

Reading 29

## Hired Hand Research

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### CASE I

After it became obvious how tedious it was to write down numbers on pieces of paper which didn't even fulfil one's own sense of reality and which did not remind one of the goals of the project, we all in little ways started avoiding our work and cheating on the project. It began for example when we were supposed to be observing for hour and a half periods, an hour and a half on the ward and then an hour and a half afterwards to write up or dictate what we had observed, in terms of the category system which the project was supposed to be testing and in terms of a ward diary. We began cutting corners in time. We would arrive a little bit late and leave a little bit early. It began innocently enough, but soon boomeranged into a full cheating syndrome, where we would fake observations for some time slot which were never observed on the ward. Sarn, for example, in one case, came onto the ward while I was still finishing up

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an assignment on a study patient and told me that he was supposed to observe for an hour and a half but that he wasn't going to stay because he couldn't stand it anymore. He said he wasn't going to tell anyone that he missed an assignment, but that he would simply write up a report on the basis of what he knew already about the ward and the patients. I was somewhat appalled by Sam's chicanery, and in this sense I was the last one to go. It was three or four weeks after this before I actually cheated in the same manner.

It was also frequent for us to miss observation periods, especially the 8 to 9:30 a.m. ones. We all had a long drive for one thing, and we were all chronic over-sleepers for another. For a while we used to make up the times we missed by coming in the next morning at the same time and submitting our reports with the previous day's date. As time went on, however, we didn't bother to make up the times we'd missed. When we were questioned by our supervisor about the missing reports, we would claim that there had been an error in scheduling and that we did not know that those time slots were supposed to be covered.

There were other ways we would cheat, sometimes inadvertently. For example, one can decide that one can't hear enough of a conversation to record it. People need to think fairly highly of themselves, and when you think that you're a cheat and a liar and that you're not doing your job for which you are receiving high wages, you are likely to find little subconscious ways of getting out of having to accuse yourself of these things. One of the ways is to not be able to hear well. We had a special category in our coding system, a question mark, which we noted by its symbol on our code sheets whenever we could not hear what was going on between two patients. As the purgatory of writing numbers on pieces of paper lengthened, more and more transcripts were passed in with question marks on them, so that even though we had probably actually heard most of the conversations between patients, we were still actually avoiding the work of transcription by deceiving ourselves into believing that we could not hear what was being said. This became a good way of saving yourself work. If you couldn't hear a conversation, it just got one mark in one column of one code sheet, and if you wrote down an elaborate conversation lasting even ten minutes, it might take you up to an hour to code it, one hour of putting numbers in little blocks. In the long run, all of our data became much skimpier. Conversations were incomplete; their duration was strangely diminishing to two or three minutes in length instead of the half-hour talks the patients usually had with each other. We were all defining our own cutting off points, saying to ourselves, "Well, that's enough of that conversation." According to the coding rules, however, a communication can't be considered as ended until the sequence of interaction has been completed and a certain time lapse of silence has ensued.

In order to ensure the reliability of our coding, the research design called for an "Inter-Rarer Reliability Check" once every two months, in which each of the four of us would pair up with every other member of the team and be rated on our ability to code jointly the same interaction in terms of the same categories and dimensions. We learned to loathe these checks; we knew that the coding system was inadequate in terms of reliability and that our choice of

categories was optional, subjective, and largely according to our own sense of what an interaction is really about, rather than according to the rigid, stylized, and preconceived design into which we were supposed to make a reality fit. We also knew, however, that our principal investigators insisted on an inter-rater reliability coefficient of .70 in order for the research to proceed. When the time came for another check, we met together to discuss and make certain agreements on how to bring our coding habits into conformity for the sake of achieving reliability. In these meetings we would confess our preferences for coding certain things in certain ways and agree on certain concessions to each other for the duration of the check. Depending on what other individual I was to be paired with, for example, I had a very good idea of how I could code in order to achieve nearly the same transcriptions. We didn't end it there. After each phase of a check, each pair of us would meet again to go over our transcriptions and compare our coding, and if there were any gross discrepancies, we corrected them before sending them to the statisticians for analysis. Needless to say, as soon as the reliability checks were over with, we each returned to a coding rationale which we as individuals required in order to do any coding at all-in order to maintain sanity.

