

This article addresses issues involved in the usefulness of cross-cultural research in family sociology. Cross-cultural research is defined as comparative research in which the data base consists of quantified ethnographic reports. The article argues that the method has been underutilized by family researchers, and that studies that have employed the method have not been taken seriously by family scholars. Possible reasons for this situation are explored. The basic conclusion is that although there are indeed many limitations on the cross-cultural method, its potentials have not yet been fully realized or exploited by students of the family.

The Utility of Cross-Cultural Data

Potentials and Limitations for Family Sociology*

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This article has two objectives. First, the nature of cross-cultural research and the data banks available to interested researchers will be discussed briefly. Second, the utility of the cross-cultural method for developing and testing explanatory theory in family sociology will be assessed at some length. The method has been dramatically underutilized by family scholars in both sociology and anthropology, in part because many researchers are simply unaware of the availability of the requisite data banks. However, many scholars who are aware of the existence of the data simply mistrust them and attach no credibility to the results of analyses performed on them. There are indeed some real shortcomings inherent in cross-cultural data that deserve careful attention in both the conduct and the evaluation of research efforts. These shortcomings, however, do not render cross-

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cultural data useless for scientific purposes. By dealing explicitly with some of the major issues in the use of cross-cultural data, I hope to contribute to a broader understanding of how they may be used productively and how their misuse may be avoided.

In terminology employed here, "cross-cultural" and "comparative" are not at all synonymous. I previously employed comparative as a generic term referring to any form of research in which two or more social systems are systematically compared (Lee, 1982). Cross-cultural research is a subtype in which the raw data consist of ethnographic reports. These reports are (or have been) content-analyzed and quantified, such that the data that are analyzed take a form similar to survey data. The unit of analysis is the society: No smaller unit of analysis is possible.

This article is restricted to an explication of the cross-cultural method because the problems encountered in cross-cultural research and the means available to resolve or minimize them are, in operational terms, quite different than those that occur in other forms of comparative research. The discussion is also restricted to issues involved in cross-cultural surveys rather than the ethnographic method of data collection, except insofar as issues pertaining to data collection affect subsequent analyses.

CROSS-CULTURAL DATA BANKS

Cross-cultural data are available to scholars interested in survey research in two basic forms. The first is represented by the Human Relations Area Files (HRAF) that contain information on over 300 societies in the microfiche version possessed by many university libraries. The contents of the files are simply pages copied from ethnographic reports, categorized and organized according to topic within societies. Except for this topical categorization, these data have not been processed. However, the categorization facilitates quantitative coding processes as coders need look only at the information contained under identified topical codes for variables of interest rather than scanning entire ethnographies.

Second, cross-cultural data are available in coded (quantified) form in several sources, including the *Ethnographic Atlas* (Murdock, 1967) and the *Standard Cross-Cultural Sample* (Murdock and

White, 1969; Barry and Schlegel, 1980). The Ethnographic Atlas is an improvement and expansion upon Murdock's earlier (1957) World Ethnographic Sample that in turn grew out of the sample of 250 cultures he employed in his landmark study *Social Structure* (Murdock, 1949). Data for the societies contained in the Ethnographic Atlas have been published in the journal *Ethnology*, most notably in the major summary in 1967, with subsequent additions. The computerized version contains codes on approximately 40 variables for 1170 societies.

The Standard Cross-Cultural Sample (SCCS) contains more extensive data on a smaller sample of 186 societies. These societies were selected according to two primary criteria: geographic dispersion, to minimize the probability that correlations among cultural traits or elements are attributable to cultural borrowing or "diffusion"; and adequacy of ethnographic information. The original publication by Murdock and White (1969) explained the method of sample selection, listed the ethnographic bibliography, and provided limited information such as economic type, geographic location, and linguistic classification for each of the 186 societies. Since that time, codes for these societies on topics including economic practices (Murdock and Morrow, 1970), cultural complexity (Murdock and Provost, 1973b), community and political organization (Murdock and Wilson, 1972; Tuden and Marshall, 1972), sexual attitudes and behaviors (Broude and Green, 1976), initiation ceremonies (Schlegel and Barry, 1979), division of labor by sex (Murdock and Provost, 1973a), the status of women (Whyte, 1978), and socialization (Barry and Paxson, 1971; Barry et al., 1976, 1977) have been published in *Ethnology*. These works are collected in a volume edited by Barry and Schlegel (1980). Subsequent publications in *Ethnology* include variables involving political decision making and conflict (Ross, 1983), spousal relationships (Broude and Green, 1983), and parental acceptance/rejection and control of children (Rohner and Rohner, 1981). This list is partial and is intended to be illustrative rather than exhaustive.

