. The classic method of prolonged field residence with heavy emphasis on participation and observation is most effective when applied to small, relatively homogenous populations whose system of meanings is widely shared and who have relatively low degrees of economic differentiation. How many communities like that can we find today? Key

informant interviewing, for all its merits of providing in-depth knowledge, cannot make claims to representativeness or account for intracultural variation (van Willigen and Finan 1991:2). To deal with most communities today, it is necessary to address internal variation by means of survey research and standardized schedules or questionnaires. This method permits the results, if the sample was appropriately drawn, to be offered with a certain level of confidence as representative of internal variation. For surveys to be useful, they must be carefully designed to meet objectives, follow standard procedures and scales to ensure comparability, and distinguish between fundamental variables and exploratory ones.

When time is severely constrained (as it often is in development consulting) or is exploratory in nature, neither participant-observation nor survey research may be the method of choice. For time-constrained studies, interactive methods such as focus groups (Merton 1989) and rapid appraisal procedures (Hildebrand 1981) may be more appropriate. They more effectively get at local idiom and at general patterns of variation than unsystematic surveys or key informant interviewing in a six-week time frame (a common length for predissertation field visits and development consulting). Increasingly mentioned in the literature is the development of projects in which the research subjects become partners in the research process (McCracken et al. 1988; van Willigen and Finan 1991:8).

This book is firmly within the canons of social science—to which a number of contemporary anthropologists do not seem to want to belong.[9] It assumes that claims to knowledge must be ultimately testable and that explanations are tested in due time through comparison (see also Hunt, Chapter 9, this volume). The rapid growth of the discipline of anthropology in the late 1960s and early 1970s brought about a cacophony of standards and measures from a variety of sources.[10] This proliferation was not necessarily all bad, as it was a part of the effort to question standards and set new priorities. Many productive approaches emerged from this period that constituted advances in measurement. Unfortunately, since that era passed there has been no comparable effort to return to agreed-upon standards for ethnographic research and reporting. Is it because we do not need them? Anyone systematically reading a large number of studies would conclude that ethnographic information, even among well-intended social science–oriented practitioners, is not often reported in formats that make comparison of findings easy or likely. The subjects of study allow unbridled relativity. The perception of culture as a "text" to be interpreted denies the possibility of comparability and measurement. The idiosyncratic reigns supreme—and though this must surely please those who deny that anthropology is a social science, it should give pause to those who aspire to a dialogue with other social and natural sciences. Noncomparable data is the surest way to guarantee irrelevancy and loss of theoretical significance.

In the rest of this book, the authors propose sets of standards for

anthropologists and other social scientists working on agrarian, agropastoral, and hunter-gatherer societies. Many components may also apply to urban groups and other ethnographic settings. Although changes and refinements in these settings may be required, the selection of topics is not complete. Priority is given to behavioral data over cultural data; readers will not find minimum levels proposed for the latter. By its very nature, culture is idiosyncratic, or, as is commonly stated today, "constructed." The authors in this volume do not examine meanings and symbols, nor is our work interpretation. Because each person constructs his or her own conception of culture, such a variable, though useful in exploring what it means to be human, is not amenable to comparative analysis—one of the goals of anthropology and one toward which we hope to contribute. Behavior, though no less structured and no less a product of people's experience than culture, has the advantage of being observable, measurable, and comparable across populations (see also Hunt, Chapter 9).

The authors do not claim that all the data they discuss need to be collected by everyone without regard for orientation. In this sense, this book differs in scope from earlier field manuals such as *Notes and Queries* or the *Outline of Cultural Materials*. The authors hope to engage the reader in a thoughtful dialogue about data collection. In this dialogue, they explore different *levels of intensity* of data collection. They hope to provide some useful resources and ideas about how to think through the choices made in the field and about what to include or exclude in a written report. Most readers will be interested in things other than the whole agrarian system; for them, the first level of intensity constitutes what we might call a *minimum data set* for an ethnographic report. This minimum data constitutes a baseline for understanding fundamental questions that keep appearing in the social sciences and to which we ultimately contribute (as discussed in Chapter 9). They also serve to locate geographically, environmentally, socially, demographically, and economically the people who are the subjects of study. To be widely useful, the data need to be standardized. This is our obligatory bow or pirouette to our colleagues in the social sciences who will be grateful for our thoughtfulness.[11] To gain their admiration, however, we will need to go much further analytically and theoretically than these minima, probing deeper into those variables most relevant to the basic objectives of the study. The authors in this volume propose other levels of data collection beyond the minima but also try to abide by the most useful standards available in the social, physical, and biological sciences.

