

CROSS-NATIONAL RESEARCH AS AN ANALYTIC STRATEGY*

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In this essay, I discuss some of the uses and dilemmas of cross-national research. I argue that cross-national research is valuable, even indispensable, for establishing the generality of findings and the validity of interpretations derived from single-nation studies. In no other way can we be certain that what we believe to be social-structural regularities are not merely particularities, the product of some limited set of historical or cultural or political circumstances. I also argue that cross-national research is equally valuable, perhaps even more valuable, for forcing us to revise our interpretations to take account of cross-national differences and inconsistencies that could never be uncovered in single-nation research.

My thesis is that cross-national research provides an especially useful method for generating, testing, and further developing sociological theory. As with any research strategy, cross-national research comes at a price. It is costly in time and money, it is difficult to do, and it often seems to raise more interpretive problems than it solves. Yet it is potentially invaluable and, in my judgment, grossly underutilized. This is hardly a radically new thesis. As Stein Rokkan (1964) long ago pointed out, to do cross-national research is to return to the preferred analytic strategy of the forefathers of sociology, a strategy that was nearly abandoned in sociology's quest for methodological rigor but now can be pursued anew with the much more powerful methodological tools available today.¹

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¹ Similarly for the United States: Armer and Grimshaw (1973, pp. xi-xii) point out that several of the early

A sensible discussion of the uses and dilemmas of cross-national research requires that I first define the domain and delineate the principal types of cross-national research. Then I illustrate some of these uses and dilemmas by scrutinizing the body of cross-national research I know best, namely my own, my rationale being William Form's (1979) cogent observation that "probably no field has generated more methodological advice on a smaller data base with fewer results than has [cross-national] comparative sociology." Using my research as a source of illustrations makes it possible to discuss the issues concretely. I review this research in sufficient detail to highlight its accomplishments and its failures, my concern being only in part with the substance of the research for its own sake; I also want to extrapolate from this concrete example, to make some more general observations. Finally, I discuss some fundamental issues about the conduct of cross-national research. In so doing, I bring in studies dealing with quite different substantive problems from those that I have addressed in my own research, and using quite different methods, to see whether my conclusions apply as well to a much broader range of studies.

presidents of the American Sociological [Association], among them, William Graham Sumner, W. I. Thomas, E. A. Ross, and Robert E. Park, "exhibited substantial interest in the comparative study of other societies." Between the 1930s and 1950s, these concerns seemed marginal to American sociologists; here they again use ASA presidents as their index, noting that, of the 20 presidents from 1931 to 1950, not one is known primarily or substantially for (cross-national) comparative work. Leaving aside the obvious question of the validity of using the interests of ASA presidents as an index of the substantive concerns of U.S. sociology, I would agree with their generalization and I am intrigued with their explanation. They see the "shift toward parochialism" in U.S. sociology of the 1930s and 40s as resulting from a combination of concern with scientific status, constricting resources, attention to immediate social problems (primarily the Depression and World War II), and the political isolationism of American society during that time. From the vantage point of 1973, Armer and Grimshaw saw a strong revival of cross-national research occurring in the 1960s. So, too, did William Evan (1975), and not only in the United States. In a fascinating analysis, Evan documented the growth of cross-national collaborations and of the "internationalization" of sociology, demonstrating as well the important role of the International Sociological Association in this process.

TYPES OF CROSS-NATIONAL RESEARCH

The broadest possible definition of cross-national research is any research that transcends national boundaries. This definition is somewhat ambiguous, though, because many studies of single societies are implicitly cross-national, in that the investigators interpret their findings by contrasting what they learn about the country they actually study with what is known or is believed to be true about some other country or countries. I prefer to restrict the term, cross-national, to studies that are *explicitly* comparative, that is, studies that utilize systematically comparable data from two or more nations.

In restricting the term to explicitly comparative studies, I do not mean to belittle the importance of studies that are only implicitly comparative. Such studies contribute importantly to our understanding; witness, for example, the distinguished series of studies of American society by foreign observers, beginning with Alexis de Tocqueville's *Democracy in America*. Consider, too, studies in which the selection of some one country is particularly appropriate for testing a general proposition—as in Kelley and Klein's (1981) use of the Bolivian revolution of 1952 to test their theory that "radical revolutions" inevitably lead to an increase in inequality, or Chirot and Ragin's (1975) use of the Romanian peasant rebellions of 1907 to test competing interpretations of the intensity of peasant rebellions. And consider, finally, those pivotal studies—Stephen Bunker's (1985) *Underdeveloping the Amazon* is a particularly good example—where some country or region of a country is selected for study precisely because it exemplifies a more general social phenomenon. I leave such research out of my purview not because it is unimportant, but because to include it would make the bounds of "cross-national" so large and ambiguous that it would be difficult to say what, other than research focused single-mindedly on a particular country, is *not* cross-national.

Within the large genre of research that is explicitly comparative, I would further distinguish four types of cross-national research of somewhat differing intent. The four types are those in which nation is *object* of study; those in which nation is *context* of study; those in which nation is *unit of analysis*; and those that are *transnational* in character.² Although these four

types of research shade into one another, their purposes are distinguishable and their theoretical implications somewhat different. My analysis will apply mainly to the second of the four types, in which nation is context of study.

In the first type of cross-national research, where nations are the *object* of study, the investigator's interest is primarily in the particular countries studied: how Germany compares to the United States, France to the Soviet Union, or India to Pakistan. Alternatively, the investigator may be interested in comparing particular institutions in these countries: the social security systems of the U.S. and Australia; the educational systems of the German Democratic Republic and the Federal Republic of Germany. At their best, as in the systematic comparisons of Finland and Poland by Erik Allardt, Włodzimierz Wesolowski, and their collaborators (1978), such studies can lead to well-informed interpretations that apply far beyond the particular countries studied. What distinguishes such research, though, is its primary interest in understanding the particular countries. In this research, one wants to know about Finland and Poland for their own sakes; the investigator does not select them for study just because they happen to be useful settings for pursuing some general hypothesis.

By contrast, I wish to focus on cross-national studies in which, to borrow from Erwin Scheuch's (1967) apt phrase, nation is *context*. In such research, one is primarily interested in testing the generality of findings and interpretations about how certain social institutions operate or about how certain aspects of social structure impinge on personality. In Burawoy and Lukacs' (1987) comparison of a U.S. machine shop with a Hungarian machine shop, for example, their primary interest is not in the United States and Hungary for their own sakes, nor certainly in the particular machine shops, but in these machine shops as exemplifying the relative efficiency of capitalist and socialist industrial enterprises. Admittedly, it may be difficult to differentiate research in which nation is object from research in which nation is context. When Robin Williams (1985) studies the use of threats in US/USSR relations, he

sons" (what he sees as two distinct *strategies* of research I see as attempts to interpret two distinct types of findings); my "nation as unit of analysis" category is ignored in his classification; and my "transnational" category may be a little broader than his "encompassing comparisons," which are limited to studies that see nations as components of encompassing international systems. (For other useful classifications of cross-national research, see Hopkins and Wallerstein 1967; Marsh 1967; Elder 1976; and Nowak 1977; see also Hill 1962.)

² I make no claim that this classification is theoretically superior to other classifications of cross-national research, only that it serves my analytic purposes better than others do. Compared to Tilly's (1984) well-known classification, my "nation as object" category corresponds roughly to his "individualizing comparisons;" my "nation as context" category encompasses both his "universalizing" and his "variation-finding compari-

clearly is interested in the US and the USSR both for their own sakes and as exemplifying superpowers in a nuclear age; there is no way of separating the two purposes. It is nevertheless generally useful to distinguish between research whose primary purpose is to tell us more about the particular countries studied and research whose primary purpose is to use these countries as the vehicle for investigating the contexts in which social institutions operate. My examination of cross-national research as an analytic strategy will be addressed mainly to research where nation is context.

This domain includes such diverse studies as Theda Skocpol's (1979) comparative analysis of revolution, and also, from quite a different theoretical perspective, Michael Burton and John Higley's (1987) analysis of the conditions under which competing elites settle their differences in grand political compromises; Donald Treiman's (1977) analysis of the stratification systems of the industrialized world; William Form's (1976) study of the complexity of industrial technology, workers' skill levels, and the quality of workers' social interactions; Janet Chafetz and Anthony Dworkin's (1986) analysis of the determinants of the size and range of ideologies of women's movements throughout the world; and my colleagues' and my comparative research on social stratification and psychological functioning in Poland, Japan, and the United States (Slomczynski, Miller, and Kohn 1981; Naoi and Schooler 1985).

