In Praise of Field Work: An Autobiographical Note

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Abstract: The author's personal experiences in field work in four different countries and on a variety of projects are reported. These experiences demonstrate the advantages of doing one's own field work, or, at least, to participate in the initial phases of team research. In this context the phenomenon of serendipity, i.e. unexpected by-products (or "spin-off"), in field work done by oneself is dealt with, as well as the experience that what one has learned in one project sometimes is helpful in other, seemingly quite unrelated, research. It is also a great asset in teaching. Finally it is shown, by a few examples, that the sociologist should not be afraid of "trespassing" on neighboring disciplines, especially history.

Recently the mail brought the advertisement of a formidable handbook on field research in the "behavioral sciences", giving advice how to prepare and execute a project, including such details as the hiring and employment conditions of a field staff. No doubt, social research as become a big enterprise. Gone are the days when a single scholar, often a Ph.D. candidate, went into the field to produce a substantial monograph on a community or on such types as the hobo or the railroader or the street corner society. And yet, I have the impression that today too many projects are thought up in the office of a senior sociologist who sends his assistants or graduate students into the field, armed with questionnaires while he (the mature sociologist) stays in the office instead of himself taking a look at the people under study. I call this the "new armchair sociology". It is my conviction, gained by experience, that valuable insights are likely to be missed in this kind of procedure. Because only through contact with the human subjects and by familiarity with their social and physical environment will one meet with those unforeseen facts and problems which often lead to significant findings.

My own experiences in field work have been in four different countries and on a variety of projects, each of which presented peculiar problems. In this paper I shall try to draw from these experiences some lessons which may be of general interest.

I first came to this country in 1926 as a Fellow of the Rockefeller Foundation. When I saw what was going on in Chicago unter Robert E. Park and elsewhere in the way of empirical research, I had difficulty fitting these studies into my conception of sociology. For, sociology in Germany at that time meant the works of men like Simmel, Toennies, Max Weber, von Wiese and Vierkandt - a general theory of social structures and processes and its application mainly to "macrosociological" problems.

However, Toennies propagated the idea of an empirical branch of social research which he called "Soziographie", adopting a term proposed by the Dutch sociologist Rudolf Steinmetz. What Toennies visualized could be described as intensive regional rather than nationwide research on social conditions and problems on the basis of demographic data but going beyond statistical description and analysis (cf. my article 1931). His own sociographic research dealt with "social problems" in his native region of Schleswig-Holstein.

I had, before I came to America, already published two books which today would be regarded as sociological studies: my dissertation on the Swedish Labor movement (1925) and a monograph on the Germans in Lithuania (1927). However, both studies were considered as being in the field of Economics.

The situation of sociology in Germany being as described, it was no wonder that when I set out for Sweden in the Spring of 1921, I had no training in interviewing. Even if I had had any funds it would not have occurred to me to stage an opinion survey to find out what the rank-and-file of the Labor movement thought. My contacts with Swedish workers
were left to chance — I travelled a good deal, visited factories and mines, adult education centers (folk hoegscholas) and on a few occasions stayed in working-men’s homes enjoying hospitality offered to a hiking stranger. I also attended political rallies during an election campaign and participated as a guest in the annual conference of the Consumers’ Cooperatives.

But my main sources were the treasures of the Archives of the Labor movement, which were maintained jointly by all branches of the movement. I spent a major part of my working time with the proceedings of party and union conferences and other documents, aided by the friendly director of the archives, a botanist who had joined the Social Democratic party as a student and was also a member of parliament. It was he who arranged many of my interviews with politicians and trade union officials.

Since I was the first young German social scientist to come to Sweden after the war, interviews were liberally granted. Even the great statesman and head of the Social Democratic Party Hjalmar Brautig who had been prime-minister received me in his modest apartment (opening the door himself) and gave me about half an hour. The leaders of the small group of Anarcho-Syndicalists, were obviously flattered by my attention and gave me half a day. It was not difficult to get people to talk. It was more difficult to think of pertinent questions, however; but since I was young and “green” I did not have to be ashamed of my ignorance.