Purpose and Scale

Before setting out to discuss what data are (or should be) minimal or standard, it is important to relate data issues to the purpose for which the study

is being conducted and to the scale appropriate to that objective. These two dimensions, purpose and scale, are closely interrelated, and discussion of one inevitably will have the other as contingent.

For studies that are broadly ethnographic in purpose and village-scaled, the proposed minimum data are likely to be the appropriate level of intensity. Only in some variables (for example, social organization) would a broadly defined study need to go into greater data intensification. It is more appropriate here to take advantage of the luxury of time and resources to engage in prolonged participation and observation and in-depth interviewing. This is the kind of study that yields detailed kinship analyses, intricate linkages between symbolic systems and practice, and precise microecological adjustments between people and local resources. Surveys and censuses are not uncommonly done; the second level of intensity provides suggestions for what to include in such household-level surveys to enhance data comparability.

More typical are studies focusing on a specific problem—e.g., pastoral adjustment to drought, adaptive responses to population growth, and shifts in household structure with declining supplies of labor. For these kinds of studies, investigators may wish to go to the second or even third level of data intensification when the variables proposed are central to the problem under study. Three of the most often neglected variables in classic anthropological writings and in contemporary training are demography, health, and nutrition. They have been included in this volume as substantial chapters not only because of their fundamental importance but also because of their past neglect. Ideally, the detail of these chapters will be taken not simply as a representation of their relative importance but rather as a resource to help readers understand how to integrate these variables into traditional approaches and strengths.[12] Further, these are variables that permit our findings to articulate with the work of social scientists, nutritionists, epidemiologists, and others who contribute to these areas of knowledge.

The problem-specific study can be executed at a number of scales, from the village level to the cross-national level. The larger the scale, the more reliant the investigator becomes on existing data sets. Several of the chapters in this volume address the kinds of caveats that use of such aggregate data require. All too often, anthropologists' awareness of the problems present in regional and national data lead them uncritically to dismiss their usefulness. Particularly in exploratory and time-limited research, these data offer a "window" into variables that may be worthy of attention and that may be overlooked unless we get familiar with these large-scale data sets. Climatic data, as per the minimum data set proposed by Wilken in Chapter 2, relies on the collection of rainfall and temperature figures over at least twenty years. Even if collected imperfectly or with a limited number of collection stations, such long-term data may be more precise than one-year

rainfall and temperature data collected in one village at one collecting point (often near the investigator's home) to predict trends over time. Depending on the range of variability present from place to place and from year to year, one-place-one-year data may be either useful or worthless. It is an excellent source as a complement to longitudinal data but not as a reliable measure of climate (an inherently statistical phenomenon).

A third category, one we have alluded to before and that is of growing significance, comprises studies concerned with development assistance, target population analysis, and social impact analysis. Commonly, such studies are of extremely limited time duration and aim to recommend certain kinds of interventions—whether to increase yields or how to increase income, reduce parasitism in humans or domestic animals, or decrease infant mortality. This kind of study is rarely village-level but is more commonly regional, sometimes even national in scale. It may focus not on all people in a region but on groups within the total population—the poorest of the poor, farmers without access to irrigation land, or women-headed households. Under these circumstances, the approach will surely not be that of participant-observation or key informant interviewing, although these may be used as complements to more systematic procedures. Such studies should rely on available statistics, informal group interviews (Rhoades 1985), focus-group interaction (Merton 1989), or "culturally adapted market research" (Epstein 1988). It is remarkable that, in a discipline that prides itself on always putting people first (Cernea 1988), anthropologists have been so slow in formalizing methods for training local people to generate their own data by making them partners in the research effort. The advantages of such a procedure become quickly obvious—people not only increase their capacity to control valuable information and use it for their own purposes but also increase both sample size and confidence in the responses when they understand the value of such research. Though it may not work in all cases, such participation has greater possibilities than most investigators have been willing to admit. The minimum data level is appropriate here; further data intensification may be appropriate depending on basic purpose and scale or investigator or local community needs.