It is useful to differentiate research where nation is context from two other types of cross-national research that are not central to my discussion here. In the first, where nation is the *unit* of analysis, investigators seek to establish relationships among characteristics of nations *qua* nations. In such research, one no longer speaks of countries by name, but instead classifies countries along one or more dimensions—their gross national product, or average level of educational attainment, or position along some scale of income inequality. A prototypic example is Bornschieer and Chase-Dunn's (1985) analysis of the relationship between the penetration of national economies by transnational corporations and the hypothesized long-run stagnation of those economies. Other pertinent examples are Blumberg and Winch's (1972) analysis of the relationship between societal complexity and familial complexity; and Ellis, Lee, and Petersen's (1978) test of the hypothesis that there is a positive relationship between how closely adults are supervised in a society and the degree to which parents in that society value obedience for children.

What distinguishes research that treats nation as the unit of analysis is its primary concern

with understanding how social institutions and processes are systematically related to variations in national characteristics. Such analyses need not treat each nation as a homogeneous entity, but may study intranation institutions and processes, as Meyer, Hannan, and their colleagues (1979) have done in their analyses of national development. Nor need research that treats nation as unit of analysis assume that each nation exists in an international vacuum. As Bornschieer and Chase-Dunn (1985, p. 65) put it, ". . . we do not contend that nation-states are closed systems. A unit of analysis does not need to be a closed system. When we compare individuals or schools we know that these units interact with one another and are parts of a larger social context. The unit of analysis in comparative research is any unit in which the process of interest is known to operate."

In distinguishing research that treats nation as the unit of analysis from research that treats nation as the context for analysis, we are again dealing with gradations, not sharp differences. As will become evident later, attempts to understand cross-national differences sooner or later require one to search for the pertinent dimensions that differentiate the nations *qua* nations. One can, in fact, argue that research in which nation is treated as context is simply a way-station to more general analyses in which the pivotal distinguishing characteristics of nations become variables in the analysis. In principle, as Rokkan (1964), Przeworski and Teune (1970), Hopkins and Wallerstein (1967), and Chase-Dunn (1982) all argue, one can and should convert descriptive differences between countries into analytic variables. I have no quarrel with this objective, only a belief that in many fields of sociological inquiry there is much to learn from research in which nation is treated as context before we are ready to translate "nations" into "variables."

Research that treats nations as the unit of analysis requires that one be able to discern which of the many differences between countries are the pertinent analytic variables; that one be able to formulate meaningful hypotheses at the appropriate level of abstraction; and—if one is ever to test such interpretations—that one have at hand or have the potential to collect data from a sizable sample of countries. It also requires much better data than are generally available in multination data sources. I hope that an essay on cross-national research written ten or twenty years from now will be able to focus much more on such research than I believe is warranted today.

And then, finally, there are studies that treat nations as components of larger international systems. Borrowing a term from economists and political scientists who have studied corpora-

tions (and I hope not distorting their usage of the term), I call this *transnational* research. Immanuel Wallerstein's (1974, 1980) analysis of the capitalist world-system and Fernando Cardoso and Enzo Faletto's (1979) analysis of dependency and development in Latin America are prominent examples. We are at a rather early stage in the development of appropriate methodologies for transnational research (Meyer and Hannan 1979; Chase-Dunn 1979; Chase-Dunn, Pallas, and Kentor 1982). Even now, though, transnational research has proved its importance by demonstrating that the nations we compare in *all* types of cross-national research are not isolated entities but are systematically interrelated.

I see all four types of cross-national inquiry as useful, each for particular substantive problems. I focus on research that uses nation as context, not because I consider this type of cross-national research inherently more valuable than the others, but because I think that for many sociological problems—particularly, I must admit, for those in which I have the greatest substantive interest—this type of research has especially great utility in the present state of knowledge. In particular, such research affords the opportunity to study each of the countries with sufficient thoroughness for intensive comparison.

ESTABLISHING THE GENERALITY OF RELATIONSHIPS AND THE LIMITS OF GENERALITY

Many discussions of cross-national research (Ragin and Zaret [1983] is a thoughtful example) contrast two research *strategies*—one that looks for statistical regularities, another that searches for cultural or historical differences. I prefer to pose the distinction, not in terms of research strategies, nor of methodological preferences, nor even of theoretical proclivities toward “tranhistorical” generalizations or “historically contextualized knowledge,” but in terms of interpreting the two basic types of research *findings*—similarities and differences. Granted, investigators' theoretical and methodological preferences make it more or less likely that they will discover cross-national similarities; granted, too, what can be treated as a similarity at one level of analysis can be thought of as a myriad of differences at more detailed levels of analysis. Still, the critical issue is how to interpret similarities, and how to interpret differences, when you find them.

Finding cross-national similarities greatly extends the scope of sociological knowledge. Moreover, cross-national similarities lend themselves readily to sociological interpretation; cross-national differences are much more diffi-

cult to interpret. As Kazimierz Slomczynski, Joanne Miller, and I argued (albeit a little too categorically) in our first comparative analysis of the United States and Poland:

Insofar as cross-national analyses of social structure and personality yield similar findings in the countries studied, our interpretation can ignore whatever differences there may be in the cultures, political and economic systems, and historical circumstances of the particular countries, to deal instead with social-structural universals. But when the relationships between social structure and personality differ from country to country, then we must look to what is idiosyncratic about the particular countries for our interpretation. (1981, p. 740)

The first half of this formulation asserts that when the relationship between social structure and personality is the *same* in two or more countries, then the unique historical experiences of each country, their distinctive cultures, and their particular political systems are not of focal importance for interpreting the relationship. The formulation does *not* assert that history, culture, and political context have been irrelevant in shaping social structures, but that the resultant social structures have a cross-nationally consistent impact on people. The explanation of this impact should be sought in terms of how people experience the resultant social structures, rather than in the historical or cultural processes that shaped those structures. Admittedly, this may not always be the best interpretive strategy. Apparent similarities can mask profound differences; what seems to call for a unitary interpretation may actually require entirely different explanations. Nevertheless, I believe that where we find cross-national similarities, the most efficient strategy in searching for an explanation is to focus on what is structurally similar in the countries being compared, not on the often divergent historical processes that produced these social-structural similarities. The basic and very simple point is that social-structural similarities may have been brought about by very different historical processes and yet have essentially similar social and psychological consequences.

The second half of the formulation directs us to interpret cross-national *differences* in terms of historical, cultural, political, or economic idiosyncrasies. Przeworski and Teune (1970) argued that what appear to be cross-national differences may really be instances of lawful regularities, if thought of in terms of some larger, more encompassing interpretation. I agree, but I also believe that developing such interpretations is an immensely difficult task. A necessary first step is to try to discover which of the many

differences in history, culture, and political or economic systems that distinguish any two countries are pertinent to explaining the differences we find in their social structures or in how these social structures affect people's lives. I do not contend that cross-national differences cannot be lawfully explained—quite the contrary—but only that the lawful explanation of cross-national differences requires more explicit consideration of historical, cultural, and political-economic particularities than does the lawful explanation of cross-national similarities.

Ultimately, the distinction between cross-national similarities and differences breaks down, and the issues cannot be so simply and neatly dichotomized. Nonetheless, it is a useful way to think about these issues. Therefore, I shall discuss the two types of cross-national research findings separately, beginning with cross-national similarities. I use the U.S.-Polish and U.S.-Japanese comparisons that my collaborators and I have carried out as my principal illustrations of both cross-national similarities and differences, my substantive concern in this part of the essay being the relationship between social structure and personality.³ The conclusions I draw are by no means limited to this substantive area.

