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In Pursuit of Life Histories: The Problem of Bias

Claire Robertson

Before beginning a critical look at the process of collecting life histories I need to give a definition of a life history and say a few words in promotion of their use. A life history, as I am using the term here, is a type of oral history embodying the story of a person's life constructed by a researcher from the informant's oral account. If the goal of oral history, or oral traditions, is often to establish what really happened by collecting and comparing various accounts of events, the goal of a life history is somewhat different. A life history differs both from an autobiography, which is a first-person written account of a person's own life, and from a biography, which is a third-person written account of a person's life based on written and sometimes oral materials. Biographies are usually written after the death of the subject; life histories can come only from the living. The goal of a life history collector is to present a sample of the type of life lived by the people being studied. Thus subjects are not chosen on the basis of outstanding unique characteristics, as is usually the case with the subjects of biographies or autobiographies. But life histories can be just as extensive as biographies, two of the best examples being Ida Pruitt's *Daughter of Han*, the story of a Chinese woman, Ning Lao T'ai-t'ai, and Mary Smith's *Baba of Karo*, the history of a Hausa woman of northern Nigeria.

Life histories, then, are a prime tool for the social historian, in particular. They are invaluable for research not only in societies where most people are illiterate, but also where they are literate. Life histories are especially useful for studying the sorts of people whose history otherwise often gets lost. We will know very little in the future about the lives of most of the women alive in the world today without collecting their life histories, because most women are illiterate and come from countries where

few or no reliable statistics are kept. Even in the United States today approximately 20 percent of the adult population is functionally illiterate, although the official count of illiterate persons is only 1 percent of the population. Thus, if women's history is to be more than the study of elites, whether white middle- and upper-class, American and European, or non-western women, we must use life histories. They are not only the stuff of family, social, and economic history, but also have relevance for local, political, and military history and the history of medicine and science. In short, we can considerably improve our grasp of all sorts of history by collecting life histories. And we must collect them *now* among old people before they are all lost to us.

While the value of life histories is indisputable,¹ there are problems with collecting and using them as historical sources. One of the most prominent is bias. I do not think it is possible to eliminate bias completely from any piece of writing, but it is possible to compensate for that bias by recognizing it and allowing for it in composing a life history. In this paper I will be drawing on my own experience in eight years of historical/anthropological research and two years of fieldwork in Ghana to explore the types of bias which arise out of the methods used in collecting a life history, the translation of the history into another language from the original, and the gender of the collector as it interacts with that of the informant. I will also describe compensatory measures which can be taken to minimize bias; these are intended to be applicable in whatever culture the researcher is studying.

Methods of Collection

While there is not a voluminous literature on the

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collection of life histories, there are several good discussions available about the methodology of collection.² I will not recapitulate these here; rather, I will confine myself to the types of bias which might arise from the mode of collection, drawing from my own experience and readings.

The collection of life histories cannot be done well without first acquiring a thorough knowledge of the culture or sub-culture in which one is working. One needs to know, as completely as possible, the history, the language, and the customs of the people one is studying. This knowledge can be gained from written and oral sources, and from participant observation. Researchers in this country should not assume that, because they are American and speak American English, they can communicate thoroughly with any given American subject. This country contains many different classes and sub-cultures whose mores must be studied and appreciated before any life history work is begun.³

A good way to attain a strong background in a culture is to live in that culture and/or to do some initial survey work, which one needs to do anyway in order to select subjects of life histories. Survey work accomplishes several goals. First, it gives a basis for fitting the collected life histories into a context, for assessing the typicality of one's life history informants. Given the paucity of analysis of life histories, this is important. Second, one can learn how to behave and develop one's interviewing skills in a less crucial context by conducting a survey or two. A good knowledge of proper behavior among the people studied is a prerequisite for doing any sort of interviewing.

