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The Dialectic of the General and Particular in Social Science Research and Teaching Praxis

Wolff-Michael Roth

Review Essay:

Carol R. Ember & Melvin Ember (2001). Cross-Cultural Research Methods. Lanham, MD: Altamira Press, 164 pages (English), Paper (0-7425-0427-1), $ 22.95, Cloth (0-7425-0426-3), $ 62.00

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Abstract: *Cross-Cultural Research Methods* pretends to be a primer on the "how to" of conducting cross-cultural research, but focuses only on quantitative methods that use secondary data in the service of generating knowledge. The book is caught twice in the dialectic of the general and the specific, by putting all its eggs into the former basket and failing to recognize the role of the latter both in research itself and in the teaching of research methods to its readers. Because I know that the students in my graduate research methods course would fail to appreciate the book, I would neither select nor recommend it to others as a resource in teaching (quantitative) research methods or research designs courses.

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1. Introduction

"This is a 'how to' book" (p.vii). Thus starts this little volume on one cross-cultural research method—rather than the plural version, methods, that the title promises. This title is probably misleading, for how would a book that introduces readers to the logic of quantitative cross-cultural research into the list of media units of a journal dedicated to qualitative research? [1]

This book fundamentally is about how to use existing ethnographic studies as (secondary) sources for comparing at once not just two but many cultures distributed across the globe. It is immediately evident that such comparisons require a reductionist approach, where complex cultures are reduced to a few measures on variables that are of interest to the researcher. Going through the

existing literature, taking information from it to provide a score on a salient variable, and using the scores in statistical analyses to reject or accept hypotheses of differences or relationships generated a priori on the basis of some theory require attention to particular problems and contingencies. The authors promise to provide a practical approach to these problems and contingencies. Apart from the fact that some of the fundamental presuppositions one has to make in such research have not been discussed, I found the book fraught with problems arising from the general/specific dialectic that will make it difficult for novices to learn from the book. That is, the authors are so deeply caught in their paradigm of generalizing that not only their research but also their teaching dwells in the general; they do not realize that without understanding the specific it is impossible to understand the general. [2]

The relationship between the general and specific is fundamental to dialectical materialism (IL'ENKOV 1977); it is also, perhaps in some guise, central to hermeneutic phenomenology (RICŒUR 1991). The experience of the specific, concrete world of objects and events always precedes the understanding of theory. HUSSERL (1973, p.61) expresses this MARXist insight in his aphoristic statement, "Die Praxis steht überall und immer voran der 'Theorie'" [Praxis always precedes theory]. For RICŒUR (1991), the general/specific dialectic is expressed in the parallel dialectic of understanding/explaining, whereof understanding is always grounded in lived experience. Thus, all explaining requires prior understanding. We cannot engage in the analysis of any text or action without always already having a practical understanding of the world. But understanding requires explaining, engagement in structural analysis, in order to unfold and develop pre-existing understanding. Without explaining we would be stuck in ideology unable to overcome our preconceptions. This general/specific dialectic operates within the paradigm presented by the authors and also creates problems for readers who attempt to understand the lesson of the book, but have to fail unless they already have an understanding of the method, some cultures, and cross-cultural comparisons. [3]

Before articulating some of these problems, I provide a brief description of the content and structure of the book. I then discuss the book under three aspects: I comment on (a) the role of the particular and the general in learning, (b) the relationship between ideology and method, and (c) coding. [4]

2. Structure and Contents

The authors structured the book more or less along the temporal dynamic of a research project. Beginning with the presentation of the logic of cross-cultural research (Chapter 1), they proceed to the formulation of the research question (Chapter 2); the role of theories and hypotheses (Chapter 3); problems related to the measurement of variables (Chapter 4); the sources of error and the impact errors have on the power of statistical analysis (Chapter 5); identification of the sampling domain (population) and issues of selecting appropriate procedures for selecting the sample on which the generalization to the domain is conducted (Chapter 6); issues arising from coding in secondary data (Chapter 7); an

introduction to the logic of some of the basic statistical procedures (Chapter 8); and questions concerning the reliability of coders (Chapter 9). A brief summary (Chapter 10), an appendix (in which a large database for conducting cross-cultural research is explained), a glossary, and reference and index sections conclude the book. [5]