³ My concern is not with cross-national similarities or differences in *personality* but with cross-national similarities or differences in the *relationship* between social structure and personality. I do not believe that current methods are adequate for assessing whether Poles are more or less intellectually flexible than are Americans, or whether Japanese value self-direction more or less highly than do Americans. Methodological experts whom I greatly respect disagree with this judgment. They believe that if you construct confirmatory factor-analytic models of the same concept for representative samples of two countries, using not only the same indicators of the concept, but also the same reference indicator to establish the metric in both countries, you can compare, e.g., the mean level of authoritarian conservatism for U.S. and Polish adults (Schoenberg 1982). This assumes not only an exact equivalence of meaning, an issue about which confirmatory factor analysis does give us considerable confidence, but also exact equivalence in the frames of reference that people employ in answering questions. I doubt, though, that "strongly disagree" has the same connotations in a Polish interview as in an American interview; the survey specialists of the Polish Academy of Sciences believe that it is difficult for Polish respondents to overcome their cultural tendency to be polite to their guest, the interviewer. We do not have a zero-point for our scales, nor any other basis for mean comparisons. This, however, in no way prevents us from accurately assessing whether, for example, the relationship between social stratification and authoritarian beliefs is of the same sign and of roughly the same magnitude for the United States, Poland, and Japan. And this, I believe, is in any case the more important question for cross-national analysis.

Cross-national Similarities

Over the course of three decades of research in the United States, Carmi Schooler and I, in collaboration with Joanne Miller, Karen A. Miller, Carrie Schoenbach, and Ronald Schoenberg, have intensively studied the psychological impact of social stratification—by which we mean the hierarchical distribution of power, privilege, and prestige (Kohn 1969; Kohn and Schooler 1983). We interpret the consistent relationships that we have found between social stratification and such facets of personality as values, orientations to self and others, and cognitive functioning as the product, in large part, of the intimate relationship between social stratification and particular job conditions. People of higher social-stratification position (as indexed by educational attainment, occupational status, and job income) enjoy greater opportunities to be self-directed in their work—that is, to work at jobs that are substantively complex, free from close supervision, and not highly routinized. The experience of occupational self-direction, in turn, is conducive to valuing self-direction, both for oneself and for one's children, to having self-conceptions and social orientations consonant with such values, and to effective intellectual functioning. It is even conducive to seeking out opportunities for engaging in intellectually active leisure-time pursuits (K. Miller and Kohn 1983). All this is true both for employed men and for employed women (J. Miller, Schooler, Kohn, and K. Miller 1979; Kohn and Schooler 1983; Kohn, Slomczynski, and Schoenbach 1986).

Structural-equation analyses of longitudinal data have enabled us to confirm even that part of the interpretation that posits a causal impact of job conditions on personality (Kohn and Schooler 1978, 1982; Kohn and Schoenbach 1983). These analyses show the relationships to be reciprocal, with job conditions both affecting and being affected by personality. Moreover, analyses of housework (Schooler, Kohn, K. Miller, and K. Miller 1983) and of education (J. Miller, Kohn, and Schooler 1985, 1986) demonstrate that the experience of self-direction, not only in paid employment, but also in housework and schoolwork, decidedly affects people's self-conceptions, social orientations, and cognitive functioning. The interpretation has considerable generality.

In the absence of appropriate cross-national evidence, though, there would be no way of knowing whether this (or any other) interpretation applies outside the particular historical, cultural, and political contexts of the United States. No analyses based solely on U.S. data could tell us whether the relationships between social stratification and personality are an

integral part of the social-stratification system typical of industrial societies, or are to be found only in the United States, or only in countries that have capitalist economies, or only in countries characterized by Western culture, with its purportedly higher valuation of self-direction. Replications of our research by colleagues in other countries (for a review, see Kohn and Schooler 1983, chap. 12), particularly the comprehensive replications that have been carried out by our Polish and Japanese colleagues (Slomczynski et al. 1981; Naoi and Schooler 1985), have made possible tests of the generality of the U.S. findings and of the validity of our interpretation. In the main, these findings are highly consistent with those for the United States, thus greatly enlarging the power of the interpretation.

Of pivotal importance here are the Polish-U.S. comparisons, particularly the comparative analyses of men, for whom the Polish study contains more complete occupational data. The principal issue to which these analyses are addressed is the specificity or generality of the U.S. findings about the linkages of social stratification to job conditions, and of job conditions to personality. Are these linkages specific to the economic and social structures of capitalist society, or do they obtain as well in socialist society?

We have found, for Poland as for the United States, that higher social-stratification position is associated with valuing self-direction, with holding social orientations consonant with such a value—namely, a nonauthoritarian, open-minded orientation, personally responsible standards of morality, and trustfulness (Slomczynski et al. 1981)—and with effective intellectual functioning (Slomczynski and Kohn in press). We have further found a strong reciprocal relationship, for Poland as for the United States, between social-stratification position and occupational self-direction (Slomczynski et al. 1981). Finally, insofar as possible with cross-sectional data, we have shown for Poland, too, a causal impact of occupational self-direction on values, social orientations, and intellectual functioning (Slomczynski et al. 1981; Slomczynski and Kohn in press). Self-direction in one's work leads to valuing self-direction for one's children, to having a more open, flexible orientation to society, and to effective intellectual functioning. Lack of opportunity for self-direction in one's work leads to valuing conformity to external authority for one's children, to viewing social reality as hostile and threatening, and to diminished intellectual flexibility. The effects of social stratification on job conditions, and of job conditions on personality, are much the same in socialist Poland as in the capitalist United States.

This does not mean that these processes are necessarily the same in all socialist and all capitalist societies, but it does mean that the U.S. findings are not restricted to capitalist countries. There is solid evidence, instead, that the interpretive model developed for the United States applies to at least one socialist society.⁴

The United States and Poland, of course, are both Western societies. Are the processes similar in non-Western societies? The Japanese study provides an excellent test of whether our interpretation of the U.S. and Polish findings applies as well to a non-Western industrialized society. In the main, the findings for Japan are markedly consistent with those for the United States and Poland. Social-stratification position is related to values, to social orientations, and to cognitive functioning in the same way, although perhaps not to quite the same degree, as in the United States and Poland (Kohn, Naoi, Schoenbach, Schooler, and Slomczynski 1987). Occupational self-direction has markedly similar effects on psychological functioning in Japan as in the West (Naoi and Schooler 1985). Thus, despite pronounced cultural differences, and despite the sharper division between the primary and secondary sectors of the economy in Japan, the linkages of social stratification to occupational self-direction, and of occupational self-direction to personality, are much the same in Japan as in the United States and Poland. The U.S. and Polish findings are not limited to Western society. Here, again, a single cross-national comparison yields immense benefits for our ability to test the generality of a set of empirical relationships and their interpretation.

Moreover, since the United States, Poland, and Japan are such diverse societies, the set of three studies provides *prima facie* evidence that the psychological impact of social stratification is much the same, and for much the same

⁴ The Polish study provides many further examples of cross-national similarity. We have found, for example, that in both Poland and the United States, occupational self-direction not only affects intellectual process, but does so consistently for younger, middle-aged, and older workers (J. Miller, Slomczynski, and Kohn 1985). We have further found that, in both the United States and Poland, the social-stratification position of the parental family has a considerable impact on the values of its adolescent and young-adult offspring (Kohn et al. 1986). The family's stratification position affects both father's and mother's occupational self-direction; each parent's occupational self-direction affects that parent's values; the parents' values affect their children's values. For present purposes, these findings are important primarily because they show how cross-national evidence strengthens the argument that the processes by which social stratification affects values and orientations, even into the next generation, are essentially the same for a socialist and a capitalist society.

reasons, in *all* industrialized societies. Admittedly, negative evidence from research in *any* industrialized society would require a modification of this hypothesis or a restriction of its generality. Admittedly, too, the interpretation speaks only to existing societies. We can say nothing from this evidence as to whether it would be possible to have an industrialized society in which one or another link in the explanatory chain is broken—a society with a less pronounced system of social stratification; a society in which social-stratification position is not so intimately linked with opportunities for occupational self-direction; even a society where occupational self-direction has less impact on personality.⁵ Nevertheless, the Polish and Japanese studies do tell us that in decidedly diverse societies—arguably, in all industrialized societies—social stratification is associated with values, social orientations, and cognitive functioning, in large part because people of higher position have greater opportunity to be self-directed in their work.