#### CASE 11

There didn't appear to be too much concern with the possibility of inconsistency among the coders. Various coders used various methods to determine the code of an open-end question. Toward the end of the coding process, expediency became the keynote, leading to gross inconsistency. The most expedient method of coding a few of the trickier questions was to simply put down a "4" (This was the middle-of-the-road response on the one question that had the most variation.). If the responses were not clear or comprehensible, the coder had two alternatives: on the one hand, he could puzzle over it and ask for other opinions or, on the other hand, he could assign it an arbitrary number or forget the response entirely.

In the beginning, many of us, when in doubt about a response, would ask the supervisor or his assistant. After a while, I noted that quite often the supervisor's opinion would differ when asked twice about the same response and he would often give two different answers in response to the same question. One way the supervisor and his assistant would determine the correct coding for an answer would be to look at the respondent's previous answers and deduce what they should have answered—thereby coding on *what they thought the respondent should have answered*, not on the basis of what he *did* answer. One example that I distinctly remember is the use of magazines regularly read as reported by the respondent being used as a basis on which to judge and code their political views. This, in my opinion, *would* be a factor in some of the cases, such as the reading of an extreme leftist or extreme rightist magazine, but to use magazines such as *Time* or *Reader's Digest* to form any conclusions about the type of person and his views, I feel is quite arbitrary.

Furthermore, I feel questionnaires should be used to see *if* consistent patterns of views exist among respondents and it is not the coder's job to put them in if the respondents fail to'

Some of the coders expected a fixed pattern of response. I, not being sure of what responses meant in a total political profile, treated each response separately-which I feel is the correct way of coding a questionnaire. Others, as I learned through their incessant jabbering, took what they thought was a more sophisticated method of treating an interview. A few would discuss the respondent's answers as if they took one political or social standpoint as an indicator of what all the responses should be. They would laugh over an inconsistency in the respondent's replies, feeling that one answer did not fit the previous pattern of responses.

The final problem leading to gross inconsistency was the factor of time. The supervisor made it clear that the code sheets had to be in the computation center by Saturday. This meant that on Saturday morning and early afternoon the aim of the coders was to code the questionnaires as quickly as possible, and the crucial factor was speed, even at the expense of accuracy. The underlying thought was that there were so many questionnaires coded already (that we *assumed* to be coded consistently and correctly) that the inconsistencies in the remainder would balance themselves out and be of no great importance. I found myself adapting to this way of thinking, and after spending two or three hours there on Saturday morning, I joined in the game of "let's get these damn things out already." It did indeed become a game, with the shibboleth, for one particularly vague and troublesome question, "Oh, give it a four."

### CASE III

One of the questions on the interview schedule asked for five reasons why parents had put their child in an institution. I found most people can't think of five reasons. One or two-sometimes three. At first I tried pumping them for more reasons, but I never got any of them up to five. I didn't want (the director) to think I was goofing off on the probing, so I always filled in all five.

Another tough one was the item about how the child's disability affected the family relationships. We were supposed to probe. Probe what? You get so many different kinds of answers, I was never sure what was worth following up. Sometimes I did if the respondent seemed to have something to say. Otherwise I just put down a short answer and made it look as if that was all I could get out of them. Of course, (the director) *did* list a few areas he wanted covered in the probing. One of them was sex relations of the parents. Most of the time I didn't follow up on that. Once in a while I would get somebody who seemed to be able to talk freely without embarrassment. But most of the time I was afraid to ask, so I made up something to fill that space.

Then there was that wide open question at the end. It's vague. Most people don't know what to say. You've been asking them questions for about an hour already. Usually you get a very short answer. I didn't push them. I'd write up a

longer answer later. It's easy to do. You have their answer to a lot of other questions to draw on. You just put parts of some of them together, dress it up a little, and add one or two bits of new information which fits in with the rest.

