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Truths and Untruths in Village Haiti: An Experiment In Third World Survey Research

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We have devised the following descriptive definition: a rural Third World survey is the careful collection, tabulation, and analysis of wild guesses, half-truths, and outright lies meticulously recorded by gullible outsiders during interviews with suspicious, intimidated, but outwardly compliant villagers. The definition is meant to be a caricature not of the villager, but of the researcher; not of all village surveys, but certainly of many. Both of us have experience in village interviewing and we are consequently touched by the caricature.

In this chapter we will present and discuss a survey research strategy applied in a Haitian village, devised to minimize the conditions which justify the above definition. The methodological complex which eventually took shape is the product of interdisciplinary collaboration, in which we — a demographer (Chen) and an anthropologist (Murray) — remained intent on applying the methods and securing the information of interest to our respective disciplines. Our research design incorporated a series of special features which we felt would

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achieve a dual purpose: to insure and verify the accuracy of the data, and to alleviate the anxiety-ridden, embarrassing, and sometimes offensive social situation that is created for the villager when a briefcase-toting socially "superior" urban stranger appears at his door to ask personal questions.

In retrospect, we harbor no illusions that we were completely successful in achieving these two goals. No social research strategy can ever guarantee complete accuracy, nor can the unpleasantness inherent in attempts to elicit personal information ever be completely eliminated. It is our feeling that the model we devised came much closer to achieving these two goals in the Haitian setting than a traditional survey would have. Because this model was devised and employed in the context of a specific village in a specific culture, we would in no way recommend its blanket application to other cultural settings. Furthermore, much more comparative material will be needed to prove or disprove the effectiveness of this model, and it is not our intent to do that here. We merely wish to present the main arguments we considered for and against the different strategies which we eventually adopted, feeling that the key aspects of the model may prove adaptable and usable by other researchers in other settings, particularly in rural areas. With this goal in mind, we shall present and discuss its central features.

The Problem: Difficulties Inherent in Third World Surveys

The decision to conduct a survey in the Cul-de-Sac Plain of Haiti was made in the context of a recently instituted public medical center and maternal-child health program in a town of the Plain, a program for which the addition of a family planning component was also being contemplated. No reliable quantified information existed on births, deaths, age and sex structure of the population, migration, conjugal patterns, family size aspirations, folk-medical versus modern medical practices, familiarity with and attitudes toward contraception, and other matters of practical as well as scientific interest. Though we included questions on all of these topics, our central interest in the survey was to elicit detailed conjugal union histories from both males and females, and detailed pregnancy histories from the females.

In planning the survey, we anticipated serious difficulties. These obstacles, though they take a particularly acute form in Haiti, generally apply to survey work in the Third World. They include logistics, locating the respondents once they are selected, inability of respondents to give ages and dates, and unwillingness to give personal information.

Logistics

The road to the research area in the Plain is in such poor shape, and so many of the communities are inaccessible to vehicles during much of the year, that the interviewers would have to live on the site during the survey. This would involve arranging for housing, food, and sanitary and bathing facilities acceptable to outside interviewers.

Locating the Respondents Once They Were Selected

The interviews would have to be done in the respondents' homes. But on normal work days, men work in the fields and do not come back until dusk. Of course, electric lights are not available in the area. Women spend weeks at a time engaged in commerce in Port-au-Prince. They rent houses there and make only occasional visits back to the village. Thus, we were afraid that a large number of people might never be contacted.

Inability to Give Ages and Dates

Few people in rural Haiti can give their correct ages or those of their children; and few people can make anything but the roughest guess concerning the length of time their current union has lasted, to say nothing of the dates and timing of past unions. Reasonably accurate ages are crucial for natality studies. Furthermore, our desire to construct various alliance indices made it essential that we have reasonably accurate information concerning duration of unions.

Unwillingness to Give Personal Information

It would be ingenuous of the researcher to assume that acquiescence to the request for an interview means that a villager will tell the truth once the interview has started, or that the villager's affability is an indicator of the veracity of what he says. On the contrary, the villager has very good reasons *not* to tell the truth. Secrecy is one of the few effective survival tactics the Haitian peasantry can use against the outside world. There are strong norms against revealing one's personal affairs and resources to strangers.

Furthermore, conjugal instability and consensual unions in the Caribbean create a special problem. Though these occur widely, the people involved all recognize them to be a socially "inferior" and less desirable way of mating and rearing children. The "correct" way is to get legally married in a church wedding, and to have only one spouse. This means that questions concerning marital history and status are

endowed with a sensitivity that they might not have in other settings. The respondents will tend to hide deviations from the ideal pattern or feel uncomfortable in reporting them.

In short, we had good reason to fear that true answers would be given only on trivial matters; the closer the questioning approached vital matters, the greater would be the tendency of the respondent to evade and give false information.