Whether or not this interpretation is correct, it does illustrate my central point: Where one finds cross-national similarities, then the explanation need not, indeed should not, be focused on the particular histories, cultures, or political or economic circumstances of each of the countries, but instead should focus on social-structural regularities common to them all.

In studying social stratification, I am of course dealing with a feature of social structure that is notably similar in all industrialized societies (Treiman, 1977). I would like to extend the argument a bit, to suggest that even where some feature of social structure is not “identical” in all the countries being compared, but only “equivalent,” it is still possible to find cross-nationally consistent relationships between contemporaneous social structure and personality. More than that, it is still appropriate to interpret these consistent relationships in terms of contemporaneous social structure, however much that feature of social structure has been shaped by the particular histories and cultures of those countries.

My illustration here comes from our analysis of position in the class structure and personality in the United States, Japan, and Poland (Kohn et al. 1987). For all three countries, we have adapted the same basic idea—that social classes

are to be distinguished in terms of ownership and control of the means of production, and control over the labor power of others—to the particular historical, cultural, economic, and political circumstances of the country. (For Poland, where ownership of the means of production is not a primary desideratum of class, *control* over the means of production and over the labor power of others is our primary criterion of class position.) The guiding hypothesis is that social class would bear a similar relationship to personality as does social stratification. Hence, we hypothesized that, in all three countries, those who are more advantageously situated in the class structure are more self-directed in their values and orientations, and are more intellectually flexible, than are those who are less advantageously situated. Our further hypothesis, again paralleling what we have learned for social stratification, is that, in all three countries, the explanation lies mainly in the greater opportunities for occupational self-direction enjoyed by those who are more advantaged in class position. The hypotheses, then, are simple extrapolations to social class from what we have consistently found to be the psychological impact of social stratification; the new element is the much greater country-to-country variability of class structures than of stratification systems.

Both hypotheses are confirmed. All three countries can be meaningfully thought to have class structures; class position has similar effects on cognitive functioning, values, and orientation in all three countries; and class affects these facets of psychological functioning for essentially the same reason—because of the intimate relationship between position in the class structure and opportunities afforded for occupational self-direction. Hence, to extrapolate, it is no bar to structural interpretation that social structures have been shaped by distinctly different historical processes.

Cross-national Differences

Interpreting differences, as I said earlier, is where things become much less certain and much more difficult. The key, of course, is the truism that if consistent findings have to be interpreted in terms of what is common to the countries studied, then inconsistent findings have to be interpreted in terms of how the countries—or the studies—differ. This truism, unfortunately, gives no clue as to which of the many differences between countries or between studies lies at the heart of the differences in findings. Prudence dictates that the first hypothesis one entertains is that the inconsistent findings are somehow a methodological artifact. As Bernard Finifter noted:

⁵ Michael Burawoy's (1979, p. 13) warning is pertinent, even though our research transcends capitalist society: “By taking the particular experiences of capitalist society and shaping them into universal experiences, sociology becomes incapable of conceiving of a fundamentally different type of society in the future; history is endowed with a teleology whose realization is the present.”

There is a curious inconsistency in the way researchers interpret results from attempted replications when discrepancies crop up. Failure to reproduce a finding in the *same* culture usually leads the investigator to question the reliability, validity, and comparability of the research procedures used in the two studies for possible method artifacts. But failure to corroborate the same finding in a *different* culture often leads to claims of having discovered "cultural" differences, and substantive interpretations are promptly devised to account for the apparent differences. (1977, p. 155)

Issues of method. The most fundamental methodological issue is whether the concepts employed in the analyses are truly equivalent. Stefan Nowak posed the issue with characteristic clarity:

How do we know we are studying "the same phenomena" in different contexts; how do we know that our observations and conclusions do not actually refer to "quite different things," which we unjustifiably include into the same conceptual categories? Or if they seem to be different, are they really different with respect to the same (qualitatively or quantitatively understood) variable, or is our conclusion about the difference between them scientifically meaningless? (1976, p. 105) (See also Almond and Verba 1963, pp. 57-72; Scheuch 1967, 1968; Smelser 1968; Nowak 1977; Marsh 1967; and Armer 1973).

The issue is so complex that a thorough treatment would require quite another essay. In this essay, instead, I simply assume equivalence of concepts and go on to consider more mundane methodological differences.

In principle, methodological differences between studies could produce either consistent or inconsistent findings (Finifter 1977). Still, when one finds cross-national similarities despite differences in research design, even despite defects in some of the studies, it is unlikely that the similar findings were actually produced by the methodological differences. Substantive similarity in the face of methodological dissimilarity might even argue for the robustness of the findings. But when one finds cross-national differences, then dissimilarities and defects in research design make for an interpretive quagmire—there is no way to be certain whether the apparent cross-national differences are real or artifactual.

It can be terribly perplexing not to know whether an apparent cross-national difference is merely a methodological artifact. I know, for example, of two studies of the interrelationship of social stratification, occupational self-direction, and personality in less than fully

industrialized societies, neither of which shows the pattern that has been consistently found in fully industrialized societies. One study was conducted in Taiwan before that island became as industrialized as it is today (Stephen Olsen 1971), the other in Peru (Scurrah and Montalvo 1975). In Taiwan, the relationship between social stratification and parental valuation of self-direction was essentially the same as has been found in more industrialized societies, but occupational self-direction fails to explain this relationship. In Peru, the correlations of social stratification with such aspects of personality as fatalism, trust, and anxiety are similar to those found in more industrialized societies, but occupational self-direction explains only a modest portion of these correlations.

Should we therefore restrict the interpretation that occupational self-direction is of central importance for explaining the psychological impact of social stratification to apply only to fully industrialized societies? Perhaps we should, and one can readily think of reasons why the interpretation might not apply to partially industrialized societies—for example, the link between social stratification and occupational self-direction may be weaker in such societies. But, since neither the Taiwan nor the Peru study is truly comparable to those done in industrialized societies (see the discussion in Kohn and Schooler 1983, pp. 293-94), the issue is very much in doubt. The Taiwan and Peru studies leave us in a quandary: They raise doubts as to whether the interpretation does apply to partially industrialized societies, but they do not provide convincing evidence that it does not.

To obviate the possibility that differences in findings are merely an artifact of differences in method—in the nature of the samples, in the meaning of the questions asked, in the completeness of data, in measurement—one tries to design the studies to be comparable, to establish both linguistic and conceptual equivalence in questions and in coding answers, and to establish truly equivalent indices of the underlying concepts (Scheuch 1968). Edward Suchman (1964, p. 135) long ago stated the matter with elegant simplicity: "A good design for the collection of comparative data should permit one to assume as much as possible that the differences observed . . . cannot be attributed to the differences in the method being used." Unfortunately, one can never be certain. The best that is possible is to try to establish damage control, to present whatever evidence one can that methodological incomparables are not so great as to explain the differences in findings. Short of that, it remains a gnawing doubt.

My colleagues and I have written extensively about the technical issues in achieving true cross-national comparability, particularly those

involved in interviewing and in index construction (J. Miller, Slomczynski, and Schoenberg 1981; Slomczynski et al. 1981; J. Miller et al. 1985; Kohn et al. 1986). So, too, have many other scholars (see, in particular, Scheuch 1968; Przeworski and Teune 1970; Armer 1973; Elder 1976; Kuechler 1986). Therefore, I do not discuss these issues further here. Instead, I assume comparability of methods (as well as comparability of concepts) and go on to the equally perplexing substantive issues in interpreting cross-national differences.

Substantive interpretations of cross-national differences. Finding a cross-national difference often requires that we curtail the scope of an interpretation, by limiting our generalizations to *exclude* implicated variables or relationships or types of countries from a more encompassing generalization. Ultimately, though, we want to *include* the discrepant findings in a more comprehensive interpretation by reformulating the interpretation on a more general level that accounts for both similarities and differences. Thus, although the discovery of cross-national differences may initially require that we make a less sweeping interpretation, in time and with thought, it can lead to more general and more powerful interpretations.

I wish that I could offer from my research an example of a powerful reinterpretation derived from coming to terms with cross-national differences. Instead, I can only share with you my dilemma in still not fully understanding some differences that I have been struggling to understand for some years. I may not convince you that discovering cross-national differences necessarily leads to new understanding, but I shall certainly convince you that the discovery of such differences forces one to question generalizations made on the basis of studying only one country. To illustrate, I use the most perplexing cross-national inconsistencies that we have found in the U.S.-Polish-Japanese comparisons (Kohn et al. 1987).

Quite in contrast to our consistent findings about the relationship of social stratification to other facets of personality, we have found a decided inconsistency in the relationship between social stratification and a principal underlying dimension of orientations to self and others—a sense of well being *versus* distress. In the United States, higher stratification position *decreases* feelings of distress; in Japan, there is virtually *no* relationship between social stratification and feelings of distress; and in Poland, higher stratification position *increases* feelings of distress.⁶ The

magnitude of the correlation is not great in any country, but the inconsistency in direction of relationship is striking. Similarly for social class: In the United States, members of more advantaged social classes, managers in particular, have a greater sense of well-being; members of less advantaged social classes, blue-collar workers in particular, have a greater sense of distress. In Poland, quite the opposite: It is the managers who are more distressed, the blue-collar workers who have a greater sense of well-being. In Japan, as in the United States, managers have a strong sense of well-being, but it is the white-collar—not the blue-collar—workers who are most distressed.

Why don't advantageous positions in the stratification and class systems have cross-nationally consistent effects on the sense of distress? On one level, this question is readily answered: Our analyses show that stratification and class matter for psychological functioning primarily because people of more advantaged position have greater opportunity to be self-directed in their work. But we find, in causal models of the reciprocal effects of occupational self-direction and distress, that although occupa-

aspects of social orientation, but affects *some* aspects of self-conception differently. In particular, in the United States, higher stratification position is associated with greater self-confidence and less anxiety; in Poland, quite the opposite.

"Social orientation" and "self-conception," however, are merely convenient rubrics; they are not underlying *dimensions* of orientation. Schooler and I (Kohn and Schooler 1982; 1983, Chapter 6) subsequently did a second-order confirmatory factor analysis of the several first-order dimensions of orientation, using U.S. data, to demonstrate that there are two underlying dimensions: self-directedness of orientation *versus* conformity to external authority, and a sense of well-being *versus* a sense of distress. Self-directedness of orientation implies the beliefs that one has the personal capacity to take responsibility for one's actions and that society is so constituted as to make self-direction possible. It is reflected in not having authoritarian conservative beliefs, in having personally responsible standards of morality, in being trustful of others, in not being self-deprecatory, in not being conformist in one's ideas, and in not being fatalistic. Distress is reflected in anxiety, self-deprecation, lack of self-confidence, nonconformity in one's ideas, and distrust. We have since shown that these same two dimensions underlie the several facets of orientation in Poland and in Japan (Kohn et al. 1987). The basic parameters of the Polish and Japanese models, in particular the relationships between second-order and first-order factors, are quite similar to those for the U.S. model. In all three countries, there is a strong positive relationship between social stratification and self-directedness of orientation. The relationship between social stratification and the sense of distress, however, is neither strong nor cross-nationally consistent: the correlations are -0.18 for the United States, -0.01 for Japan, and $+0.15$ for Poland.

⁶ In our original comparative analysis of the United States and Poland (Slomczynski et al. 1981), we put the issue somewhat differently: Social stratification has similar effects in the United States and Poland on all

tional self-direction has a statistically significant effect (negative, of course) on the sense of distress for the United States and Japan, it has no effect at all for Poland. This is in marked contrast to the cross-nationally consistent effects of occupational self-direction on intellectual flexibility, values, and self-directedness of orientation. One can, in fact, incorporate the cross-national inconsistency into an encompassing generalization: Where occupational self-direction has cross-nationally consistent effects on psychological functioning, so too do social stratification and social class; where occupational self-direction fails to have consistent effects, stratification and class also have inconsistent effects.

On another level, though, the question persists: *Why* doesn't occupational self-direction mitigate against distress in Poland, as it does in the United States and Japan? Moreover, occupational self-direction does not provide as effective an explanation of the relationships of stratification and class with distress in *any* of the three countries as it does for their relationships with other facets of personality in *all* three countries. Given the rather substantial effect of occupational self-direction on distress for the United States, we might well expect a higher correlation of social stratification with distress than the -0.18 that we actually do find. We should *certainly* expect a higher correlation than the -0.01 that we actually do find for Japan. We should expect *no* relationship, not a positive relationship, for Poland. Clearly, more than occupational self-direction is involved in explaining the relationships of stratification and class to distress. My formulation, which implies that occupational self-direction, and therefore also stratification and class, would have an impact on feelings of distress consistent with its impact on values, self-directedness of orientation, and cognitive functioning, must be revised.

It is not at all certain from the evidence at hand, though, whether the interpretation requires minor revision or extensive overhaul. I am reasonably certain that the cross-national differences are not merely a methodological artifact, for example in the conceptualization or measurement of distress. In particular, the cross-national differences are found, not only in analyses using the "higher-order" concept, distress, but also in analyses using the "first-order" concepts, notably self-confidence and anxiety (see note 6). The issues are substantive, not methodological.

In any reformulation, it is essential that we not lose sight of the fundamental principle that any explanation of cross-national *differences* must also be consistent with the cross-national *similarities*. To be valid, any explanation has to explain why we find cross-national inconsisten-

cies *only* for the sense of distress, not for values, for self-directedness of orientation, or for cognitive functioning. Explanations so broadly framed as to lead one to expect Polish or Japanese men of more advantaged position to value conformity for their children, to have a conformist orientation to self and society, or not to be intellectually flexible, could not be valid. Nor would it make any sense to explain the findings in terms of a weaker linkage of social stratification or of social class to occupational self-direction in Poland or in Japan than in the United States, or in terms of occupational self-direction being any less important for Polish or Japanese men than for U.S. men.

As I see it, there are at least five ways that my interpretation might be reformulated:

The simplest reformulation would be to limit the scope of the interpretation to exclude the sense of distress; for as-yet unknown reasons, an interpretation that does apply to cognitive functioning, values, and self-directedness of orientation seems not to apply to the affective realm. This reformulation simply curtails the scope of my interpretation, until such time as we are able to develop a more general interpretation that incorporates cross-national differences along with cross-national similarities.

A second type of reformulation would posit that the psychological mechanisms by which job conditions affect distress may be different from those by which job conditions affect cognitive functioning, values, and self-directedness of orientation. Such a reformulation might or might not emphasize job conditions different from those that I have emphasized; it certainly would posit different *processes* by which job conditions affect personality. Mine is a learning-generalization model: People learn from their job experiences and apply those lessons to non-occupational realms of life (Kohn 1985). One could argue that the inconsistent effects of occupational self-direction on the sense of distress raise questions as to whether a learning-generalization model applies to this facet of personality. Perhaps, instead, one should employ some other model of psychological process—a "stress" model is the obvious candidate—for understanding the effects of job on the sense of distress. The "stress" model posits that job conditions affect personality, in whole or in part, because they induce feelings of stress, which in turn have longer-term, off-the-job psychological consequences, such as anxiety and distress. Clearly, "stress" is a plausible link from job conditions to distress. But I think the evidence for a "stress" model, even when applied only to anxiety and distress, is less than compelling (Kohn 1985); moreover, positing different mechanisms for different facets of personality would be, at best, inelegant.

A related possibility, one that is much more to my liking, retains the learning-generalization model but expands the range of pertinent job conditions. This reformulation begins with the U.S. finding that job conditions other than those directly involved in occupational self-direction are more important for distress than for other facets of personality (Kohn and Schooler 1982; 1983, Chapter 6). *Some* of these job conditions are related to stratification and class, hence might explain the effects—or lack of effects—of stratification and class on distress. The crux of this reformulation is the hypothesis that the effects of these other job conditions on distress may be at odds with, and perhaps more important than, those of occupational self-direction. We have some pertinent, albeit limited, evidence that lends credence to this possibility (Kohn et al. 1987). In the United States, for example, job protections (such as seniority provisions in union contracts) mitigate against distress. Nonetheless, the very people who at the time of our interviews enjoyed the greatest job protections—the blue-collar workers—were also the most distressed. Blue-collar workers were distressed *because* they lacked opportunities for occupational self-direction and *despite* the job protections that many of them, particularly union members, enjoyed. Occupational self-direction and job protections seem to have countervailing effects, which may account for the relatively modest relationships of both social stratification and social class with distress, even in the United States.

For Japan, we find that believing that one works under considerable pressure of time, and believing that people in one's occupation are at risk of being held responsible for things outside of their control, are both related to distress. Although these findings may merely reflect a propensity of distressed people to overestimate the pressures and uncertainties of their jobs, it is at least a plausible hypothesis that such job conditions do increase distress. Our causal models suggest as well that either education itself, or job conditions related to education, increases distress. The countervailing effects of occupational self-direction, education, and other job conditions correlated with them both, may help explain why stratification and class have so little net effect on distress in Japan.

For Poland, we lack information about job conditions other than those directly pertinent to occupational self-direction. We do, however, have one fascinating bit of information that may help explain what it is about the conditions of life experienced by Polish *managers* that makes them more distressed than members of other social classes, quite in contrast to the situation of managers in the United States and Japan. We find that one segment of the Polish managerial

class is particularly distressed—those managers who are not members of the Polish United Workers (Communist) Party. There are too few non-Party managers for this finding to be definitive, but I think it suggestive that the non-Party managers have decidedly higher levels of distress, compared not only to managers who are members of the Party, but also compared to members of any other social class, Party members or not. The implication, I think, is that being a non-Party manager in the Polish system of centralized planning entails uncertainties, risks, and insecurities greater than those experienced by managers who are members of the Party, and greater than those experienced by managers in the less centralized systems of capitalist countries. The Polish system may hold these managers responsible for accomplishments they have neither the leeway nor the resources to achieve. By the same token, the U.S. and Japanese systems may lead managers to feel more in control of the conditions of their lives than they really are.

Our evidence suggests, then, that not only does occupational self-direction fail to have the cross-nationally consistent effect on distress that it has on other facets of psychological functioning, but also, that other job conditions associated with stratification and class may have countervailing effects. What is lacking is adequate information about these other job conditions.

A fourth type of reformulation would take greater account of the processes by which people attain their occupational positions and of the meaning these positions have to them. Slomczynski, Miller, and Kohn (1981) speculated at length about the implications of post-World War II historical developments that resulted in differences between the United States and Poland in structural mobility, job-selection processes, and the symbolic importance attached to class position—differences that might explain why social stratification bears a different relationship to distress in the two countries. These speculations still seem to me to be plausible and they are certainly potentially testable. One could similarly point to differences between Japan and the West in the structure of industry, particularly in the sharper division in Japan between primary and secondary sectors of the economy, that might be pertinent to explaining why stratification has so little relationship to distress in Japan, and why Japanese white-collar workers are more distressed than are members of other social classes.

Finally, one could broaden the scope of the interpretation even more, by taking account of conditions of life other than those involved in job and career. It might be, for example, that cross-national differences in family structure, or

in religious belief, or in whether the urban population is primarily rural in origin, or in "national culture" bear on the sense of distress. The pivotal questions, though, are not whether family, religion, rural origins, or culture account for differences in Polish, Japanese, and American men's sense of distress, but whether such non-occupational conditions help explain why *social stratification* and *social class* bear different relationships to the sense of distress in Poland, Japan, and the United States.

We do not have the evidence to test any of these interpretations. Each type of reformulation (other than simply limiting the scope of the interpretation to exclude distress) would require a different type of data. To test a "stress" formulation would require more information about the relationship between objective job conditions and the subjective sense of "stress" in one's work, and about the relationship between job *stress* and off-the-job *distress*. Similarly, to test any other model of psychological process would require data directly pertinent to that formulation. To test the hypothesis that job conditions other than those involved in occupational self-direction help explain the relationships of social stratification and social class to distress would require that we obtain much fuller information in all three countries about those job conditions thought to be productive of a sense of distress. To test the hypothesis that different processes of educational and occupational attainment account for the differential effects of stratification and class on the sense of distress would require information of yet another type: historical information about the impact of changes in the educational and occupational structures of Poland, Japan, and the United States since World War II as they impinged on particular cohorts of Polish, Japanese, and American workers. And then, finally, to test the rather vaguely formulated hypothesis that non-job conditions explain the cross-nationally inconsistent relationships of both class and stratification with distress would require information about the interrelationship of stratification and class with these other lines of social and cultural demarcation, in all three countries.

In any case, on the basis of presently available evidence, I still do not have a fully adequate explanation of why social stratification and social class have cross-nationally inconsistent effects on the sense of distress. Perplexed though I am, I value the cross-national evidence for making clear where my interpretation applies and where it does not, thus defining what is at issue. Were it not for the Polish and Japanese findings, there would have been little reason to doubt that my interpretation applies, albeit not quite as well, to the sense of distress, just as it

clearly does to values, self-directedness of orientation, and cognitive functioning.

SOME GENERAL CONSIDERATIONS

I can now address some more general issues about cross-national research that I deliberately deferred until I had offered some concrete examples. These remarks are primarily addressed to research in which nation is treated as context.

1. In whose interest is cross-national research? This seemingly innocuous question contains a range of serious ethical and professional issues. At its worst, as in the infamous Camelot affair (Horowitz 1967), cross-national research has been used in the service of political oppression. In a less dramatic way, cross-national research has too often been a mechanism by which scholars from affluent countries have employed scholars in less affluent countries as data-gatherers, to secure information to be processed, analyzed, and published elsewhere, with little benefit either in training or in professional recognition for those who collected the data (Portes 1975; Scheuch 1967). These are complex issues, where surface appearances may be misleading. But, certainly, the history of cross-national research has not been entirely benign.

Past sins and mistakes notwithstanding, cross-national research need not be employed in the service of academic or other imperialisms. My own research is again illustrative. As a matter of historical record, it was not I but Włodzimierz Wesolowski (1975, p. 98) who proposed the Polish-U.S. comparative study. He did so for precisely the same reason I found the prospect so attractive when he suggested it to me: to see whether the U.S. findings would apply to a socialist society. The study was funded and carried out by the Polish Academy of Sciences, who thought the issues important for Polish sociology and Polish society. The extension of the U.S.-Polish comparison to encompass Japan came about because Ken'ichi Tominaga, his Japanese colleagues, and the Japanese universities and foundations that funded this research were as interested as were the Americans and the Poles in seeing whether these phenomena are similar in that non-Western society.

The opportunities for genuine cross-national collaboration today, when there is a thriving, highly professional sociology in many parts of the world, are much greater than they were only a few years ago. Today it is quite possible, and advantageous for all concerned, for sociologists of many countries to collaborate effectively. The theoretical and policy issues to be addressed in cross-national research can be—in principle,

ought to be—equally important for sociologists of all the countries concerned.

2. *Is cross-national research distinctly different from research that compares social classes, or ethnic groups, or genders in a single country?* I see cross-national research as one type of comparative research. In many discussions, though (see, for example, Armer and Grimshaw 1973), the term “comparative research” is treated as synonymous with cross-national research, as if the only possible comparison were *inter-national* comparison; this I regard as hubris on the part of the internationalists. In other discussions (e.g., Hopkins and Wallerstein 1967) the term “comparative” is used more broadly and “cross-national” is limited to what I consider to be only one type of cross-national research, transnational research. And in still other discussions (e.g., Ragin 1982), comparative research is seen as that particular type of cross-national research where “society” is used as the explanatory unit.⁷ These varying usages seem to me to impede meaningful discourse. I think it best to use the commonsense meanings of both “comparative” and “cross-national.”

My own research shows that cross-national research is no different in principle from other comparative research, although in practice it is likely to be more complex, especially as one tries to interpret cross-national inconsistencies. What makes it worth distinguishing cross-national research from other types of comparative research is that a much broader range of comparisons can be made: comparisons of political and economic systems, of cultures, and of social structures. Any comparisons we make within a single country are necessarily limited to the one set of political, economic, cultural, and historical contexts represented by that particular country. I simply cannot imagine any study of the psychological impact of class and stratification, done entirely within the United States, that could have extended the scope of our knowledge, or the power of our interpretation, as greatly as did the Polish and Japanese studies.

3. *Why put the emphasis on cross-national? Why not cross-cultural or cross-societal or cross-systemic? Doesn't the term cross-national ascribe a greater importance to the nation-state than it deserves?* I use the term *cross-national* mainly because *nation* has a relatively unambiguous meaning. *Cross-cultural* can mean anything from comparing subcultures within a

single nation, for example, comparing Mexican-American and Anglo-American subcultures in the Southwest region of the United States, to comparing very large groupings of nations that share broadly similar cultures, as in William Goode's (1963) comparative analyses of historical changes in family patterns in “the West,” Arabic Islam, Sub-Saharan Africa, India, China, and Japan. Similarly, as Charles Tilly (1984) cogently argues, it is extremely difficult to define what is a “society.” And the term *cross-systemic* is so vague as to have little research utility.

I do not think that this usage of *nation* necessarily implies anything about the importance of nation, or the nation-state, as such, any more than *cross-cultural* implies (or, at any rate, should imply) that *culture* is the explanatory desideratum. Furthermore, we learn something about the importance or lack of importance of the nation-state by discovering which processes transcend national boundaries and which processes are idiosyncratic to particular nations or to particular types of nations. In choosing which nations to compare, sometimes we do mean to compare nation-states; how could Theda Skocpol (1979) have done differently in her analyses of revolutions? When we deal with governments, laws, and legally regulated institutions, the nation-state is necessarily a decisive context. But sometimes we use *nation* as a way of comparing cultures; in this case, we would choose nations with distinctly different cultures, for example, by comparing the United States to Japan, not the Federal Republic of Germany to Austria. Sometimes we mean to compare political and economic systems, as in comparing the United States and Japan to Poland, or if one wanted to minimize cultural differences while contrasting political systems, in comparing the German Democratic Republic to the Federal Republic of Germany. Cross-national research is flexible, offering the advantage of making possible multiple types of comparison within one general analytic framework.

This flexibility, it must be recognized, comes at a price: When one finds cross-national differences, it may not be clear whether the crucial “context” that accounts for the differences is nation or culture or political or economic system (Scheuch 1967). Still, one can at least try to assess which of these contexts might logically be pertinent to explaining a particular cross-national difference. And, for many types of research, one can then proceed to design new studies to differentiate among the contexts.

4. *How many nations are needed for rigorous cross-national analysis, and how should they be chosen?* For some purposes, particularly when using secondary data to establish cross-national

⁷ The issues in distinguishing cross-national research from other comparative research are discussed thoughtfully and at length by Grimshaw (1973), who, *inter alia*, reviews and summarizes pertinent earlier discussions by Erwin Scheuch and Neil Smelser. See also Marsh (1967) and Zelditch (1971).

generalities, it is desirable to include all countries for which pertinent data can be secured. Thus, Alex Inkeles's pioneering paper, "Industrial Man," (1960) gained considerably from its demonstration that the relationship between social stratification and many facets of values and beliefs is consistent for a wide array of countries. Seymour Martin Lipset's argument in "Democracy and Working Class Authoritarianism" (1959), that the working class is more "liberal" than the middle class on economic issues, but illiberal on issues of civil liberties and civil rights, was the more forceful because he marshalled evidence from several countries. Donald Treiman's (1977) comprehensive analysis of the similarity of social stratification systems throughout the industrialized world effectively utilized data from many countries and was enriched as well by information about the historical past. Janet Chafetz and Anthony Dworkin's (1986) analysis of the size and range of ideologies of women's movements gained scope and power from their use of data from a considerable diversity of countries. With similar intent, I have searched for all extant studies to establish the "universality" of a self-direction/conformity dimension to parental values in industrialized societies (Kohn and Schoenbach 1980). I have also searched for evidence in studies conducted in many countries for cross-national tests of one or another link in my explanatory schema (Kohn 1977, 1981; Kohn and Schooler 1983, Chapter 12). And, as recently as the July 1987 issue of the *American Journal of Sociology*, Alejandro Portes and Saskia Sassen-Koob demonstrated anew the usefulness of a broad comparative sweep, in showing that, contrary to all theoretical belief, the "informal," "underground" sector of the economy is not merely a transitional phenomenon of Third World development, but is instead a persistent and integral part of the economies of even advanced capitalist nations. In doing secondary analyses it is highly advantageous to utilize data from all countries for which pertinent information can be secured.

Moreover, even in collecting primary data, there can be considerable advantage to assessing the consistency of findings across a range of nations, cultures, and political systems, as Inkeles and Smith showed in *Becoming Modern* (1974) and as Erik Olin Wright and his colleagues are demonstrating anew, in a very different type of research endeavor, in their multi-nation studies of social class.

Yet, it is expensive, difficult, and time-consuming to collect data in many countries. We are rarely able to collect reliable data about enough nations for rigorous statistical analysis. Nor are we ordinarily able to study many countries in sufficient depth for intensive

comparison. It is not necessarily true that the more nations included in the analysis, the more we learn. There is usually a tradeoff between number of countries studied and amount of information obtained. In this tradeoff, investigators can certainly disagree about the relative importance of number of countries and depth of information. And the same investigator might make different choices for different substantive problems. By and large, though, I would opt for fewer countries, more information.

My own preferred strategy is the deliberate choice of a small number of nations that provide maximum leverage for testing theoretical issues. One may begin with a study in one country, with subsequent extensions of the inquiry to other countries, as my collaborators and I have done in using Poland to learn whether U.S. findings are applicable to a socialist society and Japan to learn whether such findings apply to a non-Western, industrialized society. Alternatively, one can select pivotal countries that provide maximum opportunity to test some general hypothesis, as Theda Skocpol (1979) did in selecting France, Russia, and China for her study of the causes and consequences of social revolutions, or as John Walton (1984) did in selecting the Philippines, Colombia, and Kenya for his comparative analysis of national revolts in underdeveloped societies. Whether one starts with one country and then extends the inquiry to others, or begins with a small set of countries, does not seem to be crucial. Either way, the deliberate choice of a small number of countries for *systematic, intensive study* offers maximum leverage for testing general propositions about social process.

How, then, does one decide which countries to compare? The only rule of thumb I know is that cross-national research is most useful when it can resolve a disputed question of interpretation. It follows that what is a strategic comparison at one stage of knowledge may be overly cautious or overly audacious at another.

At an early stage of my own research, for example, when I had established little more than that white middle-class parents in Washington, DC valued self-direction for their children more highly than did white working-class parents in that same city at that one time, the focal issue was Washington's atypicality. Was the Washington finding peculiar to the times and circumstances of this relatively affluent, economically secure, mainly non-industrial city in the late 1950s, or did that finding reflect a more general relationship between social stratification and parental values? Leonard Pearlman (1971; Pearlman and Kohn 1966) resolved this question by demonstrating a similar relationship of social stratification to parental values in Turin, Italy—an industrial city, less affluent and less

economically secure than Washington, and with a much less conservative working-class tradition. A more cautious choice of locale would have been an industrial city in the United States or perhaps in English-speaking Canada or in Australia. A more audacious choice would have been an industrial city in a non-Western country or in a socialist country. Turin, to my mind, was neither too cautious nor too audacious a choice: different enough from Washington that if the findings proved to be similar, the increment to our knowledge would be considerable, but not so different from Washington that if the findings had proved to be dissimilar, we would have been at a complete loss to know why. Turin was not the only city that could have served our purposes; several other West European cities might have served as well. In that state of our knowledge, though, I do not think that Warsaw or Tokyo would have been optimal choices. It would have been too difficult to interpret dissimilar findings.

Later, when we had solid evidence about the generality of our findings in Western, capitalist societies, studies in Poland and Japan became especially useful. The issue was no longer Washington's atypicality, but whether the relationships among social stratification, job conditions, and psychological functioning were peculiar to capitalist society or to Western society. Here, again, we could have chosen other countries that might have served our purposes as well: perhaps Hungary instead of Poland, or if it had been possible to do such research, there at that time, the Soviet Union; perhaps South Korea instead of Japan. It is often the case that no one country is uniquely appropriate for cross-national comparison. Other considerations—research feasibility, the availability of potential collaborators, funding, happenstance—may then legitimately enter in.

Were I to embark on a new comparative study today, the considerations would again be different, mainly because of what we now know from the Polish and Japanese studies, and because of new interpretive problems that have arisen from these studies. It would now be useful to study another socialist country and another non-Western industrialized country. It would also be useful to study a less than fully industrialized country, I think preferably (for the nonce) a capitalist country with a predominantly Western culture, perhaps a Latin American country. The possibilities for fruitful comparison do not shrink as one learns more, but actually grow.