In a recent publication, Epstein (1988) set out a list of "key cultural variables" that refer largely to social variables, most of which can be found within our minimum data set across the various chapters. As in time-limited research or comparable research, Epstein's key variables address basic considerations in human behavior, such as the culturally relevant unit of decisionmaking, how status is allocated, the structure of kinship, patterns of residence, rights to tenure in land, and dominant gender relationships. Unlike the minimum data proposed here, which have both academic and applied research in mind, Epstein's key variables are fine-tuned for development assistance using the insights of market-research and anthro-

pology. The standards proposed herein are consistent with the key variables proposed by Epstein but are more comprehensive in that they include such factors as climate, soils, crops, health, and nutrition, which are not included in Epstein's set. However, they may be less detailed on social variables than Epstein's, given that the objective is a minimal standard rather than comprehensive treatment of variables relevant to development studies per se.

Unlike the "rapid and reliable research methods for practicing anthropologists" proposed by van Willigen and Finan (1991), the standards offered here conceive of research as taking place along a continuum from time-constrained to prolonged field stays, from broadly ethnographic to highly specific and purposive research, from village-level to cross-national studies. Whatever the purpose, scale, or time availability, the standards proposed herein are graded from low intensity (minimum data sets) to medium intensity (second level of data collection) as a function of the above variables. The baseline is the minimum data set, viewed as a professional obligation to make basic information available to others. These data have been chosen for their broad availability, basic usefulness to comparison and theory, and potential to be combined with other data to generate indices of analytical value. The authors had to make some hard choices in making these recommendations, and they were not always happy to have to make such decisions. Readers may not be happy, either. Following standards is tough, and trying to get down to minima is even harder. Hopefully, readers will take them as a "first approximation," a step toward the emergence in the not-distant future of better standards. At this stage we can only hope they serve to initiate a constructive dialogue among researchers over the value and/or level of difficulty in collecting these data. It is wise to remember that any single data set may be easier to collect in one location than another and that any single minimum data set need not be central to a particular investigation. As Hunt makes clear in Chapter 9, there are an infinite number of questions to ask, but some have been of persistent significance in the analysis of human societies. We hope these minimum data get at fundamental variables in human behavior and propose levels of data collection to be implemented as appropriate. We leave it to others to fill in the outline we have provided and to propose improved standards for the comparative study of agrarian and other social systems.

The Minimum Data Set

The length of this volume might discourage some with the thought that what is proposed herein is hardly minimal. There is, fortunately, considerable overlap in the measures proposed by various authors, so that their collection does in fact serve multiple analytical purposes. Some of the minimum data are qualitative in nature and obtainable by simple observation or informal interviews; others can be derived from quantitative national aggregate data or a quick village census. This difference is not surprising given the disparity between, say, variables such as climate and diet. Whereas climate is a statistical expression of weather potential over time, diet is a product of food habits, agricultural productivity, local distribution systems, and access to tradable foods in particular populations. Aggregate diet statistics serve little purpose other than to justify food aid. Even then, a particular local measurement such as weight-for-age (to establish the percentage of children with acute malnutrition) may be a better measure than total caloric deficit, which fails to identify at-risk individuals.

We begin our data sets with climate. Climate is one of the most determinant variables in hunter-gatherer and agropastoral systems. Farmers and hunters devote considerable effort and cultural attention to mollifying its effects on plants and animals through various degrees of intervention (e.g., crop varietal selection, irrigation, fertilization, wind rows, and mobility). Though farmers cannot change climate, they can modify the microclimate at the level of the plant or animal, thereby reducing the impact of drought, wind, sheet erosion, and frost. As with all other variables that will be discussed, there is an infinite variety of climatic data that could be collected, but only some are available on a worldwide basis.

The tendency in most studies has been to cite total annual rainfall and average annual temperature, two widely available and often reported statistics. These constituted an earlier implicit standard, before we understood as clearly as we do now that annual averages make it difficult to assess agricultural potential and the impact of temperature and rainfall on crop development and seasonality. Annualized data swamps variability—which, we have come to appreciate, can be quite high. Wilken proposes in this volume a new standard that is not much more onerous to obtain but is more useful than the earlier one. He proposes collecting and reporting daily maximum and minimum temperature and daily precipitation based on a twenty-year cycle.[13] These two minima can be combined and aggregated to produce useful indices such as potential evapotranspiration, drought probabilities, heating degree days, continuous days of rain, number of continuous rainless days, monthly rainfall, seasonal patterns of precipitation, and, of course, the old standard of annual temperature and precipitation. What these new minima do is enhance our analytical capabilities and link the data to all sorts of processes relevant to crop and animal growth—and the strategies humans use to address these climatic contingencies. The value of this more disaggregated data is to increase substantially our ability to understand farmer decisions—which presumably is one of the things we are trying to understand in agrarian studies. This data is no more time consum-

ing to obtain than the annual means: If one exists, so does the other. However, anthropologists may have to specifically request the new minima because statistical offices assume that nonclimatologists only want to know the annualized data. Even purely ethnographic studies may be (or should be) concerned with drought or frost probabilities—an impossible task without the daily data.

These minima, as well as others in this volume, could change in ten to twenty years. Considerable advances are promised in the not-distant future that may permit the use of satellite imagery and georeferenced information systems to estimate climate's influence on biomass, net primary production, rates of secondary growth, leaf moisture indices, yields, and other consequences of climate. If this occurs, many new possibilities will come up. However, the minima proposed will continue to be useful whether these advances occur or not—and are more likely to be broadly useful than more precise but technically demanding information.

It is difficult to evaluate any agrarian system without recognizing the difference that variation in available soils and crops makes in the returns farmers can get from their fields. These data are not always reported by social scientists, who seem to believe they are purely agronomic variables. However, if we take the time to talk to farmers about their soils and crops, we quickly discover that every farmer is a folk agronomist capable of making remarkable associations between soils and plants. They can predict crop response under variable conditions, and in some cases they can build up soil fertility on otherwise poor soils (e.g., by using green manures, mulching, and applying village garbage). Soils data should be available from local, regional, or national agriculture departments or extension services. Given the difference that scale can make, this information should be sought, preferably for the village or region of interest to the study. Given that soil classification taxonomies vary a great deal from country to country, it is useful to report "typical" soil analyses for each major soil type present to permit assessment of chemical and textural characteristics. In Chapter 3, Nicholaides and Moran also propose the use of the Fertility Capability Classification (FCC) as a useful shorthand to bring out the most limiting factors in the "plow layer" (0–20 cm surface layer), which is the effective rooting area of most crops. Reporting soil taxonomies may be less useful, as most classification schemes ignore the plow layer and concentrate on the subsurface layer. Of particular importance are pH, nitrogen, phosphorus, potassium, cation exchange capacity, organic matter, calcium, magnesium, and carbon. Soil maps are widely available, and reproducing these on a report makes the data readily usable as long as the scale of the map is reported. A world soil map at 1:1,000,000 scale is in preparation and constitutes an improvement over the 1:5,000,000 scale of the current Food and Agriculture Organization map.

Crops are another matter. It is very easy to lapse from minimal to complete data on crops. As Netting et al. and Hunt point out in Chapters 4 and 9, respectively, minima should include a list of crops planted, with indication of both local and scientific name. This is not a difficult requirement; a conversation with a knowledgeable agronomist can ensure that local terms are scientifically precise. To list crops unaided is to invite error unless one's training includes considerable botany. In getting the list of crops, it is helpful to discuss informally what is planted when, permitting the production of an agricultural calendar, which is also a minima under social organization of labor. Asking what is planted when and by whom further permits collection of minima about the relation of gender, age-grade labor responsibilities, economic stratification, and food habits to the agricultural system. Further asking what crops are planted where allows exploration of tenure arrangement, sexual division of labor, use of irrigation or terracing, crop use after harvest, and land fragmentation.

The list of crops should also differentiate between crops planted and consumed, on the one hand, and those strictly or largely sold, on the other. Moreover, it should distinguish between staples and supplementary foods and give an assessment or estimate of yields (per man-hour or man-day and per unit of land). These are useful not only as economic minima but also as nutritional minima. Data should be reported in metric (Système International d'Unités, or SI) equivalents as well as in local measures. These must be explicit in the report to ensure equivalencies and avoid errors arising, say, because of differences between British and U.S. pints, gallons, and bushels (as Hunt points out in Chapter 9). Using metric standards avoids these sorts of errors in the use of colleagues' data.

This is no trivial matter, and its pursuit could yield interesting findings. For example, the system of weights and measures used in Bolivia has three layers.[14] At the core is the indigenous system, still the most important. Mixed with this is the traditional Iberian system of weights and measures imposed by the Spanish; finally, there is the metric system. The indigenous system is not uniform; it varies by ethnic group, each occupying distinct agricultural and ecological niches. Trans-Andean trade led to the emergence of some widely shared measures, such as the *costal,* a volume measure. As a general rule, the volume of the costal varies with altitude, which reflects the ability of the beast of burden to carry a given unit of weight, or *carga.* Local people distrust the metric system because they fear that intermediaries rig the scales against the seller. Intermediaries in fact exploit both producers and consumers by manipulating the meaning of "standard measures": In the countryside the intermediary may buy a carga weighing the local standard of 250 pounds, whereas in the city they can sell a carga that has a standard weight of 216 pounds. Thus, they could make a profit even if there were no difference in price paid—which, of course,

there is. The weight of a standard volume may vary by season and by the quality of the harvest that year. Commerce is rarely a cut-and-dried affair, and thus it is preferable to report harvest weights. A given weight must be understood as a function of complex negotiations and understandings between buyers and sellers. Lack of attention to these differences impoverishes a report and makes comparison impossible without provision for equivalencies.

In Chapter 4, Netting, Stone, and Stone make the task of studying the social organization of labor and agrarian production clear, if not simple. The three levels of intensity of data collection are most clearly explicated in this chapter. They note that the minima are names for things (the ethnoecology, if you will) and the list of activities in space and time (as noted earlier in discussing crop minima). Basically, it is a matter of reporting who does what, when, and how. It is remarkable that this requirement needs to be explicitly stated, but ethnographic reports often fail to take note of these baseline (if mundane) aspects of agrarian life.[15] It is worthwhile to note local people's perception of the constraints they face. They are far more keenly aware of what most limits their achievement of production goals (i.e., land, labor, pests, soil fertility, slope, or exploitation by others) than is any visiting scientist. This information alerts readers to particular variables that may be worth pursuing or may help interpret the behaviors reported elsewhere.

After being produced, goods are either consumed or traded.[16] The minima for understanding the distribution of what is produced is to report what is distributed by whom, and when and where it is distributed. How is it organized? Are trade and distribution largely local/regional processes, or are local producers linked to external national and international markets? On the consumption side, reports should minimally take note of what is eaten (again, local and scientific names), whether it is a staple food or a supplementary food, how it is obtained and prepared by households, when in the year it is available, and by whom it is consumed. It is basic to annotate differences in the age/sex distribution of food consumed should important differences be present. Informal interviews are probably the best way of getting at this data.

Perhaps no information is more basic than demographic data, yet probably no other kind is so often overlooked or improperly reported. As Hern notes, we must begin by reporting the *total* population. As Netting et al., Dufour and Teufel, and others in this volume point out, a population must be reported in relation to some explicit territory that encompasses it. Without this information, it is impossible to calculate (in persons per square kilometer) demographic density, density per unit of agricultural land, density per unit of irrigated land, and other basic indices relevant to the analysis of agrarian systems. Demographic data is best acquired via a

household census in small villages and from statistical offices in larger, more dispersed settlements.[17] Whether taken from local census or aggregate statistics, the minima should include not only total population but also its age and sex distribution in the standard five-year intervals.[18] Without standard intervals it is not possible to construct age-sex pyramids—one of the most useful data sets that can be reported in any study. The number of deaths and births in the past year is rarely reported and is recommended here as minima, as is the recording of who came and left the population in the past year.[19]

With this information, one can generate a number of important indices, including age-sex distribution, dependency ratios, infant mortality rate, crude birth rate, crude death rate, and rate of natural increase. In Chapter 8, these minima are confirmed by Fleming-Moran as being equally important in constructing indices of the health of a population. The percentage of newborns under 2,500 grams born per year is a particularly good proxy in assessing the health of both mother and child and is at least as informative as infant mortality rate. Other useful health minima are total days lost from work due to illness in the past two weeks and leading causes of death for infants (0–4 years old), children (5–15 years old), adults (16–44 years old) and seniors (45+ years old).

Finally, Hunt notes some very basic minima that all too often are overlooked: the latitude, longitude, and altitude of the study site(s); the availability of basic infrastructure such as roads, drainage canals, bridges, terraces; the level of technology that is commonly found (such as animal traction, mechanical traction, dibble-stick, pesticides, herbicides, fertilizers); and the general characteristics of the year in which the field data were collected (was it wetter than average? drier? or just average? were crops affected to the normal extent by pests, or more severely?). This may seem like quite a list, but in fact most ethnographic reports provide at least this much "general information." However, it is often chosen without explicit objectives. The minima proposed here offer broadly useful data that relate to major questions in the social sciences, that can generate other data and indices commonly used in analysis, and that argue for standardization of units to enhance data sharing and comparison.

Going Beyond the Minima

All the chapters in this volume go beyond the minima to more intensive levels of data gathering. These levels of intensity of data collection are hierarchically nested in each other, permitting relational approaches to the database and letting each author use his or her best judgment on the most fundamental data to collect for each data set.

Wilken notes that greater precision in the assessment of climate can be obtained from having data on insolation differences across the year, which permit a better assessment of evaporation rates and seasonal drought stress or wilting. Most of the additional data Wilken recommends are actually consistent with that suggested by Netting et al. and Nicholaides and Moran: socioeconomic and ethnoecological figures about management strategies of farmers to deal with existing local climatic constraints.

Soils data beyond the minima could focus on changes in pH, macronutrients, cation exchange capacity (CEC), and organic matter under different management regimes and crops. Of particular interest today is how populations manage organic residues to achieve sustainable yields. Attention and observation of these folk approaches to mulching and recycling will become increasingly important and ensure that one's work will be used rather than ignored. Beyond the minima, crop varieties and their characteristics can be discussed in terms of how well they resist drought, pests, and other problems and what their yields seem to be. Is yield important to the population? Is plant protection and reduction of risk important to them? Why?

Going beyond the ethnoecology of production and labor organization, Netting et al. suggest a gradual intensification beginning with household surveys to examine intrahousehold labor dynamics and gather detailed data on demography, health, exchange, and nutrition—thereby fulfilling many objectives simultaneously and efficiently. Demographically, at this higher level of intensity the focus ought to be on maternal and child health, especially mortality and fertility rates. Beyond the economic data in household surveys, a third level of intensity would have us explore the dynamics of marketplaces, which play such an important role in either encouraging or dampening producers' incentive, and use twenty-four-hour-recall surveys and diaries of food consumed to quantitatively establish dietary intake. Less intensive may be the anthropometric measurement of children, with special attention to age, sex, height, and weight. These can be used to establish their growth and development rates compared to "standard" populations. Such comparisons help determine whether the population experiences chronic or acute malnutrition, whether children of different ages are at different degrees of risk, and whether they catch up by the time they become adolescents and enter the reproducing population.

Conclusion

In this brief introduction I have not done justice to the complex and detailed chapters that follow. I have tried to tantalize the reader to go to the particular chapters and to engage each author in a dialogue on method. As

Kuper has reminded us, "advances in anthropology actually occur through the development of new observational techniques, the refinement of models, the definition of new foci and above all by the continual interplay of new studies and old studies; that is, by comparison" (1983:204). There is plenty of room in these tasks for creativity and imagination—without the need to revert to fiction. If our colleagues are provoked to question their ethnographic writing and data collection and are moved to come up with better (and still broadly applicable) standards, our task will have been successful. Each author has found this exercise difficult as well as thought provoking. Not one of them has felt that his or her ethnographic practice has lived up to the standards proposed herein. This is as it should be. If we are to strive in earnest for greater reliability, our studies will need to have more comparable data, and we will have to encourage, rather than discourage, restudies of previously researched communities. Our goal in this volume has been to start the process of discussion on standards for data collection and reporting so that data sharing and comparison may become more possible in the future than it is now.

Notes

The author thanks Robert Netting, Robert Hunt, and Sutti Ortiz for constructive suggestions on this discussion.

1. Pelto and Pelto (1978) remind us that researchers need to develop methods to protect themselves from their own biases and value judgments: "[T]raining in anthropology does not rid the investigator of conscious and unconscious biases" (p. 36). They cite several examples from the ethnographic literature.

2. Cf., for example, Royal Anthropological Institute 1874, Radin 1933, Epstein 1967, and Ellen 1984 in England; Griaule 1957, Cresswell and Godelier 1976 in France; Jongmans and Gutkind 1967 in Holland; and Murdock et al. 1971, Williams 1967, Pelto and Pelto 1970, and Bernard 1988 in the United States.

3. In the words of the volume, "to be a handy *aide-memoire* to the trained anthropologist doing field work and also to stimulate accurate observation and the recording of information thus obtained" (RAI 1951:27). It is still widely used in countries whose anthropological research communities are in early stages of development and is appreciated there because it takes little for granted—always good advice in training novices into the practice of a discipline.

4. Described by Ellen (1984:3) as "naive guidance for amateurs."

5. The sixth edition was delayed because of World War II. The committee formed in 1936 had to be reconstituted in 1947 and was able to produce the edition only in 1951. The committee that prepared it was an impressive one, including H. J. Fleure, Le Gros Clark, Evans-Pritchard, C. D. Forde, J. H. Hutton, Radcliffe-Brown, R. Firth, M. Fortes, and F. Myers. They were assisted by, among others, E. R. Leach, S. F. Nadel, F. B. Steiner, and I. Schapera. The volume was divided into two parts—physical anthropology and social anthropology—with the bulk of the 369 pages of text devoted to social anthropology.

6. The first edition of the *Outline* (1945) was a revision of a draft prepared

in 1937 and circulated to approximately "100 specialists in various fields of knowledge" (p. v).

7. This is not surprising, given that a discussion of method inevitably means exposure of the self, "from which many anthropologists have shrunk. . . . Nevertheless, the attempt has to be made if anthropologists are to retain credibility in the face of charges that their work is primarily a series of aesthetic constructs, a set of ego-trips into the exotic" (Ellen 1984:vii).

8. This myopia is not a purely anthropological failing. Ecologists have faulted some of their colleagues in a similar fashion for making the particular the basis for theory rather than seeing it as one case among many, requiring systematic testing and comparison to be validated to the status of theory (Franklin, Bledsoe and Callahan 1990).

9. The development of a postmodern anthropology has exacerbated whatever lack of standards of ethnographic reporting may have existed. Postmoderns revel in the uniqueness and individuality of ethnographic writing (some even making their ethnographic writing into explicit ethnographic novels and other forms of fiction). Insofar as such writing is judged to be great literature, it may be read beyond the current generation of postmoderns. In far too many cases, the ethnographies give too little information about the people, their ecological and economic context, their health, their growth and decline in numbers, or even their forms of religiosity. The aggressiveness of postmodernists has intimidated far too many colleagues into practicing social science outside the academy or into not teaching firmly grounded social science standards to a new generation.

10. Note how many of the sources cited in note two came out in the late 1960s and early 1970s.

11. And at other times critical of our data as well. But that is how sciences advance—by making improvements in the precision with which we can describe and explain phenomena in terms of reproducible data.

12. Thus, the topic most important to the core of anthropology, social organization, may very well be one of the shortest chapters in this volume, because most practitioners have considerable expertise in this area and may only require a brief discussion of minima to collect and report on this data. By contrast, health, nutrition, and demography are less often adequately covered in training; thus, the chapters on these subjects are longer, showing both the relevance of such data and how to collect it and report it.

13. The meaningful period of record is a function of the purpose of the study. Twenty years is generally acceptable for most purposes but not for all. The amplitude of events increases with time. It is probable that during a twenty-year period, higher and lower temperature and rainfall will occur than, say, in a ten-year period. However, twenty years may not be sufficient if the study is focused on erosion or on infrequent natural hazards such as earthquakes, which require a longer time-series for confidence.

14. I want to thank Jeanette Rawlings, who came up with this fascinating and complex system of weights and measures while pursuing her doctoral studies in the Sucre market region. This brief explication is but a summary of her rich material.

15. Again, in *Notes and Queries* anthropologists were advised in considerable detail on this type of data, but with a greater emphasis on kinship and politics (RAI 1951:63–171).

16. Consumption and distribution, by contrast, were given short treatment in *Notes and Queries* (1951:171–173).

17. *Notes and Queries* included demography among its basic data but warned against the use of data gathered by others (1951:59). It advised, instead, that the favored procedure was to personally carry out a village census. For populations too large to census completely, they gave advice on sampling and estimating population trends that is still useful.

18. The standard intervals are 0–4, 5–9, 10–14, 15–19, 20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64, 65–69, and 70+ for each sex and commonly laid out in a pyramid for effectiveness of presentation. The construction of innovative intervals may be useful for some questions, but it should not replace reporting standard intervals if data sharing and comparison are to be possible. Nor is it useful to aggregate intervals—say, by using a 50+ interval if the number of people over 50 is small—as this leads to data loss and reduces the potential for comparison.

19. *Notes and Queries* suggested enumerating name, origin, area, sex, age, names of parents with ages if living or with ages at time of death, kin or status group, association membership, material condition, and children with their ages (RAI 1951:61.

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2

Minimum Climate Data for Comparative Analysis in Agriculture

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Agrarian systems are largely responses to climate. In fact, the crops and management practices by which farming systems are commonly distinguished represent adjustments to opportunities and constraints offered by the environment. The primary element of environment is climate, followed by soils and vegetation, which themselves reflect the climate regimes under which they develop.

Although climate sets limits to plant and animal growth, agrarian systems operate in an environment not of absolutes but of climatic probabilities in which success is likely but not certain. The odds are improved by managerial strategies that include selection of climate-tolerant crops and varieties, scheduling of field operations, and climate-modifying practices. As Porter (1965) succinctly put it: "[R]isk is not given in nature, it is a settlement negotiated between an environment and a technology." There are also social strategies for identifying and coping with general climate conditions and extreme events. Thus, the relationship of an agrarian system to its climate is interactive and has three aspects: the climate itself, managerial responses, and socially and economically conditioned perceptions, evaluations, and strategies. Comparative analyses should include data on all three aspects.

Climate Data

Weather consists of atmospheric elements (such as temperature, precipitation, wind) and events (storms, droughts) that occur at a particular time.

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