The Solutions Adopted

We decided from the outset to give full recognition and high priority to these problems, and to devise a research strategy that would meet them head on. Each of the tactics which we used has been used by researchers in other contexts, but to our knowledge they have never been joined into a single methodological complex. It is in their combined operation that these methods achieved unusually high accuracy and effectiveness.

Complete Coverage of a Single Community, Using External Checks and Verification Procedures Rather Than Extensive Coverage of a Broad Geographic Region

Technically satisfactory sampling is very difficult in many parts of the Third World. Survey researchers are frequently unable to locate a large portion of the subjects selected randomly, and end up interviewing whomever they happen to find. While one hopes that the interviewers will locate enough of their sample so that the results can be generalized, and that people will report accurately to them, there is little guarantee of either. Thus, the accuracy of the all-important measuring stage is very much left out of the researcher's hand.

The issue of accuracy can only be resolved by devising research strategies which *make the occurrence of errors or misstatements less likely*, and in which these errors and misstatements can be detected and rectified when they do occur, as they inevitably will. Our first step in this direction was to move away from scattered coverage of a broad geographic region. Instead, we opted for an intensive survey in a single community.

The anthropologist had been active in a Haitian village for over a year before the survey took place. From his extensive knowledge of this village and his general familiarity with the surrounding region, we had many reasons for considering this village typical of the Cul-de-Sac Plain. The principal and almost sole male livelihood was cultivating small plots and raising livestock; and the women were active in trade

— two features which the literature indicates are central features of life in rural Haiti. More specifically, the community's principal crop, in terms of acreage, was sugar cane, typical of the communities throughout most of the Cul-de-Sac Plain. Furthermore the community was practically inaccessible to ordinary motor vehicles, especially during the rainy season, and thus had the same degree of relative isolation as most communities in this region.

The houses in the community were for the most part wattle-daub cottages with thatched roofs, just like the vast majority of Plain communities. In terms of settlement patterns, the houses were organized in clustered compounds, not in linear fashion, again typical of the Plain. The lack of medical care was also typical of the region (although a small clinic had been established four months before the study began). In terms of religion, most villagers, though nominally Catholics, were practitioners of *vodoun*, the Haitian folk-religion, as are most of the inhabitants of the Plain. In short, we had every reason to believe the village was generally representative of communities in that region.

Having adopted as our research strategy the intensive survey of a single community, it was imperative that our coverage of that community be indeed complete. Thus we interviewed every male and female who had ever produced a child, or who had ever been involved in a conjugal union. In light of the late average age of first union for males in the village, we also interviewed all males over 25 who had not as yet been involved in a conjugal union, to examine the possible factors associated with late onset of first union. Two separate questionnaires were designed, one for each sex. All in all, we interviewed some 420 males and females in a community with a population of nearly 1,100.

To assure that every relevant individual would be contacted, we did a presurvey in which we obtained the names of every individual in every house in the village. Then, we went through the lists of names, house by house, with four local informants, verifying the household membership and making sure that we knew which individuals had ever had a spouse or a child. On the basis of this elicitation, we created a sublist of everybody in the village who had to be interviewed. We feel that this was the only reliable way to make sure that every relevant person in the community was reached. In short, our initial research energy was dedicated not to sampling a broad area of the region, but rather to insuring that each relevant individual within a specific population was identified and contacted.

But the objection can still be made: technically speaking the results of such a survey cannot be generalized to any broad region. But we

have already alluded to the impossibility of doing technically satisfactory random sampling in an area such as the Plain. The researcher must aim for *credibility* and *plausibility* rather than scientific certitude.

Several months before our survey was run, a group of social scientists from a U.S. university came down to do a survey in the Cul-de-Sac Plain. Following traditional procedure, they used trained urban interviewers, singled out several communities in the region, and sent out the team to visit these communities one at a time, spending one or two days in each community, interviewing whoever could be found, and moving on to the next area. Postsurvey comments heard in the following days indicated that the survey was a threatening experience for many of the peasants. Open joking and boasting was made as to who had concocted the wildest answers and who had managed to escape the interview altogether. Those who were "caught" at home by the outsiders compliantly submitted to the questioning for the most part, but defended themselves by telling lies.

Though such a survey may collect thousands of questionnaires, the fact that the "sample" consists of those who happened to be sitting at home on the survey day, and that the protocols are filled with purposeful lies, weakens the credibility of the results. Technically this sample is no more generalizable to the entire region than is a careful study of the people of one community. Although it is not possible to guarantee that the village we chose is representative of the entire Plain, we feel that a careful one-village study is more credible in the long run, because of the external accuracy checks which complete coverage permits, and because the researchers will be sure of contacting representatives of all economic, social, and religious groups in the community.

In short, we are recommending a method which entails (1) the expenditure of more resources per respondent to ensure accuracy and (2) the use of a careful cluster or community approach rather than an ambitious regional approach. The emphasis is in making sure that *all* relevant individuals from one (or more, if resources permit) well-chosen representative community are *carefully* interviewed and that the external checks which this intensive method permits are applied to ensure that the information collected in the questionnaires is reasonably accurate.

Aging Procedures: Baptismal Records, Sibling Placement, and Peer Matching

Age is probably the control variable most frequently used by researchers to analyze the distribution of other variables, especially in

natality studies. Ambiguity in age placement of individuals damages the credibility of subsequent analysis.

Demographic surveys in many Third World countries share the problem of trying to record the ages of people who do not know their exact age and are not fundamentally interested in knowing this (Brass et al. 1968; Rukanuddin 1968; CELADE 1969). The researcher has no way of knowing which answers are correct and which are mere guesses; hence he must view all questionnaire responses as to age with equal suspicion.

We will discuss here a series of methods which we used to get highly certain, externally validated ages for the entire population and in which guessing with numbers, either on our part or the respondent's, was minimized. These methods involved elicitation with informants and were time consuming. But in studies in which age will be a control variable, it is unsatisfactory to settle for ambiguous ages when exact ages can be obtained with a bit of work.

The use of baptismal records. Most parents on the Cul-de-Sac Plain baptize their children, usually within the first few months after birth. The majority of people are Catholics. Even most Protestants were baptized in the Catholic church, since they tend to be converts to Protestantism.

Although not many individuals retained their own baptismal records, the Catholic church in the nearby town had kept a copy of baptismal records since 1905. Each entry contained the child's name, the names of its parents and the community where they lived, the birth and baptism dates, and the child's status as legitimate or natural.

These records were naturally a gold mine for our survey. Since we were working with a specific community, we had only to type out each of the births listed for that community on its own slip, rearrange the slips in alphabetical order, and look for the slip which corresponded to each respondent in the survey. Because many of the people born in the community since 1905 had died or moved out, we ended up typing out many unnecessary slips; but this was a quicker procedure than searching individually for each respondent's name in the almost illegibly hand-written register.

There were a few shortcomings in the baptismal records. For reasons which we did not determine, entries up to the mid-thirties tended to list the names of all male children as "John" and all female children as "Mary." Furthermore, the widespread use of nicknames in the village means that an individual's "good name" (the name given at baptism and entered on the record) is rarely used and occasionally forgotten. It was not always easy to associate the name on the baptismal record with the name by which the villager was known.

We hired two knowledgeable villagers as informants to help us identify the names listed as parents of the baptized child among the villagers. We then arranged the slips in order of birth for all the children born to each woman. Having done this we were able to associate the names on the baptismal record with the children reported in the pregnancy history of female respondents. By means of this tedious method, we were able to locate the baptismal slips for some 640 of the 1,100 people in the community.

Sibling placement technique. Many villagers could not be found in the church records, and others were born outside the village and had come in to live on the property of their spouse's parents. Among those born in the village, many could be aged by a method which relied on the elicited birth order of the children in the village. If a person had siblings in the village who did have baptismal records, it was a simple matter of finding out the ages of the sibling that preceded and the one that followed him, and then assigning an age halfway between the two. There would not be a discrepancy of more than one or two years. This technique provided us ages for another 160 people in the community.

Peer-matching technique. Most of the remaining people were placed by a modified version of the peer-matching technique used by several researchers (Klein 1972; Hurault and Vallet 1963). Originally we had built into the questionnaire peer-matching questions by which each village-born respondent was asked to recall the names of two "friends" he used to play with as a small child who were also still in the village, and was then asked if these individuals were younger, older, or the same age as himself. The idea was that if we knew the age of the childhood peer, we would also know the respondent's age. This method slowed down the interview, was confusing to the respondents, and produced mediocre and contradictory results.

Late in the survey the idea arose of making a chronological list of all villagers whose baptismal records we had and to ask unaged respondents to place themselves with respect to the names on the list. To test the exactness of the calculations the respondents would make, we first asked several respondents whose ages we *did* know to place themselves with respect to the names on the list. The results of these preliminary tests were highly accurate; people remember very well who was older, who was younger, and who was the same age as they were when young children. People who migrated into the village as adolescents or young adults were also aged by this method. Their estimates as to whether they were older or younger than so-and-so were probably less reliable than those of village-born people, but these cases constituted a small minority of the respondents.

In summary: By rather tedious work with baptismal records and village informants we were able to age with confidence 90 percent of the 1,100 total population. The aging techniques which we have presented here are obviously most applicable in communities where most of the people were native-born and know each other. But most Third World people live in precisely that type of community. Even if a researcher does not have the benefit of baptismal records or other documents, if the exact ages of a small number of key people in the community can be found, the sibling placement and peer-matching techniques will permit the assignment of highly reliable ages to many other individuals in the population. The principal prerequisite is the willingness on the part of the investigator to work patiently with informants and perhaps to be modest in the number of respondents his study aims to encompass.

*Use of Baptismal Records
and Comparisons of Male and Female
Versions of the Same Union
as External Checks
in Accuracy of Natality and Union Data*

The baptismal records were originally copied and consulted with the sole aim of providing accurate ages. But the fact that they contained the parents' names and the child's birth date endowed them with a value we had not foreseen. Because parents baptize their children, most child-producing unions will end up recorded in the church registry. The baptismal records proved to be a *powerful external check* on the fertility and union information gathered in interview.

Furthermore, the fact that we interviewed males as well as females, and that the interviews generally took place on separate days (we interviewed most of the males first), and that only in exceptional cases were the spouses present at each other's interview, meant that in effect we had different versions of the same union. In the case of dissolved unions, if a woman said she had been together with a certain man, then we could search to see if that union was also reported in the man's interview, and vice versa. Because the community is highly endogamous, and because we thus interviewed almost every partner to every union, we had a very accurate method of verifying union histories. Omissions and discrepancies were easily detected. How this was handled will be discussed when we deal with the question of the use of village interviewers.

The decision which most deeply affected the course of the survey was to employ villagers from the research community as our interviewers. This procedure, highly unusual in survey research, has antecedents in the practice of many anthropologists who have used local assistants to help them gather data.

The idea of using village interviewers arose only after survey preparations were well underway. We had begun making plans for hiring and training interviewers from Port-au-Prince, but were contemplating with distaste the prospect of having to set up housing, eating arrangements, sanitary facilities, and bathing arrangements in an isolated, poor community where all of these commodities were in short supply.

Over and above the logistical problems, we had other misgivings about these outside interviewers. We were afraid that the villagers would refuse to speak to them, or that we would be collecting questionnaires filled with misstatements, as had occurred in other surveys. As we were testing the instrument with four village informants, we were told quite frankly that the villagers would not give *étrajè* (outsiders — anybody, foreign or Haitian, who was not born in the immediate vicinity of the community) straight answers to the questions being asked. Since none of these informants was eligible to become an interviewer himself (none could read or write) there was little reason to suspect ulterior motives in their statements.

We eventually decided to use five villagers as interviewers, trusting that their literacy would improve with practice and familiarity. Thanks to a governmental literacy campaign several years back, there were several men in the community who could read and write Creole, albeit very hesitatingly. In the preparation of the instrument, we used the Creole orthography which they had originally learned. When they were tested, they were able to make their way slowly through the questions and write down practice answers given to them.

We took great care in choosing the interviewers. Social considerations were as important as literacy. There were several young men who were semiliterate; but there is a strong tendency in the village to categorize (to *dismiss* might be a better word) as a *ti-moun*, in effect a child, anybody who has not yet entered a union and produced children. Adulthood in the village is defined as much by social as by chronological criteria. We took only persons who were adults by local standards. (We made one exception and employed one "child" in his mid-thirties as an interviewer. Questionnaires he collected indeed contained more evasive and facetious answers and we eventually had

to dismiss him from the work.) We did not have to make a decision about using female interviewers; no adult females in the village could read.

Our guiding strategy was to choose interviewers who could capitalize on preexisting bonds of kinship, exchange labor, friendship, and ritual co-parenthood to carry out interviews which would minimize respondent anxiety and the tendency to conceal.

The interviewers we finally chose fell in the middle range of a subjective economic ranking that was subsequently done on all village males. Perhaps because they were among the few literates in the community, they demonstrated an occupational and social versatility. Two of the interviewers gave part-time reading lessons to preschool children. One was a tailor. Three were active protagonists in most of the healing ceremonies, mortuary rites and sacrifices, and the public religious dances which characterize the Haitian folk-religious complex (commonly called "voodoo" by outsiders).

But despite these side activities, all of the interviewers were part of the same peasantry as everybody else in the village. They all cultivated small plots of land and their wives were involved in the commercial activities which characterize the rural Haitian female. Two interviewers were involved in polygamous unions. In their occupations and life styles they were true members of the peasant subculture. Though their prestige was greatly increased by their employment as interviewers, throughout the survey they maintained the external symbols of peasant Haiti. Some interviews were done as both parties relaxedly sipped *clairin* (Haitian cane liquor); and some interviews were done in which both parties were unselfconsciously barefooted.

The interviewers were carefully trained and the questionnaires they completed were rigorously scrutinized for omissions and inconsistencies. But we left them a great deal of leeway to cover the community as they saw fit. Nobody was assigned sectors of the community; everybody was free to find his own respondents, on the basis of his own personal networks. They also worked their own hours, adjusting their schedule to the times of day they knew they would be able to locate more respondents. They displayed a skill, far superior than available to outsiders, in locating every villager and arranging for an interview at a later date if it was not convenient at the moment. The women in the village presented a special problem. They often spend weeks at a time in Port-au-Prince doing commerce. The interviewers knew exactly when they would be back and exactly where to find them. There was not one single adult present in the village who was not at least contacted and asked for an interview.

The village interviewers became very proficient at the mechanics of interviewing and functioned beyond our expectations. Of course, extra time was required for their training. Generally speaking, they were less skillful at administering open-ended questions, but had little difficulty with the check-off type items.

The selection of interviewers has remained an issue of methodological interest over the years. Merton (1947) has raised the general problem that great discrepancies in status between observers and subjects may militate against genuine rapport and inhibit the responses. This has been found in studies of workers by middle-class interviewers (Lenski and Leggett 1960), of blacks by whites (Bindman 1965, Loewenstein and Varma 1970), and of women by men and vice versa (Cisin, Kirsch, and Newcomb 1965). After an extensive review of empirical studies, Carol Weiss concluded that respondent-interviewer similarity appears to increase the validity of interview data, especially when the inquiry topic is directly related to the specific characteristic (for example, ethnic group, religion). Interview topics with a high loading of social undesirability can be discussed more freely with same color, same sex, or same class interviewers (Weiss, Bauman, and Rogers 1971). By using the villagers as interviewers, we maximized the similarity between respondents and interviewers, and this approach proved to work very well in our study.

However, it is also argued that too close a similarity between respondent and interviewer can lead to biased responses, and that too friendly and personal a relationship may inhibit giving certain kinds of answers, socially undesirable answers in particular. By using the village interviewers did we forfeit any possible benefits that might have come from anonymity?

The argument for anonymity states in effect that there is personal information which a person will tell a stranger who comes to his door, but would hide from a neighbor or relative. There are good reasons for feeling that the whole question of the benefits of interviewer anonymity is irrelevant in rural Haiti and possibly in many parts of the rural Third World.

Discussions of anonymity and secrecy will remain fuzzy unless we make a distinction between different types of information:

1. Truly secret information which a villager will hide from his neighbors as well as inquiring outsiders — certain types of economic information fall into this category in rural Haiti.
2. Information about a villager which is common knowledge to his neighbors but which, because of its potentially delicate nature, might be concealed from inquiring outsiders — questions concern-

ing marital status and marital history fall into this category in the studied village.

3. Information which is completely nonthreatening and which will not be concealed, such as, What crops do people plant here? What year did the hurricane cause floods here?

These are the three major categories into which information will tend to fall if classified on the basis of its ease in being elicited. The argument for anonymity in village surveys, however, in effect posits a *fourth* type of information: information which a villager will hide from his neighbors but will reveal to a stranger who comes to his door. While this category is theoretically possible, researchers who have personally gone through the harrowing experience of walking up as a stranger to a village door and eliciting personal information will probably sympathize with our skepticism concerning the supposed willingness of villagers to give personal information to anonymous outsiders. Certainly the degree of evasion will vary from setting to setting; but we feel from our own experience in village interviewing in different societies that for every villager who will pour out his heart to the urban interviewer, there are many more who will feed him a line. In rural Haiti the inquiring stranger will not only fail to get *personal* information from a villager, but even in those areas of his life known to other villagers, many Haitian peasants, though they may obligingly talk to the interviewer, will concoct a version of their affairs and life history which will send any eavesdropping neighbor into stifled laughter.

Not all investigators agree with this position. Stycos (1954:10), for example, reported from his experience in Jamaica and Puerto Rico that "one factor which *may* have been in our favor was the higher class level of the interviewers; for many poor families, it was flattering to have an educated middle-class person show an interest in them." We remain unconvinced and suspect that in general the percentage of village respondents who feel "flattered" will be substantially lower than the percentage who feel threatened, or at least annoyed. Because empirical evidence on these matters is extremely scarce, each researcher must formulate guidelines based on personal experience and common sense. But even taking into full account the different cultural and political factors which could influence village receptivity to a survey in different settings, we feel that, in general, if a piece of information is so secret or threatening that a villager hides it even from his neighbors, he is unlikely to reveal it to a representative of the urban middle or upper class who suddenly appears at his front door.

In the long run, if survey researchers have used urban interviewers, practical, rather than theoretical, considerations have probably been the guiding motive. For surveys covering a broad region, it is simply more convenient to resort to a corps of highly literate and mobile urban interviewers than to search out and train semiliterate community members to do the same job. In the case of our own survey, however, it was much easier to use villagers, and we were thus motivated to reexamine and call into question the alleged benefits of anonymity. In retrospect we are convinced that the choice which was most logistically convenient to us — villager interviewer — was also the choice which turned out to have the stronger methodological justifications.

Remuneration of Each Respondent

A final step which we felt to be essential for the success of the survey was the remuneration of the respondents. We explained the connection of our survey with the newly established medical program, but we purposefully avoided giving the villagers the erroneous impression that they were in any way obliged, morally or otherwise, to allow themselves to be interviewed, and the interviewers were on several occasions clearly instructed in this matter. We were fully aware that, though the findings of the survey would optimistically benefit the region by permitting the design of more realistic public service programs, the content of the survey was largely scientific. The respondents would be doing us a favor, rather than vice versa.

Depending on the number of children or unions an individual reported, the interviews could last over an hour. It was unrealistic and unfair to expect the villagers to submit willingly and cheerfully to this type of lengthy personal questioning unless they would receive something very concrete in return. Thus, at first we proposed that each person who was interviewed should be given a *gourde* (20 cents), which by local usage is the rate villagers pay each other for two hours of work in another person's garden.

We were not surprised at the negative reactions this suggestion provoked from several Haitian professionals separately consulted. The villagers should not be given any direct payment, they argued. This would ruin the terrain for future researchers; henceforth the peasants would demand remuneration from every researcher who came to their doors. Furthermore, they said, their reward would come when the findings were used to benefit them; the peasants must be taught to cooperate with people who come to help them.

The entire issue of remunerating respondents merits a brief discussion. There are, it seems, four commonly applied tactics which survey

researchers in the Third World may use to induce people to answer their questions.

Posing. Posing (or allowing oneself to be erroneously perceived) as some sort of emissary from the government, endows the researcher with the concomitant subtle coercive power that in some societies makes it difficult for the peasant to refuse outright to cooperate. This deception can also be achieved by the skillful introduction of "the government's interest in this research," into the preinterview introductory statement.

Arousing false hopes. Another tactic is actively promulgating (or passively permitting the uncorrected promulgation of) the deceptive impression that the study is being run by people who have singled out this community or region and have concrete intentions to introduce a specific economic or social improvement in the community. Though in most cases a particular region or community has been chosen for scientific reasons, if the researcher talks about the possible benefits that might come from this research project, the respondents may get the impression that the researcher's immediate purpose is to *help* the community, rather than *study* it, as is in fact more often the case.

Eliciting the respondent's cooperation. The researcher may avoid either of the above poses by taking steps to make very clear the basic investigative nature of his interest in the community and soliciting cooperation from the respondents out of generosity, idealism, respect for science, or any other motive which the researcher feels he can call upon. In this alternative the respondent is made clearly aware that the interviewer is asking a favor of him and that he is free to refuse.

Remunerating the respondent. This is basically the same as the previous tactic but offers to remunerate in one fashion or another those respondents who do cooperate.

There may be other strategies that researchers have used, but these four, or some combination of them, probably occur with greatest frequency. The first alternative would be quite unethical in most surveys. The fact that a researcher might have discussed his study with government officials and secured official permission, in no way changes the voluntary nature of respondent cooperation. Since the villager might not know this, any researcher who mentions the government without at the same time mentioning clearly and explicitly the lack of obligation to be interviewed, is in effect practicing deceit. The second alternative must be rejected with equal vehemence. To create, by emphasizing the future benefits of the research project, an impression by which villagers are subtly led to believe that their community has been singled out for a program of governmental or foreign aid, of

which the survey is a necessary first step, is an unscrupulous misrepresentation whose fundamental purpose is to trick Third World villagers into unremunerated cooperation with research.

In our survey, we clarified to ourselves, to the interviewers, and to the villagers that cooperation in the survey was a favor to us. In choosing, then, between the third and fourth alternatives listed above — asking for free help or giving something in return — we chose the latter. Stereotypes of the “friendly peasant” notwithstanding, villagers of the Cul-de-Sac Plain are highly (and justifiably) unreceptive to the inquiring approaches of outsiders. They are furthermore firm believers in the principle that few things in life are free. By offering to remunerate respondents, we simultaneously re-emphasized the voluntary nature of their cooperation, increased the chances that they would in fact cooperate, and created in effect a “contract” whereby we could justifiably send the interviewer back in case we discovered omissions or false statements.

In order to lessen the objection of those who had opposed our giving monetary rewards, we asked the village interviewers their opinion of certain alternative compensations to give the respondents. They suggested that the next best thing to money would be a *ti-komisyô*, a small gift packet containing a pound of rice and a pound of beans (together worth about 30 cents), which would be sent as a thank-you gift to each villager who allowed an interview. Food-gifts are a common means of expressing solidarity and gratitude in rural Haiti, and in this community rice and beans were luxury foods during much of the year.

We accepted this suggestion from the interviewers, as well as their suggestion as to the manner in which the gift packet should be delivered. It proved to be a genuinely appreciated and sought-after gift, and induced many people to be interviewed who otherwise might have refused.

Over and above the gift packet, we were very fortunate in the offer of Dr. Ary Bordes, of the Centre d'Hygiene Familiale in Port-au-Prince, to send a vaccination team to vaccinate everybody in the village who so desired, as general thanks for cooperation in the research. Although people in other parts of the Plain had been vaccinated, the inaccessibility of this particular village had meant that no vaccination team had ever gone there. Thus, many adults and children were vaccinated for the first time.

We were by no means the first researchers to remunerate a survey population. Stycos (1954) paid his Jamaican respondents 75 cents each in cash or food for a six-hour interview. There, the respondents often asked bluntly, “What do I get out of it? Other people have been

around here asking questions and nothing ever happens.” In their study of birth planning attitudes and practices in Pittsburgh, Spillane and Ryser (1972) paid each respondent 20 dollars for coming to a designated place to be interviewed. They claimed that the 56 percent successful response rate would not have been achieved without offering to pay the subjects for their participation.

Let us examine the principal objections to remuneration.

Such remuneration will destroy the terrain for future researchers. This is true only in a limited sense. Generous compensation for time spent will incline villagers to receive future researchers, if not with open arms, at least with a generally positive reaction. The objection really means that villagers will no longer let researchers study them for free. But even this is not certain. If the investigator is truly the vanguard of an action project whose benefits will be direct and immediate to the researched community, he will probably get cooperation. If he is not part of such a specific, imminent action project, then he has no right, in our opinion, to expect such unremunerated cooperation in the first place. The remuneration has to be adjusted in light of what the respondent would get for a similar amount of time spent in other activities in his socioeconomic setting. Thus some surveys will cost more per respondent than others. In our case, the cost of the remunerations amounted to less than 5 percent of the entire survey budget. But this should be anticipated as a fixed cost when funding is solicited, every bit as indispensable as interviewer salaries.

Remuneration commercializes and thus cheapens social research. Not so; it merely helps remove one of its most exploitative aspects. The humanitarian motives which so easily bubble forth in introductory explanations and justifications to the respondents tend to obscure the equally strong academic and career interests of the social scientist. Though the investigator may believe that his research activities will help improve the situation of the people studied, the prime beneficiary of most research projects is probably the researcher himself.

Respondents in some societies would resist payment for “talking” as being offensively commercial. It is true that in some settings people would find it very strange that they should be paid for talking. But reciprocity is the norm in most cultural settings. Perhaps money or food-gifts will be inappropriate in some settings. The task is to discover the culture-specific and setting-specific rules for reciprocating. The survey researcher is in a situation where compensation would be especially appropriate; he usually remains a total stranger to his respondents. In any society, the stranger who has received a favor, especially from people much poorer than himself, is most ungrateful if he departs without making some concrete gesture of thanks.

Summary and Discussion

With the goal of making the interview situation less threatening and the information more accurate, we devised a research strategy in which we: (1) studied the population of a single community intensively; (2) used birth records and elicitation with informants to obtain age data; (3) used these same baptismal records as well as comparisons of the male and female reports of the same union to check and improve the accuracy of the data; (4) used residents of the village itself as interviewers; and (5) compensated each of the respondents. We must now discuss how this methodological complex in fact worked, and whether it succeeded in reaching the basic goals we originally had in designing it.

The Interview Atmosphere

A principal goal of ours was to reduce intimidation and subtle coercion, and to create an atmosphere where the respondents were not participating reluctantly and where the interview itself was more relaxed. It would be naive to think that we were completely successful in every case; but there is good reason to believe that we did succeed in the survey as a whole. Though not everybody looked forward to being interviewed, very many people did, perhaps principally because of the remuneration.

In the first few days we had a terrifyingly high refusal rate. One discouraged interviewer made the glum prediction that only "40 out of every 100" villagers would agree to be interviewed. The only consolation was that we had succeeded in creating an atmosphere where people did not feel coerced into talking; most people were refusing. Rumors had spread about "obscene questions" and the interviewers were discouraged.

But when the word spread about the compensation, and the basically innocuous content of the questionnaire became known, the atmosphere changed. Refusals ceased and most initial refusers informed the interviewers that they had changed their minds. In not a few cases, people who had not yet been interviewed asked the interviewers when it would be their turn. And we even had a few people from neighboring communities who asked if they could be interviewed.

If the remuneration secured cooperation, it was the presence of village interviewers that provided a relaxed atmosphere during the interview itself. Each interviewer ended up surveying the people in the section of the community where he lived, proceeding along his own personal kinship and friendship network. Despite introductory

explanations, the respondents on the whole certainly could not have understood the purpose of the questioning; but the fact that they were going to be compensated created a rationale for their participation, and the fact that a relative or friend was interviewing them made the whole curious thing quite nonthreatening. In a survey in which some 420 people were interviewed, we ended up with only a half-dozen permanent refusals.

Were the Answers Accurate?

There is no question about the higher than usual accuracy of the ages, made possible principally by using the baptismal records. Myer's index (U.N. 1955), which measures the extent of digit preference, was calculated for the ages reported by the respondents on the one hand, and those derived through baptismal records, sibling placement, and peer-matching techniques on the other. Magnitude of departure from zero indicates the extent of digit preference. Myer's indices for the reported ages and the derived ages were reduced from 25.1 to 9.5 and from 31.8 to 16.3 for males and females respectively, giving strong evidence that the derived ages are of higher quality.

What about those errors which enter a survey by virtue of false answers? The effectiveness of the strategies we adopted resulted from their *combined operation* in the following fashion.

Right from the outset we demonstrated the seriousness of our intentions with regard to accuracy by sending back interviewers if a questionnaire had omissions, internal inconsistencies, or childbearing and union information which contradicted the information we had on baptismal records or on questionnaires of other villagers. We made it clear that sooner or later we would catch any serious omissions or misstatements in these areas. Early in the survey we had to dismiss the youngest interviewer because of his repeated failure to turn in an error-free questionnaire. The significance of these measures was less in terms of the specific errors we "caught," than the general nonsense atmosphere we were able to instill about the whole survey. On some items we tolerated suspicious answers — for example, people claimed to go to church much more frequently than they actually did. But on other items, especially the childbearing and union history, we were quite inflexible in our demands for exactitude. Though we ended up singling out about a third of the cases for rechecking with the interviewers, the information on the questionnaires was overwhelmingly consistent with the information in the baptismal records and became even more so as we demonstrated to the interviewers our ability to detect errors.

Thus we had in effect a structured system of external checks which made false statement easier to detect and hence less likely to occur. The village interviewer would generally know if the respondent gave false answers; and we in turn would generally know when the interviewer turned in a questionnaire filled with inaccuracies.

With respect to this question, we can mention in passing that informal comparisons were made between data gathered in our survey and those collected in a previous census in the community by Murray without using external checks.

The following deviations from truth tended to occur in the first survey:

1. Former unions might be concealed
2. Current extra-residential unions might be concealed
3. Children by a former spouse who are living elsewhere might not be recorded
4. Children by a former spouse who are living with the respondent might be falsely attributed to the current spouse

In other words, some respondents tended to distort their histories to an outsider to give the impression that he or she has departed as little as possible from the cultural "ideal." We feel that our use of village interviewers and other external checks was instrumental in minimizing the occurrence of this understandable tendency.

There is one final issue that must be clarified. Granted that the use of village interviewers and baptismal records is a powerful tool, is it not also a dangerous tool? Are we not in effect making village interviewers privy to private information about their fellow villagers? No, we are not. The baptismal records can detect only events that have been public. If a woman has a child whom she baptizes in the nearby church, everybody in the village knows about it at the time. If the father of the child recognized the child, that is also public knowledge. It is "secret" only in the sense that it might be denied to a stranger if the union has dissolved. In using these checks, we were not giving the interviewers access to any information which they or other individuals in the community did not also possess. The use of checks merely discouraged the interviewers from being accomplices in the "conspiracy of silence" which the village might understandably enter into against outsiders. The door to door survey is a poor tool for getting truly private information; but with modifications it can be used to elicit more public sorts of information.

As for truly private information — for example, How many cows do you have? — the person who has strategically deployed his 15 cows around the Plain to conceal his wealth will understate their number to

the village interviewer as surely as he will to the stranger. But whereas the stranger will get "two cows" for an answer, the village interviewer, who knows the general economic position of the respondent is more likely to be told nine or 10, which is still false, but a lot closer to the truth.

Conclusion

In this chapter we have discussed some major problems of social research in rural areas of the Third World. We have criticized a "haloed" version of social science, in which the studied community is given the impression that the research is for their own benefit, and that the researchers should therefore be assisted. Besides the ethical question, we have discussed our approach to some practical aspects of field work. Our design was based on the assumption that unless preventive and corrective measures were taken, serious errors might enter our questionnaires, some of them unintentional (such as poor estimates of ages), but others based upon a subtle, unspoken agreement among the villagers to keep information from outsiders.

The effect of such errors is far more serious if the researcher is unwilling to recognize them. Many survey investigators have devised ways of dealing with these difficult issues, such as constructing *internal* consistency checks. The research strategy we have presented here, which relied on *external* checks, worked very well for us in a Haitian village. Other researchers who decide to undertake a survey may find certain parts of it applicable to their own situation as well.

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