In my work in Ghana, my rudimentary initial knowledge of the Ga language was improved tremendously by spending a year doing survey work before ever beginning the collection of life histories. I learned the proper forms of politeness, the relevant vocabulary, the kinds of household situations I was likely to encounter, the geographical area, and how to use my recording equipment, as well as the cultural facts and details necessary to make a life history comprehensible. My survey respondents learned to regard me at least as a familiar face, at best as a friend, often seen walking around the central city neighborhood of Accra where I was working. This allowed me to exercise some discrimination about the informants I chose to continue with to obtain complete life histories. I was working in a slum neighborhood where it was quite important to overcome the people's initial distrust of a foreigner by extended familiarity. After a year of survey and archival work I decided to collect a few extended life histories from some of my best informants, the more friendly and informative among a sample of seventy-two women from whom I had already collected skeletal life histories in the survey data.

I worked with Mankah, an interpreter, even though by that time I could understand much of what was said. She was a nineteen-year-old middle school graduate whose grandfather was a friend. She helped me all through the survey work and was familiar with the goals of the research. Her presence provided both advantages and disadvantages. For Central Accra Ga people it is very important to know a person's family origin. Many of my

informants knew her grandfather, a well-known character whose house was located in the middle of the area I was studying. Even though Mankah herself was raised in a suburb of Accra, my informants could immediately place her in context, and that contributed to their trusting both her and me. Most interviewing began only after survey informants had inquired into Mankah's ancestry and present connections; she served as a cultural passport of sorts.

A disadvantage attached to Mankah's local connections, however, was that the women were sometimes not willing to go into sensitive topics that they feared might be broadcast around the neighborhood. We assured them that what they told us was confidential, but few informants were trusting enough to believe us. Despite this factor, some women were very frank indeed on certain sensitive topics. The fact that I was not asking them specifically about their relations with their neighbors allowed us to avoid some of the touchiest subjects. As it turned out, they volunteered a fair amount of information on the topic of neighborhood affairs, so indirection and sympathetic listening sometimes may be a good way to elicit sensitive material. In the end, I felt that the advantages of having Mankah along outweighed the disadvantages, especially after I learned more Ga and was able to crosscheck her translations. I then compensated for her errors through my own knowledge and through having the tapes of all interviews translated by other research assistants. Her few errors tended to arise from inattention rather than misinterpretation; she did not make what might be called creative distortions by imposing her own views. I felt, therefore, that she had not biased the responses unduly. She usually did exactly as she was told; this was perhaps a mixed blessing, since some interviews might have been more productive had she thought about what was being said. On occasion, I found it helpful to pretend total ignorance of Ga because the informant was telling Mankah things that she assumed I would not understand. Having an interpreter allowed me considerable flexibility: I could decide whether and how much of my knowledge of Ga to demonstrate to my narrators.

My procedure for collecting life histories was to interview an informant for an hour or two every morning for a week, or as long as it took to finish what she wanted to tell me. Most people tire of talking after some time, and repeated contacts help to build trust. We explained to them the purpose of the exercise before beginning, and they were all cooperative. They were asked to tell us about their lives from the beginning, or topically, as they preferred. These interviews came after they had answered the intensive survey questions, which took an hour or so on three or four successive days and gave us the initial outlines of their lives. Later, in writing up the life histories, these interviews were all blended together.

Initially with each informant it took some shifting of gears to move from their responding to my questions to their taking the initiative in telling me about their lives. We had mixed success with this: some women were unstoppable and did a superlative job; others never entirely made the switch. With most women the interviews became, as I had wanted, an exchange of information.

These women also wanted to know about my life and country. There is something exploitative about “milking” someone else for personal information without offering something in exchange. The eight women whose life histories I completed also were all friends by the time we started the life history collection, so that two-way conversations were more appropriate.⁴

What bias came out of these aspects of the methods used? First, people concentrated on what interested them most. For me, this was extremely valuable. For instance, several women talked a great deal about religion (both Christian and indigenous) and healing, two topics I had not inquired about extensively and of which my knowledge was most deficient. Second, the women tended to present values rather than norms. They idealized their behavior in presenting generally flattering self-images—to keep my friendship and to preserve their self-respect. Again, for my purposes this was good, since previously we had talked more about actual behavior. I could then contrast the two. If I had not already had the other information, however, some distorted accounts of behavior would have resulted.

Most of these women were quite self-assured and did not want to change anything in their lives. Some were simply fatalistic, with the same results. Most were elderly, in their sixties or older, and this made me suspect that self-confidence may also have been a reason for my choice of informant. This led me to consider the possible biases introduced by that choice. From among the original representative sample of seventy-two, itself selected from among 232 large survey respondents, I chose ten to interview for life histories, eight of which were completed. These women were all chosen because they were cooperative and were my friends; most were rather self-confident, outgoing types. It is easier, of course, to get a life history from an extrovert, but perhaps this biased the results because those with a more introverted reflective nature were not included. However, I had not discovered many such women in the larger sample, so extroversion was perhaps more typical for these women. This bias was therefore the result more of cultural than of personal factors, I concluded.

Because I wanted women with much experience behind them I also chose fifty as the lowest cutoff point for the age of the life history subjects. In a few cases their age led to occasional rambling incoherence and repetitiveness. Sometimes I had to interview the subject on her “good day,” when her memory was functioning well. One woman among the intensive survey of seventy-two was eliminated from consideration for a life history interview because alcoholism affected her reliability. Flexibility in scheduling interviews was obviously necessary. Aside from their age, occupation, Central Accra backgrounds, and extroverted natures, the women had very little else in common: a few had been successful large-scale traders, others petty ones. Some were very religious, others not. Some came from high-status families, some from the middle, and others from the lowest strata. If one was an ex-slave, another was the sister of Lady X, the wife of a prominent parliamentarian in Ghana. I purposely tried to get this sort of variation, as well as variations in number

of children and occupations; that aspect, at least, was easy to control.

It occurred to me that the actual recording of the interviews might bias the results by increasing inhibitions. However, unlike other researchers, I had no trouble with this aside from the technical problems of interference in the audio quality because of passing chickens, dogs, and children, and malfunctions in the equipment. I requested permission to record the intensive survey and life history interviews, and withheld people’s names; instead the informants were identified by numbers on the tapes, which have now been deposited at Indiana University. Although a few women felt uncomfortable with the tape recorder at first, all granted permission, and most soon forgot it was there. If something went wrong with it in the middle of an interview, rather than ruining the spontaneous quality by stopping, I simply continued and later relied on my notes. By the time I started the life histories, in any case, the women were familiar with the tape recorder from the survey work and were uninhibited. In fact, they gave rather the contrary impression that they liked the importance conferred on the interviews by the use of the tape recorder.

All interviews were conducted with as much privacy as possible under the circumstances. Central Accra houses are usually built around a courtyard in which most women spend much of their time when at home. The problem was that so did everyone else. Thus we usually adjourned alone to a sleeping room. Curiosity often attracted an audience to the door, but this quickly evaporated if I started off on a dull topic. After the “observers” left we got down to more interesting things. I preferred privacy for the interviews in order to minimize the bias induced by the informant’s efforts to deceive, offend, or not offend the listeners, as the case may have been. In one survey case, I had to stop the interview because the woman’s argument with her teenage daughter distracted her attention and distorted her answers. If I could not reduce outside presences to a minimum I did not use that particular woman as a subject. This procedure, of course, contributed to my receiving an idiosyncratic point of view, but that was what I wanted. Given a different purpose, I might have gathered everyone in a household, singly or collectively, to critique or add to a person’s life history. On occasion I did check out an account with another person. However, unlike some collectors of oral traditions, I was trying not to find out what “really” happened by comparing diverse accounts, but rather to obtain people’s own accounts of their lives.⁵ In analyzing the results, I then weighed the possible distortions caused by the donor’s own personality, politics, goals, or ignorance before presenting something as “fact.”

The location of the interviews also imposed another bias on occasion: brevity. The one-story clay and stucco structures in Central Accra usually have tin roofs and few, if any, windows. The temperature outside hovered around 80° to 90° Fahrenheit most of the time; inside it was commonly over 100°, and the humidity was around 98 or 99 percent—another reason that most people stayed outside. Anyone working under such conditions must have cool all-cotton clothing and a plentiful supply of handker-

chiefs. As time went on, I became more accustomed to the heat and humidity, but it took the initial seasoning provided by the surveying for me to be able to do the longer interviews involved in collecting life histories.