Many years later in connection with a study of parliamentary government in West Germany (cf. my papers 1959b and 1962), the interview situation was more difficult: the trick was not to appear quite uninformed and yet not to seem to know too much. In both cases my informants would have been less willing to cooperate had I approached them with pencil and paper and a questionnaire or even a notebook. I had to train myself to remember the quintessence of the conversation and to write it down later. Today one could perhaps use tape recorders. However, this technique is likely to have an inhibiting effect on interviewed persons: instead of speaking spontaneously they will give more consideration to what they are going to say.

The Swedish Labor movement comprised various kinds of groups and tendencies, ranging all the way from reformist social-democrats to left wing socialists, communists and anarcho-syndicalists. There were also links with tee-totallers and pacifists — movements which were neither labor nor socialist. One had to learn to distinguish between ideas essential to the labor movement — in later years when I wrote a general treatise about Social Movements (1951a; also 1967) I called these “constitutional” ideas — and those merely accidental.

The work on my dissertation involved also a great deal of reading on economic history and social problems. From the comprehensive Government Report on Emigration I learned certain things which were useful in my later migration studies. Among these “spin-offs” was the insight that long distance migration, even emigration to a foreign country, may not be regarded as a greater adventure than migration within one’s country, if many people from the same community have gone to the same places abroad. An old woman in a village had asked the pastor to hold a memorial service for her son who had died somewhere in Minnesota. When the pastor pointed out that the deceased was not a member of the parish, adding “you would not ask me to do this if he had died in Goeteborg” — she resorted: this was different, the community where her son had died was one where most people from her parish had emigrated, it was, she thought, a part of the home community.

The study of the Germans in Lithuania which I undertook in 1925 was a project of the Institute of East German Economics at the University of Koenigsberg. The director who assigned me this project visualized an “enquête”: census-type questionnaires were to be distributed among the Germans and a more extended questionnaire should be filled in by their leaders, such as schoolteachers and pastors. This turned out to be unfeasable. The German farmers and artisans who made up the main body of the minority were not accustomed to this kind of inquiry and they were also afraid of political repercussions in view of the strong nationalism prevailing in this newborn state. I soon realized that I had to make as little “noise” as possible in pursuing this project.
This added to the excitement and the challenge of my task. Strangely, while a lot had been written about the Germans in Latvia and Estonia there was hardly any literature about the Germans in Lithuania, although it bordered directly on East Prussia. Even the Lithuanian census figure of ca. 25.000 Germans was disputed by the leaders of the German minority as much too low.

Thus once again I had to conduct interviews with knowledgable persons in leading positions in the German group. But this time I really went into the “field”, travelling many weeks from farm to farm, and from town to town. Sometimes I was able to use the railroad or a river boat but most of my travelling was “by horse and buggy”, often not even so comfortable but on a farm wagon filled with hay or straw.

I had no assistant, but sometimes I was accompanied by one of the two German representatives in the Lithuanian parliament. We often stayed overnight on a farm belonging to one of his numerous kinfolk; this was a great help in a country where hotels were rare and often not free of bed-bugs. But we ran into a difficulty: every morning before breakfast, we were offered a drink of home-made Vodka. That was not so bad in view of the rich meal that followed; but this was repeated a couple of times in the course of our morning visits on neighboring farms. In order to preserve out scholarly sobriety and yet not to offend our hosts, we soon decided to pose as tee-totalers.

A most serious problem was notetaking. It would have been psychologically impossible to bombard our hosts with questions: how many acres, how many horses and cattle he owned, etc. This had to come out incidentally in the course of the informal conversation and perhaps at a tour of the stables and barns. The size of the farm often had to be inferred from the number of horses or cattle. My wife, who knew something about statistics, was horrified when she learned that all my „data“ were in one little pocket-size notebook.

In the towns I usually stayed with the protestant pastor, who in most cases was also the leader of the Germans in his parish and therefore one of my main informants. Hospitality was graciously granted as one would expect in a rural-pre-industrial society. Here again one had to feel one’s way and could not simply start interviewing as soon as one had crossed the doorstep. However, there was usually no difficulty in getting the informants to talk; they liked to share their problems with the visitor from the “Reich”. Some of the pastors ran a school for German children or boarded pupils attending school in town. In these instances there would be entertainment, an evening party, arranged for the visitors, often playing old-fashioned parlor games or singing songs no longer known in the Reich.

