

S U P P L E M E N T

PROBLEMS AND HAZARDS OF FIELD WORK

The Case of the Filipina Researcher

with an

INTRODUCTORY NOTE

by

THE ISSUE EDITOR

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INTRODUCTORY NOTE

One of the most exciting phases of social science research is fieldwork. But the excitement has its own counterpart in frustration and sometimes discouragement. This, however, has not been written on in formal essays, much more so if the fieldwork were undertaken by the fairer sex. The famous women social scientists particularly the anthropologists, like Margaret Mead, Cora Dubois and Ruth Benedict do not give us any insight into the problems and hazards of their fieldwork in their respective research areas. We can draw these insights only from reading their works.

In the Philippines, the Filipina social science scholars, like Mary Holnsteiner and Mary Gonzales of Ateneo, Celia Tagumpay-Castillo and Gloria D. Feliciano * of U.P. do not also provide us with accounts of these problems and hazards as they encountered them in their field work, at least in articulated essays.

The present collection of three essays attempts, in very modest terms, to present the problems and hazards of field work as the Filipina researcher encounters these in the process of data collection. The novelty of these essays lies perhaps in its being the first of its kind in Philippine social science research. Except for an essay on a semi-urban area, at least these essays cut across the cross-section of Philippine society, viz., rural-agricultural and sub-urban, industrial-commercial. These researchers are graduate students in the Asian Center—two now in the last stages of their theses, and one has already passed the oral defense of her thesis.

“Research in a Cockpit” by Marialita M. Tamanio brings to focus a very important problem of data gathering for her work, for as she discussed the approaches she used she was faced with the extreme value judgments of the community, which endangered her rapport with the people. “Research in a Pampanga Barrio” by Realidad Q. Santico also presents relatively similar problems and hazards of her field work but in a different setting—that is, the community at large. Hence her problems and the hazards she was faced with were broader in magnitude and more intense in involvement.

* Gloria D. Feliciano, however, has written a very important paper which touches upon “The Limits of Western Social Research Methods in Rural Philippines: The Need for Innovation,” (*Lipunan*, Vol. I, No. I, 1965, pp. 114-127), which more or less implies some problems of research in the field.

The cynicism of a relatively urbanized community like Santolan, Pasig, Rizal seems to be the major problem that Carolyn C. Israel encountered in her field research on "Kinship and Socialization in a Suburban Community." These and many others, however, are presented in the essays with relative lucidity.

To present in broad outlines, at least within the purview of the researchers' field work, some guideposts in approaching the possible problems and hazards of a female researcher is the reason these essays were written. They may in one way or another generate essays of the same nature as to give prospective female researchers pre-field work insight into what to expect as they themselves embark on similar ventures. If these essays in the collection shall have given impetus to further ventures then they shall have justified their being presented here.

Issue Editor

RESEARCH IN A COCKPIT

MARIALITA M. TAMANIO

"CLASS DISTINCTION SEEMS TO BE ERASED IN THE SABONG" IS A VIEW expressed by one of my professors in one of our encounters in Asian Studies courses; a view which challenged me to conduct a more intensive investigation on the *sabong* among the Tagalog. This included fieldwork for twelve months. It was divided into three phases: first, four consecutive Saturdays of interviews with my uncles—who are cockfight *aficionados*¹ in the province, the next days being Sundays were spent in the cockpit as a participant-observer; second, four consecutive weekends with similar routine as in the first phase, but in three other different localities; and third, the succeeding six months with residence of five days within one week in the research areas.

In the first stage my primary intention in conducting fieldwork was to collect data for two term papers in the Asian Studies 292, Research Topics in Asian Studies, because the first term paper which I wrote for this course received a failing mark. In the initial fieldwork I collected some data which were not utilized in my previous papers. This motivated me to conduct further investigation in connection with another course, Anthropology 284, Seminar on Culture Change, with Dr. Willis E. Sibley. Thus, in the second phase, I worked on the comparative study of the functions of cockfighting among the Tagalogs living in the cities and in the towns. After the first two phases, my interest in cockfighting intensified. I decided then to extend the study on cockfighting as a socio-political element in the Tagalog community.

Conducting field studies on a subject such as this, I anticipated that the expansion of the limited knowledge of it, the methodology and its validity, the choice of the research areas, and the financial resources available, would be the major problems. My information on cockfighting was primarily based on feature articles occasionally published in local weekly magazines, and from conversations with uncles while I was still in the province. In the course of my research I gained a wider perspective and a deeper insight on the subject-matter, thereby amplifying the prior limited knowledge I had in the beginning of my research.

Apart from having insufficient knowledge on the subject, my only background on theories and methods in research was a reading of the works of

¹ Cockfight *aficionados* in this essay will refer to those men who go to the cockpit regularly during Sundays and holidays, but they do not necessarily bet every-time.

Basu,² Feliciano,³ and Mead.⁴ Depending on this, and working on the assumption that there is social stratification/structure in the cockpit founded on mere impressions gleaned from published articles, I drew up a set of structured interview questions. The questionnaires included the choice of fighting cocks, their care, how and when the informant began to be involved in cockfighting as well as its consequences on the informant himself, the importance of cockfighting in the community and finally the community elites who frequent the cockpit.

Initially I tested the questionnaire when I interviewed my uncles and cousins who live in the province, but later on I had to revise the questions according to the nature of the involvement of the informants in the game, i.e. whether he is an "asensista",⁵ "sentenciador",⁶ "llamador",⁷ "soltador",⁸ "tahor",⁹ "kristo",¹⁰ etc. There were also questions which were inhibitive and could not be asked and answered in the cockpit since it is a public arena. For example—

"Ano ang 'ginagawa'¹¹ ninyo sa alangang araw?"
 What do you do during weekdays? or
 What is your occupation?
 "Nagiging simula baga ng paminsan-minsang away ninyo [ng inyong asawa] ang pagsasabong?"
 Does your going to the cockpit/cockfighting cause family quarrels once in a while?
 "Nanalo/Natalo baga kayo? Magkano?¹²
 Did you win/lose? How much?