In "The Logic of Cross-Cultural Research" (Chapter 1) the authors lay out some of the assumptions of the kind of research they conduct. Among these assumptions they list: the use of variables and measurement as prerequisites for cross-cultural comparisons, generalization based on statistical inference, the use of contingency tables, the distinction between synchronic and diachronic comparison, and the need to have variability as a precondition for doing quantitative work in cross-cultural research. They forget to note—or perhaps fail to understand—that in the scientific paradigm that they pretend to espouse, causal relationships require experimental variation of the independent variable. Drawing on observation-based data, they cannot try "to see how cultural traits may be causally related to each other" (p.16). If the causal relationships are derived from theory, procedures other than experiment—for example, intuition and understanding-based elaboration—have been used to establish causal relationships. [6]

The brief Chapter 2 provides a classification of different types of research questions (causal, consequence, and relational), and an explanation of the difference between dependent and independent variables. The chapter fails to note that interesting research questions, those that expand the knowledge of the field, cannot be framed unless one knows the field, ascribes to its assumptions and values, and is familiar with the going (acceptable) theories. [7]

The brief Chapter 3 deals theories and hypotheses, explains the difference between them, describes the nature of scientific laws, and provides demonstrations why theories cannot be proven and why hypotheses can only be falsified—based on POPPER's (1959) articulation of the logic of scientific discovery, which social scientists consider to be pretty well passé. [8]

In Chapter 4, "The Art of Measurement," the authors articulate issues surrounding reliability and validity, explain and exemplify the differences between nominal, ordinal, and interval and ratio measures, and discuss the differences between face, content, and convergent validity. Readers can find a brief exposé on GUTTMAN scales, which are hierarchically ordered descriptors so that a higher order descriptor normally includes the presence of all lower order descriptions. There is also discussion of primary data, usually gathered by researchers themselves in the service of a particular research or research program, versus secondary data, derived from existing ethnographic texts that were written on the basis of very different intentions. [9]

How to minimize error and how errors affect the detection of presumed (causal, correlational) relationships is the topic of Chapter 5. The authors discuss random and systematic error, the relationship between levels of inference to be made in

coding and the level of error that is likely to occur, the effect different time frames (under which primary data were collected) have on error, how to minimize ethnographer, informant, and data coder errors, and how to check the quality of data. [10]

Sampling issues constitute the content of Chapter 6; these issues are particular to cross-cultural research based on secondary data, for the existing literature does not cover all cultures with equal depth and breadth. Thus, researchers interested in making quantitative cross-cultural comparisons for making generalizations valid to a population of cultures, need to be careful in selecting their sampling frame so that its random sampling from the frame becomes representative of the population that the generalization is to be about. A large multi-page table lists similarities and differences between databases and articulates some of the advantages and disadvantages of working with each. The authors describe different databases and how to use "proportionate" or "disproportionate stratified sampling" to meet the need of their research. GALTON's problem, concerning the lowered validity of cross-cultural generalizations when information on two or more cultures is not independent—because of historical ties between the cultures—is introduced in a way that leads me to conclude that it is a significant issue in the culture represented by the authors. [11]

Some generic information on coding and the problem of unreliable coders as well as a coding example constitute the contents of the relatively brief Chapter 7. The chapter is annoying in the attitude it, as the entire book, takes with respect to coders—who are both cheap (why they don't do a good job) and expensive labor (why researchers can't hire as many as they want), depending on the context; most coders therefore cannot be trusted inherently. Also, the authors specify that the preference to keep coders "in the dark about hypotheses" (p.94) because they might contaminate the data, a recommendation that most qualitative researchers would reject as unethical and undesirable. [12]

Chapter 8 introduces the reader to the logic of using statistics for summarizing quantitative data (e.g., why and when to use the measures of central tendency mean, median, or mode) or for making inferences about relationships between different kinds of variables based on their use of categorical, ordinal, interval and ratio measures including t-tests, analysis of variance, chi-square and FISHER's exact tests, and correlations. The authors introduce the idea of tests of significance (p-values), without nevertheless articulating, for example, why a p-value of 0.05 is the hallmark for rejecting or accepting hypotheses or the relationship between the p-value and the number of cases in the sample. [13]