A last factor worth mentioning about methods is that I paid my informants if more than an hour's time was involved. This is not recommended by most anthropological manuals, but my conscience required me to break the rule. Most of these women were retired or were still market trading. They were businesswomen, although poor by American standards. They considered their time valuable, and my interviews were quite lengthy. I carefully calculated the amount of pay to make it sufficient that they would not feel exploited, but not so much as to encourage dragging the interview out or participating solely for gain. I paid only when interviewing was completed. Early on, one or two women in the intensive survey dropped out when they discovered they would not be paid until the end. Subsequently there were no more problems stemming from payment, nor did it bias the selection of informants further. In practice, I varied the amount paid according to performance and need, but the women did not know this ahead of time and it did not therefore affect the interviews. None of the women knew each other, so they did not exchange this information. The life history interviewees would probably all have participated without pay, but I felt that that would have been an imposition on their good nature.

The Perils of Other Languages

This aspect of collecting life histories in Accra made me more uncomfortable than any other, and it may have introduced the most bias. In essence, I was dealing with three languages—my own American English, Ghanaian public school English (the language of my interpreters), and Ga, the language of my informants. In a sense the initial danger from Ghanaian English was greater than that from translation into Ga because I was not as alert to it. One was a difference in dialect and the other a complete language shift, so that I tended to ignore the former until I was hit over the head with it.

Before going to Ghana I attempted to learn Ga—from grammars and from tutoring by a fellow University of Wisconsin graduate student who spoke it. There is and was no regular formal instruction in Ga available in the United States, although it has been offered on occasion at summer language institutes. The tutoring arrangements at the University of Wisconsin and in Accra did not materialize because of other time commitments of the tutors. There is not, in any case, a large pool of tutors for Ga or other languages that are not widespread; Ga is spoken only in and around Accra. Because most first-language Ga speakers have never been taught Ga as a written language (first recorded in the mid-nineteenth century by missionaries), they cannot analyze its structure. Coming from a learning tradition which emphasizes language structure, I spent too much time trying to find an instructor from that tradition. Ultimately, after wasting several months, I simply hired an interpreter and started interviewing.

My Ga improved considerably as time went on, but it is a difficult tonal language. The Central Accra dialect is particularly difficult because it is very rapid and has many elisions. An impediment to learning it was the fact that housing difficulties in Accra generally, and especially in Central Accra—where the average density of population is over twenty persons per room—forced me to live elsewhere, with my husband in university-provided housing on the outskirts of town. I could not then practice total immersion in the language and culture of the Ga, a distinct disadvantage.⁶ I attempted to compensate for my ignorance by having the tapes translated by other research assistants and discussing with the translators, informants, and Ga language experts any problems.⁷

Although I learned a considerable amount of Ga and a great deal about Ga, I still feel that I do not understand sufficiently the ambiguities in the language, and that this has biased my results. For instance, English and Ga divide up and describe the parts of the body and colors somewhat differently—rather like cutting up a chicken differently. Conversely, they have one catchall verb for “to like” or “to love” and do not make the distinctions we do in such matters. Because I was dealing in depth with husband-wife relationships, I found this rather difficult to handle. Another example comes from a translation of an answer given by an ex-slave to the question, “Why didn’t you go to the District Commissioner to get your freedom [after slavery was abolished on the Gold Coast]?” She answered, “The District Commissioner, what did I have to do with him?” In writing up her life history I initially interpreted this to mean that she had no need for his services and was satisfied with her lot. I was then fortunate enough to be able to talk with her again when I returned in 1978, and she explained that appealing to the District Commissioner would have done no good anyway because she had nowhere to go (she had been captured at a very early age), and that people such as she could have very little to do with such an exalted creature as the British District Commissioner. The basic problem, then, was one of incomplete comprehension; I shudder to contemplate other gross errors I may have committed in interpretation; I could only be as careful as my limited knowledge allowed. I do not feel, however, that systematic bias in one direction arises out of incomplete comprehension of a language, unless one is strongly ethnocentric.

The other chief language bias arose out of distorted comprehension of differences in dialect between Ghanaian and American English. Once I became aware of this problem it was easier to handle than the other one. I asked my interpreter and translators to define for me what they meant by certain English terms. Thus, “too much” means “a lot,” and “at all” means “no” or “none.” There were also individual idiosyncrasies in translation that I had to note. I believe I caught most of the systematic ones; I may have missed more of the individual ones. Again, I do not think that these imposed an overall bias on the data in a particular direction, although they may have promoted misinterpretation of certain points.

The Influence of the Gender of the Collector

Although some fieldworkers have considered the bias

imposed by the gender of the researcher on the collection of data, it is unusual to find any consideration of the gender of the donor.⁸ The interaction of the two is particularly important.

Before setting out to do fieldwork in any given situation, a researcher must do an analysis of his or her own preconceptions and feelings of appropriateness about gender roles. If these are very strong and very narrow, one had better be cautious about studying gender roles in another culture. In the African context, our knowledge has been impeded greatly until the last ten years or so because western researchers of both genders have often projected onto African women a number of ethnocentric assumptions. To name only a few, it has often been assumed that (1) husbands support wives, (2) marriage is the most important ceremony and institution, (3) spouses share property, and (4) women's economic activities outside the home are insignificant. But, in fact, (1) most African women support themselves and also provide the food for their children, (2) funerals, which express the solidarity of patrilineages and matrilineages, are more often the most important ceremony, (3) most Africans do not practice community of property in marriage but rather share resources with members of their own lineage (family of origin),⁹ and (4) African women most often do upwards of 70 percent of the agricultural labor (with great variations from place to place), as well as trading and other activities outside the home. These activities are generally long-established parts of their expected and accepted roles.¹⁰ Thus, many African women find irrelevant the American feminist emphasis on promoting women's work outside the home,¹¹ and, instead, are quite interested in obtaining better access to their husbands' incomes, a right that many Americans take for granted. Without a keen perception of such differences any collection of life histories is hopelessly biased and may focus on irrelevant issues.

Therefore, one needs to get as much information as possible about gender roles in the culture one is studying beforehand in order to program that into the research design. One can then consider which gender is more important for doing the proposed study competently. For instance, if one wants to study marriage patterns in a society where strict sex segregation is observed in public and in private because of certain taboos, then one should accommodate oneself to getting reliable information only from the same sex. Even if, as a stranger, one manages somehow to adopt a neuter role and gets permission to breach taboos, the persons of the opposite sex are unlikely to be comfortable in an interview and their usefulness as informants is therefore impaired.

The best situation is to have a pair of researchers of different sexes who get both sides. This is preferable even in societies where sex segregation is not strict, since men's and women's realities are often very different. Most women scholars are aware of the considerable bias imposed in most disciplines by a predominance of male researchers. But this is particularly true in conducting fieldwork; until relatively recently male bias was usually unstated and unnoticed as scholars went ahead to describe what the old men told them as *the* history of that people.

Happily, we now know enough to try to correct this bias.

Another bit of hard thinking needs to be done, once one arrives in the field and begins interviewing, as to what role one wants to play in that society. Peggy Golde's book¹² should be required reading for its superlative discussions of that topic alone. One will, of course, be thrust willy-nilly into a role by the people one is studying, depending on their perceptions of one's status and behavior. One needs to choose a role, if possible, that will allow one access to the kind of data one wants. For example, in many societies a woman researcher who wants information about political and/or religious rituals must achieve some sort of "classificatory" male status. Laura Nader managed to do this in both Zapotec and Lebanese fieldwork experiences.¹³ But a woman studying women or children needs to assume a role compatible with those of the indigenous women.

Margaret Mead and others have maintained that women's roles in many societies are more restricted than men's,¹⁴ but there is also an element of freedom involved for the fieldworker who is considered to be inconsequential because inferior. Women are often perceived as less threatening than men. The government of Ghana, although aware of my research, was not concerned about it because I was a woman dealing with women's affairs. Another helpful tendency is that people view foreigners, in general, as eccentric. On occasion, I unscrupulously used eccentricity as an excuse for actions designed to get something necessary out of the governmental bureaucracy quickly, but I did not do this in the fieldwork. Quick shifts in character or role are likely to promote suspicion and are therefore not advisable. In any case, unaccountability for one's actions, whether because female or foreign, can sometimes be an advantage. The combination of these aspects may permit a foreign woman to go places and ask questions a man would be forbidden, and to act with autonomy because her actions are not considered important anyway.

Some of my experiences illuminate the problems associated with interviewing in a sex-segregated society. In Central Accra Ga society adult men and women often live in separate compounds composed of patrilaterally or matrilaterally related groups of relatives of the same sex. Thus, a woman would often live with her mother, sisters, and daughters (her sons were to join their father at around age six). Husbands request their wives to come to them at their compounds when they want to have sexual intercourse. A failure to request or a refusal to come, if repeated, usually means separation. The women do the cooking and laundry at their compounds and send it to their husbands via the children.

The Central Accra situation was ideal for woman-to-woman interviewing. Because husbands and wives were usually not co-resident, the husbands did not put a crimp on freedom of speech by denying me or the women permission to talk, or interjecting and overriding with their own opinions. All of these things happened later when I interviewed in a suburb of Accra where husbands and wives were more often living together in the same house. The ideal of submission for women operated even more strongly where men had substantially more education than

their wives and were co-resident with them. On several occasions I came away with an incomplete questionnaire because a husband came home and forbade his wife to continue talking, even though we were discussing innocuous topics. The point was that the husband's permission had not been sought first (which was not possible in a large-scale survey situation). On other occasions, I had either to omit a woman from a survey or stop temporarily when a husband insisted on being present and overrode his wife's opinions. The presence of male siblings did not usually have this effect, since those relations were generally more egalitarian.

A further factor that affected relations with the women was my marital and maternal status. In 1971-72 I was married but had no children, which the women found sad and a bit suspicious. For them, infertility usually has uncomplimentary causes—promiscuity, for instance. But many were sophisticated enough to grasp that I might operate by different rules. When I returned in 1977-78 I had one child, and this smoothed matters considerably. Marriage and motherhood are so important in many societies that a woman who is single or childless is automatically viewed with great suspicion. One should try to find locally acceptable reasons or roles to explain an unusual situation.

Ignoring the problem will not solve it, either. People may not respond in a helpful manner to someone they think to be promiscuous or a witch. I was fortunate in that they assumed I was respectable. In fact, I took care to obtain an introduction from the local Ga and national government authorities, to present my credentials when requested in the form of a card with my degrees, affiliations, and address on it, and to have my work blessed by a well-known priestess of the Ga religion before beginning. I also was very fortunate to be helped in the latter by an anthropologist colleague, who introduced me to the priestess and helped me to find a research assistant/interpreter. Any efforts along these lines will more than pay off and must be made. One result of these "bona fides" plus the interactions of personality, was that informants in 1971-72 concentrated on telling me remedies for childlessness. A bond was thus made with one of the life history subjects who was also childless. Later the fact that I had only a son forged a stronger bond with another life history subject who only had sons: we bemoaned together what we would do in our old age without daughters. Whatever one's circumstances, it should be possible to identify with those of some of the informants and thus cement bonds.

But what happened when I tried to interview men? My calling cards were particularly important to the men, who were more often literate and asked questions about the nature of my research. Some were well-educated and/or well-read. The men were generally more informative than the women on matters of custom and history. I had to be careful, though, because some of the best educated were liable to quote to me the two leading Euro-American ethnographers of the Ga, rather than to speak from their own firsthand knowledge. Many men were willing to talk at length on various topics, but these were more often impersonal ones than with the women. Some were notice-

ably reluctant to discuss marital and financial affairs and views of women. Some older men had difficulty remembering how many wives and children they had had. Family matters were less important to them than to the women. In fact, many men were contributing only nominally to the support of their children and not at all to the support of their wives, both of which practices were socially unacceptable. I concluded that it was not only the gender difference between the researcher and the subject that biased their answers, but also the social situation. A male researcher would have gotten more on attitudes toward women, no doubt, but would probably have had the same problems with collecting the marital histories.

Because of my foreign and high educational status, and the relatively high local standards of politeness, I rarely had to worry about unwanted sexual advances. For some men I was "classificatorily" male. However, on one occasion a strange young man came up to me in a market where I was conducting a survey and patted me on the cheek while making the sort of remarks in English that might be addressed to a prostitute. His gesture and words could only be considered insulting by Ga standards; I responded appropriately by insulting him loudly in such a way—"Who do you think you are, God's gift to women?" and so on—that he fled incontinently with the jeers of my friends, the market women, following him out of the market. Although I spoke in English, several nearby schoolgirls translated quickly for the audience's benefit, and much hilarity followed the incident. I was very proud of myself for reacting as a Ga woman would in the situation, and grateful to my friends for supporting me by furthering the insults. Such concerted efforts are one of the chief means Ga women use to force men to treat them respectfully. I also prevented some touchy situations involving sexual advances by being purposely obtuse. To avoid such hassles it is a good idea to find out beforehand, if possible, what the local standards of polite behavior are in heterosexual encounters, and what parts of the body are considered provocative. The Ga do not consider the breasts to be as sexually enticing as the waist, and many women are barebreasted in the compounds; wearing a halter top, however, would have caused an unwelcome sensation.

Male-to-male researcher-subject interaction has all of the advantages of the female-to-female interview plus even more. In reading accounts written by some male researchers,¹⁵ I realized that the help they got in their research, in terms of both finances and personnel, reflected the generally higher status of men in most societies. Old boys' networks are generally more powerful than old girls' networks because of men's greater ease in achieving high-status positions. I received a great deal of help from important men in the government and Ga power structure, but there was usually a final barrier that I could not breach because I was female. Despite being "classificatorily" male for some purposes, I was still female when it came to friendship: in Ghana, as in many other societies, there is no other conceivable relationship between a nonrelated premenopausal woman and a man than a sexual one. Thus, the kind of friendship with high-status men which can be so important for furthering

research was inhibited. Male researchers have the great advantage, when interviewing male subjects, of potential friendship, just as women have with other women, and the quality of the data reflects this.

Men interviewing men will also not have to worry as much about their subjects' getting permission to talk or their answers being biased by submissive ideals of behavior. I found a strong disparity between the submissive social ideals regarding women's behavior toward men and the women's actual behavior in the matters of money and love affairs. Such discrepancies are inevitable when strong dominance is asserted by one group over another (the closest analogy is in the literature on slave behavior). Because of their dominance, male informants will not have as much need to cover up their nonconformist behavior by lip service to conformity.

However, men interviewing men may encounter bias arising from other sources inherent in social structure. Although I have been concentrating here on the bias arising from gender differences, it should be noted that status differences caused by age and education can be as important as, or even more important than gender as criteria for allocating authority.¹⁶

As a result of the biases that are likely in cross-gender research when the researcher and the informant are of different genders, I feel that my data from women are more reliable in general. For life histories, in particular, same gender interviewing is highly preferable. The ideal of having different gender pairs of researchers will seldom be achieved. It is, however, perfectly acceptable to deal with single-gender samples, as long as one considers the possible biases involved. Indeed, one may have to settle for single-gender samples in many situations. But whether or not one chooses a single-gender sample, one needs to consider one's own and one's subjects' biases caused by gender roles very carefully in analyzing the data.

In summary, a number of biases can be introduced into life histories by the collection methods used, the use of language, and the interaction of the gender of the researcher and the informant. All of this is not to say that life histories should not be collected, but rather that care must be exercised in their collection and analysis, and that some situations are more productive than others. Life histories remain one of the best ways open to us of understanding culture, personality, and the history of humankind.

NOTES

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1. Oscar Lewis, *La Vida* (New York: Vintage Books, 1966), Introduction, has an excellent discussion of the merits of collecting life histories, as well as the difficulties and time commitments involved.

2. The "classic" works are: Louis Gottschalk, Clyde Kluckhohn, and Robert Angell, *The Use of Personal Documents in History, Anthropology and Sociology* (New York: Social Science Research Council, 1945), and L. L. Langness, *The Life History in Anthropological Science* (New York: Holt, Rinehart and Winston, 1965). In addition to the works mentioned in other footnotes, Thomas Rhys Williams, *Field Methods in the Study of Culture* (New York: Holt, Rinehart and Winston, 1967), Solon T. Kimball and James B. Watson, ed., *Crossing Cultural Boundaries* (San Francisco: Chandler Publishing Co., 1972), and Eleanore S. Bowen, *Return to Laughter* (Garden City, N.Y.: Doubleday and Co., 1964), may be of use, as well as Michael A. Rynkewich and James P. Spradley, ed., *Ethics and Anthropology* (New York: John Wiley and Sons, 1976).

3. A good example of a researcher who did not make this assumption and who was able to collect valuable life histories after doing a great deal of participant observation among an American sub-culture is Carol Stack in *All Our Kin* (New York: Harper Colophon Books, 1975).

4. The importance of reciprocity in the exchange of information in fieldwork has been emphasized elsewhere. Peggy Golde, "Introduction," *Women in the Field: Anthropological Experiences*, ed. Peggy Golde (Chicago: Aldine Publishing Co., 1970), p. 8, called a person "who obtains information without revealing her own identity and goals" a spy, who will arouse suspicions that she will use the information against the people.

5. Jan Vansina, *Oral Traditions* (Chicago: Aldine Publishing Co., 1965), has a definitive discussion of how to discern bias and resolve conflicting accounts, and a far more detailed analysis of the influence of the circumstances of collection on a testimony. However, he is dealing with establishing a unified "truth," for the most part, which did not concern me.

6. George M. Foster and Robert V. Kemper, ed., *Anthropologists in Cities* (Boston: Little, Brown and Co., 1974).

7. Mr. A. A. Amartey, the head of the Ga section of the Ghana Bureau of Languages, was of tremendous help in this and other matters.

8. The best work available on this topic is probably Golde's *Women in the Field*, a fascinating collection of accounts of fieldwork experiences, many of which consider the impact of being a woman on the data collection.

9. I think here of the startling statement by a moneylender informant who, when asked what interest she charged her husband on loans, replied, "For others I charge 50 percent [per annum], for him, only 30 percent."

10. Lack of knowledge of these four factors, as well as male bias, has negatively affected most so-called "development" policy carried out by U.S. AID and international agencies. See Barbara Rogers, *The Domestication of Women* (New York: Tavistock, 1980).

11. Likewise, lower- and working-class American women, who have worked outside the home for generations in many cases in jobs that are sheer drudgery, are often not concerned about their right to work or equal pay for ghettoized jobs, but rather would like to retrain for better jobs or retire and live on a well-paid husband's income.

12. See Golde.

13. Laura Nader, "From Anguish to Exultation," in *Women in the Field*, pp. 104, 111.

14. Margaret Mead, "Fieldwork in the Pacific Islands, 1925-1967," in *Women in the Field*, p. 323; Golde, "Introduction," in *Women in the Field*, p. 8.

15. George N. Appell, ed., *Ethical Dilemmas in Anthropological Inquiry: A Case Book* (Waltham, Mass.: Crossroads Press, 1978), pp. 139-40; Michael J. Lowy, "Me Ko Court: The Impact of Urbanization on Conflict Resolution in a Ghanaian Town," in *Anthropologists in Cities*, pp. 153-78.

16. Elsewhere I have suggested that in precolonial times (the nineteenth century and earlier) age took precedence over gender in allocation of authority. Colonialism then imposed European ideas which deprived women of power while educating younger men, reversing the means of allocating authority. See Claire Robertson, *Sharing the Same Bowl; a Socioeconomic History of Women and Class in Accra, Ghana* (Bloomington: Indiana Univ. Press, forthcoming).