Any reader with research experience can probably recall one or more cases in which he observed, suspected, or participated in some form of cheating, carelessness, distortion, or cutting of corners in the collection or processing of research data. He probably thought of these instances as exceptions-an unfortunate lapse in ethical behavior or a failure of research directors to maintain proper controls. I would like to put forth the thesis that such behavior on the part of hired data collectors and processors is not abnormal or exceptional, but rather is exactly the kind of behavior we should expect from people with their position in a production unit.

The cases I have presented do not constitute proof, of course. Even if I presented ten or twenty more, my efforts could be dismissed as merely an unusually industrious effort to record professional dirty linen (or I might be accused of making them up!) and not at all representative of the many thousands of cases of hired researching carried out every year. Rather than multiply examples, I would like to take a different tack and examine the model we have been using in thinking about research operations and to suggest another model which I believe is more appropriate.

The ideal we hold of the researcher is that of a well-educated scholar pursuing information and ideas on problems in which he has an intrinsic interest. Frequently this ideal may be approximated when an individual scholar is working on his own problem or several colleagues are collaborating on a problem of mutual interest. Presumably such a researcher will endeavor to carry out his data collection and processing in the most accurate and useful way that his skills and time permit.

When a researcher hires others to do the collecting and processing tasks of his research plan, we often assume that these assistants fit the "dedicated scientist" ideal and will lend their efforts to the successful conduct of the over-all study by carrying out their assigned tasks to the best of their ability. As suggested by my examples, I doubt that hired assistants usually behave this way even when they are junior grade scholars themselves. It becomes more doubtful yet when they are even further removed from scholarly tradition and from the direct control of the research directors (e.g. . part-time survey interviewers) .

It seems to me that we can develop a more accurate expectation of the contribution of the hired research worker who is required to work according to somebody else's plan by applying another model which has been worked out in some detail by sociologists-namely, the work behavior of the hired hand in a production organization. First, let us look at one of the more thorough of these studies, Donald Roy's report on machine shop operators.<sup>1</sup>

Roy's workers made the job easier by loafing when the piece rate did not pay well. They were careful not to go over their informal "quotas" on piece

<sup>1</sup>Donald Roy, "Quota Restriction and Goldbricking in a Machine Shop," *American Journal of Sociology*, 57 (March (1952),427-42.

rate jobs because the rate would be cut and their work would be harder. They faked time sheets so that their actual productive abilities would not be known to management. They cut corners on prescribed job procedures to make the work easier and/or more lucrative even though this sometimes meant that numerous products had to be scrapped. Roy's calculations show that the workers could have produced on the order of twice as much if it had been in their interest to do so.

But it is *not* in their interest to do so. The product the hired hand turns out is not in any sense his. He does not design it, make any of the decisions about producing it or about the conditions under which it will be produced, or what will be done with it after it is produced. The worker is interested in doing just enough to get by. Why should he concern himself about how well the product works or how much time it takes to make it? That is the company's problem. The company is his adversary and fair game for any trickery he can get away with. The worker's aim is to make his job as easy and congenial as the limited resources allow and to make as much money as possible without posing a threat to his fellow workers or to his own future. The company, in turn, is placed in the position of having to establish an inspection system to try to keep the worst of their products from leaving the factory (an effort often unsuccessful—the inspectors are hired hands, too) and of devising some form of supervision to limit the more extreme forms of gold-bricking and careless workmanship.

Almost all the systematic research on "restriction of output" and deviation from assigned duties has been done on factory workers, office clerks, and other low prestige work groups. This is mostly because such work is easier to observe and measure, but also because much of this research has been controlled in part by those in a position of authority who want research done only on their subordinates. However, there is evidence to indicate that work restrictions and deviations in the form of informal group definitions and expectations are probably universal in our society. They can be found among business executives and in the professions, sports, and the creative arts. They are especially likely to crop up when one is working as a hired hand, and almost all productive activities have their hired hand aspects. A professor may work hard on scholarly tasks of his own choosing and perhaps even on teaching a course which he himself has devised, but he becomes notoriously lax when he is assigned to a departmental service course which he does not like—spending little or no time on preparation, avoiding his students as much as possible, turning all the exams over to a graduate assistant, and so on.