The final substantive Chapter 9 is devoted to issues of reliability in its various forms pertaining to test-retest (how similar are a persons results on the same or similar tests taken twice), inter-observer (how similar do two observers rate the same situation), and inter-rater situations (how similar do two raters rate the same piece of given data). It highlights once again the fundamental distrust the authors have in observers and coders, as being biased and not following coding

instructions. The authors discuss four approaches of dealing with different coding: using one (more trustworthy) rater's codings, summing or averaging codes, resolving differences, or dropping cases in which raters have "serious" disagreements. [14]

3. Some Comments

In addition to extensive experience in qualitative research, my background includes a formation as a physicist and statistician. Reading the book came easy to me in one sense, because I was familiar with the various aspects of the research method presented in the book. On the other hand, in many places, the text presumes a prior understanding of the field as a whole and of quantitative cross-cultural research in particular. I attempted to come to grips with the role my own familiarity plays in reading the book, by trying to see the task from a novice's perspective. I found that with respect to the statistical aspects, too, the book assumes too much background understanding to be useful to, for example, the graduate students teach in introductory research designs courses. I traced the uneasiness to the use of general descriptions and cooked up, fake examples that are impossible to understand unless one is also familiar with concrete cases. That is, the authors wrote a book that was supposed to be practical with too many non-practical examples to make sense. Here, I consider the use of examples from the natural sciences as inappropriate because the concerns in those fields are very different than in the social sciences—I consider them inappropriate even if they had not been tainted by wrong conceptions, as this is the case in the present volume.[1] [15]

3.1 On the role of the particular and the general in learning

The authors teach the advantages and disadvantages of correlational research by drawing on graphs (e.g., Figure 1.2, p.15) of the same type that our research shows give trouble even to research scientists with 6 to 25 years of experience (ROTH, BOWEN & MASCIOTRA 2002). Reading and interpretation require previous understanding, which can be developed through critical analysis (RICŒUR 1991). When, however, the text dwells in abstract statements, students will find it difficult to grasp its meaning. The following two statements are but examples from the domain of using generic objects and descriptions when concrete, meaningful cases should have been used to assist the novice reader

1 As a physicist and science educator, I cringed when I read examples given in the context of measurement. The authors have a misunderstanding about temperature as a proxy of heat (p.37), which of course is only the case when everything else is the same. But one cannot compare the heat of two substances by comparing their temperature, as claimed by the authors, for (a) only changes in heat can be obtained and (b) changes in heat involve other parameters such as mass and heat capacity. Furthermore, the authors claim that molecules can come to an absolute standstill (p.41), which is wrong because it would violate the uncertainty principle. According to the authors, "energy is generated when molecules are moving" (p.37), when in fact energy is simply a state variable allowing one to track systems given the principle of conservation of energy. The authors claim that water freezes because of the relation between state (ice, water) and temperature (p.28), when in fact water has a constant temperature while it freezes, 0 °C, and the change in state is not a function of the temperature but a function of the change in order (physicist call it entropy).

for whom, as one can infer from the second statement, the book was evidently designed:

> "You could say that the Society X people have this unique [pottery] design because it is customary, but this answer is an uninformative tautology. It merely says that they have this design because they had this design before." (p.22)
>
> "If you find the terms dependent and independent variable confusing, think of independent as free to vary and the dependent variable as not free to vary because it depends on some other trait." (p.23, emphasis in the original) [16]

Both examples suffer from the lack of specificity that would allow novice readers to make connections to their own experiences: any mathematics' teacher knows the difficulties many students face in thinking about Xs and Ys. *Praxis always precedes theory*. If readers do not have concrete cases, how are they to understand and develop theory? We do understand the general because we already have had lived experience with and prior understanding of the particular. We do not understand the general in the absence of the particular, in the same way that we cannot have thoughts (noesis) without content (noema). One does not understand the meaning of dependent and independent variables by providing another description of the two and in the absence of suitable examples from the everyday world of the reader. [17]

The same case can be made for the use of hypothetical case material. For example, take the following rallied by the authors in the service of making inferences about the differences between two conditions or cases.

