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Doing Social Research Under Conditions of Radical Social Change: The Biography of an Ongoing Research Project*

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In February 1992, while I was in Warsaw working with my collaborators on our study of social structure and personality under conditions of radical social change, I received word that I had been designated this year's Cooley-Mead awardee. The award was conditional on my giving an address that had not been committed already for publication. But a paper embodying the conceptualization and design of the research already had been so committed (Kohn et al. forthcoming), and the research had not proceeded nearly far enough to have produced any substantive findings that could provide the basis for an address. Lamenting this state of affairs, I commented to my Hopkins colleague and friend, the anthropologist of Romania, Katherine Verdery, that the only conclusion I could draw thus far from my efforts was that it was damned near impossible to do research under conditions of radical social change. Katherine properly retorted, "*Your* kind of research. Not ethnographic research." She was right, of course, and her remark stimulated me to write a quasi-ethnographic account of the project itself.

If there is a rationale for this paper above and beyond providing me with an excuse for telling some of my favorite anecdotes, it is simply this: One can learn a great deal about social and psychological phenomena by examining the problems one encounters in trying to study them.[1] Our problems have resulted mainly, albeit not entirely, from the research infrastructures of Poland and Ukraine falling into disarray. But why should *research* infrastructures *not* fall into disarray when *all* the institutional structures of Eastern European societies are disintegrating, as these countries make their uncertain transitions from centralized political and economic systems to whatever will be?

I have not the slightest doubt that each of my collaborators—Krystyna Janicka, Valery Khmelko, Bogdan Mach, Vladimir Paniotto, Kazimierz Slomczynski, and Wojciech Zaborowski, all of them deeply involved in the project—would tell a rather different, although consonant, history of the research. This is one participant's account, based mainly on extensive field notes and innumerable memoranda written very shortly after the events described here.[2]

PREHISTORY OF THE PROJECT

The start of it all was an event that marked the beginning of my awareness of those remarkable Poles. It took place at a world congress of the International Sociological Association (ISA) in Varna, Bulgaria, in September 1970. The program listed a session organized by the Soviet Sociological Association on social stratification in socialist society. That session turned out to be a sparring match between Soviets and Poles, with Hungarians joining in support of the Poles and East Germans in subservience to

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[1] For a fascinating demonstration of the validity of this observation, see William Form's (1976, pp. 277–99) depiction of what he learned about labor-management relations in the automobile industries of four countries while attempting to gain access to auto workers in those countries.

[2] Having been trained by Bill Whyte in the fine art of taking field notes, and being compulsive, I have several hundred pages of notes—raw materials enough for such an endeavor.

the Soviets—almost all of it in English, as if for my benefit.

This was a time of imposed orthodoxy in Eastern Europe, and the head of the Soviet delegation, a commissar named M. N. Rutkevich, was an especially severe imposer. The Soviet line—I will caricature it, but only slightly—was "Yes, we do have some occupational *differentiation* in socialist societies, but certainly not social *stratification;* that's impossible under socialism." The Polish response, put forth by their leading Marxist scholar, Wlodzimierz Wesolowski, in essence was "We've read our Marx, too, but we've also done surveys, and our findings come out remarkably similar to those of the West Europeans and the Americans. In socialist Poland, we certainly do have social stratification, and our system of social stratification is not much different from that of capitalist societies." This response infuriated Rutkevich and his followers. Their reaction seemed to spur Wesolowski and his compatriots to the energetic pursuit of what I only later learned was the Poles' favorite indoor game, baiting the Soviets.

Who are these incredible people? I had to find out. Under the constrained circumstances of the Varna Congress, the best I could do was to move to where the Poles were sitting and exchange what Americans call "business cards" and Japanese more appropriately call "name cards." This gesture was followed in later weeks and months by exchanges of books and reprints.

Four years later, at an ISA congress held in Toronto, I attended a similar session, this time co-chaired by Rutkevich and Wesolowski. Although the session was called "Transformations of Social Structure in the U.S.S.R. and Poland," it dealt with much the same issues, and the exchange was just as spirited. On this occasion, though, I received news from Wesolowski: I was on a list of people who would be invited to visit Poland, though the time of the visit was indefinite ("Ours is a planned society, so everything has to be worked out well in advance. . . ."). The invitation actually came only a few months later, and I visited Poland soon thereafter.

I will compress the most fascinating week of my professional life into one brief meeting, the climax of the week, on its penultimate day. I had begun the day with an intensive three-hour discussion with Stefan Nowak, the leading *non*-Marxist Polish sociologist. He then took me to visit Wesolowski at the third of Wesolowski's three offices; the other two were at the Academy (where he did his research) and the University (where he taught his students). The third was at the Institute for the Study of Fundamental Problems in Marxism-Leninism of the Polish United Workers' (Communist) Party, where Wesolowski was officially second, and operationally first, in command.

In his office at the Party Institute, with a large picture of Karl Marx looking down on us (and an empty hook, from which I supposed Vladimir Illich Lenin also was supposed to look down on us), Wesolowski put his proposition to me: "We have enjoyed your lectures and we have enjoyed having you in Poland. Now let us talk business." For a moment, I thought that Karl winked at me; I certainly looked up at him in some disbelief, hearing these words in that setting. The proposal, though, was simple and compelling. Wesolowski wanted to replicate my research in Poland; the aim of the inquiry was to learn whether my findings and interpretations about social stratification, job conditions, and personality would apply in a socialist society.[3] What could please me more? Of course I wanted to cooperate in such an endeavor.

Wesolowski went on: The study was to be theirs, paid for by them. They would own the data. As he put it, they had had too much experience with Big Brother to want any other arrangement. He asked me to be a technical

[3] The evening before, he had shown me a passage in his just-published book (Wesolowski 1975) where he had written of my *Class and Conformity* (as he translated roughly on the spot): "This controversial book cries out . . . to be tested in a socialist society..to see how universal are its conclusions." The main themes of that book (Kohn 1969), whose applicability to socialist Poland he proposed to test, were as follows: People's positions in the social stratification hierarchy have profound effects on their personalities; these effects occur primarily because stratification position strongly affects more proximate conditions of life, particularly job conditions; and job conditions, in turn, profoundly affect personality. Specifically, a higher position in the stratification order affords greater opportunity to be self-directed in one's work—that is, to work at jobs that are substantively complex, are not subject to close supervision, and are not routinized. The experience of occupational self-direction leads in turn to a higher valuation of self-direction for oneself and for one's children, and to a more self-directed orientation to self and to society.

consultant to the study, "and let's see how things develop from there." His proposal would become a model—I came to think of it as "the Wesolowski model"—that I would follow in all my cross-national research.

Wesolowski proposed that his protege, Kazimierz Slomczynski, come to spend a month at the National Institute of Mental Health (NIMH), where I was then employed, to translate Carmi Schooler's and my interview schedule into Polish and discuss that translation with me as he worked. Nowak said, "Mel, you're lucky. He's the best sociologist of his generation we have." I was delighted. Costs? "Since Slomczynski is a Polish citizen, we can pay his air fare to New York on LOT in zlotys. But he'd need transportation from New York to Washington, and he'd need living expenses in Washington." No problem: even if NIMH should balk at this minor expense, I could pay for a rail ticket. In any case, he would live with my wife and me.