The Ethnographic Atlas and the SCCS both have their own distinctive uses, as I will discuss below. In addition, it is possible to combine precoded data from the SCCS with original codes on the same societies (usually from the same ethnographies) obtained from the HRAF or other ethnographic sources. The microfiche version of the HRAF available at Washington State University, for example,

contains data on 122 of the 186 societies included in the SCCS, enabling researchers to code additional variables of interest. Ethnographic sources are also reported in each of the articles that has contributed codes to the SCCS, so the enterprising researcher interested in expanding this data set may return to the original descriptive account or accounts of each society whether or not they may be found in the HRAF.

Although there are other cross-cultural data banks in existence, these three are the most widely used and, overall, probably the most useful and accessible. Most of the material that follows pertains to the utility of these three data sources, and examples are drawn exclusively from them.

These data banks constitute a potentially valuable resource for family sociologists and others interested in the analysis of macrolevel properties and processes. Because many sociological generalizations pertain to societies as distinctive social units, and because sociology is often defined as the study of human society, it is incumbent upon us to employ societies as the units of analysis in at least some of our empirical investigations. The properties of social systems, about which many of us are prone to generalize, cannot be apprehended by studies of single social systems because in noncomparative studies these properties are necessarily constants. The scientific method requires variation in its objects of analysis. The properties of social systems may therefore be studied only through comparisons of multiple systems, and cross-cultural research is one method of conducting such comparisons.

However, studies employing cross-cultural data appear quite infrequently in the pages of family-related journals. Those that have appeared have received very little attention in the way of subsequent citations, although they have dealt with topics (kinship relations, mate selection, divorce, marital power, socialization, etc.) of general and pervasive concern to the discipline. This suggests either that the utility of cross-cultural data is actually very limited or that family scholars have not taken advantage of their potential.

I do not intend to turn this article into a plea for attention. I do intend to inquire into the reasons for the minimal effect of cross-cultural studies on the progress of the discipline. The main reasons for this may be that family scholars who are not themselves involved in cross-cultural research feel either that the method and findings of

cross-cultural studies have no relevance for contemporary issues or that the method is scientifically unsound and the results of studies that employ it are thus untrustworthy.

These objections may be further specified as a series of potentially useful and legitimate concerns about the quality of cross-cultural research and its relevance to the cumulation of knowledge. The following list of objections to cross-cultural surveys is intended to be representative rather than exhaustive, but does reflect what I believe to be the most common and serious questions regarding the utility of the method.¹

These objections generally take one or more of the following 7 forms:

- (1) The ethnographic method of data collection produces observations of unknown reliability and validity.
- (2) Many ethnographic studies were done some time ago and are no longer accurate descriptions of the cultures to which they pertain.
- (3) Error is inevitably introduced in the process of codifying or quantifying ethnographic observations for purposes of comparison.
- (4) Samples of cultures are inherently nonrandom and nonrepresentative, minimizing or eliminating investigators' abilities to construct inferential generalizations or estimate population parameters.
- (5) Correlations among cultural traits may be attributable either to "functional" relations between traits or to cultural borrowing (diffusion), and these two kinds of explanations cannot be distinguished.
- (6) Cultural traits are inherently incomparable across cultures because each trait derives its meaning from the cultural context in which it is embedded and this meaning is necessarily lost in the process of comparison.
- (7) Because cross-cultural data banks contain no information on modern industrial societies, the information they do contain is irrelevant to contemporary issues and concerns.

Each of these points is valid to some degree and in certain contexts. Each one must be considered by researchers interested either in doing cross-cultural research or in evaluating the utility of such research for other purposes. However, singly or collectively, they do not merit the conclusion that the cross-cultural method in general has no value. The remainder of this article consists of a preliminary, but I hope

systematic, evaluation of each of these objections to the cross-cultural method.

METHODOLOGICAL ISSUES IN CROSS-CULTURAL SURVEYS

Even the most dedicated producer or consumer of cross-cultural research must be aware of the circumstances under which, and the degree to which, the method is appropriate and useful. Such awareness also allows the researcher to anticipate certain problems in the conduct of his or her research, and to take steps to minimize the effects of these problems. I will deal here with the 7 problems listed above; the order is arbitrary.

(1) The ethnographic method of data collection produces observations of unknown reliability and validity. Researchers accustomed to the rigorous measurement standards of contemporary survey and experimental research often hesitate to make use of descriptive statements based on simple observation. Yet such statements constitute the raw data on which cross-cultural analyses are based. The ethnographic method consists almost entirely of nonparticipant observation and the use of "key informants," purposively selected on the basis of their presumed knowledge of their own cultures. The ethnographer observes, interrogates, and records observations. He or she normally can produce no interview schedule or questionnaire, no list of items comprising scales, no evidence of the reliability or validity of measurement devices, and certainly no evidence that the observations that he or she actually made are representative of all possible observations in the culture in question. How, then, can we place any confidence in ethnographic data?