> "Assume we are comparing two societies with different levels of fertility. We may think that the difference is due to a greater need for child labor in one society because there is more agricultural and household work to be done in that society." (p.76) [18]

The authors suggest that the inference may sound plausible. But it is not immediately evident why fertility—at least if defined as a biological concept rather than birth rate—ought to be a function of social factors such as need for child labor. If someone has trouble seeing that the example "may sound plausible," then it will be difficult to impossible to follow the argument that the authors attempt to make. [19]

The larger context of the example is also interesting because the authors claim that the difference between two cases cannot be attributed to the difference on another variable. This may be true for the kind of data that these authors work with, but it is not true in general. The very logic of analysis of variance in the context of experimental research is aimed at identifying whether or not one variable, such as different teaching methods, has a causal relation to another, such as student learning outcomes (as measured by a posttest). Although there are some concrete examples and even a coding exercise, the text remains too aloof from the concerns that a beginner would have to be useful. Thus, the entire

chapter on statistical analysis is of little value unless the reader already knows statistics; yet, as the above quote shows, the authors expect that some readers may not even be familiar with the difference between dependent and independent variables. Furthermore, it was difficult to understand just what the different databases consist of, how they present themselves, how one searches for information. Thus, I found it difficult to grasp the entire discussion of and comparison between different databases. Some concrete rendering of bits of information would have helped a long way. [20]

3.2 On ideology and method

The book suffers from an unresolved tension between the concrete and the abstract, the general and particular, both in its theoretical underpinning and in the way that the material is being presented. The authors seem to be uninformed of some of the important research on coding, and the role that knowledge of particulars informs the work of coding (GARFINKEL 1967). [21]

BOURDIEU (1992) noted that the greatest enemies of research are researchers themselves, when they engage in "abstraction which ignores itself as such" (p.226). BOURDIEU particularly writes against the empire of those monomaniacs of individual methods, bending the entire world to fit their Procrustean beds of their choice. Above all, BOURDIEU notes that researchers are caught in the preconstructed that is everywhere. Thus, the cross-cultural anthropologist as

> "[t]he sociologist is thus saddled with the task of knowing an object—the social world—of which he is the product, in a way such that the problems that he raises about it and the concepts he uses have every chance of being the product of this object itself." (p.235) [22]

This book lacks a reflective and reflexive approach. In this day and age, I would have expected a book with *Cross-Cultural Research Methods* (note the plural!) as title to be more reflective about its own presuppositions. For example, the authors make the fundamental assumption that "similarities cannot be seen or recognized until we think in terms of variables, qualities or quantities that vary along specified dimensions" (EMBER & EMBER, p.4). There are not only serious critiques of the variable approach (e.g., HOLZKAMP 1991) but also (dialectical) alternatives for thinking about generalization, how to achieve it, and its relation to the specific and concrete (IL'ENKOV 1977). [23]

There is a continuum of representations, beginning with originary, lived biographical experience on one side and highly abstracted structural formal symbols, often mathematical equations, on the other side; this continuum has been articulated in terms of world/sign (LATOUR 1993) and testimony [Zeugnis]/structure (MÜLLER 1973) oppositions. Abstracting from experience, that is, going from the world/testimony pole toward the sign/structure pole requires work, summarizing over many experiences and testimonies. This work is done in "centers of calculation" (LATOUR 1987). This work is not expended in vein, for it is related to observing many cases, lending itself to generalization and

knowledge. Because observation, like surveillance, knows something about many testimonies, however little this may be, this knowledge is associated with power and control.

> "La surveillance devient un opérateur économique décisif, dans la mesure où elle est à la fois une pièce interne dans l'appareil de production, et un rouage spécifié dans le pouvoir disciplinaire." (FOUCAULT 1975, p.206)[2] [24]

The fundamental questions, "Cui bono?", that is, "Who benefits from this research?" and "Whose power is supported by such analysis?" never seem to be asked. Whose interests are served with cross-cultural comparisons that take a Western epistemology as ARCHIMEDES' point of absolute reference? A little does of HABERMAS (1971), and interrogation about the interests that are served with this kind of research, and a bit of "radical doubt" (BOURDIEU 1992) with the authors' own preconceptions would allow readers to evaluate much better the relative value with, and problems of, the methodology elaborated here. [25]