"Restriction of production" and deviation from work instructions is no longer regarded by students of the sociology of work as a moral issue or a form of social delinquency. Rather, it is the expected behavior of workers in a production organization. The only problem for an investigator to work practices is discovering the details of cutting corners, falsifying time sheets, defining work quotas, dodging supervision, and ignoring instructions in a given work setting.

There is no reason to believe that a hired hand in the scientific research business will behave any different from those in other areas of productive

activity. It is far more reasonable to assume that their behavior will be similar. They want to make as much money as they can and may pad their account or time sheet if they are paid on that basis, but this type of behavior is a minor problem so far as the present discussion is concerned. They also want to avoid difficult, embarrassing, inconvenient, time-consuming situations as well as those activities which make no sense to them. (Thus, they fail to make some assigned observations or to ask some of the interview questions.) At the same time they want to give the right impression to their supervisors—at least right enough so that their material will be accepted and they will be kept on the job. (Thus, they modify or fabricate portions of the reports in order to give the boss what he *seems* to want.) They do not want to "look stupid" by asking too many questions, so they are likely to make a stab at what they think the boss wants—e.g., make a guess at a coding category rather than having it resolved through channels.

Even those who start out with the notion that this is an important piece of work which they must do right will succumb to the hired hand mentality when they realize that their suggestions and criticisms are ignored, that their assignment does not allow for any imagination or creativity, that they will receive no credit for the final product, in short, that they have been hired to do somebody else's dirty work. When this realization has sunk in, they will no longer bother to be careful or accurate or precise. They will cut corners to save time and energy. They will fake parts of their reporting. They will not put themselves out for something in which they have no stake except in so far as extrinsic pressures force them to. Case No. I is an excerpt from the statement of a research worker who started out with enthusiasm and hard work and ended with sloppy work and cheating when she could no longer escape the fact that she was a mere flunky expected to do her duty whether or not it was meaningful. The coders in Case II soon gave up any effort to resolve the ambiguities of their coding operation and followed the easiest path acceptable to their supervisor. In this case, the supervisor himself made little effort to direct the data processing toward supplying answers to meaningful research issues. We must remember that in many research operations the supervisors and directors themselves are hired hands carrying out the requests of a client or superior as expeditiously as possible.

Many of the actions of hired hand researchers are strikingly analogous to restrictive practices of factory operatives. Interviewers who limit probing and observers who limit interaction recording are behaving like workers applying "quota restriction," and with interacting hired hands informal agreements may be reached on the extent of such restrictions. To fabricate portions of a report is a form of goldbricking. The collusion on the reliability check reported in Case I is strikingly similar to the workers' plot to mislead the timestudy department. Such similarities are no accident. The relationship of the hired hand to the product and the process of production is the same in each case. The product is not "his." The production process gives him little or no opportunity to express any intrinsic interest he may have in the product. He will sooner or later fall into a pattern of carrying out his work with a minimum of effort,



inconvenience, and embarrassment—doing just enough so that his product will get by. If he is part of a large and complex operation where his immediate superiors are also hired hands with no intrinsic interest in the product and where the final authority may be distant and even amorphous, quality control of the product will be mechanical and the minimal effort that will get by can soon be learned and easily applied. The factory production situation has at least one ultimate limitation on the more extreme deviations of the hired hands: The final product must "work" reasonably well in a substantial proportion of cases. In social science research, on the other hand, the product is usually so ambiguous and the field of study so lacking in standards of performance that it is difficult for anyone to say whether it "works" or not.

What is more important is the effect of the hired hand mentality on the *nature* of the product. Workmen not only turn out less than they could if it were in their interest to maximize production, but often produce shoddy and even dangerous products." In the case of research, the inefficiency of hired hands not only causes a study to take longer or cost more money, but is likely to introduce much dubious data and interpretations into the process of analysis. Our mass production industrial system has opted to sacrifice individual efficiency and product quality for the advantages of a rationalized division of labor. The same approach has been applied to much of our larger scale scientific research and the results, in my opinion, have been much more disastrous than they are in industrial production with little compensating advantages.