Despite the common elements of ethnographies and the basic constants of the ethnographic method, it is not particularly sensible to conclude that the method itself is either good or bad. Ethnographies vary in quality just as do surveys and experiments. No scientific study is valuable or worthless simply because of the method of data collection it employs; the critical issues are how well and how appropriately the method is implemented.

Within anthropology the ethnographic method is quite highly developed and has been for some time. Ethnographers are not simply

cultural journalists. Naroll (1962, 1970) and others have proposed methods of "data quality control" regarding ethnographic descriptions that rely primarily on the known qualifications of ethnographers in terms of training, time spent in the culture, knowledge of the language, and so on. These factors have been taken into account in the construction of cross-cultural data banks. The ethnographies used for the *Ethnographic Atlas* and, particularly, the SCCS were explicitly and carefully chosen for trustworthiness according, in part, to the qualifications of the ethnographers. The information contained in these data sets was not provided by wandering missionaries, as is commonly imagined, but by trained ethnographers. Furthermore, in the substantial majority of cases in the cross-cultural data banks, multiple ethnographies were consulted by the coders. Thus, the criterion of replicability can be (and has been) applied to the evaluation of ethnographic descriptions.

However, these points do not eliminate the possibility that multiple ethnographers may agree on erroneous descriptions. The task of accurately describing the total way of life of a group of people is enormous and sins of both omission and commission are to be expected. In order to assess the implications of the inadequacies of ethnographic data that must inevitably exist, although we cannot always identify the specific errors, we must consider one more property of ethnographic data and one property of their use in cross-cultural surveys.

The societies studied by ethnographers are typically, if not uniformly, small and relatively undifferentiated. The method is less appropriate for the study of large, complex, heterogeneous social systems.² This is why such systems are not included in cross-cultural data banks: Other methods of studying them are preferable. The ethnographic method, however, needs to be evaluated in the context of the type of social system to which it is applied. The ethnographer's task is to describe the culture of the group he or she observes. Culture consists of shared patterns of behavior, thought, belief, ritual, and so on. In other words, the ethnographer looks for and reports the common elements of the way of life of a relatively homogeneous group of people, and it is that very homogeneity that is the object of the ethnography. This does not imply that the members of undifferentiated cultures are all alike but, rather, that their similarities and commonalities, not their differences, are the objects of ethnographic descriptions.

Research methods employed in differentiated societies, such as the sample survey, are designed to tap individual heterogeneity in behavioral traits, attitudes, and so on, to quantify this heterogeneity, and to ascertain whether variation in one property of a unit (individual) is connected to variation in another property or properties. The ethnographic method does not yield data on intrasocietal, interindividual variation, nor is it intended to do so. Instead, it yields observations on intrasocietal homogeneity; variation is given by comparisons of multiple cultures that differ from one another. In this respect the ethnography differs from the survey in purpose and, therefore, in kind not in degree. It is not a less preferable means of accomplishing the same task but, rather, a method that is uniquely adapted to the accomplishment of a specific purpose: the description of culture. A researcher whose objective is to describe the culture of a relatively small, homogeneous group of people would be foolish to draw a sample and conduct a survey; the ethnographic method is much better suited to the task.

But even the most highly qualified ethnographer may misdescribe some element of a culture. Informants may disagree, selective perception cannot be entirely eliminated by training, an unusual event may be mistakenly taken as typical, and so on. Nothing said thus far should imply that ethnographic descriptions are inherently or uniformly accurate, although they are probably much more accurate than most nonethnographers believe.

Admitting the possibility (indeed, the inevitability) of at least occasional ethnographic error, what are the implications? In evaluating these implications I make two assumptions. First, it is likely that ethnographer error is random rather than systematic across large samples of ethnographies.³ In other words, observational errors are essentially uncorrelated. Second, the task of the cross-cultural researcher is to document and explain regularities or consistencies in patterns of human behavior. That is, to establish "explanatory and predictive principles . . . to which the individual phenomena conform and by virtue of which their occurrence can be systematically anticipated" (Hempel, 1952: 1). In the search for such regularities random measurement error introduces noise into the explanatory system and operates to reduce the magnitude of observed correlations among cultural traits. Ethnographic measurement error thus increases the probability of making an error of Type 2—failing to reject a false null hypothesis—and decreases the probability of

making an error of Type 1—rejecting a true null hypothesis. As Naroll (1968: 263) points out, “evidence of random error is evidence that the true correlation must be even higher than we suppose. For random errors tend to lower correlations, not raise them.” Thus, to the extent that ethnographic error affects correlations among cultural traits, it operates to produce conservative estimates of these correlations and, consequently, conservative (stringent) tests of hypotheses.