The same questionnaires underwent several revisions, corresponding to the different phases of the research. Modifications, however, were still made during the actual interviews depending upon the interests of the informants.

² Minendra Nath Basu, *Field Methods in Anthropology and Other Social Sciences*. Calcutta: Bookland, Ltd., 1961.

³ Gloria D. Feliciano, Limits of Western Research Methods in Rural Philippines: The Need for Innovation. *Lipunan* Vol. 1, No. 1 (1965), pp. 114-127.

⁴ Margaret Mead, Report of the Committee on Ethics. *Human Organization*, Vol. 8, No. 2, (1949), pp. 20-21.

⁵ The *asensista* is the owner of the cockpit. If he himself could not manage the cockfights and he employed a 'manager', the latter assumes the same title.

⁶ The *sentenciador* is the referee or the judge of the game.

⁷ The *llamador* is the equalizer of the basic bets or *guarden* or *warden*. He is also called *taga-tawag*, *taga-kasá ng pusta*, *cazador* or *pagador*.

⁸ The *soltador* is the '*taga-bitiw*' or '*taga-butaw*' which literally meant 'the one who throws' or 'the one who lets go' the cocks inside the *rueda*.

⁹ The *tahor* is a person who bets a large amount of money for a cock in a game quite regularly.

¹⁰ The *kristo* is a person who bets for somebody else. He is responsible in the collection and payment of the bets he made for that somebody. Just in case he made a mistake in counting the amount of their bet and the cock they betted on won, the *kristo* must pay the exact amount agreed upon (from his pocket).

¹¹ 'Ginagawa' is a euphemism in these areas for one's occupation.

¹² All translations from Tagalog to English in the subsequent references to the local language are free renderings.

In the course of the research, original attempts to utilize the tape-recorder, camera, even note-taking, were dismissed. The use of these was possible only in the interviews with my relative-informants. Apparently, the utilization of such instruments destroys the fluidity of communication. Moreover, the informants refused to give information especially on the political affairs and economic status of other informants in the community. The difficulty lies in the inaccuracy of my memory of everything I gathered during the interviews. Thus, there was the necessity of going back to them and of asking the same questions.

During the third phase, I finally elicited answers to the questionnaires. The informants, however, insisted that they should remain anonymous especially where the information should be held in confidence. Hence, the use of fictitious place-names and identities of informants in this essay. To complete the data, and to gain further first-hand perspective, I still felt the need to sit in the cockpit myself, and subsequently to reside in the communities. Hence, the insufficient information I gathered from direct interviews was augmented by residence in the particular areas. Furthermore, it has provided me with a deeper insight into the roles of those directly involved in the game in their respective communities.

The work was conducted in the Tagalog areas for the following reasons: the accessibility of facilities—like transportation, housing and ‘contact persons’, and my knowledge of the language of the area of research. These factors are indeed very important taking into consideration the limited time and funds available. From my hometown the two cities¹³ are only an hour’s drive and the two towns¹⁴ an hour and fifteen minutes away.

Under the town category, are *Niing*, a more than ninety year old municipality in the north central part of Batangas Province, and *Maculot*, a ten year old municipality in the southwestern part of Quezon Province. [There is a wide gap between their founding dates as municipalities.] On the other hand, the research areas under the city category are *Ilaya City* and *Balintawak City*. These two cities are the metropolis in the respective provinces where they are located. They were formerly the capital towns of the provinces of Batangas and Quezon. Having colleges and vocational schools within their territorial jurisdictions, both offer educational opportunities in their respective locations at lower costs. They, too, are the recreational centers of the areas.

There are cockpits which are more accessible from the University of the Philippines campus. However, the composition of the *aficionados* in these areas is relatively heterogenous. This factor alone would require more expenditure, timewise and moneywise. In the chosen research areas I have

¹³ The term *cities* is based primarily on the political units of the Philippine political system.

¹⁴ *Ibid.*

a fore-knowledge of a number of families that may help me identify other families whose members are *aficionados* of the game.

A problem arose when I decided to reside in the communities during the period of field research. The elder brothers of my parents insisted that I commute to and from the research areas for fear that I might get into "dangerous situations". There have been a number of rape cases in Maculot. Thus, a compromise was arrived at with my relatives. Instead of residing in Maculot, I resided in the adjacent community, Ilaya City, which is also one of the research areas. Two-thirds of the third phase interviews were conducted in the evening, due to the fact that my informants work at daytime.

Financial assistance was another major concern from the first stage. About ₱40.00 per weekend was the budgetary requirement for the first two stages. Fortunately, the Asian Center appropriated a Thesis Aid for its students among whom I was a recipient.

Prior to the work, I prepared a schedule (of data gathering) so that my term papers could be submitted on time. However, I did not anticipate that to keep pace with my timetable would be a major problem, as well as the cause of other minor ones. To keep me within the limits of my schedule was a problem encountered while establishing rapport with the informants at the same time getting as much information as possible from them.

This could be explained by the fact that this field of research is a recent development in the Philippines, and as yet, unfamiliar to many, at least from the scientific point of view. This requires a longer time to make the value of research comprehensible to the informants. This did not exclude my relatives. Between my relatives and me the research introduced another level of relationship, which did not exist before. The problem was indeed difficult. My particular activity, especially my presence in the cockpit, significantly affected their own status in the community. For instance, the eldest brother of my mother upon knowing I would enter the cockpit was very resentful, and said:

"Nakahihiya kung magbabad ka sa sabungan, ka babae..."

It is shameful if you will stay in the cockpit, you a girl..."

It was only after a great deal of persuasion that my relatives and the other informants were convinced that it was necessary for me to go to the cockpit and not *depend* on magazine articles. These attitudes I believe can be traced to the very nature of the topic I was dealing on. Apparently, cockfighting is "no woman's business" in Philippine society.

Indeed, there are women in the cockpit, but they are operators of the *carinderias*,¹⁵ vendors of household goods or women porters employed by the *asensistas*. It was only in Balintawak City that I had occasion to see

¹⁵ *Carinderias* are small stores selling lunch and snacks.

women who sat in the *palko*.¹⁶ They were elderly women most of whom were wearing the *sayas*,¹⁷ and gray-haired too. These did not exceed twenty in number.

Among my relatives I chose *Kakang*¹⁸ Imoy and *Kuya*¹⁹ Temyong to serve as my key informants, because they were regarded as "professional sabungeros"²⁰ in the neighbourhood.

On the first Sunday of my fieldwork, Kuya Temyong accompanied me to the cockpit in Balintawak City. He often spent his Sundays in Balintawak, for cockfighting season in our town does not begin until after harvest-time in November through early June. I gathered that the operation of most cockpits in towns are suspended during the rainy season, because the greater portion of the *aficionados* are farmers.

Kuya Temyong left me with the *asensista* after introducing me to him (the latter). This gave me familiarity with the actual cockpit situation, hence the subsequent work in other areas are conducted without his aid. The *asensistas*, therefore, became my initial 'contact persons' in three other areas.

The *asensista* in Balintawak is the son of the owner, and a college student, which made it easier for me to explain my purpose in going to their cockpit. While I talked with him, middle-aged men around us made such comments as:

"Boy, bago ba 'yan?"
 Boy, is [she] new?
 "Magkano?" with a winking of one's eye.
 How much?
 "Baka naman taga-riles 'yan?"
 [She] might be from the railroad?

I knew that in Balintawak the row of 'red houses' is located along the railroad. I was irritated. Were it not necessary for me to finish the term papers for my courses, I would have "blown my top!!!" The *asensista* must have noticed that I was about to lose my temper, when he said in an authoritative tone that I am his friend from a university in Manila, that as long as I was there he would be responsible for my safety. He continued that should I encounter difficult situations in the cockpit, such would be tantamount to offending him. These statements from the young *asensista* silenced them but they still looked at me with suspicious eyes.

¹⁶ The *palko* is the 'general admission' area of the cockpit-building.

¹⁷ The *sayas* are the traditional floor-length skirts of Filipinas which are for everyday use.

¹⁸ *Kaka* is a respect address to the elder brothers, sisters, and even to the third degree cousins of one's parents.

¹⁹ *Kuya* is also a respect address, but to the elder brothers and cousins.

²⁰ "Professional sabungeros" refer to the men of the community who go to the cockpit regularly on Sundays and holidays, except maybe when they are sick, and usually stay in the cockpit the whole day. They bet regularly.

From this incident, I learnt that the *asensista*, inspite of his age, is an authority figure inside the cockpit. Even those who are older has to reckon with him. It may be added that in the Tagalog communities the safety of a stranger is attributed to his relationship with an authority figure.

During the third phase of the research I was informed that the *aficionados* misinterpreted my presence in the cockpit, and that disguised as a student, I was a woman of ill-repute looking for prospective customers. They do not know what research means; they do not know me personally, and logically all these fit into the fact that there are 'red houses' in the areas where the research is being conducted.

In connection with this, in a conversation with some of my new friends, I became aware of another situation—that is, a proposition to become someone's concubine. They considered me lucky for the nature of my research is very conducive to such a situation. The proposition they said comes from the political elite of the communities. Apparently, this was due to the stereotyped "easy-to-get" image of girls residing in Manila. How I escaped from such an experience was a guessing game. Probably it was because I greeted my informants in Tagalog with a handshake, which latter gesture is considered as an index of a highly educated person.

As I resided longer in the research areas my informants developed more ambivalent impressions towards me. In the first two stages I did not immediately tell them I came from the University of the Philippines, taking into account the "radical image" of U.P. students to those outside the campus. Whether or not there is truth in this "image," it did not matter, for to be identified with "radicals" might restrain them from freely associating, and cooperating with me. When they learnt that I came from U.P., there were *aficionados* who approached me and introduced their friends as "kristos", "llamadors", etc., whom I could interview anytime if I wanted to. It seemed that those who were introduced to me did not only pretend to be such, since I always saw them performing the functions of what they professed to be. I noticed my informants became relatively more responsive after knowing where I am studying.

I was anxious when the informants refused to give me any definite amount of their approximate income from cockfighting; neither did the *asensista*. Frequently, they said that what they get from cockfighting was "pang-inom lamang".²¹ I realized later that the informants might not really know exactly how much they earn in one year because they are either farmers or daily wage-earners. According to them they feared I might be a Bureau of Internal Revenue agent, investigating probable taxable amusements of their community. I confused them further when I began inquiring about

²¹ "Pang-inom lamang". Literally, meaning 'for drinks only'. In these Tagalog areas one of the favorite pastimes is drinking *lambanog* or fermented wine made from coconut sap.

the legal cockfighting days and how it should operate. They thought I might be a Philippine Constabulary agent trying to investigate unlawful business in their localities.

These impressions were disclosed to me only during the third phase when finally the rapport with my informants was established. At this period I was considered "hindi na iba."²² This kind of relationship was strengthened when a number of families requested me to be the godmother of one of their children. The problem now was how to come up to their expectations as a ritual sponsor, for this involved some monetary considerations. Being a graduate student, another unfamiliar status to them, they expected a "better gift" from me.

Another difficulty was eliciting reliable information during the intervals of the *soltadas*,²³ owing to the presence of onlookers and the very short period of the intervals. Added to this was the particular time of research. The field study coincided with the beginning of the cockfighting season—that was October. The number of *soltadas* steadily increased each weekend. I did not want to annoy them unnecessarily. The spectacle inside the *rueda*²⁴ was more interesting to watch than to talk 'nonsense' with a stranger-researcher.

Domestic harmony and humility as social desiderata in Tagalog society were factors which hampered the accuracy of data being gathered. The *aficionados* avoided answering questions on domestic problems arising from their involvement in cockfighting. For example, the case of Mang Pedro from Maculot. When asked if his "devotion" to his fighting cocks caused family misunderstanding once in a while, emphasizing *once in a while*, his immediate response was a vehement *no*. I did not pursue the subject because he left for the *rueda*. Barely twenty minutes after, I followed him when he brought his wounded winning cock to the "cock-doctor".²⁵

Mang Pedro unconsciously started telling his "cock-doctor"-friend that he was very hesitant to bring the winning cock to the fight. Early that morning he found the cock hanging from the *batalan*.²⁶ It made him very mad at his wife who was not careful in driving away their dog, which frightened the cock. Apparently it was a bad omen. Mang Pedro added, he did not want to stay home after a "civil war" and the only place where he could cool-off is the cockpit.

²² "Hindi na iba" is a colloquial which means that somebody is accepted by the community even without any consanguinal or ritual relationship existing between them.

²³ The *soltada* is the game of one pair of cocks.

²⁴ The *rueda* is the platform where the *soltada* is held.

²⁵ The *cock-doctor* is the one who cures the wounded cocks inside the cockpit. His paraphernalia are merthiolate, tincture of iodine, penicillin ointment, gauze, scissors, cotton, shaving blade, needle and thread. All of these are placed in a small wooden box. His service fee ranges from ₱0.50 to a few pesos depending on the seriousness of the cock's wound and his prestige as a 'good' doctor.

²⁶ *Batalan* is a roofless appendage of the kitchen in Philippine barrio houses.

"Kung pupunta naman ako sa sabungan at mag-mimiron lamang ay wala ring kuwenta."

If I will go to the cockpit to be an onlooker only, it is not so nice.

While we were on our way home I asked Mang Pedro again why he responded negatively before. He explained that there were other people around. He said:

"Ang batang ito, ibabandera ko bang away naming mag-asawa sa mga usyoso doon?"

My child, will I go around telling those kibitzers about our quarrel?

My intention in following Mang Pedro to the "cock-doctor" was to see what the latter was doing. It turned out to be an unexpected check on Mang Pedro, giving me an idea of another way of checking on my interviewees. The situation presented above showed me the disparity between the "ideal" and the "real" in relation to cockfighting, as perceived by my informants.

The onlookers were a nuisance to me in the initial stage, but then I realized they (the onlookers) also did unconsciously help me, with their unsolicited comments. For instance, I was asking one informant on his wife's attitude towards his being involved in cockfighting. Mostly the answer was that his wife does not mind anymore, because she (the wife) is already accustomed to it, but the onlookers would make comments like:

"'Oy hindi ah, narinig kong 'outside de kulambo' ²⁷
ka noong isang araw."

Hey, I heard you were not allowed to sleep
with your wife the other day.

"Pare, magsabi ka na ng totoo. Tayo-tayo lamang
naman eh."

Friend, tell us the truth.

In order to find out who was telling the 'truth' I had to go to their homes and talk with them further. During the third phase of the fieldwork when I met the wives of the *aficionados*-informants I was amazed to find out how vocal their wives were about their domestic problems.

After the social amenities on meeting the informants and their families, I would start asking them how they became cockfight *aficionados*. When the topic turned to the ways wherein the cockpit or cockfighting was exploited for political gains and their opinions, I put away my notes to affect an air of "conversation". After two weeks I went back to "visit" them and had another "conversation" to countercheck on the prior statements. This was how I conducted the third phase of my study.

²⁷ "Outside de kulambo" is a colloquial which means the husband would have to sleep somewhere else but not beside his wife.

Being a transient in the community most of the young people in the neighborhood, both male and female, came to my boarding house. It was mostly in the form of *harana*.²⁸ Although they came every night and stayed until midnight I appreciated their coming because the topic of our conversation was mostly on cockfighting. They were in fact another set of informants. Indeed, in my fieldwork it was not informants and information I was in need of but to keep with the pace within my schedule while still trying to establish rapport with them.

Problems and hazards obstructing one's fieldwork, especially when least expected, indeed cause discouragement, but as the discipline of economics would put it, "in the business cycle there is the period of depression, recovery and abundance." The barriers encountered in fact are profitable lessons which I could not have learnt elsewhere. I realized that a student will not fully understand the difficulties facing a researcher unless he himself would go out into the field and test the methodological theories learnt in the classrooms and from the books. Now I understand better the "field-workers' complaints". What are being taught formally in schools are only guidelines, unlike the algebraic expression, wherein $X \times Y = XY$.

²⁸ *Harana* or serenade.

RESEARCH IN A PAMPANGA VILLAGE

REALIDAD Q. SANTICO

IT HAS ALWAYS BEEN ASSERTED THAT "FIELDWORK IS INTERESTING AS any other social activity, both as a matter of sociability and satisfying intellectual curiosity."¹ While there is truth in such a statement, it must not be construed however that fieldwork is all fun. There is much more to it—as a matter of fact, there are a number of unexpected obstacles and barriers which one cannot learn through formal classroom instruction but which one encounters only as he goes out into the field to conduct his own field-research.

It is, therefore, within this perspective that such an article has been conceived, with the end in view of presenting interesting facts about the problems and hazards of field research. It is also the purpose of this brief article to provide students of anthropology with insight as to what to expect when conducting their own field researches.

With F. Landa Jocano's advice and motivation, I conducted my field research in Pampanga, with Kalayaan,² a typical farming village in the municipality of Santa Ana,³ as the area of study for my M.A. thesis in the Asian Center. There were a number of reasons why Pampanga was chosen. First, its accessibility to Manila gave me the opportunity to consult with my thesis adviser on weekends or from time to time whenever need and problem arise since commuting to and from Pampanga is less than two hours by bus. Second, the fact that most of the materials written about the Capampangans are either historical in nature, survey reports or impressionistic articles, this study therefore, will be the first attempt at a descriptive analysis of the structure, function, and value orientation of the Capampangan family and social organization.

The method employed in this research is largely anthropological in nature and this includes: residence in the community under study, participation (without subjective involvement in local problems) in the day-to-day activities of the people under observation, interviews, socio-economic surveys, gathering case studies which illustrate the general principle of social

¹ Buford J. Junker, *Fieldwork: An Introduction to the Social Sciences*, Chicago: University of Chicago Press, 1960, p. 12.

² Fictitious name of the barrio. This has to be done to preserve the integrity of the person concerned.

³ Fictitious name of the municipality where this study has been undertaken.

relations, and library research for the historical documentation of historical events in the community.

To facilitate such community study, I lived in Kalayaan with a number of families for a period of eleven months, from July, 1968 to June, 1969, thereby affording me the chance to gain insight into the child-rearing practices of barrio mothers, their beliefs, and the interactions between members of the family.

On the 15th of July, 1968, I paid the Mayor a courtesy call and informed him of my plans to do fieldwork in Kalayaan. I had to do this because I felt that out of propriety, the Mayor should at least be informed of my objective in doing research work within his municipality. This was done because I knew that the support coming from the town officials could in a way expedite my research work in the barrio. The Mayor himself showed interest and concern over the prospect of putting Kalayaan and Santa Ana in the limelight. I was given all the assistance and cooperation by the Mayor himself and his staff. I did not realize that such a move would prove hazardous and detrimental to my research project. The Mayor then introduced me to the Barrio Captain who happened to be in his office on that very morning. The Mayor explained to him the plan and the research work and the Barrio Captain accepted willingly the task of extending all possible assistance as soon as I was ready to conduct the census survey.

The fact, however, that I proceeded directly to the Mayor's office before informing the Barrio Captain caused much confusion at the start of my fieldwork. The Barrio Captain showed much indifference and resentment to the research project when he outrightly defied the Mayor's request to accompany and introduce me personally to his people. He relegated his duty to Tata Caryo, one of his councilmen, who later on became my chief informant. For a time, the Barrio Captain and I were not on speaking terms. One day, while my informants and I were conducting our census survey, he ordered a municipal policeman, who happened to be with him at that time, to summon us by blowing his whistle. I did not pay attention to them and I advised my informants to do the same thing. He blew his whistle thrice and when he failed to attract our attention, they stopped and approached the house where I was conducting my interview. They created all kinds of tricks to attract our attention. And in the weeks to come, he did everything within his capacity as barrio official to make me desist from doing fieldwork. And when the Mayor came to see me and inquire about the progress of my work, I told him the whole truth. It did not occur to me that it would lead to further complications and more conflicts. He took immediate action by conducting an investigation of his own. The policeman was to be dismissed but I mediated and told him that it was too harsh a decision. The policeman was just warned and advised to be more courteous to strangers. And in the case of the Barrio Captain, the Mayor acted as the

middleman in order to right whatever wrong had been committed by both parties concerned. From that time on, my identity with the Mayor started to gain the attention of the people. Talks started to circulate around that I was favored by the Mayor during the confrontation meeting between the Barrio Captain and me. But these talks did not affect me much at the beginning for the effect came very much later.

Since I do not speak the Capampangan dialect fluently but can understand it, I had to ask the assistance of an interpreter, Anding, who also became my guide and bodyguard throughout the period of my fieldwork. I attempted to study the dialect in my desire to understand better the interactions among the barrio people and also to record the data without the assistance of an interpreter. My zeal and enthusiasm were hampered by the lack of cooperation on the part of my relatives to extend the necessary help in terms of guiding me as to proper stress and intonation. Everytime I attempted to talk in the dialect, my relatives and other informants giggled and criticized my intonation. This discouraged me from pursuing the plan to speak the dialect fluently before leaving the field. Nevertheless, I did not find language a real barrier to the spontaneity of my informants since most of the barrio people speak and understand Tagalog. It was only with the very old respondents that I had difficulties in communicating because they do not understand any single Tagalog word. In cases like this, I sought the help of an interpreter.

I was called, and known to the small children and my other informants as a Tagalog since I conducted my interviews in Tagalog or English instead of the Capampangan dialect.

The first few weeks of my fieldwork were devoted to establishing rapport with the barrio people by joining them in their conversations and accepting invitations to baptisms, weddings, picnics, and birthday parties. I did not encounter much difficulty in gaining their confidence because the barrio people were cooperative. They showed willingness to share pieces of information which they considered relevant to my study. However, there were at least three respondents who displayed resentments to my interviews and observations. One mistook me for a Bureau of Internal Revenue agent and refused to commit himself by withholding minute details that I asked about his family. Only after assuring him that I was not in any way connected with any government agency, and that the data I would gather in the barrio would be used for my thesis at the University of the Philippines, did he oblige to respond to some of the questions asked.