I was quite surprised to discover that many customs and other culture traits of this German minority were survivals from the eighteenth century, the period of the main immigration. Certain types of food dated even further back to the seventeenth century when the ancestors of some families had left the archbishopric of Salzburg, in Austria, fleeing from the persecution of protestants.

It was especially in the area of culture that I benefited from my first-hand contacts and from the informal, unstructured approach I had to use. Many observations which proved to be significant were made quite casually and would have escaped me had I sent a group of graduate students into the field armed with structured questionnaires. There was of course no language barrier as in the case of my Ph.D. dissertation for which I had to learn Swedish. Except for a few statistical terms I did not learn Lithuanian. However, the Germans often did not have clear ideas of such concepts as nationality, citizenship and ethnic origin.

On the other hand my knowledge of French and English proved useful in background research on the history of Lithuania and the Lithuanian movement for independence from Russia. (By the way, many of the leaders had been students at the University at Koenigsberg where special attention was paid to the Lithuanian language and Lithuania.)

While the focus of my study was on the present condition, the history of the German
minority and its background also had to be treated. This led to work with primary sources in church-archives in Lithuania and in the Prussian State archives in Koenigsberg and Berlin. There had been in the 17th and 18th centuries diplomatic exchanges between the governments of Prussia and Poland (which at the time ruled Lithuania) about serfs who had run away from East Prussia. In the town of Skuodas (or Schoden) I came across the charter of this German settlement which had been founded by a Polish baron in the 16th century. I mention these excursions into historical research because it seems typical for German social scientists of my generation that we never shied from "trespassing" on other disciplines territory as so many Americans seem to do today.

An ethical problem arose when I had to contact Lithuanian officials. In view of the tension between the national government and the German minority I had to conceal the true objective of my study by pretending I was studying the economy of our neighboring state. Also as a byproduct of my inquiries I gained some insights into the German minority leaders' view of Lithuanian domestic politics; these I conveyed in a memorandum to the German Foreign Office. This was done with the approval of my superior.

The monograph resulting from my research, *Die Deutschen in Litauen* (1927), was the first comprehensive study of this group of Germans in the border country. Ten years later it was withdrawn from trade by the Nazis. No reasons were given, but one can guess what they were: I had depicted the German group as politically insignificant, culturally isolated from the fatherland and in the process of losing its identity through intermarriages with Lithuanians. This was a major heresy because according to Nazi doctrine, the "Germandom" was "in the blood".

Actually it was amazing that these people, in spite of having almost no contact with the Germans across the border — peasants do not travel —, had preserved German language and culture at all. In this respect they were like the well-known groups of ethnic Germans in Eastern Europe, all the way from Estonia in the north to Rumania and other Balkan countries in the south. Even in Russia there existed at that time large, well preserved German settlements. When a few years later in the U.S. I observed the quick assimilation and acculturation of Germans — except where they lived in large compact rural settlements — I formulated this theory: if an immigrant group moves from a country of advanced technology and economic development into a country which in these respects is relatively backward, they are likely to preserve their entire culture complex. If however the country of immigration is more advanced in these respects than the country of origin, assimilation and acculturation will occur soon, especially in the case of individuals who are rising in the socio-economic stratification system. Even in these cases, however, certain nonutilitarian culture traits are likely to be preserved in the second and third generation, e.g. certain kinds of food, and the use of the mother tongue in religious observances.

My study of migratory mobility in the U.S.A. (1929) did not involve systematic field work. But the very idea came to me as a result of conversations with all sorts of people. Whenever I asked somebody where his home was I would get a long story of his parents' moving from one place to another several times while he was young and of his own subsequent moves. I soon found that some travellers from Europe had made comments on this peculiar feature of American society; yet nobody had attempted a scholarly study of it and of its effects on social relationships, groups and norms of conduct. Studies on immigration and also on migration within the U.S. were numerous, but they were not dealing with the frequency of change of residence *within the lifetime of an individual*. This was the new point of view in my study. Today the Bureau of the Census publishes data at certain intervals on change of residence which give a fairly satisfactory approximation of migratory mobility in general and by occupational categories, and other break-downs. But in 1927 one had to attempt very rough estimates of the relative mobility of farmers, agriculture workers, various categories of industrial workers and other occupational groups from the Census reports stating how many residents of a state were born in other states and from special studies on migratory harvest workers, lumberjacks and other categories of highly mobile wage-earners.
The U.S. Department of Agriculture and the Department of Labor provided many reports which I found useful; officials in both departments gave me advice and information. Much had to be inferred from data collected for other purposes, and also from a vast range of literature, including autobiographies and novels. These sources were especially valuable in dealing with the social effects of mobility. Some insights were gained quite incidentally from newspapers and magazines.