There are obvious limitations on the utility of ethnographic reports. However, these limitations do not make them unsuitable for use in cross-cultural surveys as long as researchers are aware of them and adjust their inferences and conclusions accordingly.

(2) Many ethnographic studies were done some time ago and are no longer accurate descriptions of the cultures to which they pertain. This concern has considerably less scientific merit than does the first. Although cross-cultural data banks may indeed give researchers and students an increased appreciation of the tremendous range of human behavior, in no other sense are they useful for pure description of any particular or general empirical reality. The fact that a specific culture may no longer exist as described, if indeed it exists at all, makes the description more valuable as the information it contains can no longer be reproduced.

The cross-cultural researcher is, of course, interested in the formulation and testing of general propositions that are independent of time and space. The fact that an ethnography done in, for example, 1880 is not an accurate description of its subject culture as it exists in 1980 does not mean that the 1880 ethnography has become obsolete but, rather, that the culture has changed. Nor does this fact make the ethnography any less valuable for testing hypotheses about relations among variables. The case possesses values for variables that were presumably accurate at the time of observation: There is no reason to conclude that these observations become less useful over time.

Furthermore, there is no reason that a culture described in 1880 cannot be compared with one described in 1980, unless there are historical connections between the cultures (see point 5 below). As long as the cases are independent, the time frame or historical period from which observations are drawn is immaterial to subsequent comparative analyses. If we are interested, for example, in studying the relation between type of economy and marital structure (Lee,

1979) and we find that polygyny occurs most frequently in horticultural societies (Osmond, 1965), more recent evidence from formerly horticultural societies to the effect that polygyny is no longer practiced does nothing to disconfirm the hypothesis that polygyny and horticulture are related. One might, of course, perform a historical analysis to ascertain whether changes in economic type are followed (or preceded, for that matter) by changes in marital structure, but this would be another type of study and would constitute an additional independent test of the hypothesis.

In some ways earlier ethnographic reports may be more valuable than recent ones. Murdock and White (1969), in constructing the Standard Cross-Cultural Sample, generally selected the earliest period for which satisfactory data were available for each society in order to avoid possible effects of contact with European cultures in later years. None of the cross-cultural data banks offers an accurate description of the world's cultures as they currently exist, but description is not their purpose. They do offer data that are suitable for the testing of hypotheses involving relations among the properties of social systems. For this purpose, which is also the purpose of cross-cultural research, the time period in which the observations were made is irrelevant.

(3) Error is inevitably introduced in the process of codifying or quantifying ethnographic observations for purposes of comparison. Because the raw data employed in cross-cultural surveys are written descriptions of cultures done by many different ethnographers, these descriptions must be coded or quantified in some form to allow for statistical analysis. The coding process, which is similar to content analysis, is a difficult task. The data in the Ethnographic Atlas and SCCS have already been coded, whereas researchers employing the HRAF or other ethnographic sources must do their own coding.

In evaluating the utility of precoded data such as the SCCS, several criteria are important. First, the variables must be clearly defined both conceptually and operationally. This is critical so that users of the data know exactly what variables they are analyzing, and also to impart some confidence that both coders and survey researchers agree on the meanings and indicators of the variables. Variable definitions, dimensions, and scale points are generally quite clearly described in the articles that have contributed codes to the SCCS, but there is variation in the quality of these descriptions both

across articles and across variables within articles. It is vital that the survey researcher investigate thoroughly the definitions and scales employed to measure the variables of interest in each study, and that authors of cross-cultural surveys pass this information along to their readers. The adequacy of these definitions must be evaluated on a case-by-case basis, so information necessary for these evaluations must be transmitted through all stages of the research process. Unfortunately, this has not always been the case in cross-cultural family research; our performance in this tedious but necessary task could certainly be improved.

The most common method of maximizing reliability in the coding process for the precoded data sets has been to have some or all of the ethnographies coded by two or more individuals working independently. The coders then compare results and arrive at consensus in cases of disagreement. If the disagreement cannot be resolved, the variable is either coded as missing or an indication of limited confidence in the final value is entered in the data set. My preferred method involves a variation of this procedure in which each variable (for each society) is coded independently by two coders and their original codes are retained for subsequent analyses. This allows the reporting of intercoder correlations that may be treated as estimates of reliability. If either coder decides that there is insufficient information to make a judgment, the case is treated as missing. If the two codes differ substantially, the case is also dropped from further analyses. Final values are assigned by summing the two judges' codes, which is mathematically equivalent to taking the mean; this moderates the effects of extreme codes. This procedure provides the reader with the maximum possible amount of information on the reliability of measurement operations.

Reliability, however, does not guarantee validity. The best indicator of the validity of cross-cultural codes is evidence that the resulting variables behave as expected in relation to other variables according to some clearly articulated theory. This is one of several reasons that cross-cultural surveys should be conducted only to test theoretically based hypotheses, not to search for correlations that will be subjected to *post hoc* explanations. If two variables are correlated in a manner predicted by a hypothesis deduced from a sound theory, the hypothesis becomes more credible. Such an outcome also suggests that the variables as measured are reasonable indicators of the variables as conceptualized.

If, on the other hand, a variable does not behave as expected there are two general types of appropriate explanations that cannot always be perfectly distinguished. First, it may be that the hypotheses involving this variable have been improperly deduced from theory, are insufficiently specified in terms of scope conditions, or are just plain wrong. Second, the variable may have been poorly measured. It is perfectly possible for multiple coders to agree on the same mistakes. But, once again, such occurrences will normally result in the error of underestimating true correlations or failing to reject a false null hypothesis.

The message here is identical to a point made under (1) above, to the effect that measurement error, if it is random rather than systematic, operates to decrease rather than inflate the magnitude of observed correlations among variables (Marsh, 1967; Kobben, 1968; Naroll, 1968; Lee, 1982). According to the conservative standards of science, underestimation of true correlations is the less serious error. Random measurement error will have this consequence in cross-cultural studies whether the error is ethnographic (contained in the original observations) or ethnologic (resulting from quantification processes). There is no doubt that such error occurs in cross-cultural analyses, as in any other form of social or behavioral research, but the admitted existence of measurement error does not render cross-cultural data sets or analyses that employ them useless. Instead, as in any other research, caution must be exercised.

(4) Samples of cultures are inherently nonrandom and nonrepresentative, minimizing or eliminating investigators' abilities to construct inferential generalizations or estimate population parameters. Cross-cultural data banks are nonrepresentative, in part, because they contain no entries for modern industrial societies, for reasons discussed earlier. Less commonly recognized is the fact that these data banks do not constitute representative samples of non industrial societies. Murdock (in Marsh, 1967) estimated that roughly 5000 distinct societies have existed in human history; of these, 1170 are included in our version of the Ethnographic Atlas, which is the largest existing cross-cultural data bank in terms of sample size. However, those societies contained in the Ethnographic Atlas are not representative of those not included. Smaller societies, those that ceased to exist some time ago, and those that left no records are either underrepresented or not included at all. Furthermore, in many cases

ethnographic sources on a "society" are actually descriptions of a distinct, relatively autonomous subgroup of the society that may or may not be representative of other subgroups. The societies contained in the Atlas constitute an availability sample in the strictest sense of the term: They are societies on which ethnographic information is available.

Murdock (1967: 114) suggested that researchers should sample from the Atlas, rather than using its entirety, to test hypotheses. Although this is frequently sound advice, the reasons for this strategy have been widely misunderstood. A random sample of the societies contained in the Atlas yields neither a random nor a representative sample of any population except the Atlas itself; a random sample of an availability sample does not allow inference or parameter estimation any more than does the availability sample. Sampling from the Atlas (on a stratified rather than a random basis) is advisable because it can correct the differential representation of regions of the world that characterizes the Atlas and because it can minimize the probability that observed correlations are attributable to diffusion (discussed below). But there is no way to obtain either a random or a representative sample of the world's cultures from cross-cultural data banks, because certain kinds of cultures are not included in these banks and thus have a zero probability of being included in any sample drawn from them.

Cross-cultural researchers must unavoidably restrict their efforts to types of research in which the absence of a random sample is not a major drawback. As experimental researchers have known for decades, it is quite possible to conduct social and behavioral research on nonrandom samples and to advance the cause of science by doing so. One does not predict the outcome of a presidential election, or estimate any other population parameter, with a nonrandom sample. However, if one wishes to test a hypothesis that X is related to Y, one needs a sample in which X and Y vary. If X is indeed related to Y in the manner predicted, the hypothesis becomes more credible. The objective here is not to estimate population parameters on the basis of sample statistics, but to subject hypotheses to empirical tests. Cross-cultural data banks are useful for this purpose and this purpose only. Fortunately, random samples are not essential for this purpose.

(5) Correlations among cultural traits may be attributable to either "functional" relations between these traits or to cultural borrowing

(diffusion), and these two kinds of explanations cannot be distinguished. If we are interested in why culture A possesses trait Y, there are at least two general types of explanations that must be considered. First, trait Y may be a consequence of the existence of trait X in the culture; evidence that X and Y covary would support a hypothesis of a causal, systemic, or other functional relation between these traits, and culture A would constitute an instance of their covariation. But second, culture A may possess trait Y because the members of culture A observed the members of neighboring culture B practicing this trait and adopted it. This is the process of cultural borrowing or diffusion. It is problematic because diffusion may produce correlations among cultural elements that appear to be functional or causal but that are, in fact, due to historical connections between cultures. It is a problem of nonindependence of observations.

The problem of diffusion, although potentially quite serious, has been vastly overrated as an impediment to accurate interpretations of correlations among cultural traits. There are several reasons for this conclusion, the most important of which is that there are now ways of checking for the influence of diffusion. Naroll (1968) summarized a number of methods he developed that are based on the premise that diffusion proceeds along lines of geographic proximity: It is, in other words, a consequence of direct contact between members of different cultures. This is an eminently reasonable assumption given that we are dealing with cultures that lack advanced communications technology. Thus, geographic proximity may be employed as a variable and its influence may be controlled statistically.

Alternatively, samples of cultures may be selected that are widely separated in space and/or time. This is why Murdock (1967) suggested sampling from the Atlas; the Atlas contains many cultures that have historical connections between them. The SCCS (Murdock and White, 1969) was created for precisely this reason; it consists of cultures that are widely dispersed and, therefore, correlations among their characteristics are unlikely to be due to diffusion. We have made it a routine practice to check any correlations observed from the Atlas on the SCCS. A correlation that exists in the Atlas but not the SCCS is probably diffusional; one that exists in both samples reflects, in all probability, a functional relation. As samples of cultures that are not geographically proximate are now routinely available, it is no longer the case that diffusional and functional correlations cannot be satisfactorily distinguished. It is still true, however, that scholars

should treat analyses that fail to consider and check for the influence of diffusion with considerable skepticism.

(6) *Cultural traits are inherently incomparable across cultures because each trait derives its meaning from the cultural context in which it is embedded, and this meaning is necessarily lost in the process of comparison.* This philosophical position, known as “historicism” or “holism,” developed in anthropology around the turn of the century as a reaction to unilinear evolutionism, a school of thought that dominated comparative theory at the time. Its basic premise is the unquestioned observation that all cultures are unique. Each element of a culture derives its meaning and significance from the complex of cultural elements to which it belongs, so abstracting a few elements from multiple cultures for comparative purposes is invalid because the meanings of abstracted elements cannot be retained. Thus, only whole cultures may be compared. But cultures are unique, and unique phenomena provide no basis for comparability (Zelditch, 1971: 276). Therefore, the comparison of cultures is a literal impossibility and nothing of any benefit may be learned by such comparisons.

The logic of historicism has many contemporary adherents and instances of its application occur with considerable frequency in debates over the merits of comparative research. For example, Goody (1973) argued that polygyny has different meanings in Asia than in Africa and that instances of its practice drawn from these two regions are thus incomparable. Holy and Blacking (1974), in a critique of a cross-cultural study of marriage ceremonies by Rosenblatt and Unangst (1974), contended that real knowledge is generated only by intensive studies of whole cultures:

Although case studies establish nothing of generality, only intensive contextual analysis of marriage ceremonies within single societies can advance causal explanations which will take into account the intentions, motives and reasons of the actors themselves (Holy and Blacking, 1974: 60).

Some scholars (Zelditch, 1971; Elder, 1976) consider the difference between the comparative and historicist positions to be a “paradigm clash,” not subject to resolution by logic or evidence. Others (Przeworski and Teune, 1970; Warwick and Osherson, 1973) contend

that there are logical fallacies contained in the historicist argument that make its conclusion, that meaningful comparison is impossible, demonstrably incorrect. I agree with the latter position.

I grant the points that all cultures are unique in terms of the configurations of traits or elements of which they are composed. It is also true, as Zelditch (1971) points out, that comparisons must be based on the common properties of discrete phenomena. However, folklore tells us that snowflakes are also unique. Snowflakes nonetheless possess certain common properties that allow their comparison and categorization: Indeed, we (presumably) know they are unique because we have compared them. If each snowflake was entirely different from every other snowflake, we would have no generic term by means of which to refer to this class of phenomenon; indeed, they would not constitute a class. Snowflakes share certain properties by means of which we distinguish them from raindrops, hailstones, third basemen, and other aspects of our experience. Furthermore, these properties are measureable and allow for comparisons among different types or classes of snowflake. In other words, snowflakes possess different values of a common set of variables. The presumed fact that no two snowflakes possess identical values of all possible variables in no way prevents us from analyzing their properties.