The choice of countries should always be determined by asking whether comparing these particular countries will shed enough light on important theoretical issues to be worth the investment of time and resources that cross-

national research will certainly require (Galtung 1967, p. 440). One must always ask: If I find cross-national consistencies, will this particular cross-national comparison extend the scope of my interpretation enough to have made the venture worthwhile? And if I find differences, will this particular cross-national comparison shed light on crucial interpretive problems? Cross-national research is always a gamble; one might as well gamble where the payoff is commensurate with the risk.⁸

5. *What are the costs of doing cross-national research?* If, as I have argued throughout this essay, the advantages of cross-national research are considerable, so too are the costs. These costs are considerably greater than most investigators realize, great enough to make a rational person think twice about doing cross-national research when it is not needed or when it is premature.

Securing funds is always problematic, even (as in my own research) when financial support is obtained in the countries that are participating in the research. This, however, is only the first and by no means the most serious difficulty. Establishing collaborative relationships that can be sustained and will develop throughout the course of what can be counted on to be difficult research is much more problematic (Hill 1962; Sarapata 1985). Both the greatest benefits and the most difficult problems of cross-national research come from the collaborative relationships. If a good collaboration is like a good marriage, rewarding yet difficult, then a good cross-national collaboration is akin to a cross-cultural marriage that manages to succeed despite the spouses living much of the time in different countries, sometimes with considerable uncertainty about passports, visas, and the reliability and timeliness of mail delivery, and despite working in different institutional settings with conflicting demands and rewards. And still, it's far preferable to the alternatives. More than that, without good collaboration, many types of cross-national research are simply not possible.

The methodological pitfalls are another set of obstacles to good cross-national research; I have touched on some of them earlier in this essay. It would be hard to exaggerate the amount of time, thought, and analysis that must go into the effort

⁸ A corollary is that, if one wants to gamble audaciously, do so where the payoff will be considerable. A splendid example is provided by Nancy Olsen (1974). She not only extended to Taiwan the scope of our U.S. findings about the relationship between closeness of supervision and parents' values for their children, but also extended the scope of generalization about the institution in which close supervision is experienced, from paid employment to the family itself.

to achieve comparability of methods, concepts, and indices. There are also issues in the standards of research employed in different countries. Sometimes these issues become acutely problematic when one least expects them. As a simple yet telling example: The reason why we do not have Polish data about some of the job conditions that may be pertinent to distress is that the survey research specialists at the Polish Academy of Sciences refused to include questions about job conditions that did not meet their criteria of objectivity in a survey for which they were professionally responsible. Even when we appealed to them that cross-national comparability required their repeating the defects of the earlier U.S. study, they would not yield. They were as zealous in imposing their justifiable, yet irrelevant professional standards as were the clearance officers of the U.S. Department of Health, Education, and Welfare, and of the Office of Management and the Budget, in imposing their not nearly so justifiable requirements.

And still, there are yet more difficult problems, problems of interpretation. Particularly when one finds cross-national differences, an expert knowledge of all the countries is essential—a knowledge most easily achieved, of course, by collaborators who have expert knowledge of their own countries (see Kuechler 1986). Even when such collaboration exists, though, sharing knowledge, interpreting within a common framework, even having enough time together to think things through at the crucial junctures, does not come easily.

Unless one has a good reason why research *should* be cross-national, it generally isn't worth the effort of making it cross-national. Operationally, this means that one should do cross-national research either when a phenomenon cannot be studied in just one country (for example, the causes of revolutions) or else when some phenomenon has been well substantiated in one country and the next logical questions have to do with the limits of generality of what has been learned. In principle, but rarely in practice, it may be worth embarking on a cross-national study of a less well researched problem if you have good *a priori* reason to believe that important theoretical issues can be more effectively addressed by conducting the research in more than one country. I remain a strong proponent of cross-national research, but I would not wish to mislead anyone into thinking that its very considerable advantages do not come at equally considerable cost.

6. Finally, to return to a question that has pervaded this essay: *What role does history play in cross-national interpretation?* In posing this question, I most decidedly do not mean to cast doubt on the utility of historical analysis as a

method for doing cross-national research. I regard the persistent debate about the relative merits of historical and quantitative methods in cross-national research as a wasteful distraction, addressed to a false dichotomy.⁹ Each method is appropriate for some research purposes and inappropriate for others. Best of all, as Jeffery Paige (1975) demonstrated in his analysis of the relationship between agricultural organization and social movements in 70 developing nations, is to combine the two. My question concerns, not historical analysis as method, but history as explanation. At issue, of course, are the competing merits of idiographic and nomothetic explanation. I can hardly do justice to this complex question in the closing paragraphs of this essay, but I would at least like to point out that the issues are somewhat different when analyzing cross-national similarities from what they are when analyzing cross-national differences.

As I have argued throughout this essay, the interpretation of cross-national *similarities* should not focus on the unique historical experiences of each of the countries. One seeks to discover, instead, social-structural regularities that transcend the many differences in history, culture, and experience that occur among nations. This is true even in inquiries—Walton's (1984) *Reluctant Rebels* is a good example—where the evidence is mainly historical but the analysis searches, not for historical idiosyncrasies, but for historical commonalities. The intent in all analyses of cross-national similarities is to develop generalizations that transcend particular historical experiences in a search for more general explanatory principles. In short, the method may be historical, the interpretation should be sociological.

In a broader sense of history, of course, cross-national analysis, just as any other type of sociological analysis, cannot be ahistoric, even when much about history is only implicit in the interpretation (Sztompka 1986). To compare the impact of social stratification on personality in the United States and Poland, for example, assumes that we are comparing industrialized states that have shared much of Western history.

⁹ The methodological debate takes place on two levels: the type of analysis used *within* each nation and the type of analysis used for *comparing* nations. I see nothing of value in the first part of the debate; one uses whatever methods are appropriate to the task. The second part of the debate deals with real issues, for example, the meaningfulness of using "samples" of nations, the utility of statistical tests when basing one's analysis on the entire set of existing countries, and the difficulties of having to test multiple interactions on a necessarily small number of "cases" (see, e.g., Ragin 1982). This literature, despite its antiquantitative bias, offers some useful cautions.

That one is a capitalist state and the other a socialist state can be viewed, depending on how you read the broad sweep of history, as a comparison of different economic-political systems or as a comparison of different levels of political development. In either case, even though history is not treated explicitly, historical considerations are certainly there implicitly. And when one compares fully industrialized to partially industrialized societies, historical issues are necessarily at least implicit. Nevertheless, in interpreting cross-national similarities, history need not be at the forefront of attention.

In interpreting cross-national *differences*, by contrast, historical considerations cannot be merely implicit; history must come to the forefront of any interpretation. For example, after demonstrating remarkable parallels in both the causes and consequences of the French, Russian, and Chinese revolutions, Skocpol (1979) had to explain differences, particularly in revolutionary outcomes, in terms of historically unique circumstances. Similarly, when I find that social stratification and social class do not have the same impact on the sense of distress in the United States, Poland, and Japan, I have to look to the separate historical developments of the three countries, to try to discover what may explain the inconsistent findings. I maintain, though, that even in interpreting cross-national differences, explanation cannot consist merely in explicating pertinent historical differences. The object is not an understanding of history just for history's sake, but the use of history for understanding more general social processes. The interpretation must be historically informed, but sociological interpretations, even of cross-national differences, are quintessentially trans-historical.

EPILOGUE

In the preface to *Class and Conformity*, I made a declaration of faith: "The substance of social science knowledge comes from the process of speculation, testing, new speculation, new testing—the continuing process of using data to test ideas, developing new ideas from the data, doing new studies to test those ideas" (Kohn 1969, p. xii). I take this occasion to re-affirm this fundamental tenet of my scientific faith. Its relevance to this essay is, I trust, obvious: In the process of speculating, testing, and speculating anew, cross-national research, properly employed, provides uniquely valuable evidence. There is no other evidence so useful for confirming social-structural interpretations, or for discovering their limitations. Either way, cross-national research is of pivotal importance for the development and testing of sociological theory.

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