It was the kind of project where one had to use imagination and be able to see the relevance of apparently unrelated phenomena. What for instance did it mean when a church put up a sign: “Make our church your home, easy chairs, men like them”? Or what was the significance of the fact that in California one found an abundance of state-societies, especially of Iowans and other mid-westerners? These expressions of a longing for contact, for new “roots” were the counterpart for the restlessness, the search for material gains that motivated migration. To put it in more theoretical terms: we are dealing here with a longing for “Gemeinschaft” in a society where the forming of intimate personal relationships is hampered by a high degree of migratory mobility and purposive gesellschaftlike relationships predominate.

The preparation of the study of political tendencies and voting among rural people in Schleswig-Holstein which I began in 1932 and continued even after the Nazis had seized power (see my 1945 and 1963) involved getting acquainted with types of farming and the particular local problems in agriculture. Much information had to be obtained by interviewing county administrators, prominent farmers and other leading people in rural communities, and, of course, politicians. I also learned much from chance conversations in village inns where I spent the night and in stores, or just while I travelled, which I did to a large extent on foot.

Sometimes one gets unexpected insights as a by-product of personally conducted field work. In the course of my project I asked many people about “neighborhood” customs, since I had a hunch that close neighborhood relations might contribute to high percentages of votes obtained by the leading party. One farmer gave me a detailed explanation of the customs prevailing among neighbors, such as mutual aid at funerals and on the other hand the “right” of neighbors to participate in weddings and other joyful family festivals. He remarked that in his case not only the people next door and across the street were his neighbors (German farmers as a rule live in cluster villages) but also the family on a farm more than a mile outside the village. It turned out that this farmstead had once joined his but had been moved to its present location sometime in the eighteenth (17) century in connection with a more rational arrangement of property holdings. This led me to conclude that neighborhood relations among these people...
were not simply relations between people but between farmsteads, a kind of social mortage resting on the farm regardless of who occupied it. My conclusion was confirmed by other villagers, among them a school teacher who had acquired a former farm house and, as he said, all the neighborhood duties and privileges that went with it. He emphasized that neighbors had priorities over friends when it came to inviting people for a wedding or baptism. This was the inception for a paper on "The normative element in neighborhood relations" which I wrote many years later (see my paper 1959a).

Another field observation drastically proved the effect of kinship on voting. The people on the island Maaholm had in 1932 voted nearly 100% for the Nazis although in preceding elections they had almost unanimously voted for the Communist party. I went there to get an explanation for this puzzling swing of opinion. My best informant was a young fisher with whom I consumed several glasses of grog in the village inn. From him I learned that the population consisted of only three or four large families — or one might say "clans" — interrelated by kinship and marriage. It appeared that the vote was practically controlled by the heads of these families. A second factor contributing to the unanimity in voting was that practically everybody was engaged in commercial fishing and dependent on the wholesalers who set the prices for the catch. The economic condition I might have found out from other sources, but the kinship element would have escaped me had I not gone into the field.

Another puzzling case was presented by two neighboring villages one of which had a high percentage of Social Democratic and the other of Communist votes. Both had the same kind of industry. By talking to people on the spot, I learned that the labor force in the first village was native and firmly organized in socialist unions, while the workers in the other village were predominantly outsiders, migratory workers, a fact which resulted in a high labor turnover. This situation resembled certain areas in the American West where once the I.W.W. (Industrial Workers of the World) had their strongholds among highly migratory workers. In political ecology I found again and again that closer inspection of unexpected or deviant local voting patterns often pays off through discovery of unanticipated factors of voting "behavior" — in principle, this is what imaginative natural scientists do when an experiment produces unusual results.