Thus began more than a decade and a half of close collaboration with Slomczynski, who spent long periods working with me at NIMH and later at Hopkins. In turn, I visited Poland regularly. The project culminated in Slomczynski's and my publishing one book in Polish (Slomczynski and Kohn 1988) and another in English (Kohn and Slomczynski 1990); the latter was dedicated to Wesolowski as follows: "To Wlodzimierz Wesolowski, whose idea it was." We had found—with one major and intriguing exception[4]—that the U.S. findings and interpretation did apply to then-socialist Poland.

During those years, there occurred many important events that bear on the current research. Prominent among them were the advent of Solidarnosc, the imposition of martial law, and Wesolowski's dramatic resignation from the Party when it invoked martial law; reportedly he was the highest-ranking Party official to do so. On a bureaucratic pretext, he was forced out of the University—where he could contaminate students—but not out of the Academy. He had long since left the Party Institute.

ORIGINS OF THE SOVIET CONNECTION

Concurrently, almost by happenstance, I was involved in reestablishing relations between the American and the Soviet Sociological Associations. These relations had withered with the end of the *relative* freedom of the Khrushchev regime and the reimposition of orthodoxy under Brezhnev.

The involvement had begun at dinner one evening in December 1983 in Barcelona, during a meeting of the ISA Executive and Publications Committees. The official translator for Khatchik Momdjan, the Soviet member of those committees, told me, "Professor Momdjan would like to discuss possibilities for improving relations between Soviet and American sociologists." I readily agreed, on the rationale that it might not do much good—this was pre-Gorbachev—but at any rate couldn't *hurt* world peace. So, at dinner the next evening, the three of us sat aside from the others and talked.

The conversation began slowly, painfully so. Momdjan said something in Russian, which was translated as "Professor Momdjan says that it would be desirable to have more contacts between American and Soviet sociologists." I replied in kind, raising the ante just a bit. We went on in this manner for three or four interchanges, barely beginning to approach anything concrete, when I decided to speed up the process with a proposal for making a first small step toward meeting Momdjan's worthy objective. To my astonishment, the "translator" accepted my proposal and made a further, more substantial one of his own. I pointed to Momdjan, who had understood neither what I had said nor his "translator's" reply. The translator, who of course had caught my intent, said, "This is taking too long; I'll tell him later." So Momdjan—who was president of the Soviet Sociological Association at that time—was not a free agent (not that I had thought he was).

Eventually the three of us worked out

[4] The principal cross-national *difference* is in the relationships between social structural position and a sense of distress, in contrast to a sense of well-being. In the United States, managers have a strong sense of well-being, and manual workers are the most distressed; in Poland, nearly the opposite. Similarly, in the United States, the correlation between social stratification position and distress is negative; in Poland it is positive. Part of the explanation is that occupational self-direction does not have the cross-nationally consistent impact on distress that it has on other facets of psychological functioning. A more fundamental reason is that job conditions other than those directly involved in occupational self-direction, such as job risks and uncertainties and the protections from those risks and uncertainties, have effects countervailing those of occupational self-direction (see Kohn and Slomczynski 1990, ch. 8).

arrangements for a delegation from the Soviet Sociological Association to come to the 1985 ASA convention and present a set of papers about Soviet research on the sociology of work, the theme of that year's convention.[5] It wasn't a great set of papers, although the papers and the ensuing discussion revealed quite a lot, perhaps more than the presenters intended. Of far greater import than the session itself, the Soviet presentations led to the establishment of a series of joint US-USSR symposia in sociology and—with the advent of the Gorbachev era—to other innovations such as Soviet students coming to do graduate work in U.S. departments of sociology, several U.S. sociologists—I among them—giving lecture tours in the Soviet Union, several Soviet sociologists lecturing in the United States, and expanded exchange programs for mid-career sociologists.

At the first of these joint symposia, held in Vilnius, Lithuania in July 1987, I met Vladimir Yadov, the preeminent Soviet social psychologist and sociologist of work. I had been exchanging books and papers with Yadov for several years, but I had never met him in person; he hadn't been allowed out. Yadov and I explored the possibilities of his replicating my research in the Soviet Union. He could see no way to do it, though, because he was in disfavor with the authorities. He had few resources for any research and none at all for research on so ideologically sensitive a topic as social structure and personality.

I next saw Yadov in October 1988, at the second of the US-USSR symposia, which he and I co-organized at a conference center near Baltimore. The U.S. participants were greeted with startling news: the Central Committee of the Soviet Communist Party had legitimized sociology. One of their first acts had been to rename an Institute of the Soviet Academy of Sciences that had masqueraded under several other names, at long last giving it its proper name: the Institute of Sociology. More startling still, Yadov—who once had been expelled from the Leningrad branch of an earlier incarnation of that Institute—had been named director. He would now have authority and resources. All he would lack was time to do research.

With that in mind, Yadov had included as part of the Soviet delegation two Ukrainian sociologists, Valery Khmelko and Vladimir Paniotto, both of whom he respected greatly. With Yadov's encouragement, Khmelko and I had some exploratory discussions. (Paniotto, the methodologist, gave the lead to Khmelko, the social psychologist.) We spoke not so much about a research project per se as about the possibility of his spending two or three months with me at Hopkins (where I was now employed) if he could get a fellowship. All of this was exceedingly tentative, but nonetheless encouraging. The interest was there on both sides, but it wasn't at all clear how we could do it.

THE BEGINNINGS OF THE ACTUAL PROJECT

The next major steps towards a Ukrainian study occurred in December 1989. At that time I spent three weeks in the Soviet Union in a complicated mixture of activities, among them attending the third of the US-USSR symposia. This one was on public opinion research.

The morning after my arrival in Moscow, Yadov told me that I was to divide my time between Moscow and Kiev, attending the symposium in Moscow, lecturing in both places, and discussing research with Khmelko and Paniotto in Kiev. So, immediately after the symposium, my wife and I went off by the night train for four days in Kiev.

Our discussions about the possibilities for collaborative research were held in Khmelko's office at the Ukrainian branch of the Marxism-Leninism Institute of the Communist Party of the Soviet Union. This Institute was analogous to the one where Wesolowski had proposed the Polish study a decade and a half before. Khmelko told me that he had proposed, and his colleagues and chief had approved, a survey on social psychological factors that might facilitate or interfere with the development of "self-regulation"—a term approximately equivalent to my "self-direction." I would not have preferred to state the issue in this way. To my mind, Soviet

[5] That session, which I chaired, was my last public appearance as an employee of NIMH, for earlier I had decided that the U.S. government was not large enough for both Ronald Reagan and me, and it was evident that he would have the votes to be reelected. I also supposed—correctly, as it turns out—that I would have a better shot at getting research funds from the U.S. government by applying for grants through a university than as an employee of NIMH. That, however, lay in the future—farther in the future than I imagined at the time.

sociologists were too disposed to treat psychological variables as independent variables in causal analyses, and social structural variables as dependent.[6] I would have preferred to talk about the reciprocal relationship between social structure and personality. But since both the social structural and the psychological variables were to be included in the same survey, there would be a basis for data analysis.