When the tasks of a research project are split up into small pieces to be assigned to hired hands, none of these data collectors and processors will ever understand all the complexities and subtleties of the research issues in the same way as the person who conceived of the study. No amount of "training" can take the place of the gradual development of research interests and formulations on the part of the planner. Since the director often cannot be sure what conceptions of the issues the hired hands have as a result of his explanations and "training," he must make dubious guesses about the meaning of much of the data they return to him. If he attempts to deal with this difficulty by narrowly defining the permissible behavior of each hired hand (e.g., demand that all questions on a schedule be asked in a set wording), he merely increases the alienation of the hired hand from his work and thus increases the likelihood of cutting corners and cheating. As he gains in quantity of data, he loses in validity of meaningfulness."

I do not want to give the impression that the hired hand mentality with its

2] want to emphasize once again that in a business setting, supervisors and executives, as well as production line workmen, participate in aspects of the hired hand mentality. None of them may have an intrinsic interest in the quality of the product. (See, for example, Melvin Dalton, *Men Who Manage* (New York: Wiley, 1959), esp. chaps. 7, 8, and 9.) The same is the case in much large-scale research.

3]In this discussion] am assuming there *is* someone (or a small group of colleagues) who has initially formulated the research problem or area of concern because of intrinsic interest and curiosity. In much of our social science research, we do not have even this saving grace and the research is formulated and carried out for various "political" reasons. In such cases, we cannot count on having anyone interested enough to try to turn the accumulations of data into a meaningful explanatory statement.



attendant difficulties is simply a characteristic of the large-scale on-going research organization. We may find it at all size levels, including the academic man hiring a single student to do his research chores. The argument may be advanced that assignment of specified tasks by the director of a study is essential to getting the job done in the manner that he wants it done. My answer is that such assignments are often not effectively carried out and it is misleading to assume that they are.

Let me illustrate this point. A researcher wants to do a study of the operation of a given institution. He has some definite notion of what aspects of behavior of the institutional personnel he wants information about and he has some ideas about the manner in which he will go about analysing and interpreting these behaviors. He finds it possible and useful to engage four trained and interested assistants. Let me outline two ways the study might be conducted.

A Through a series of discussions, general agreement is reached about the nature of the study and the manner in which it might be conducted. Some division of lab or is agreed upon in these discussions. However, none of the field workers is held to any particular tasks or foci of interest. Each is allowed to pursue his data collection as he thinks best within the larger framework, although the field workers exchange information frequently and make new agreements so that they can benefit from each other's experience.

B The director divides up the data collection and processing in a logical manner and assigns a portion to each of the assistants. Each field worker is instructed to obtain information in all the areas assigned to him and to work in a prescribed manner so that his information will be directly comparable to that of the others. The director may use a procedural check such as having each assistant write a report covering given issues or areas at regular intervals.

Which is the preferred approach? Judging from my reading of social science journals, most research directors would say Method B is to be preferred. Method A, they would maintain, produces information on subjects, issues, or events from one field worker which is not directly comparable to that collected by another field worker. They would also object that if each field worker is permitted to follow his own inclinations even in part, the total study will suffer from large gaps. These accusations are quite true-and, I would add, are an inevitable result of dividing a research project among a number of people. What I disagree with, however, is the assumption that Method B would not suffer from these defects (if indeed, they should be regarded as defects.) It is assumed that the assistants in Method B are actually carrying out their assigned tasks in the manner specified. In line with my earlier discussion of the behavior of hired hands, I would consider this highly unlikely. If the information produced by these assistants is indeed closely comparable, it would most likely be because they had reached an agreement on how to restrict production. And, whether the study is carried out by Method A or by Method B, gaps will occur. The difference is that the director of Study A-assuming he had

succeeded in making his assistants into collaborating colleagues-would at least know where the gaps are. The director of Study B would have gaps without knowing where they are-or indeed, that they exist-because they have been covered over by the fabrications of his alienated assistants.