Two middle-aged female respondents on the other hand, questioned my presence in the barrio during the first few weeks of my stay. When I tried to converse with them to show my desire to make friends with them, they always found excuses to evade me. I learnt later from some reliable sources that these women used the socio-economic position of an individual

as the yardstick for friendship. This came to my knowledge accidentally when one day, I decided to bring my car to the field as I brought with me a few things I needed in the field. It took the people by surprise—they could not believe that I own a car because they always see me clad in simple dresses and slippers as I conducted my interviews. People from that time on regarded me differently and then these particular women approached me the following morning and invited me to have lunch with them. I declined the invitation politely but could not refuse to have merienda with them. I took this opportunity to interview the family but it went the other way around. They started asking questions about my economic status in Manila, my family, my educational background and other pertinent data about my life. From that time on, things went on smoothly for me and my informants.

To know the genealogy of the family, I conducted a house-to-house census of the barrio. This gave me the opportunity to meet and know the barrio people personally. I started from one end and worked gradually to the other. As I went from house to house, I explained to the people the objectives of my study. I told them that without their help, I would not be able to finish my thesis. It took me one whole month to accomplish this, in spite of the help of Tata Caryo, my guide-informant. After this census, I picked out at random the families I intended to use for intensive interviews. Sometimes, I took down notes while interviewing an informant. At other times, I made recapitulations of the interviews when I reached home. This was done because some informants were sensitive to note-taking.

I did not use standard questionnaires but I had well in mind the problems to be discussed. My interviews were conducted at the informants' homes from 8:00 in the mornings until 5:00 in the afternoons. To accomplish this, I stayed with a different family each day in order to observe variations and similarities of their day-to-day activities. Despite the fact that the social nature of the interviews enabled the informants to feel at ease and talk with increasing freedom about their personal lives, there were some who were still indifferent to interviews and observations. For instance, last August 15, 1968, I went to visit a housewife in the far end of the barrio at 8:00 in the morning. She was attending to her hogs and when I greeted her, she did not bother to stand, she just glanced at me and continued with her chores. I stood near her for about five minutes, with the thought that she was just finishing her work. After ten minutes had passed, she told me frankly to leave the place as she was very busy and told me to come back in the afternoon. I took my leave and I promised her I would come back in the afternoon. I felt very much embarrassed, depressed, and decided instead to stay at home and cancel all my appointments for the whole afternoon. I never expected that fieldwork could be deplorable at times.

But after that incident, I mustered myself and came up with the thought that I could not accomplish anything by indulging in depressive thoughts or

by spoiling the whole project with what my colleagues (classmates) call a minor occurrence—it is part of the game.

I continued with the interviews which were always informal and unstructured. I visited the families unannounced so I would be able to observe them in their most unguarded moments. There were times however, when I could not help but accept invitations to drop by on them. At one time I accepted the invitation of a family to visit and interview them because they claimed they were left out or discriminated against. To oblige them, Anding, my guide and I accepted the invitation one Saturday morning. The family prepared for the interview, the house was very clean, the children had their baths, they were instructed to behave properly as there were visitors expected for the day, and the housewife prepared a sumptuous meal. This was the very reason why I objected so much to structured interviews, the people do not act naturally and the real activities of the family are veneered by their desire to project only the best in them. Thus, after the first interview, I always made unannounced social visits. I found out that their behavior during the first interview was not generally consistent with their behavior during the second interview. Doing this enabled me to check and recheck my data.

One of the most important hazards of fieldwork which I encountered during my stay in the barrio was to be identified with an authority—the town Mayor. Since he showed concern and attention to the difficulties I met as a researcher, he was always around to extend his personal assistance and cooperation to the success of my research work. He made all the necessary arrangements for possible meetings and interviews with the elite group in the barrio and he accompanied me personally during these interviews. Because we were seen together most of the time, people began to identify me with him and vice versa. The barrio people dissented in their interpretations of the Mayor's interest in the project. Some of those who knew him long enough belied the accusations while those "dirty-minded" people, particularly the women, made sweeping conclusions which proved to be morally damaging, disappointing, and disgusting for both of us.

I was not aware of all these malicious talks until one day, August 15, 1968, to be exact, the Mayor's driver dropped by and told me that the Mayor was inviting me out for lunch because it was the birthday of one of the municipal employees. I accepted his invitation with the idea that it might prove beneficial to my research work—I had always my project in mind. And when I boarded the Mayor's jeep, I noticed the womenfolk by the windows, staring at us but I did not give any importance to it because I knew that they do it to everybody.

The people made speculations and conjectures when the Mayor himself brought me home alone. My guardians in the barrio confronted me and informed me about their custom. They told me to refuse the invitation of

the Mayor to go out because people started to put color to our relationship. My line of thinking did not run parallel to their thinking because I knew that I was not guilty of their accusations. I respected his office as well as his honesty and sincerity. But my guardians insisted that in the barrio, people consider it immoral for girls to go out with married men, and to violate this injunction in their custom is a direct defiance of the community norm. I decided to pull out from the field for at least one month and started organizing my data at the University while I prepared for my language examinations. I did not explain to anybody the tight situation where I was in, except to my adviser, F. Landa Jocano. In fact, it was he who suggested my leaving the field for at least a month to put an end to such gossip.

On September 30, 1968, I went back to the field with the thought that my absence had normalized conditions in the barrio. I arrived at 6:00 in the evening. I learnt from one of the people I met on the way to my foster parent's home, that the Mayor had been waiting for me since 3:00 in the afternoon but left just five minutes before I arrived. The following morning, the Mayor came and inquired why I left the barrio hurriedly without informing his office. I told him the whole truth and he decided to call on the older members of the community to a meeting to explain his concern for me and my research project. But I knew that the meeting and the explanation did not in any way alter the credulity of the people. The situation was further aggravated by the fact that my return to the barrio coincided with his (Mayor) yearly leave of absence and I was told that he usually took his vacation leave between October and January every year. But people mistook this for something else. Thus, the Mayor had plenty of time to spare and assist me with my research project. I learnt to take the people's talk and the gossip in stride and became "calloused" to their criticisms. Fieldwork taught me to accept the fact that the problem of learning to be a good researcher is like the problem of learning to live in a society.

KINSHIP AND SOCIALIZATION IN A SUBURBAN COMMUNITY

CAROLYN C. ISRAEL

THIS PAPER ATTEMPTS TO PRESENT A BRIEF PERSONALIZED NARRATION of the methodologies and techniques of field research employed in a suburban community—Barrio Santolan, Pasig, Rizal. It includes the various problems encountered from the first time the research team set foot in the community until the final day of withdrawal. In so doing, it is hoped that this paper may provide some hints or guidelines for future anthropological field researches particularly those which may be conducted in semi-urban communities.

This project was financed by the Social Science Research Council of the University of the Philippines, and jointly conducted by Dean Eva Gonzales of the College of Home Economics and Dr. Felipe Landa Jocano of the Department of Anthropology. Unlike many previous projects of the University of the Philippines, this was an inter-disciplinary research wherein graduate students of various academic fields like Anthropology, Asian Studies and Nutrition studied the community simultaneously. This is significant in the sense that the research area and its ramifications were perceived and analyzed with the use of varied approaches and from different view points.

The anthropological aspect of the research dealt on the kinship and family structure and patterns of child-rearing. It formally started in July, 1968 with Dr. F. Landa Jocano as Senior Researcher, and Melissa de Lara and the writer as research assistants who at the same time are graduate students in Nutrition and Anthropology respectively. Later, Lerma de Lima and Luz Sevidal, also graduate students in Anthropology and Asian Studies joined the team. Occasionally, Home Economics students participated in data gathering both for training and for meeting some term paper requirements.

APPROACH AND PREPARATION FOR FIELD WORK

The initial research activity was an ocular survey of several barrios of Pasig, initiated to determine which area was most suited for the projects. Of the barrios visited, Santolan was chosen on the basis of its relative proximity to the University of the Philippines, its population density and other demographic features, as well as the apparent willingness of the barrio officials to extend their help and cooperation to us. The nearness of the barrio to

the said school was indeed a very important determining factor since we were taking up some graduate units and had to attend classes once or twice a week. As soon as the research area was chosen, the next activity we did was to gather as many written materials available about the barrio. The College of Home Economics had conducted earlier a demographic and socio-economic survey. This material together with those taken from the files of the provincial and municipal offices of Rizal and Pasig respectively, Presidential Arm on Community Development, Philippine Rural Reconstruction Movement, Rural Health Unit Clinic and Commission on Elections lessened the difficulty of approaching the community, a problem which is usually encountered by field workers and students of "primitive" societies. In other words, these materials furnished us with introductory information about the community, *i.e.* land area and topography, climactic condition, size and nature of population, means of economic livelihood, and health and sanitation. From the reports of the community development workers we were more or less briefed on the attitudes and reactions of the barrio folks towards strangers in general. In like manner we got some hints on how to deal with them to enable us to gain their confidence and cooperation. In addition, monographs on similar studies in the Philippines as well as articles and books relevant to the project were compiled and carefully studied. During the entire period of the project we had regular conferences with the Senior Researcher for purposes of evaluation, with particular emphasis on the methods and techniques we were using in the field.

The focal point of interest was the kinship institution and socialization practices in the barrio. To get, however, a holistic perception of the barrio life, information on the other aspects of the culture like economy, politics, religious and magical beliefs and practices, folk medicine, etc. were equally gathered. Such initial knowledge indeed provided significant insights on how to actually proceed with the project; for instance, where to position or locate the researchers in the area in order to obtain the maximum opportunity for observing community behaviors and activities, what techniques of research are best suited to the situation, and what aspects of the people's life are least sensitive for probing. Later on, it likewise proved that a prior knowledge of the varied aspects of the culture helped immensely in analyzing and understanding better the kinship and family systems. It was only after these information were obtained and studied carefully that we went into a depth study of the subject of interest.

INITIAL CONTACT

The first activities my fellow-researchers and I did during the initial visits to the barrio was to be acquainted with the political leaders composed of the barrio captain and councilmen, the rich and influential barrio families as well as personnel of agencies who were conducting studies and

welfare-projects in the community. We also scouted for Santolan residents who were then enrolled at the University of the Philippines to act as our initial guides and informants. Since the College of Home Economics had conducted an earlier survey, we requested one of the workers to orient us in the locality and to introduce us to the above-mentioned persons. Thus, the initial trips to Santolan were solely characterized by walking about the barrio noting down the locations of the houses, chapel, schools, clinic, stores, etc. With all the impressions thus far gathered, we then planned on how to divide the community among ourselves. I stayed in a sitio where the majority of the people belong to the lower economic stratum while my fellow-researcher located herself in another sitio where resided most of the affluent—the old families of the barrio.

Meanwhile, we undertook the task of establishing rapport with the people. The Rural Health Unit clinic which had a favorable reputation seemed to be the most accessible agency to temporarily affiliate with in order to get close to the people. Hence, we "hang around" the clinic, talked with the mothers and other patients, and sometimes played with their children. Otherwise, we alternately joined the Rural Health Unit midwife in her house-to-house visits on patients. In some of these occasions I was able to witness and assist in a delivery of a baby as well as administer post-natal medical care. On the other hand, my companion helped in treating a person who was sick of meningitis and influenza. Apparently, situations like these become rich sources of observations and close studies pertaining to family life as well as beliefs and practices about delivery and infant care.

Later on, the barrio captain also introduced us to the parish priest, faculty members of the Parochial High and Public Elementary Schools and some old men from whom we secured many valuable data on the history of the barrio. Since reciprocal relationship on a give-and-take basis usually reinforces friendship, we returned favors by accepting the invitation to give lectures on Anthropology before high school students and by providing the barrio officials with copies of the surveys and other documents gathered pertaining to the barrio.

PARTICIPANT-OBSERVATION AND OTHER TECHNIQUES OF INQUIRY

After a month of regularly visiting the barrio, we formally became participant-observers by actually residing there. Unlike in most rural areas or among some ethnic minorities of the country, there is little choice of residence open to an outsider in this suburban area. Not all families are willing to allow a non-kindred to stay and live with them. As such we had to contend with the very few choices of a place for board and lodging. However, I was fortunate enough to live with one of the highly respected and relatively affluent family in the sitio I was assigned to. The house was a

two-floor structure which made it possible for me to observe from the top floor without being noticed, the behaviors of children and adults alike in the nearby street, stores and houses. Furthermore, the family keeps a store and by helping them run it, I met more people, heard gossips and learned of their attitudes and sentiments toward certain objects and individuals. At nights, young men gathered here and conversed about the events of the day, local and otherwise, while sipping beer or rhum.

As days progressed and as our circle of acquaintances expanded, opportunities for community participation likewise increased. I heard mass on Sundays, and Wednesday evenings, attended birthday parties, joined outings of young people, participated in baptismal rites, wake and funeral ceremonies, political meetings and many other barrio activities. During the barrio fiesta, I helped clean the house and prepare the food. However, caution was taken in order not to get emotionally involved; not to influence or interfere in the planning and decision-making of these activities. During ordinary days, I went from house to house to observe and talk to whoever was at home at that given time of the day. In the afternoons I sometimes stayed in the stores or rest house called *lamyaan* where adults spend their leisure time conversing and/or bantering with each other. The other areas for direct observations were held in the ricefields, nearby river, factories, market place, schools, etc. As such, photographic recordings of individual and group behaviors whether at work, play or study were carefully noted down.

In addition to being a participant-observer the project also utilized the technique of formal and informal interviews. The latter was conducted whenever I was with anybody coming from the barrio and while observing community activities. All responses elicited from the informants were written down as soon as I arrived home or after each conversation. On the other hand, formal interviews were guided by a schedule of structured questions and administered to persons who are more or less specialists in the particular field of our inquiry. For example, we formally interviewed the medicine men or *albularyos* on matters pertaining to folk medicine, the priest on the social and religious aspects of the study and the barrio officials on political matters. Daily activities and impressions were carefully noted down in a diary which I kept throughout the duration of the field work.

Finally, the project utilized gadgets like camera, maps, measuring tools and tape recorder to facilitate and render more accurate the data collected.

PROBLEMS IN FIELD WORK

Compared to field studies in "primitive" societies, suburban research apparently has lesser difficulties to reckon with particularly in terms of living accommodations and adjustments. In our case, the field problems were minimized not only because the barrio is already complete with the modern conveniences of living but also because we can speak the language of the

locality which is Tagalog. This saved us the time, money and effort which would have been otherwise spent in learning the *lingua franca* or in hiring interpreters. Furthermore, the barrio is only about twenty to thirty minutes ride from the University of the Philippines and from the commercial center of Cubao. It was, therefore, very easy to get reading materials and supplies whenever the demand for these arose.

The problems I encountered are probably similar to those being experienced by researchers in urban setting. For instance, I had some difficulty in explaining to the people our presence there and in convincing them that the materials solicited would not be used to their disadvantage. I had to repeatedly explain to every barrio folk I talked with, the nature and goal of the project. Secondly, there was the difficulty of interviewing many of those whom I considered valuable informants. This is due to the fact that they either worked in the office, factory or went to school during week days. Their free times were in the evenings and during weekends. Moreover, being a girl restricted my chances of going out at nights not mainly because of fear of harrassment but in order to avoid unnecessary adverse public opinion. This limited my research activities and I was not able to go to the nightly gambling sessions of mahjong and cards and drinking fests of the barrio men, and other occasions which could have been rich sources of information. But even during weekends it was also difficult to find some of the informants in their homes since they either went to the movie, *sabong*, church or visited friends residing in another barrio. However, this problem was partly solved later on, when I started arranging interview appointments at their most convenient time.

Like any urbanizing community in the Philippines, Santolan is fast becoming a heterogenous society. Immigrants from all regions of the country are continuously settling in the barrio because of the job opportunities generated by the sprouting factories and flourishing cottage industries. Even residents of nearby urban centers are moving in due to the conversion of large tracts of agricultural lands into housing subdivisions. This cultural diversity which markedly exists at the time of the research inevitably rendered more difficult the observation and isolation of the traditional cultural traits from those overlays.

To students exposed in the field for the first time, research is both exciting and many times discouraging. Exciting because of the challenge of translating into action the knowledge learned in the classroom and experiences contained in books by earlier field scholars. On the other hand, an initiation to field work is discouraging since one is suddenly made to realize that there is a wide gap between classroom ideas and barrio situations. The research tools which appeared perfect in the books became shrouded with shortcomings once personally tested in the field. For instance this research proved that the use of anthropological methods and techniques alone in

studying transitional communities are far from sufficient. It has to be reinforced by more precise tools of scientific inquiry like those of psychology, sociology and the natural and biological sciences. Although there are standard and prescribed methods and techniques of field research available in books for students to follow, the test for its suitability at a given time and situation would largely depend upon individual judgment and the conditions obtaining in the research area. At times it would be necessary to modify or readjust one's procedure of inquiry if only to maintain a favorable researcher-informant relationship and to get as much information as possible. In other words, a researcher must be well-trained—sometimes self-trained—in the application of these tools and sensitive enough to know when to use them. But probably, it is only through constant field work that a student can hope to be a good researcher, because as the saying goes "experience is still the best teacher".

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