3.3 On coding

Quantitative researchers (monomaniacs?) attempt to purge interpretation and the role of experience from their research. Interestingly enough, interpretation and experience, which play a central role in understanding hermeneutics, creep into the quantitative paradigm, and especially during coding. The EMBERs repeatedly discuss the problem of having coders go through textual materials and code them not only in consistent but in fact in the same way. These authors, as others working in the same paradigm, fail to deal with the questions why different coders may be coding differently and more poignantly, why different coders may code some text or situation in the same way. The more limited assumption is that there is a truth out there accessible to all intelligent, willing, and careful people/analysts. A broader and less stringent assumption would be that common codings are the exceptions and therefore need to be explained. There are two issues to be raised, the first dealing with the relation between coder and situation the second with the relation between coders of the same situation. [26]

First, the coding issue arose and was subsequently researched in a project where sociologists wanted to find out about the work processes and social organization in psychiatric clinics by reconstructing them from the clinics' records (GARFINKEL 1967). Studying the processes by means of which graduate students coded these records, GARFINKEL came to the conclusion that the coders' understanding of how clinics work allowed them to arrive at conclusions about what the records say. That is, understanding how clinics worked already entered the coding of the data, although how clinics work was supposed to be inferred from the codes. Coders arrive at definitive codings even if the documents are only marginally adequate. Thus, coders

2 This sentence translates about like this: "The surveillance becomes a decisive economic operator to the extent that it is both an internal piece of the production machinery and a specified set of wheels in disciplinary power."

> "will be able, there and then, to contrive ways of dealing with these difficulties, and are able to do this because they are able to draw upon their understandings of which things may possibly, and actually, happen in places like psychiatric clinics." (SHARROCK & BUTTON 1991, p.150) [27]

Second, we can infer, therefore, that different coders equally familiar with the objects and phenomena under investigation will have fewer difficulties and are more likely to come to common inferences than coders with different levels of familiarity, that is, with different levels of prior understanding that enters, as embodied in the understanding/explaining dialectic, any effort of structural analysis (RICŒUR 1991). Without such understanding of concrete situations that the generalization is supposed to summarize and explain, even natural scientists have been shown to have trouble reading and interpreting texts and graphs although these had been culled from undergraduate textbooks and courses of their own domain (ROTH, BOWEN & MASCIOTRA 2002). On the other hand, the more extended the collective experience of coding data, the more consistent the interpretations and categorizations will be (SCHOENFELD 1992). That is, people who have a lot of shared experiences and cultural background are very similar in the ways that they perceive and describe records. The question of how to obtain high inter-coder reliabilities is thereby solved. [28]

4. Conclusion

A critical step that any book on method has to pass before I make a recommendation is the "how-would-my-graduate-students-rate-it" test. For more than a decade, I have taught research design, statistics, and qualitative research methods to graduate students from different disciplines including nursing, education, conflict management, native governance, and other departments within the school of human and social development. Based on this experience, and in particular students' evaluations of different textbooks, I know that their reception of *this* book would be very unkind. I would neither select the book nor recommend it to others for selection as a resource in their teaching of research methods, cross-cultural or otherwise. (Of course, I would get the blame rather than the author because I selected it.) [29]

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Author

Wolff-Michael ROTH is Lansdowne Professor of applied cognitive science at the University of Victoria. His interdisciplinary research agenda includes studies in science and mathematics education, general education, applied cognitive science, sociology of science, and linguistics (pragmatics). His recent publications include *At the Elbows of Another: Learning to Teach by Coteaching* (with K. TOBIN, Peter Lang, 2002), *Science Education as/for Sociopolitical Action* (ed. with J. DÉSAUTELS, Peter Lang, 2002), and *Being and Becoming in the Classroom* (Ablex Publishing, 2002).

Wolff-Michael ROTH has published essay reviews in previous *FQS* issues. These include Grenzgänger Seeks Reflexive Methodology; Auto/ Biography as Method: Dialectical Sociology of Everyday Life and The Politics and Rhetoric of Conversation and Discourse Analysis: A reflexive, phenomenological hermeneutic analysis.

Contact:

Wolff-Michael Roth

Lansdowne Professor, MacLaurin Building A548, University of Victoria, BC, V8W 3N4, Canada

Phone: 1-250-721-7785

E-mail: mroth@uvic.ca

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