It is ironic that established researchers do not ascribe the same motivating forces to their subordinates as they do to themselves. For many years research scientists have been confronting those who pay their salaries and give them their grants with the argument that a scientist can do good research only when he has the freedom to follow his ideas in whatever way seems best. They have been so successful with this argument that university administrations and research organization directorates rarely attempt to dictate-or even suggest-problems or procedures to a researcher on their staff, and the more prominent granting agencies write contracts with almost no strings attached as to the way in which the study will be conducted. Yet research directors fail to apply this same principle to those they hire to carry out data collection and processing. The hired assistant's desire to participate in the task and the creative contribution he might make is ignored with the result that the assistants' creativity is applied instead to covertly changing the nature of the task.

There has been very little discussion in our journals and our books on research methods on the relationship of the hired hand to the data collected. Whatever discussion there *has* been can be found in the survey interview field where there have been some studies of the effect of such demographic factors as age, sex, and race, sometimes measured personality traits, on "interviewer bias." The nature of the interviewer's status in a research organization is seldom discussed in print. The problem of interviewer cheating, although a common subject of informal gossip, is seldom dealt with openly as a serious problem. When Leo Crespi published an article twenty years ago in which he expressed the worry that cheating was seriously affecting the validity of much survey data," those who responded (mostly survey organization executives) stated reassuringly that few interviewers cheated and that they had pretty effective ways of controlling those who did." If the analysis offered in this paper is correct, the first part of this reassurance is almost certainly wrong. The low-level flunky position which most interviewers occupy in survey organizations" should lead us to expect widespread deviations from assigned tasks. The survey executives who responded give no convincing evidence to the contrary. As for the second part of the assertion, their descriptions of their control measures indicate that they can hope to block only the cruder, more obvious, and repeated forms of cheating. The postal card follow-up may eventually catch the interviewer who makes contacts, but fabricates demographic data (to

<sup>1</sup>Leo Crespi, "The Cheater Problem in Polling," *Public Opinion Quarterly* (Winter 1945-1946), pp. 431-45.

<sup>2</sup>"Survey on Problems of Interviewer Cheating," *International Journal of Opinion and Attitude Research*, 1 (1947), 93-107.

<sup>3</sup>Iulius A. Roth, "The Status of Interviewing," *The Midwest Sociologist*, 19 (December 1956), 8-11.

fill a quota sample) or completes only part of the interview and fills in the rest in a stereotyped manner later on. (Even here, many of his interviews may be used before he is detected.) However, from the cases of hired hand interviewing which I am familiar with, I would say such crude cheating is not the most common form of cutting corners on the job. Far more common is the kind found in Case III where the interviewer makes his contact, obtains a fairly complete interview, but leaves partial gaps here and there because he found it time-consuming, embarrassing, or troublesome, felt threatened by the respondent, or simply felt uncertain about how the study director wanted certain lines of questioning developed. With a little imagination, such gaps can be filled in later on in a way that is very unlikely to be detected in a follow-up interview. If, for example, a supervisor in Case III had returned to the respondents and asked them whether the "five reasons" listed on their interview form were accurate reflections of their opinion, probably most would have said yes, and the few who objected to one or two of the reasons could have been dismissed as the degree of change that one expects on re-interview.<sup>1</sup>

Some gimmicks for catching cheaters may even put the finger on the wrong person. Thus, one approach to detecting cheating is to compare the data of each interviewer to the group averages and to assume that if one deviates markedly from the group, he is cheating or doing his work improperly. This reasoning assumes that cheating is exceptional and will stand out from the crowd. I have already suggested that the opposite is often the case. Therefore, if the cheaters are working in the same direction (which is readily possible if they have reached an informal agreement or if the question is of such a nature as to suggest distortion in a given direction), it is the "honest" person who will deviate. In the study alluded to in Case III, for example, one of the interviewers always left spaces open on the "five reasons" item. At one point the director reprimanded him for not obtaining five responses "like the rest of the interviewers." The director preferred to believe that this man was not doing his job right than to believe that all the rest were making up responses.