If it is true that all cultures are unique, it is also true that all individuals are unique. The application of the historicist argument to comparisons of individuals seems warranted by the logic of the position, but would lead to the conclusion that there can be no general laws of human behavior. As W. H. Auden (quoted in Brodbeck, 1968: 1) commanded: "Thou shalt not sit with statisticians nor commit a social science." If we accept this dictum we admit that there is no meaning that can be attached to any social or behavioral research, and that knowledge of human behavior may be generated only by intensive case studies of single individuals. The evidence of over a century of systematic behavioral science does not support this conclusion.

I agree that the comparison of whole cultures is impossible, but I feel that this is due to the sheer magnitude of the task. Indeed, the description of a whole culture is impossible for the same reason. It is undeniably true that the phenomena that are compared in cross-cultural research are specific properties of cultural elements that are abstracted from their respective cultural contexts, and that meanings

attached to these elements are lost in the processes of abstraction and comparison. The conceptual vocabulary of science serves to categorize and dimensionalize common properties, not idiosyncratic meanings. The common properties, however, are the objects of comparison.

Social phenomena do not have a property of being "comparable" or "not comparable." "Comparability" depends upon the level of generality of the language that is applied to express observations. The response to the classical objection to comparing "apples and oranges" is simple: they are "fruits" (Przeworski and Teune, 1970: 10).

Thus, we may grant the point that all cultures are unique in their totality, but this does not prevent us from comparing their common properties or from learning something via the comparison. We will not learn about the unique meanings attached to social or cultural phenomena in different systems from this process, but this is not the sort of knowledge that is the objective of comparison. For this, the ethnography itself is the best source. The utility of the cross-cultural method lies in its potential for increasing our understanding of the general laws that govern the systematic interrelationships among the common properties of human experience. The conclusion that there are no such general laws does not follow from the premise that all cultures are unique.

(7) Because cross-cultural data banks contain no information on modern industrial societies, the information they do contain is irrelevant to contemporary issues and concerns. To deal with the issues involved in this point, we need to once again consider the generality of scientific concepts and their empirical indicators. It is true that many of the specific variables contained in cross-cultural data banks have no direct counterparts within the confines of the United States. However, at a slightly higher level of abstraction there are numerous parallels and many ways in which our understanding of contemporary issues might be augmented by analyses of cross-cultural data. Perhaps the best way to make this point is by example.

Some time ago, Ackerman (1963) conducted a study of cross-cultural variation in divorce rates. The frequency of divorce was estimated for 62 societies from the HRAF, and this variable was correlated with other properties of these societies. Among other

things, Ackerman found that in societies with bilateral descent systems those that practice community and/or consanguine endogamy have lower divorce rates than those that are exogamous. Although the United States has a bilateral kinship system, there appears to be little general value in this finding as rules of endogamy and exogamy are virtual constants in the United States and do not vary across individuals. That is, they are system properties and thus cannot be used to explain variation in the occurrence of divorce within systems.

However, Ackerman (1963) employed cultural rules regarding endogamy and exogamy as indicators of a more abstract and general concept: the extent to which the affiliations of spouses are "conjunctive" or "disjunctive." He concluded that divorce rates are low when the social affiliations of spouses are overlapping (conjunctive, as indicated by endogamy), and high when spouses belong to and interact with different social networks (disjunctive, as indicated by exogamy). Although the exact measurement of these concepts cannot be replicated in contemporary American survey research, the applicability of the theory is obvious. Nonetheless, this study has rarely been cited by family scholars who are not themselves engaged in cross-cultural research.

Pearson and Hendrix (1979) built on the work of Ackerman and others and, based on an examination of cross-cultural data, suggested that the divorce rate varies directly with the status of women. This is, or should be, an exceedingly controversial hypothesis. It clearly has direct implications for the current situation in the United States and other contemporary societies. Obviously, divorce is not a topic that may only be investigated cross-culturally: It is currently one of the most critical issues in family sociology. It may be addressed from the perspectives of both social psychology and social organization, among many others. Cross-cultural studies of the divorce rate are useful not because they enlighten us about the current situation, but because they can help to build and test theories that may be applicable across time and cultures.

It is clearly not the case that any and all knowledge generated by cross-cultural research is directly or immediately applicable to the concerns of other scholars. However, it is usually the case that the theories employed by cross-cultural researchers to develop hypotheses are similar, if not identical, to the theories that the rest of us draw upon to construct our own explanations of social phenomena.