Of course I was concerned about potential bias to people's answers in a survey conducted under the Party's auspices. This concern was at least partly assuaged when I learned that the survey was to be carried out by the Ukrainian branch of Tat'iana Zaslavskaya's Public Opinion Center, as "progressive" an institution as could be found in this highly politicized society. My principal remaining concern was with the content of the interview schedule.

Khmelko's colleagues and chief had approved the inclusion of many of the psychological variables from my surveys, and also had agreed to measure both social stratification and social class as I measure them. The latter would have been unthinkable a couple of years before. At the Vilnius conference, my paper on class position and psychological functioning (later published as Kohn et al. 1990) had been roundly attacked for its attempt to conceptualize and index social class in socialist Poland. To many Soviet social scientists, the very idea of social classes existing in a socialist society implied the Stalinist formulation: two classes, one stratum.[7] But no longer; now it was possible for a Party institute to accept the reality of social classes in the Soviet Union.

Khmelko asked what else I would be interested in including. I replied that job conditions were crucial—they were the heart of my theoretical model explaining how social structural position affects and is affected by personality. Khmelko told me that he *hoped*—but had no assurances—that he would be able to include my questions about the most important facet of occupational self-direction, the substantive complexity of work. He did not have the resources to include questions about the other major facets of occupational self-direction—closeness of supervision and routinization—or about other job conditions. He asked instead, "Do you want to pay for those questions?"

Even with my foreknowledge of the ad hoc ways in which Soviet research institutions had to fund their studies, I wasn't quite prepared for the possibility that a Party institute itself would be seeking outside funds. The idea of my applying for research funds in the United States to support a survey conducted under the auspices of the Ukrainian Communist Party was too ludicrous to try to explain. Moreover, it was altogether inconsistent with the Wesolowski model.

My answer must have come as a shock to Khmelko: "Not on your life! If the Party Institute wants a study, they should pay for it. There is no way I can get a grant to buy a few questions in a survey whose quality I don't control." I counter-proposed: "I am available as a consultant and possible collaborator in the data analysis. My price is that the study has to interest me . . . and unless the study includes the crucial job conditions, it has zero interest for me." Khmelko accepted all of this with remarkably good grace, and we moved quickly to technical questions of translation and implementation.

As we left the Institute, I made the lame joke that the last time a Communist Party Institute had sponsored a replication of my research, the research had prospered but the Party (the Polish Communist Party) had gone under. It did not seem remotely possible that the same fate could be in store for the Communist Party of the second most populous republic of the Soviet Union.

Back to Moscow, where there were three events of note for the study, two happy and one dismal. The first was Yadov's telling me that his Institute probably could fund the

[6] This had become especially apparent in a debate among the Soviet participants in the Baltimore symposium, in the discussion of my own paper (later published as Kohn 1989); this was a critique of *American* sociologists' failure to take social structure into account sufficiently in their social psychological inquiries. One of the Soviet participants—Ovsei Shkaratan—thought the criticism applied at least as much to Soviet as to U.S. sociologists; as the debate went on, I came to agree with him. After that time, in my public lectures in Moscow and Kiev, I even took to criticizing Soviet sociologists for not taking Marx seriously because many of them knew only a politicized Marx, seen through the writings of Lenin and Stalin.

[7] In that formulation, although theoretically there could be only one true social class in socialist society—the working class—there remained as a historical anachronism a second class, the peasantry; and within the working class, the intelligentsia constituted a distinct stratum.

proposed study. The second was a telephone call from Paniotto; he had just received word from the International Research and Exchanges Board (IREX) that he was invited to visit Columbia University from mid-January to mid-February. We decided that I would try to get IREX to extend his stay, with a modest increase in stipend, so that he could work with me at Hopkins on a Russian translation of Schooler's and my interview schedule. The third event—the dismal one—was startling in its implications for a possible Ukrainian survey.

At dinner one night, Boris Grushin—a particularly well-informed Soviet sociologist—told me that the American participants at the recent public opinion conference had been shown a "Potemkin village." The Soviet participants had implied that they conduct surveys much as we do in the West. Grushin told me, though, that few Soviet surveys are based on face-to-face personal interviews. The usual procedures are to send out mail questionnaires, to distribute paper-and-pencil questionnaires at places of work, or to have a so-called interviewer watch the respondent fill out a questionnaire. This last alternative at least assures that the person who fills it out is the designated respondent, but in no sense is it an interview. With few exceptions, the only interviewing is conducted in telephone surveys. In a country where few people have private telephones, however, the sampling is necessarily abysmal. Grushin's news did not bode well for a Ukrainian study.

The next major event was Paniotto's month-long visit to Hopkins. I had managed to get IREX to arrange for an extension of time and to give Paniotto an additional $1,000 stipend, most of which he invested in a computer. Although I didn't realize it at the time, that computer made our future research possible.

What I had learned in Moscow about Soviet methods of survey research made me eager to learn precisely how surveys are done in Ukraine. To my dismay, Paniotto told me that Ukrainian surveys were a variant of the general Soviet pattern: an untrained, underpaid woman delivers a paper-and-pencil questionnaire to the respondent's home and returns a day or two later to pick it up. Her only substantive role is to make certain that the respondent has answered all the questions. I contended that this method could not possibly give the detailed information we would require. After two weeks of often spirited and sometimes strained discussion, Paniotto agreed that it would not be sensible to compare data from Ukraine collected by self-administered questionnaires to data from the United States and Poland collected by interviews. The clear implication was that my Ukrainian collaborators would have to learn how to conduct an interview-based survey, and I would have to help them learn.

The next issue was language. Ukrainian practice had been to conduct all surveys in Russian. I was surprised that Paniotto followed this practice because I knew he was an advisor to Rukh, the Ukrainian nationalist movement. The possibility that Ukrainians might resist or resent being interviewed in Russian seemed to be one of those things that are more apparent to outsiders than even to engaged insiders. When I suggested giving the respondents a choice of being interviewed in Russian or in Ukrainian, Paniotto agreed—and did not think that it would be difficult to develop linguistically equivalent interview schedules. (Now, only 2½ years later, it is standard operating procedure in Ukraine to conduct surveys in the two languages.)

Finally, we turned to the painstaking work of translating. It was a repeat of Slomczynski's efforts of a decade and a half earlier, when Slomczynski had translated the U.S. interview schedule into Polish. The one major difference was that Paniotto understands both English and Polish, so he could make use of both versions of the interview schedule in preparing the Russian translation. To my delight, he has an uncanny gift with language. Not only did he raise perceptive questions about the meaning of the English-language version of the questions, but whenever he discerned a difference in nuance between the English-language and the Polish-language version, the two versions almost invariably had proved to be less than fully equivalent in our measurement models.