Large survey organizations have at least made some attempts to control the cruder forms of cheating. In most studies using hired hands, even this limited control is absent. The academic man with one or a few assistants, the research organization study director with one or a few small projects, usually has no routine way of checking on the work of his assistants. If he duplicates much of their work or supervises them very closely, he may as well dispense with their services. If he gives them assignments without checking on them closely, he is in effect assuming that they are conducting their assignment more or less as directed and is accepting their products at face value. This assumption, I assert, is a dubious one. And since it is a common practice nowadays to farm out much of one's research work—quite often to accumulate

<sup>1</sup>I have even heard the argument that it makes no difference if perceptive interviewers make up parts of the interview responses with the help of information from other responses because their fabrications will usually closely approximate what the subject would have said if he could have been prompted to answer. But if we accept this argument, a large portion of the interview should have been eliminated to begin with. It means we already claim to know the nature of some of the relationships which the study is purportedly investigating.

research grants only to hire others to do the bulk of the work—the dubious nature of hired hand research is a widespread problem in small as well as large scale research, in surveys, in direct observation, and in various forms of data processing.

I do not want to suggest, however, that the major failure of hired hand research is the lack of control of cheating. Rather, the very fact that we are placed in a position of having to think up gimmicks to detect cheating is in itself an admission of failure. It means that we are relying for an important part of our research operation on people who have no concern for the outcome of the study. Such persons cannot have the kind of understanding of the data collection or data-processing procedures which can come only with working out problems in which the researcher has an intrinsic interest and has gone through a process of formulating research questions and relevant ways of collecting and processing data.

I can hear the objection that much social science cannot be done without hired hands. But we should at least be aware of the doubtful nature of some of the information collected in this way and construct our data collection and processing in such a way as to reduce the encouragement of cheating and restriction of production as much as possible (See Crespi's list of "ballot demoralizers") More important, however, I believe the need for hired hands has been greatly exaggerated. Why, for example, must we so often have large samples? The large sample is frequently a contrivance for controlling various kinds of "errors" (including the "error" introduced by unreliable hired hands). But if the study were done on a much smaller sample by one person or several colleagues who formulated their own study and conducted it entirely by themselves, much of this error would not enter in the first place. Isn't a sample of fifty which yields data in which we can have a high degree of confidence more useful than a sample of five thousand where we must remain doubtful about what it is that we have collected? Often a large-scale study tries to do too much at one time and so ends up as a hodge-podge affair with no integration of ideas or information ever taking place because it is, in effect, *nobody's* study. How often have you read the report of a massive study expending large amounts of money and employing large numbers of people where you were disappointed at the paucity of the results, especially when compared to a far smaller project on a similar issue conducted entirely by one or a few people?

Let me repeat that I am not singling out large-scale operations as the only villains. The current structure of professional careers is such that often small studies are turned over to hired hands. We tend to be rated on how many studies we can carry on at the same time rather than on how thoroughly and carefully we can carry through a given line of research. Soon we find that we do not have time for all of the projects we have become involved in and must turn some over to others of lower professional status. This might not be so bad if we were willing to turn over the research work wholeheartedly. We might simply act as entrepreneurs to funnel funds to others and to provide them with

"Leo Crespi, *op. cit.*, pp. 437-39.

appropriate clearance and an entre to research settings. We can then leave the specific formulation of the problem and procedure (and the credit for doing the work) to the person we have helped out. Such is often done, of course. However, there are many instances in which the senior researcher believes those he has hired cannot be trusted to formulate their own plans, or professional career competition convinces him that he cannot "afford" to give up any of his studies to others. In such cases he is likely to maintain a semblance of control by mechanically structuring a research plan and making assignments to his assistants. This, as I have indicated, is the way to the hired hand mentality with its attendant distortions of research data.

What is a hired hand? So far I have been talking as if I knew and as if the hired hand could readily be distinguished from one who is not. This, of course, is not true. The issue is a complex one and information on it is, by its very nature, not very accessible. It is a crucial question which deserves study in its own right as part of the more general study of the process of "doing research."

Let me attempt a crude characterization of hired hand research, a characterization which hopefully will be greatly refined and perhaps reformulated with further study. A hired hand is a person who feels that he has no stake in the research that he is working on, that he is simply expected to carry out assigned tasks and turn in results which will "pass inspection." Of course, a hired assistant may not start out with the hired hand mentality, but may develop it if he finds that his talents for creativity are not called upon and that his suggestions and efforts at active participation are ignored.