The variables differ primarily at the operational or measurement level. For scholars who can see beyond the specifics of operationalization there is a wealth of relevant knowledge that has been generated by the cross-cultural method but has remained largely untapped.

CONCLUSIONS

There are many methodological and theoretical problems faced by both producers and consumers of cross-cultural research that cannot be dealt with here. The 7 that have been discussed should be taken as examples, not as a taxonomy of issues. Furthermore, none of the individual issues covered here has been examined in full detail. However, sufficient information has been presented to allow the formulation (or perhaps restatement) of some general principles that bear upon the utility of cross-cultural research for the concerns of family scholars.

The first principle is that if cross-cultural surveys are useful at all, they are useful only for the testing of theoretically-derived hypotheses, not for description or parameter estimation. This point is critical. It places a definite limitation on the utility of cross-cultural data, but it also means that criticisms of the method based on the descriptive inadequacies of the data are misguided: No descriptive utility is ascribed to such data, except for the trivial case of heuristically useful illustrations that may be drawn from the original ethnographic sources.

Second, only hypotheses dealing with relations between the properties of social and cultural systems may be tested by cross-cultural surveys. Issues pertaining to interindividual variation within systems are beyond the reach of this method. This is also an important limitation.

Third, within the confines established by the first two principles the cross-cultural method offers a wealth of opportunities to expand our knowledge of family behaviors and relationships. It does so by providing an arena within which hypotheses may be tested. Many hypotheses tested or testable on cross-cultural data emanate from theories that also have implications for contemporary situations, practical concerns, and issues involving individual variability. Cross-cultural research, if carefully and properly employed, can contribute to the development, refinement and, ultimately, the generality of our

explanatory theories of family phenomena. Although such research is afflicted by imperfections in sampling, measurement, and explanatory ambiguity, these problems are not insurmountable and are, in various manifestations, endemic to all forms of social research.

One major limitation of cross-cultural research that is not likely to be overcome is that there will not be much more raw data generated by original ethnographic research.⁴ This is because the number of cultures in existence that are uncontaminated by the influence of Western civilizations is approaching zero. We can no longer assume that geographic dispersion is a safeguard against diffusional correlations for recently collected ethnographic data. This means that for many analyses sample sizes will remain relatively small. Furthermore, in this type of research one who is dissatisfied with the quality of the original data cannot simply return to the field to make additional observations to add to or replace the original ethnographies. Even if this were logistically possible, the cultures have changed.

In the future, most new data suitable for cross-cultural analyses will be generated by ethnologists—those who code and quantify existing ethnographic reports and render them suitable for quantitative analysis. This process is ongoing in *Ethnology*. The cross-cultural data banks contain valuable information on a plethora of family-related phenomena, and even more information on factors that affect or are affected by these phenomena. We could learn much more about families if we intelligently and selectively made greater use of this resource.

NOTES

1. Most of the objections examined are not attributed to any particular author and may thus appear to be inventions of a defensive mind. They are not. The fact of the matter is that scholars who take exception to the cross-cultural method for one or more of the following reasons tend to ignore it, to not write about it. Methodologically we tend to report to our colleagues why we do what we do, not why we don't do something else. The objections examined here have been distilled from multiple sources: legitimate concerns of practicing cross-cultural researchers; anonymous reviews of manuscripts; comments of discussants and others on papers presented at professional meetings; discussions with colleagues in both formal and informal settings; and concerns expressed by graduate students in seminars. The fact that these concerns are rarely expressed in print means that they are somewhat fugitive and difficult to

respond to in a systematic fashion. I appreciate the opportunity to present both sides of these arguments here.

2. It can, of course, be applied to the study of small segments of large societies; witness the growth of urban anthropology. However, the resulting observations cannot be taken as representative of such societies in their entirety. In general, the more complex and differentiated a social system, the less representative an ethnographic description of some specific instance of its culture will be. Ethnography is traditionally based on direct observation and the number of things that can be directly observed is finite.

3. For logic and evidence that support this assumption see Marsh (1967), Kobben (1968), Naroll (1968, 1970), and Elder (1976), among others.

4. An anonymous reviewer has suggested that it is possible to generate ethnographic-type observations on complex societies if one is willing to modify requirements for direct observation. For example, observations to the effect that the United States is characterized by nuclear families, monogamous marriages, bilateral reckoning of kinship, and so on, would engender little disagreement. Problems would arise in areas such as husband-wife power relations, parental socialization values, and other phenomena in which the variation is so great that singular typifications of cultural practices are literally impossible and estimates of central tendencies deceptive. Although space does not permit an intensive examination of this possibility here, it may deserve careful consideration in the future.

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