After my emigration to Louisiana, I directed three projects requiring field work: a study of new ground settlers in the Yazzo-Mississippi backwater area north of Vicksburg (see my paper 1941), a study of part-time farming among oil refinery workers in East Baton Rouge Parish and a study of Displaced Persons from Eastern Europe in Louisiana and Mississippi (see the monograph 1951b). In all three projects prepared questionnaires were used but some valuable information would have escaped us had I not made my own observations in the field and participated in some of the interviews. For example, the occupation of Displaced Persons given on the official rosters was not always the true occupation because "farmers" and "agricultural workers" had the preference. A few of these "farmers" turned out to be foresters and one or two were really "Oberfoerster", that is graduates of a forestry-academy. In one instance a woman listed as "farmer" who quite successfully and contentedly worked as a field hand on a plantation, told me she had been a book-keeper in a candy factory in Riga. When I challenged her, she said proudly "but I am a farmer. I owned a farm in Latvia". In this case, as in some others it helped that I had been in Latvia and Lithuania.

My knowledge of German also proved useful in some instances. We discovered that some of the children had learned German before coming to Louisiana; they were now able to speak three languages: I learned this by accident when the young son of a Ukranian plantation worker instead of translating my questions to his father who knew very little English began to answer them himself: I asked the father whether he knew German, he said "ein bischen" (a little bit) but when I continued the interview in German the boy again quickly answered in that language which he said he had learned "im Lager" (in the camp).

In this kind of project it is often difficult to maintain the role of an impartial investigator
and not to change into that of a mediator between the interviewer and his employer (or "sponsor" as in this case the employer was called). We did this in a few cases where my European background helped me to diagnose the nature of the misunderstanding. For example, one sponsor had offered to take "his" Displaced Person to stores like Sears in order to help him to establish credit. The Pole thought this was a trick to trap him into a sort of peonage. In another case a Polish couple had complaints about the high iron content of their well-water. When the sponsor told us he had suggested they drink the rain water collected in a cistern I knew there would be a new conflict. I remembered from my childhood how I had been warned never to drink from a rainwater-barrel. And sure enough, our Polish friend was insulted and we had to assure him that in rural south Louisiana everybody used rainwater since there were no underground wells. I say "my Polish friend" because in this case I had broken the rules and taken a personal interest in the family. It was a family which in terms of educational and occupational qualities had been definitely "misplaced", a fact of which the sponsor was quite aware.

Two of these projects were carried out with one graduate student, who got a MA Thesis out of it. The new ground settler study was started with a graduate assistant, who was later joined by a more experienced field worker employed by the sponsoring government agency. In all three projects the number of cases was not large, nor could we establish a strictly representative sample because we did not know the total number of units and because we had to take as many "cases" as we could contact. However, in all three cases, we found that after a certain number of interviews we did not learn anything new of importance. All the oil refinery workers who owned farms had grown up on farms, all of them wanted their children to grow up in the country and most of them regarded farm ownership as a security for old age. The new ground settlers, whether black or white, were former share croppers who hoped to become independent farmers; all of them were paying exorbitant prices for what they thought was fertile river bottom land — in which they were mistaken. It seems quite logical that in dealing with a highly homogeneous category of people one is bound to reach diminishing returns when adding new cases.

Of course, this rule may not hold where one is dealing with problems of greater complexity than those I had to deal with.

In this essay I have tried to show the advantages of doing one's own field work, or, at least, to participate in the initial phases of team research. In this context I dealt with the phenomenon of serendipity, i.e. unexpected byproducts (or "spin-off") and with the experience that what one has learned in one project sometimes is helpful in other, seemingly quite unrelated, research. This compensates for the disadvantages resulting from failing to specialize in one particular field. It is also a great asset in teaching.

Finally I tried to show, by a few examples, that the sociologist should not be afraid of "trespassing" on neighboring disciplines, especially history. In my case it was a practical matter to inquire into the "history" of my subjects. But there is more to it than that: one should be aware of the fact that social phenomena of sociological relevance must be seen in historical perspective, since they occur in "historical time". This, however, would require separate treatment.

Selected Bibliography:


