

RESEARCH METHODOLOGY IN VILLAGE STUDIES*

by

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“Let every man be his own methodologist;
let every man be his own theorist;
let theory and method again become part of
the practice of a craft; stand for the primacy
of the individual scholar; stand opposed
to the ascendancy of research teams of technicians.
Be one mind that is on its own confronting the
problems of man and society.”

C. Wright Mills ; The Sociological Imagination

The need for village research in Asian countries such as ours deserves no particular emphasis. Without understanding the village and the villager we cannot understand the country or its people. In the study of such villages, particularly in the Asian region (or sometimes more broadly in any country of the under-developed world) there is an urgent need to look at the present methodologies adopted in research: Are these methodologies comprehensive, strong, appropriate and effective enough to elicit the information needed particularly in relation to the solution of social problems and development? Are the peculiar research problems we are faced with adequately covered by an effective methodology to understand them, suggest solutions as well as to render assistance to implement these solutions translating them into action?

My experiences in the village has enabled me to identify two main problems vis-a-vis methodology in village research : (1) The necessity to modify the methodologies already employed by us and (2) the urgency to invent new “tools” to deal with situations which cannot be met effectively by the use of older methodologies, even with considerable modifications.

In discussing solutions to these problems it is essential that we view the development of methodology of village studies in its correct historical perspective.

During the British times some interested British officers inspired by either the formal training in anthropology, which was considered as a useful subject of study for British officers in order to understand the natives, or on informal desultory reading on the subject, took a keen interest in collecting information pertaining to various aspects of village life. Of these officers almost all did collect information through the help and assistance of the

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Government officials employed under them. Although most of them did have a smattering of the language and culture of the people, this knowledge and understanding was not quite adequate to "feel" the life of the village in its varied perspectives.

The method of collecting information was thus based on the official hierarchical system. The high British official would exercise his authority, summon his subordinates and ask them to collect a particular piece of information desired by him. The official authority and the hierarchical system shaped the collection of the data. The 'field workers' were the lowest rank of the hierarchy and along some point in the hierarchical system there were the 'interpreters' who in their role were both interpreters of language as well as that of culture. They did not confine themselves to the translation of the information in linguistic terms but also meddled with its texture by attempting to explain the culture behind it to the British official who probably did not understand it from that point of view.

The Sri Lankan administrators of that¹ time, some of whom except perhaps a few such as Ananda Coomaraswamy² followed this 'official-hierarchical interpretative' role. Even when they went to the village this hierarchical role kept them away from the villagers. In the case of British officials, however much they have desired, this role, plus their complexion, perforce kept the people away from them. In the case of indigenous officials, the majority of them kept themselves away from the people, because in imitating the British colonial officers they had developed a superior attitude towards their countrymen. They felt that they were an elite clan, often educated in English public schools, also in virtue of their elite family status and more than all, revelling proudly in the luxurious sub-culture of officialdom which they as a ruling clan, speaking English, inherited and enjoyed.

Even after independence (1948) this elite group continued the hierarchic "interpretative" role, which in its turn dictated the methods used by them in village studies. The influence of this role is seen in the present hierarchical system of research teams employed in village studies. Those at the top being the supervisors and those at the base, the "field-workers or research *podiyans*". The distance between these two groups is such that except for a cursory official visit, very often a supervisor does not come into contact with the village. Even if he goes the motivation for the field-workers springs from such an interpretative position. The field-workers are "officials" i.e. Government officers or those relying on such officials attempting to obtain information.

The hierarchical distance is present even in the way research data is processed. The 'field-workers' mechanically collect the data in the field. They are interpreted, very often by research officers or supervisors. Then

another group would write the report. The supervisor is supposed to coordinate the entire work. The 'touch' of the grass-root level where research data is collected is never felt by those above the field-workers at any stage other than by accident. Very often, if the supervisor visits the village, it would be in the aggressive official style of the British administrators.

The interpreter role still exists today in village research. It functions effectively at two different levels. The foreigner who wishes to do research in our villages engages an Interpreter to translate what other people say. Some astute research workers avoid the pitfalls of employing an interpreter by learning at least the rudiments of the language of the people so that the interpreter cannot at all times mislead him. They also ensure the escape from cultural misinterpretation by learning about the culture beforehand. In addition to an interpreter, they may hire a few persons with specialized knowledge in order to test, clarify and compare the translations of the interpreter.

Notwithstanding all these cautions, it is possible for an interpreter to mislead a person not properly exposed to the language and culture of the people in all its possible dimensions. This could happen to a foreigner as well as to an indigenous scholar. The interpreter could be the "contact man", "the informant", "opinion leader" or any one with whom you may establish a link to engage in research in the village. Your reliance on him or the reliance of the field-worker on him means that one is being subjected to a different kind of interpretation. The interpreter who is serving you (for example, let us say as the opinion-leader), when asked, would try to provide you with "short-hand" information. Instead of explaining in full about an incident or person he would shorten it according to his caprice.

The "official" status expresses itself to the villager when you try to contact him through the Grama Sevaka who sometimes is considered a leader in the village, etc. When you, as a stranger, introduced by a friend with an official letter goes there with your file, questionnaire, interview or schedule, etc. you are an "official" for all purposes irrespective of your excellent introduction. You cannot escape that. The response to you from the village is the typical response you get from a villager to a typical official. This typical response always prompts the villager to present the data in "short-hand".

The manual of training which you and I give to our field-workers makes them learn all what is in the manual. This 'copy-book' training can give birth to a set of field-workers who act almost in a mechanistic way. They present the questionnaire or interview people almost in the same way, asking the same question bereft of any originality or inventiveness as if they are embarrassed by the people they meet. In the 'manual' we never make them

aware as to how they could get away from this hierarchic attitude. We cannot do so, because most of us subsist on it. If the hierarchic structure is lost, we would not be able to contact the villager. The entire field of research, international and national, is based on this hierarchic structure which I believe is the bane of village studies today.

I wonder whether at least in village studies we could do away with these different categories of a research hierarchy. Each one, in such a research team, sans a hierarchy, should have contact with the grass-roots level on his own. Without that experience one cannot analyse and theorise on it. If you remain at the top awaiting what your field-workers bring to you, when you analyse these facts (as cold facts) you are doing the other end of the work that an interpreter mentioned by us, usually does. You are interpreting data like a "computer", data which have a feeling in them because they deal with the human beings in the village. At least in the case of a computer, the data is fed into it in a way that a computer understands. But you as a supervisor and with your "long feet" you may have touched the village at some level but without that real grass-root experience or feeling you can only misinterpret the data collected.

The primary idea is not only to collect statistical data which by themselves would have no meaning without the qualitative elements in them. The collection of data is possible by the use of appropriate techniques which can be implemented under conditions that the people chosen as respondents are agreeable to this task. In order to make this happen the rapport between the investigator and the people is essential.

Rapport does not mean getting an official, friendly introduction to someone in the village and thereby treating this introduction as a licence or permit to ask any type of question from him. What right have you got to invade people's privacy and expect them to answer your questions spending their precious time for you? In a village where a survey with questionnaires and interviews was done, we asked the interviewers their reaction to it. Eighty three percent of them said they did not like to be so questioned, although out of courtesy they tolerated it. We cannot have people like guinea pigs and treat them as if they were in laboratory conditions. Unless the people are happy with us, happy to answer our questions, there cannot be any rapport between them and the research workers.

Rapport in research involves the creation of a hospitable environment in the field so that our research subjects do spend some time with us and answer our questions willingly or join us in conversation. This requires a wealth of fore-knowledge about the village, its people, its culture and life. We must study the people beforehand, know the best possible approach

to their individual and community life and also how to make them happy in conversation with us. How could we do this if we as research workers are not aware of their village life and without an understanding of its varied aspects?

Rapport is not only establishing a link with the villager and the village, it is also the happy frame of mind that could motivate him and the villager to co-operate and also to maintain relations with each other afterwards. It is sad to observe how some of us neglect the villager and the village as soon as our interviews or researches are over. We forget that the rapport established by us is an open rapport that should continue on behalf of future research workers coming to the village. This is the reason why when a research is finished in a Sri Lankan village, it is often difficult for a new research to be started there. The "rapport line" has been broken at the end of the previous research and it is now very difficult to re-establish it again.

One word pertaining to ethical conduct vis-a-vis research should be mentioned here. When we engage in village studies, if the people are to know that we are there to help them in a problem, to assist them in translating into action what we would find out in research, certainly the right rapport could be established. By helping them to translate into action, at least part of our suggestions at the end of the research or the making of the findings available to them in some way, would ease us of an ethical responsibility and add a further strength to the rapport already established. As I said earlier our ethical responsibility is double-edged. On one side we owe it to the village and on the other side to our own researchers of the future who may come into this village for purposes of research.

By virtue of selection and by the stereotype training we give, the majority of our research workers fall into the category of official interpreters or mechanical collectors of data. I strongly feel from my subjective experience that a real researcher is indeed born. The training component is evidently essential but, having observed five or six generations of my own students who were given the identical training I could say that only a very few were really competent research workers. Individuals with the identical educational background, exposed to the same training may come out as different types of research workers while in the field. Now I judge a research worker only from his activity or performance in the field. If I have the opportunity, I would select him, given the minimum educational background, from the performance in the field. How close he could get to the people, the extent of the sympathy he has towards them, his pleasantness of manner and dedication as well as enthusiasm, the inordinate curiosity to know 'more' would be the criteria that I would look in a potential researcher for the practical field of research.

I am really unhappy that most of us engaged in village research forget about these essential qualities of research workers. Today I think twice before I assign a post-graduate student to field-work. The question that I would ask him is "what could your research give to the people? It would give you your M.A. or Ph.D. But how about the people?". Then they have nothing to give to the people, but are only motivated in exploiting the village and earning a degree, is it correct methodology to support them? Your narrow attitude naturally circumscribes your approach to the people and your rapport established is thus a deformed one.

Are the instruments presently in vogue such as the questionnaire, interview, observation including participant observation, etc., effective for collecting data at the village level? With regard to all these instruments there is one thing in common : That is those who administer it do it in such a manner, that by their deportment, language, etc., they use and the style in the use of the instrument, they all create a certain antipathy towards research workers coming to the village with their tools. The questionnaire, the file, the young man and the woman going from house to house, the stereotyped questions asked, identical type of mannerisms and response inherited often by living in campus or in the city, the dress, all in their total effect contribute to the fact that this is not a happy experience to the villager. Perhaps it is no surprise that in a random-stratified sample of such research workers, when asked whether they enjoy this experience over 71 per cent replied "no". This as well as our observations show that the researchers—the field-workers—do not enjoy their encounters with the people. They do it just to go on "with their job".

If the attitude is so, how could you expect them to motivate the people, make them happy, share their experience, be happy themselves, and thus ensure the collection of correct data? This is a question of research method that needs our attention in the present time. However precise and effective our instruments are, if the human being we select to operate them is not successfully attuned to the task at hand, could we be happy about the tools we use and imagine that the instruments would be successfully employed?

I have experienced a number of researches where the questionnaire or the interview schedule was drafted elsewhere and sent here to be used. This type of common instruments were considered necessary in international research in order to standardise data collection. But such instruments or machines need "tropicalising", in view of the distinctive nature of our culture and society. Moreover in translating them into Sinhala or Tamil the spirit of the Sinhala or Tamil culture and the language should be correctly felt.

In order to do this, whatever the objectives of the research, it is necessary to consider the instruments intended for use locally, most preferably in relation to the actual setting itself, the village.

I have asked many times from my colleagues whether pre-tests, pilot-studies actually are relevant in Sri Lankan village studies. We devise a questionnaire, take it to an area where we expect to meet a population similar to what we have in mind, administer it and assess its effectivity. In Sri Lanka, on many instances, I have seen my colleagues administering it to their friends. Whether they do it with their friends or in a village closer to the intended research village, I believe they pay very little attention to the person who administers it, how he does it, who will be there with him when he does it, the actual details of the scenario of administering it, its effects on others in the village, the aftermath following the administering of the questionnaire.

We studied a village frequented by two or three teams of researchers. Our idea was to find out exactly the effect of the administering of the questionnaire. The technique adopted was to select youngsters from the village with a sound educational background and train them to record incidents of research workers administering such a questionnaire. After being exposed to such researches people appear that have developed an almost uniform way of reacting to them. The other findings, in short, are as follows.!

- (a) The 'scenario' of the administering of the questionnaire interview depended very much on the rapport that the researcher could establish with the villagers.
- (b) The reaction differed by the personal appearance and mannerisms as well as the enthusiasm of the research worker, the location where the administering of these questionnaires etc. took place, who were the dramatis personae present in the scenario, what exactly happened in the village just before and after the "visit," the information passed on verbally from one individual to the other on the research and the researcher.
- (c) The effectiveness of the links established by the researcher : (1) How did he come to the village ? (2) Whom did he thereafter meet ? (3) What was the process that led him to "my" house ? (4) Where did he go from there ?
- (d) The rapport initially established with some one in the village would echo and re-echo as he goes from house to house, each time bringing out a community-echo to the extent he had been able to meet the people, and this community echo would be determined by the total

response he has received in the community. His success in motivating people would depend on this 'community-echo'—how positive or responsive it is.

In the case of village studies, rather than formulating a questionnaire and taking it to the village, I have often attempted to design the questionnaire in the village itself. If the objectives of the research are clear, why not put them to the villagers and ask them about the questions which they think are relevant and important? If participation of the people is considered as something desirable in research, why not involve the people in such participatory work at the initial stage of the research i.e. the time when research instruments are devised? If people could be induced to join, that is another way of motivating them in the research.

The usual accusation against the above strategy is that people will come to know of the questions or research objectives beforehand and this would accordingly condition their responses. This is indeed absurd because even after formulating questionnaires outside, you have to carry them to the village. Once you asked questions from one man, the nature of the questions would be spread from mouth to mouth. If this is a serious objection, one could very well select another village similar to the one in hand, and get the villagers involved in preparing the instruments.

Participatory research is often spoken of as a kind of research in which people are induced to participate together with the researchers. The most important factor here is that in order to make or induce the people to participate, the researcher should participate in the activities of the village people first. This participation of the researcher, if done for the purpose of securing information, would be "seen-through" by the people. Even if it is not "seen-through" then, at the end of the research, when the research worker finishes and goes away the people would eventually come to know why he had been there. The feeling 'he cheated us' then would colour people's attitudes to subsequent research rendering it difficult to establish a rapport with the people thereafter.

In the village the best method of research is to work and live with the people.⁴ There is no substitute for this in mastering the alphabet of village society or learning the grammar of the local society and culture. The concept of a researcher coming to the village, living in the nearby rest-house or hotel or in the house of a well-to-do villager is now no longer tenable. The role of a researcher going on a fact-finding academic cruise in the village, with his papers and the pencil, is now a common sight. But this has to disappear

soon. The researcher is not a different man doing a different job. His job is to understand the people. There is no better way to understand the people other than by working and living with them.

The grammar of the village life and grammar of village culture has to be understood only by living and working with the village people. In such work your commitment to the research and the village is emphasised. Your understanding of them in work would bring you closer to them. You would learn about them in varying dimensions which you could never reach through question and answer sessions even in well-arranged interviews.

It is for this purpose that I devised what I call "Living-in-Experience," a method whereby you totally immerse yourself subjectively in the village or community life as one of them. You de-educate yourself in the process to re-educate yourself along the lines of a villager. When you see the village from their point of view and experience it thus, it is easier for you to understand their problem. Now you may compare it from your theoretical point of view and even use the instrument such as questionnaires, interviews, etc., more profitably and more precisely in the light of the expert knowledge gained through "Living-in-Experience."

I have explained the essentials of this method elsewhere.⁵ But let me summarise its chief characteristics so that the reader could grasp the essentials quickly :—

1. Unlike in the participant observation method the researcher here gradually orientates himself to the position of one of the members of the group he is researching on.
2. Dissociating himself gradually from his own theoretical orientation, parting from his education he becomes re-educated from the people's point of view.
3. While in the group no notes are taken down openly. Experiencing their life from all possible angles would be the main task of the researcher.
4. A fairly long stay with the group would be necessary but this also could be met by selecting trainees living there for a long time and teaching them the research work. There are three main ways how one could do this :—
 - (a) The researcher himself could gradually get interested in the life of the people during a comparatively long period of time or different spells of time each not so long.

- (b) He may use individuals who are living in the group from their birth and having trained them make them living-in-researchers.
 - (c) He may introduce others having carefully planned their entry into the group. These "outsiders" are introduced as people in the village and not as strangers. The 'link' that establishes them in the village is thus of great importance.
5. Having completed the "living-in-experience" the researcher is able to use the tools such as questionnaires, interviews and even observation in a more meaningful and an efficient way. Let me illustrate this too. In a village, a questionnaire and the interview technique was used to ascertain the villagers' pattern of eating. The data when collected and interpreted showed that the people are eating more than one nutritious meal a day. This was quite contradictory to the number of children and adults found who showed obvious signs of malnutrition. We desired to test the efficacy of the tools used.

The technique to be adopted was the "Living-in-experience." We selected carefully a number of research workers, about 5, who had very close relations with the village. Having studied the families who are related to them, we found out suitable points of entry to each family to make the "landing" in the village as soft and unobtrusive as possible. Those selected by us fortunately were individuals who had in the recent past been into the village to "see" their relatives from time to time. In a family there was an alms giving and that was a grand opportunity for the researcher—the relative—to come into the family in order to help them in the alms giving. In the case of another family a member of the family fell ill, the researcher—the relative came to see the sick man and made a welcome stay. In the case of the other families too such points of entry—the harvest time, the occasion of a wedding, the need to take a child to school—were chosen.

The entries thus made, the researchers were able to prolong their stay by virtue of the initial necessity. "Grand-father is ill. I will stay a few days to fetch him medicine." Such a request was welcome to the family and very acceptable to every one in the village. When they stayed thus they were not 'guests'; they were members of the household where they lived. If they were guests by virtue of being guests their "line of visibility" would have been limited. The inmates would eat something and serve the guest something else. But as it was there was no awkward visibility and as such everything was visible to the researcher from a very close perspective.

The data thus collected from a sample of families was quite in contrast to what we earlier got through questionnaires, interviews and even through observation. The earlier researchers on certain occasions even became participant observers but as participant observers their line of visibility was limited. You saw only what they decided for you to see and nothing more.

In order to check what we have obtained as data we devised a means known as "participant action." In the village school where we had won the support of the Headmaster we arranged a contest. Three classes were selected and in most of these classes one or more members of substantial village families were found. The contest was to write down what each person ate each day for breakfast, lunch and dinner and to guess what the person nearest to you in the class could have eaten. The ruse worked well. Some boys having discussed the question among themselves told each other what they have eaten and listed them accordingly. What the elder in a higher class listed, we compared with what the younger brother or sister in a lower class wrote. This was a good check to test the veracity of the statements made. The results of this were compared with the sample-study we made with "living-in-experience." Not surprisingly they tallied with each other closely.

In formulating a questionnaire, conducting an interview or observing an event, those instruments could be made more effective if utilised side by side with Living-in-experience. My research workers are advised to live in the village for some time even though they may not utilise this technique of Living-in-experience in research. They are expected to first devise ways of "landing" in the village softly and then explore the possibilities of working with the people. There is no substitute to working with people to gain experiences of their life. It is only by engaging in such work, they are asked to administer the questionnaire or open their interviews. Very carefully during this working period they are ingeniously made to test the questionnaire, interspersing a question from the questionnaire now and then among lot of other talk to see how the response to the question is. This is a better way than the usual "pre-test" with its attendant sombre rituals. We call this "work-tests" where at work, quite unknown to the people the instruments (questionnaires, interview methods) are tested, bit by bit carefully.

The instruments of data collection, if they are not properly utilised, will not help us to get the accurate data we need for our scientific analysis. A sociologist born and bred in the culture would sometimes come to erroneous conclusions because of the fact that he utilised these instruments incorrectly. The following illustration where a native sociologist defines a characteristic of Sri Lankan culture is an interesting example. Here the sociologist investigated a common religious characteristic of the Buddhists, i.e. the basic ethics

of Buddhism, the Five Principles of abstaining from killing, stealing, indulging in wrongful sexual misconduct, telling lies and taking intoxicants. These Five Principles known as "Sil" forms the basic ethics of Buddhist life. But the sociologist looks at it from "borrowed eyes and ears", i.e. through the eyes and ears of those who had observed and attempted to define it earlier, most probably foreigners who never understood it properly. He thus calls Sil "a partial ascetic withdrawal from the world, lasting usually for a day." In Buddhism there is no emphasis on ascetic practices or ascetism. The scholar himself has not sensitised his own observation to understand this characteristic correctly. But he had attempted to notice how others had seen it and perhaps wrongly conceptualised it. This is indeed one of the greatest pitfalls on observation and description which the researcher had to be cautious.

A scholar when he does not exercise his faculty of observation correctly sometimes tends to gloss over what he sees. I call this "sociological blindness." In such a gloss over when he attempts to theorise,⁷ particularly trying to find solutions to problems, then certainly the entire exercise becomes ludicrous. The following example shows how even an indigenous scholar could indulge in "sociological blindness." "Begging is a legitimate occupation in some cultures. Since giving alms to the poor (and in Buddhist countries to monks) brings merit to the giver in a future life the presence of beggars is perpetuated by the cultural mores. The maimed, those with festering sores, are common on pavements and public places, outside temples, etc. and the greater the appearance of physical deformity, diseases and penury the greater the merit to the donor. It is only in emergency situations that attempts can be made to end begging."⁸

The sociologist is not correctly sensitised to what he observes. He lacks the deep understanding of the culture that would have enabled him to develop the proper sensitivity towards what he observes. The implied comparison of giving alms to the Buddhist monks with that of giving alms to the poor is one point where his shallow understanding of the culture emerges. It is further shown when he says that the merit that comes to the giver of alms increases according to "The greatness in the appearance of the physical deformity." Here he errs in an unpardonable manner. In Buddhism there is no theory or teaching which says that merit increases according to the size of the physical deformity. The quality of merit depends on CETANA or volition and not on other factors such as the smallness or greatness of deformity. The "interpreter" characteristic of scholars during the British times spoken of earlier in this paper comes to our mind here. In those times the British administrators interested in research had an "interpreter" to

explain what he observed. In scholars who are not sensitised properly to our culture, the 'interpreter' in them motivates them make such erroneous conclusions.

This becomes very serious when such a scholar attempts to suggest remedies for social problems. In the above example in suggesting a solution to the social problem of beggars, the sociologist says that end of begging could be attempted only in emergency situations. In his note he elaborates this saying thus "Thousands were arrested in Bombay in a drive to end begging but this was possible only under emergency conditions. Beggars and cripples were moved out of Colombo just before the Non-Aligned Conference, August 1966."

He has never observed or even attempted to utilise a research tool and understand the problem of the beggars. But he offers an ideal solution. The solution is to take the beggars away (arrest them) under emergency conditions. What happened to the beggars after that? Where they were taken to? Has he observed it or come to know about it through other research sources? Is the beggar problem solved, at least to a certain extent after these mass arrests, herding the beggars off by force during the period of emergency regulations? For him, it appears that one of the ideal solutions for social problems is the Caprice of the rulers at work during the period of emergency regulations that lead to the mass arrest of beggars. It shows us, how in looking at our own society, our own sociologists, born and bred in Sri Lanka, forget the precise tools and the sensitization necessary to gather information and interpret the facts so gathered in a scientific way. Such examples are warnings to all of us, showing us the pitfalls of our own methodologies into which we ourselves might fall, if we are not wary of such pitfalls.

Notes and References

1. Nandasena Ratnapala, *Sinhalese Folklore, Folk Religion and Folklife*, Colombo 1980 introduction.
2. Ananda Cumaraswamy is the well-known Sri Lankan born writer, Geologist, Art-Critic and Philosopher best known by his work "Medieval Sinhalese Art."
3. The Findings would be published by the Sarvodaya Research Institute, Ratmalana.
4. The method was first used in Sri Lanka by me in my 'The Beggar in Sri Lanka'; Colombo 1979. In the second edition (to be published shortly), I have included a detailed analysis of this method used later by me also in several other studies.
5. Ibid.
6. Susantha Goonetilleke : "Civilization Flows—Social Development and Change : 'Case of South Asia'" Paper submitted at a Seminar at the Institute of Social Studies, The Hague, May 18-22, 1981.
7. Ralph Peiris, *Asian Development Style*, 4, 5.
8. Ibid. 114.