By the time Paniotto left, I was still not certain that there would be a Ukrainian study. I was dismayed at how much work lay ahead before Paniotto and Khmelko would be able to do a high-quality interview-based survey. I was reassured, however, that my Ukrainian collaborators would be able to produce an interview schedule fully comparable in meaning to the U.S. and the Polish versions.

I asked Paniotto to carry back to Khmelko a long letter, in which I broached the question of interviews versus questionnaires and raised any number of other technical issues. I also suggested:

> Both the U.S. and Polish studies were done at times of *relative* social stability. . . . The Ukrainian study will be done at a time of great social change. In some respects, this makes planning and executing a Ukrainian study much more difficult. But it also represents a potentially huge advantage, if you can somehow study the process of change. . . . This is a large question for extended discussion.

That extended discussion was to take place the following June.

PLANNING IN EARNEST

When I returned to Kiev in June 1990, my goal was to see whether it was possible to make the Ukrainian study into something real and substantial. From there I was to go to Warsaw, to learn whether there were any prospects for a new Polish study.

Officially I was in Kiev to represent IREX in establishing relations with the Ukrainian Academy of Sciences. This official role secured me a room in the Communist Party's hotel, with an assured source of food, not too easy to obtain in Ukraine even then. My IREX role also provided access to officials of the Academy, which gave me a first-hand picture of the Academy being run by officials whose Academy and Party roles could hardly be differentiated.

My colleagues' situations had changed greatly since my earlier visit. Khmelko had authored a "Democratic Platform" for the Ukrainian Communist Party, a call for the transformation of the Party into a democratic socialist party. At that he was given an ultimatum by the Party Committee of the Institute: either withdraw from the Democratic Platform faction of the Party or leave the Party Institute. He left the Institute. Khmelko also had become more and more heavily involved in intra-Party politics; he was even elected to the first (and last) partly democratically elected Party Congress.

In the meantime, the sociologists of Ukraine had created an independent sociological organization, the Sociological Association of Ukraine. Khmelko was elected first vice-president and Paniotto vice-president for international relations. The Association created a research center, to be financed by contracts with government agencies and foreign customers. Khmelko had been appointed its director, so he had a job—albeit a job with no assurance of salary or longevity. Paniotto remained for the time being at the Ukrainian Academy of Sciences, but also would work closely with Khmelko in developing the research center.

For our study—if there was to be a study—these changes had several major implications. Any survey we did would no longer be part of a larger Party-Institute survey. Thus we would no longer have to compress our questions into a composite interview schedule, and I would no longer have to be concerned about how Party sponsorship of the survey might affect people's answers to our questions. But it also meant that there was no longer an assured source of funding for the fieldwork. Moreover, and perhaps more important, Khmelko and Paniotto now would be creating a field operation from scratch. This was a formidable undertaking, but also an opportunity to create something that hardly existed in the Soviet Union—a research center that could carry out surveys based on face-to-face personal interviews. It was very exciting, and a bit intimidating as well.

Khmelko, Paniotto, and I had 10 days of discussion, with interruptions for my meetings with officials of the Academy, our meeting with the officers of the Sociological Association, my lecturing at the Higher Party School (where I was astonished to be questioned by several anxious members of the audience of successful Party officials about the future of that Party), and Khmelko's several Party meetings. Despite these distractions, we worked out a plan for research. The study was to be essentially a replication of the surveys that my collaborators and I had conducted in the United States and Poland, and that Naoi and Schooler had conducted in Japan. The main focus would be on whether our findings and interpretations[8] applied to a portion of the Soviet Union—Ukraine—in its "processes of democratization" and its transition from a centrally planned and administered economy to "a market economy." We

[8] These findings and interpretations—by now expanded from an interpretation of the effects *of* social stratification *on* values and orientation, to an interpretation of the *reciprocal* effects of position in the social structure, job conditions, and personality—are spelled out in Kohn (1987), Kohn et al. (1990), Kohn and Schooler (1983), Kohn and Slomczynski (1990), Naoi and Schooler (1985), and Schooler and Naoi (1988).

had considerable discussion about which questions to include in the survey, what new questions had to be invented, and how to achieve comparability between the Russian-language and the Ukrainian-language version of their interview schedule.

We also decided that Khmelko and Paniotto would attempt to secure funds for fieldwork from Soviet and Ukrainian sources—the "Wesolowski model." We were confident that Yadov would try to provide as much support as possible from the Soviet Academy of Sciences, but we were not at all certain that he would succeed. We were far less confident that we could count on the assurances of support we received from officials of the Ukrainian Academy, mainly Communist Party functionaries. We also explored other possibilities. Our estimates of financial need were modest—too modest, for they were based on previous Ukrainian experience in conducting surveys, and did not take full account of how much more expensive an interviewer-based survey would be.

I committed myself to applying to U.S. sources for financial support of those portions of the data analysis that would be carried out in the United States. These costs included both Khmelko's and Paniotto's coming to Johns Hopkins for extended periods, when I would give them intensive training in LISREL and we would analyze the data collaboratively. We even signed a formal agreement, which they thought might be useful in applying for Soviet and Ukrainian funds.

As I look at this agreement in 1992, only two years later, it seems quaintly dated: the aim of the research is stated as "collecting and analyzing data that are necessary for assessing and forecasting the mutual influence of social structure and personality during *perestroika*." That formulation, which now included the notion of reciprocal effects, was deliberately put in terms of the buzz-word that seemed most likely to elicit Soviet support, *perestroika*. In fact, the agreement called mainly for extending the comparative studies of the United States, Poland, and Japan to Ukraine, but the term *perestroika* signified a new emphasis on social change.

During this time, I was further developing what was then still only the germ of an idea: everything we had done in our U.S., Polish, and Japanese studies had been done under conditions of relative social stability. Our findings, our conclusions, our interpretation might very well not apply under conditions of radical social change. If this was so, we could learn some very exciting things, which might lead us to modify our interpretation considerably.

A COMPARATIVE STUDY

Next I traveled directly to Warsaw, where I attended a conference on social change in Eastern Europe and had a series of discussions with Wesolowski, with Slomczynski, and with Bogdan Mach.

Some years back, Wesolowski had enlisted Mach, his next-generation protege, to translate a set of Carmi Schooler's and my papers into Polish, and from those papers to edit a book (Kohn and Schooler 1986). In working with Mach on that book, I came to recognize him as a first-rate sociologist as well as an extraordinary human being.[9] I was eager to collaborate with him, but I didn't quite know how to do it.

All three were enthusiastic about the prospect of a Ukrainian study; all three shared my excitement about making radical social change the focus of the study; and all three immediately recognized the central methodological limitation of the planned study: we had no baseline data for Ukraine. Like it or not, the 1978 Polish data would have to stand as a proxy for baseline Ukrainian data. The prospects for a rigorous analysis would be immensely improved if we could do a restudy of Poland, thus making possible a truly comparative study—comparative over time in Poland, as well as cross-nationally comparative.

Who would carry out the new Polish study? I saw two issues as potentially problematic. One was that Slomczynski and his research team (of which Mach was a member) then were planning to make a study of social change in Poland—not of the social psychology of social change, as I would want to study it, but of changes in Polish social structure as such. Somehow, though—and to this day I am not entirely sure how it happened—the news of the planned Ukrainian

[9]Just how extraordinary a human being Mach is was demonstrated dramatically by his underground activities under martial law in Poland. As I learned only recently—not from him—he had been responsible for finding safe housing for some of the leaders of the *Solidarnosc* underground and for finding places for the leaders to meet.

study enticed first Slomczynski and then the other members of his research group into putting aside those plans. Instead they signed on to a replication and major extension of our 1978 study of social structure and personality.

The other potential problem was that Slomczynski was now teaching at Ohio State University and spending much of his time in the United States. He could not play the central role in a new study that he had played in the earlier study. But Mach was eager to participate, as were Krystyna Janicka—who had played a major part in designing and conducting the 1978 survey—and Wojciech Zaborowski, whom I hardly knew at the time, but whom both Slomczynski and Wesolowski held in the highest regard. It seemed then—and still seems—a splendid research team.

As I wrote in a memo to myself while in Warsaw, "I think that the pieces have come together, at least potentially: a *comparative* study of Poland and Ukraine, focusing on social change. We already have a comparative study of the U.S., Poland, and Japan under conditions of apparent stability; what happens under conditions of radical social change? It's a very exciting prospect!"

What about funding? Fieldwork would be much more expensive now in Poland than it had been in 1978 or would be in Ukraine. Not only were Polish prices rising more and more to meet the level of world prices, but Polish interviewers had long been underpaid and were now catching up. Of even greater concern, the traditional method of funding research in the Polish Academy of Sciences—direct allocations from the Academy—was ending, and it wasn't clear what method, if any, would replace it. That problem was to haunt us in the ensuing months. Still, from the beginning, we all thought the Wesolowski model best: our aim should be to secure Polish funding for the fieldwork and to obtain U.S. funding to bring Polish collaborators to Johns Hopkins for collaborative analysis and (except for Slomczynski, who was already expert) for training in LISREL.

From Warsaw I went to Madrid for a meeting of the ISA Executive Committee and the World Congress of Sociology. It was a ghastly two weeks; the tone was set when I was mugged by three men on my first day in Madrid. The Executive Committee meetings were even worse. But there were two happier events in Madrid, which are directly pertinent to this story.

I introduced Slomczynski to Paniotto. All three of us were very much aware, and somewhat amused, that an American was bringing Polish and Ukrainian sociologists together in a collaborative endeavor. Paniotto readily grasped the advantages of a Polish-Ukrainian comparative study; all along, he and Khmelko had intended to compare their Ukrainian data with the 1978 Polish data, and a new Polish study fit into their plans.

The second event was Yadov's assuring me of full financial support for the Ukrainian survey. Even though subsequent circumstances prevented him from making good on that promise, his endorsement of our research proved invaluable.

FUND RAISING AND INTERVIEWER TRAINING

The major activities of the next several months—both mine and my collaborators'—were devoted to raising funds for the research from U.S., Polish, and Soviet sources. Our three efforts were intimately interrelated. Not only did *we* view all three as essential to the overall project, but each of the funding agencies sought validation in the actions of institutions in the other countries.

The National Science Foundation (NSF) and the National Council for Soviet and East European Research (NCSEER) were greatly impressed that our research had the endorsement of the Polish Academy of Sciences, the Soviet and Ukrainian Academies of Sciences, the Polish Sociological Association, and the Sociological Association of Ukraine. These endorsements demonstrated that the proposed research was of potential value to the relevant scientific institutions in the host countries, not only to U.S. sociologists. Moreover, both NSF and NCSEER were aware of the virtues of the Wesolowski model in terms of the quality of the data that would likely be secured: this was to be the very antithesis of "dollars for data." The Polish and Ukrainian authorities, in turn, were greatly impressed by the scientific imprimatur of the National Science Foundation and of the National Council for Soviet and East European Research. But I'm running ahead of my story; many events occurred and we faced many anxieties before we put it all together.

In July 1990, soon after I returned home, I read an article in *ASA Footnotes* about NSF's interest in supporting collaborative research in Eastern Europe. It seemed to be made to order, as indeed it was. As I settled in to work on a proposal, though, I got scared about the size of the budget: I was asking for funds for coding and quality control for both surveys; for bringing two Ukrainians and two Poles to Hopkins for a semester each; for computers for Kiev, Warsaw, and Hopkins; for salaries for research assistants; for travel; and astronomical amounts for overhead. These items added up to so much that I was afraid the very act of asking for such a sum would ruin my chances of receiving any funds at all. On the advice of Murray Webster, the director of NSF's sociology program, I did propose a full budget of all necessary expenses. Eventually the project was approved, and NSF gave me nearly half of what I had asked for. Certainly this was enough to begin the study and to convince the Polish and Ukrainian authorities that the project met NSF's scientific standards. The rub was that now I had to submit a new budget, in the amount actually granted, explaining how I could do the research at that reduced cost "without sacrificing either scope or quality." As my wife put it, I had to prove that I had been a liar when I originally told NSF that it would cost twice as much to do the job.

To make the task even more complicated, I had learned meantime from Bob Randolph, the executive director of NCSEER, that the Council would be willing to supplement a partial grant from NSF if their reviewers thought the study would contribute significantly to our understanding of Eastern Europe. NCSEER required an application much like NSF's, along with a budget showing precisely what I intended to do with the funds that NSF was providing and what more I would do with the requested supplementary funds. Having explained to NSF how I could do the job perfectly well with the funds that they were able to provide, I now had to explain to NCSEER why these funds were insufficient. I made my proposal and the NCSEER approved it.

Mine was the least difficult part. I was dealing with stable, established institutions with effective and well-understood mechanisms for making decisions. Meanwhile my Polish and Ukrainian collaborators were dealing with institutions in process of disintegration and with new institutions groping to develop rational and effective procedures, or they were inventing their own institutions.

Initially my Polish collaborators had hoped that their fieldwork would be funded by the Polish Academy of Sciences, just as the 1978 survey had been, but this hope dissolved quickly into uncertainty. When I visited Warsaw in January 1991, the director of the Institute of Philosophy and Sociology—where Janicka and Zaborowski were employed and where Slomczynski's research was located administratively—promised administrative support, including support for training the Ukrainians in survey methods. The director of the newly established Institute of Political Studies, where Mach now was employed, also assured us of administrative support. In what amounted to a public endorsement of the study, he even invited me to present a seminar about our proposed research. Whether public endorsement of the research and promises of administrative support would translate into financial support, however, was as uncertain to the Institute directors as to us. The only certainty was that even administrative support was conditional on my receiving a grant from NSF.

Then the Polish government made a radical change in its mode of supporting research. The government now required scientists employed by the Academy to apply for individual grants from a newly created entity, the State Committee for Scientific Research. It took some time for the State Committee to develop application forms and procedures; which delayed our undertaking. My collaborators developed twin proposals—one through each Institute—for two separate components of the overall study. In the ensuing competition, peer reviewers' judgments of both projects were extremely favorable. Eventually the two linked proposals were approved and funded. Even after the awards were announced, though, we suffered horrendously long delays and agonizing frustration. The months had slipped by—months that we had intended to devote to designing new questions to ask in the survey, but which had been devoted instead to administrative matters.

My Ukrainian collaborators faced even more difficult circumstances. The Ukrainian research initially was to have been supported by the Party Institute, but Khmelko had left that Institute. In any case, the Party and its Institute had ceased to exist. Then the research was to be supported by the Soviet

Academy of Sciences, but Yadov's assurance of full financial support was trimmed to token support when Paniotto left the Ukrainian Academy to take a position at the University of Kiev and it was no longer possible to fund the research by a transfer of funds from the Soviet Academy to the Ukrainian Academy. Later, when the Soviet Academy was transformed into the Russian Academy, even token financial support was no longer possible. The Ukrainian Academy showed no interest in supporting my collaborators. All the sources from which we thought we might get financial support for the Ukrainian fieldwork had dried up.

Khmelko and Paniotto, however, are nothing if not inventive. They transformed their newly created Research Center into a thriving survey organization. The Center is an enterprise whose primary office is one corner of the Paniottos' bedroom and whose secondary office is one corner of the Khmelkos' living room. Its equipment consists of the computer that Paniotto bought with the IREX funds he didn't need for subsistence when he was living with my wife and me. Yet despite the lack of physical resources, Khmelko and Paniotto have conducted surveys for local authorities in Ukraine, for the Research Institute of Radio Liberty, for other Western news organizations, and for the United States Information Agency. The Ukrainian fieldwork will be supported very substantially by the profits that Paniotto and Khmelko have made in conducting these client-sponsored surveys. Now they also have financial support in the form of a grant from the newly created and elegantly named Commission of Scientific and Technological Progress of the Cabinet Ministries of Ukraine. Khmelko and Paniotto are husbanding that grant, however, to support a planned, and potentially invaluable, follow-up study a year from now.

The client-sponsored surveys have provided not only the financial resources for conducting our intended survey, but also extremely valuable experience in conducting surveys. Moreover, by a marvelous stroke of good fortune, they have furnished expert interviewer training for Ukraine in the person of Michael Haney, of the research staff of Radio Liberty.

We still needed to find some way to train the Ukrainian investigators in methods of intensive pretesting. I had hoped to enlist the help of the Methodology Section of the Institute of Philosophy and Sociology of the Polish Academy of Sciences, which had done an outstanding job in pretesting the 1978 Polish interview schedule. When I was in Warsaw in January 1991, Slomczynski and I appealed to the director of the Institute for the services of Andrzej Wejland, a key member of that section, to help train the Ukrainians in his methods of intensive pretesting. Wejland, who is fluent in Russian, was eager to participate. The director approved his doing so as party of his regular duties. The costs to us would be minimal: perhaps transport from Warsaw to Kiev, perhaps not even that.

In the fiscal crises that befell the Institute, however, a new director abolished the Methodology Section, despite our strong appeals. Wejland and his close associate, Pawel Danilowicz, were kept on in temporary positions, but found the arrangement unsatisfactory. They resigned and went into business as a private survey firm. We hired them for pretesting, both in Poland (mainly using the Polish grants, supplemented by the quality control funds in my NSF grant) and in Ukraine (using the NSF funds). In effect, we had to jerry-build our own temporary institutional structure where the existing structure fell apart. For our study, this ad hoc solution has proved entirely satisfactory. Whether it is equally satisfactory for Polish social science is another question altogether.

We also needed office space in both Warsaw and Kiev, quiet places to meet, plan, and work. In Warsaw, until the grants were in hand, we met in one or another of the collaborators' apartments or in noisy cafes. The Ukrainians still work that way, except that in Kiev it is difficult to find even a noisy cafe; we have spent many precious hours looking for a place to meet. The Polish team finally has an office, financed from the overhead on their research grants. Soon the Ukrainians, too, should have an office. At last they have found a satisfactory institutional home for their Research Center in the Graduate Department of Sociology of the University "Kiev-Mogila Academia," where both Paniotto and Khmelko have been appointed as professors. The Academia is a medieval university, closed in 1815 by the Russian Tsar, later converted by the Communist regime into a training school for political officers of the Soviet Navy, and now reconverted into what I like to think of as a new medieval university. Amidst the institutional disarray, some things have turned out quite well.

Even so, dealing with these administrative and organizational problems has interfered greatly with our ability to work on the actual research—partly because so much time was expended on these matters, but also because my collaborators have found it exceedingly difficult to invest themselves in issues of research design and question wording when our ability to do the study at all has been so problematic. Progress on the research has been excruciatingly slow.

The delays certainly have been extremely frustrating. In at least one sense, they also have been costly: the Polish grants are in zlotys, and there is considerable inflation in Poland. With each passing month, the number of respondents we will be able to interview becomes smaller. In a strategic sense, however, it is difficult to tell whether we have been *dis*advantaged or conceivably even *advantaged* by falling behind in our schedule. Would it have been better to conduct the surveys when we originally intended to do so, or would it be preferable to wait until the processes of social change have progressed farther? At what point in the process of radical social change can one best study its psychological ramifications? How can one tell in advance when that time might be?

RESEARCH DESIGN AS A REFLECTION OF SOCIAL REALITY

To this point, in my effort to emphasize the research *process* rather than the *content*, I have deliberately touched only lightly, and mainly in footnotes, on the substance of the planned research.[10] Now, if I am to do justice to the intended theme of this paper—how the problems encountered in the research reflect the very social phenomena that the research attempts to study—I must tell you about some of the changes we have made in our research design and in the substance of our inquiry. These changes reflect what has been happening, and what seems to be impending, in Polish society and also increasingly in Ukrainian society. For the past two years we have been scurrying constantly to keep up with the social and political changes of Poland and Ukraine.

A major strategic issue in planning the Polish portion of the inquiry had been whether it would be preferable to do a follow-up of the respondents in the 1978 survey or to conduct a new cross-sectional survey based on a currently representative sample of Polish adults. Early on we had decided on a new cross-sectional sample, on the rationale that for a study of *social* rather than of *individual* change, it was more important to secure a sample representative of the Polish population today than to have longitudinal data. Still, to establish causal models of change, we had planned to reinterview a representative sample consisting of perhaps 500 of the original respondents. This was to have been an important part of Mach's research grant proposal.

On a visit to Lodz in January 1991, however, we received confirmation of something we had long feared: the names and addresses of the 1978 respondents had been destroyed deliberately during the period of martial law, in what now seems to have been an overzealous effort to protect the respondents' anonymity. One must remember that this was at a time when the *New York Times* showed photographs of army tanks surrounding the building that houses the Institute of Philosophy and Sociology, and the police confiscated copies of the Institute's journal, *Sisyphus*.

On hearing the disheartening news that a follow-up of the 1978 survey was not possible, Mach and I went for a long walk and a large beer. Two hours later we had a new design, capitalizing on the existence of one especially valuable set of records that fortuitously had *not* been destroyed: the names and addresses of a subsample of 177 of the men, those who in 1978 had one or more children in the 13-to-17 age range. Each man's wife and a randomly selected child had been interviewed about a year and a half after the main survey, in a study of the transmission of values in the family. Mach now intends to reinterview all three members of these triads. True longitudinal analyses will be exceedingly valuable, even with a small N. Moreover, such analyses can provide useful information for the simulated longitudinal analyses that we intend to do with the cross-sectional data that we shall collect for

[10] For a much more complete statement of our theoretical rationale and research design than I can provide in the space allotted here, see Kohn et al. (Forthcoming).

much larger samples of Polish and Ukrainian men and women. Moreover we have a long-standing interest in the intergenerational transmission of values (Kohn 1983; Kohn, Slomczynski, and Schoenbach 1986), which now we can study under conditions of radical social change.[11] Thus an intended but infeasible longitudinal study of Polish men has been transformed into a longitudinal study of Polish families.

We have had to make equally great changes in the design of the cross-sectional surveys, and for much the same reason: to keep up with changes in the societies we are trying to study. Initially we had intended to interview representative samples of employed men and women in both Poland and Ukraine, much as we had done in the earlier Polish, U.S., and Japanese studies, except that this time our primary samples would include women as well as men. To that end, the Poles and I spent a high-pressure month in January 1991 (joined for a week by Paniotto) and again in July (this time joined by both Khmelko and Paniotto) developing new questions that attempted to capture the changing conditions currently experienced, likely to be experienced, hoped for, or feared by employed men and women in Poland and Ukraine. Some of these questions have to do with occupational structure and conditions of employment: ownership and other forms of control over resources and labor power, changes in organizational structure, the changing nature of unions and of workers' relationships with their unions, changes in the bases on which people are paid, and technological developments. Many questions pertain to risks, uncertainties, and job protections—both the reality (including the structural bases of the uncertainties and protections) and the perceptions thereof. The risk of unemployment, of course, looms especially large.

During the July 1991 meetings, Slomczynski convinced us that studying only the employed would not do justice to many of the very people who might be affected most strongly by radical social change. He proposed that we add to the Polish research design special studies of the three categories of people that either did not exist under socialism or that have been greatly affected by the current changes: 1) the unemployed (defined as people who have lost their jobs and are now actively seeking new employment)—a new category in a country where unemployment previously was disguised, and where people for the first time are finding themselves without formal employment or paychecks; 2) employers ("capitalists")—a growing category, whose members would be included in any sample of the employed, but in numbers too small for intensive analysis; and 3) a startling new category in Poland—women who were employed earlier in the state sector of the economy but who now, with the dismantling of child care and other facilities, can no longer afford to be employed and have become housewives. We decided to interview special samples of 500 people in each of these categories. At that time the Ukrainians deferred any decision about doing the same.

By January 1992, it was apparent to all of us that because of the delays in funding and the postponement of fieldwork, too much time had elapsed to allow us to employ such a complex research design. We might never be able to study the special samples, the very people who might be affected most by social change. Almost as problematic, the delay between our cross-sectional survey of the employed and our special survey of the unemployed might be so prolonged that the two surveys would no longer be comparable. This eventuality would be especially troublesome if we found the unemployed to be more distressed than the employed but had no way of knowing whether the employed might have become equally distressed in the interval. Therefore we revised the design of the Polish survey, expanding the sample from employed adults only to the entire adult population. Later I prevailed on the Ukrainians to do the same.

Reluctantly we dropped the rural segment of the Polish sample (just as in the 1978 study): in part to husband our financial resources for interviewing adequate numbers of urban respondents in each of the employment situations, and in part because we simply did not have time to design questions that would be truly appropriate to rural respondents. On learning this, the Ukrainians decided to drop the rural segment from their survey as well: in part for the same reasons

[11] In pursuit of this interest we also intend, in both the Polish and the Ukrainian studies, to interview the spouse and one child of every respondent in the cross-sectional surveys who has one child or more in a designated age range, probably 13–17.

that we had found compelling for the Polish study, in part because there would no longer be comparability of the rural Ukrainian population to a similar Polish population, and in part because it was becoming increasingly difficult to find transportation in the rural areas of Ukraine, the gasoline shortage being acute and public transport being very limited in those areas. (This is telling evidence that the research infrastructure is certainly not the only infrastructure in disarray.)

We faced three major challenges in expanding the samples to include all adults living in urban areas.

The first was to develop a battery of questions—my Polish collaborators call this "the sorting machine"—to ascertain the respondent's employment situation. This undertaking turned out to be most formidable, requiring weeks of work and a special pretest. The strategy that we developed was to ask a series of questions designed to classify the respondent in the most appropriate employment category, followed by batteries of questions appropriate to people in each of the categories. We begin by asking whether the respondent is employed 15 or more hours per week; if so, we ask about his or her job conditions. If the respondent is not employed 15 hours or more per week, we ask whether he or she is looking for work; if so, we ask questions about job seeking; if not, we ask whether (in this sequence) she is a housewife, he or she is a full-time student, he or she is a pensioner, and so on. At whatever point we receive a positive response, we shift to a set of questions appropriate to the respondent's employment situation. The Ukrainian survey does not use so elaborate a procedure for classifying the respondent's employment situation, but relies more on the respondent's self-classification.

A second challenge sounds simple but has proved to be exceedingly complex: how does one decide what is the respondent's principal job? In the Poland of 1978, this question was not problematic. Some respondents held more than one job, but almost always the principal job was the one held in the state sector of the economy; other jobs were auxiliary to that. In 1992, with their economies in transition, many Poles and Ukrainians hold two or even more jobs, and determining which one is primary has become highly problematic. Because of limited interview time, we could not inquire about all of them in detail. Working out the procedures for determining which is the respondent's principal job has proved to be anything but simple.

Because the heart of the theoretical model is that the lessons learned on the job are generalized to nonoccupational reality (Kohn and Schooler 1983), the prime desideratum in deciding which is the respondent's principal job is what that person actually does in his or her work, not that person's subjective assessment of the work. Thus we use as our principal criterion the amount of time spent on each job, rather than (for example) the respondent's subjective estimate of which job he or she considers most important or most satisfying.

The third challenge was the most demanding of all: on a crash basis, we had to develop new batteries of questions for key segments of the population: the unemployed, full-time housewives, students, the retired, and the disabled. Our most important objective was to develop questions about the substantive complexity of their principal activities, analogous to those we ask of employed respondents about the substantive complexity of their work in paid employment, it being a theoretical necessity that we include such measures as a likely intervening link between social structural position and psychological functioning.

We could build on the questions that Carmi Schooler and I had developed long ago for measuring the complexity of housework, of schooling, and of activities in retirement, but these were only a start. Mach and I sketched out others. Then Janicka, Mach, and Zaborowski did a herculean job of translating and refining those questions and inventing others. My own usefulness decreased precipitously as we moved from English-language formulations to Polish-language implementation.

I spent January through June 1992 (using a blessedly timed sabbatical) shuttling back and forth between Warsaw and Kiev, flying Aeroflot and keeping my fingers crossed that I would arrive in one piece. My principal role during these crucial months was to coordinate the Polish and the Ukrainian studies as well as I could, working on research design and question wording with both research teams and trying to keep each team abreast of what the other was doing. I also played an emotionally difficult, quasi-administrative role—pressuring the Poles to speed up their sometimes excruciatingly slow pace in getting interview schedules ready for pretesting, while restraining the Ukrainians, who were eager to begin field work but whose timing was held hostage to the completion of the Polish interview schedules. We all desperately wanted the two surveys to be as closely comparable as possible, but this was proving very difficult to accomplish. Probably my most useful activities were bringing Wejland and Mach to Kiev to help in the translation of Polish questions into Russian, greatly enlarging the scope and intensity of pretesting in both countries, and facilitating Wejland's full participation in the Ukrainian pretests. In a peculiar way, my frequent presence also provided both my Polish and my Ukrainian collaborators with a useful excuse to put aside other responsibilities to work concentratedly on our joint endeavor.

By June 1992, pretest versions of both the Polish interview schedule and the Russian-language version of the Ukrainian interview schedule were ready. Intensive pretests now have been conducted in both countries: Wejland and Danilowicz have conducted the Polish pretest, and Wejland has traveled twice to Kiev to teach his methods of intensive pretesting to the Ukrainians and to assist in the Ukrainian pretest. In mid-July the Poles and the Ukrainians met to discuss the results of those pretests. From what Slomczynski and Khmelko tell me, the pretests went well in both countries. By early August 1992—when I had to bring my tale to a conclusion, if I was to present it at the ASA convention that month—my collaborators were busily refining their interview schedules. Both surveys were to go into the field in early fall, almost a year and a half behind schedule.

Every aspect of the research—the timing of the surveys, the samples we chose, our conceptualization of what is entailed in radical social change, the very questions we ask our respondents—has been affected profoundly by our own experience of social change. By the same token, every modification we have had to make in our research plans has given us insight into the meaning of radical social change to those whose lives it affects.

CONCLUSION

Attempting to study the social psychology of radical social change has proved exceedingly difficult, sometimes exceedingly frustrating, and yet always exceedingly interesting. Our experience in this research—both of being buffeted about by radical social change and of being forced to cope with such change—helps us to understand the very processes we are trying to study.

Admittedly, there are obvious and striking differences between the situations we have encountered in attempting to carry out this research and the situations most people in Poland and Ukraine encounter in their everyday lives. Obvious and striking differences also exist between my own situation—operating from a home base in the United States and being subject to the vagaries of radical social change only in my research role—and those of my Polish and Ukrainian collaborators, who have been affected in all aspects of their lives. Nonetheless I am impressed by how closely our experiences in attempting to do research reflect those of the people of Eastern Europe in pursuing their lives. I am also impressed by how greatly the changes in *research* institutions reflect (and even result from) changes in other social institutions. Research institutions are so integral to the society that they are necessarily affected by any profound changes in that society.

Observing the disintegration of research infrastructures in Poland and Ukraine has provided insight into what is happening to many other institutions as well. From our own experiences in attempting to secure funds and to conduct this research, for example, we have learned how extreme budgetary shortfalls can threaten the very existence of even well-established institutions. From the repeated need to delay our plans so that we could deal with previously unknown obstacles and contingencies, we have learned something about how radical social change prevents people from getting things done in the ways, and on the schedules, that previously were normal and achievable.

Disintegration, though, is only part of what we have witnessed. We also have seen the creation of new research-supporting institutions—the State Committee for Scientific Research in Poland and the Commission of Scientific and Technological Progress of the Cabinet Ministries of Ukraine. These new agencies represent sharp departures from past methods of funding research in Eastern Europe. Rather than following the past practice of allocating research funds to institutions and their component units, they provide grants to individual research projects—the Polish agency even bases its evaluations of those projects on formal peer reviews. These radical departures from past institutional practice embody a major change in the very conceptualization of the primary unit of research: such a change clearly reflects a fundamental shift in ideology, affecting all the institutions of Eastern European countries.

We also have witnessed the beginning of a process of transformation of existing research institutions. The Polish Academy of Sciences, to take a notable example, is making strenuous efforts to reorganize itself to meet new and perilous challenges.[12] Perhaps the most radical innovation we have encountered is the resurrection in Ukraine of the Mogila Academy. Reconstituted on a model unknown in the former Soviet Union, this university combines research with teaching. Moreover, it has no ties to the previous regime.

Although we have participated in, fostered to some degree, and benefited directly from these transformations, I am not at all sure whether they will succeed, or, if they do succeed, whether they will be an improvement over previous institutional arrangements. It is too early to tell. It is not too early, however, to observe not only disintegration, but also the creation of new infrastructure and the transformation of the old, based on quite different organizational and ideological principles. There is no question in my mind that the particular developments we have encountered in conducting this research reflect similar institutional transformations occurring more generally in Poland and Ukraine.

The lessons of our research are not limited to what we have learned from our encounters with formal institutions. We have also learned something about what is happening in the process of radical social change from our need to modify the design and content of our studies. In facing the need to expand our research design to encompass people not currently employed, and to develop new batteries of questions that assess the job-equivalent activities of people in nonjob situations, we have gained considerable understanding of the changes now taking place in the class structures of Poland and Ukraine. Even the relatively minor task of identifying the respondent's principal job has given us some understanding of changes now occurring in many people's conditions of work.

Finally, we have learned some valuable lessons about the social psychology of radical social change from being forced to cope with such change ourselves. Certainly we have learned more than we cared to learn about how frustrating and discouraging it can be to have one's expectations constantly undermined, and never to know whether yesterday's ground rules apply today or whether today's will apply tomorrow. After living with radical social change for some time, you come to think that no one can make commitments, and that no one who does make commitments can fulfill them.

But radical social change also means that new types of initiative are possible. Even the ingenuity that my collaborators have displayed in overcoming the obstacles they have encountered may help us to understand how people are attempting to deal with institutional change. At the extreme, resorting to privatization as a means of supporting research—as Khmelko and Paniotto have done in Ukraine and as Wejland and Danilowicz have done in Poland—is far from unknown in Eastern Europe today. And, although few Poles and few Ukrainians enjoy the support of the National Science Foundation and the National Council for Soviet and East European Research, "joint ventures" with foreign capital are the dream of many and are becoming reality for some.

[12] We have participated in two of these undertakings: the beginnings of a reorganization of the Institute of Philosophy and Sociology of the Polish Academy of Sciences, based on research teams (such as ours) rather than on administrative units; and the formalization of the graduate teaching program, in defense against criticisms of the Academy for not doing enough teaching. In the catalog of the newly formalized graduate program, my course "Cross-national Research on Social Structure and Personality" was the first course listed, in an attempt (I assume) to signify the international nature of the program

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