From specific examples from the research world and by analogy from research on hired hands in other occupational spheres, I am convinced that research tasks carried out by hired hands are characterized, not rarely or occasionally, but *typically*, by restricted production, failure to carry out portions of the task, avoidance of the more unpleasant or difficult aspects of the research, and outright cheating. The results of research done in part or wholly by hired hands should be viewed as a dubious source for information about specific aspects of our social life or for the raw material for developing broader generalizations.

Of course, this leaves open the question of what constitutes a "stake in the research" and how one avoids or reduces the hired hand mentality. Again, I have no specific answers and hope that issues will receive much more attention than it has up to now. A stake may mean different things in various circumstances. For graduate students, a chance to share in planning and in writing and publication may often be important. For interviewers or field workers, the determination of the details of their procedure may be crucial. In an applied setting, the responsibility for the practical consequences of the research findings may be most important."

"The "human relations in industry" movement has given us some useful suggestions about the circumstances which alienate workers and executives, and also ways in which industrial employees may be given a real stake in their jobs. See, for example, Douglas McGregor, *The Human Side of Enterprise* (New York: McGraw-Hill, 1960), Part 2.

It would also be worthwhile to examine the conditions which make for hired hand research. Here again, I have little specific to say and this subject, too, needs much more investigation. However, I will suggest a few factors I consider important.

**Size** Hired hands can be found in research staffs of all sizes from one on up. However, it is clear that when a very small number of researchers are working together, there is a greater possibility of developing a true collegue-ship in which each will be able to formulate some of his own ideas and put them into action. The larger the group, the more difficult this becomes until the point is probably reached where it is virtually impossible, and the organization must be run on the basis of hierarchical staff relations with the lower echelons almost inevitably becoming hired hands.

**Subordination** If some members of the research group are distinctly subordinate to others in a given organizational hierarchy or in general social status, it will be more difficult to develop a true colleague working relationship than if their status were more closely equal. The subordinate may hesitate to advance his ideas; the superordinate might be loath to admit that his lower-level co-worker be entitled to inject his ideas into the plans. Formal super-subordinate relationships can of course be muted and sometimes completely overcome in the course of personal contact, but certainly this is an initial, and sometimes permanent, basis for establishing hired hand status.

**Adherence to Rigid Plans** If a researcher believes that good research can be done only if a detailed plan of data collection, processing, and analysis is established in advance and adhered to throughout, he has laid the basis for hired hand research if he makes use of assistance from others who have not participated in the original plan. Sticking to a pre-formed plan means that others cannot openly introduce variations which may make the study more meaningful for them. Any creativity they apply will be of a surreptitious nature.

In their research methods texts, our students are told a great deal about the mechanics of research technique and little about the social process of researching. What little is said on the latter score consists largely of Pollyannaish statements about morale, honesty, and "proper motivation." It should be noted that appeals to morality and patriotism never reduced goldbricking and restriction of production in industry, even during the time of a world war. There is no reason to believe that analogous appeals to interviewers, graduate students, research assistants, and others who serve as hired hands will be any more effective. If we want to avoid the hired hand mentality, we must stop using people as hired hands.

Glaser and Strauss state that we regularly "discount" aspects of many, if not most, of all scientific analyses we read because we consider the research designed onesided, believe that it does not fit the social structure to which it

was generalized, or that it does not fit in with our observations in an area where we have had considerable experience."

I would like to suggest another area in which we might consistently apply the "discounting process." When reading a research report, we should pay close attention to the description of how the data were collected, processed, analyzed, interpreted, and written up with an eye to determining what part, if any, was played by hired hands. This will often be a difficult and highly tentative judgment, requiring much reading between the lines with the help of our knowledge of how our colleagues and we ourselves often operate. However, we can get hints from such things as the size of the staff, the nature of the relationship of the staff members, the manner in which the research plans were developed and applied, the organizational setting in which the research was done, mention made of assignment of tasks, and so on. If there is good reason to believe that significant parts of the research has been carried out by hired hands, this would, in my opinion, be a reason for discounting much or all of the results of the study.