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# 'Circle No Bicycle': Fieldwork in Nyanza Province, Kenya, 1994-1995

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# 'Circle No Bicycle': Fieldwork in Nyanza Province, Kenya, 1994-1995

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Kenya, Social Networks, Wealth, Family Planning, Behavior, Qualitative, Interviews, Focus Groups, Fieldwork, Household Survey, Questionnaire, Survey Research, Interviewers

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"'Circle No Bicycle':  
Fieldwork in Nyanza Province, Kenya, 1994-1995

Susan Cotts Watkins  
with  
Naomi Rutenberg, Steve Green, Charles Onoko, Kevin White,  
Nadra Franklin and Sam Clark<sup>1</sup>

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<sup>1</sup> The study of women's social interaction by Rutenberg and Watkins was funded by a grant from the Carolina Population Center's Evaluation Project (USAID) to the Futures Group and the University of Pennsylvania; the study of men's social interaction was funded by a dissertation research fellowship from the Population Council to Steve Green. Green also received funding from the Association for Voluntary Contraceptive Sterilization. Exploratory work by Watkins, the participation of Nadra Franklin and Sam Clark of the graduate program in demography at the University of Pennsylvania, and some of the participation of Green was made possible by grants from the Mellon Foundation to the University of Pennsylvania. The study was directed by Charles Onoko.

There were two stages to our research.<sup>2</sup> We began with 10 long interviews with women and 10 with men in each of our four sites (a total of 40 men and 40 women) in June and July, 1994. In this stage, we asked men with whom they talked about wealth flows and family planning, and we asked women with whom they talked about women's health problems and family planning. In this stage, our primary interest was in gaining a general picture of the patterns of social interaction and, particularly, the content of their conversations.

In the second stage, we surveyed approximately 800 men and 800 women (about 200 men and 200 women in each of the same four sites) in December and January, 1994-95.<sup>3</sup> In this stage, our primary interest was in describing the social networks of our respondents more precisely. We did not ask much about the content of their conversations or their own attitudes, but rather about some aspects of their social behavior: where they had lived and with whom they talked, where, how frequently, and the characteristics of those with whom they talked. When we were in the field, we did a lot of looking and listening. At least one of the principal investigators (Watkins, Rutenberg for the female survey, Green for the male survey) was in the field at all times during both phases (Watkins was there throughout). Our two colleagues from Kenya (Alan Ferguson, of GTZ/Ministry of Health, and David Wilkinson, of Innovative Communication Services) were in the field at various stages for a few days. We were also helped by the collaboration of three graduate students from the University of Pennsylvania: Kevin White, who participated in the qualitative phase and was instrumental in developing the sampling procedures for this stage, and Nadra Franklin and Sam Clark, who with Steve Green supervised data entry in the field during the second phase.

Crucial to gathering the data were our study director, Charles Onoko, and the five Kenyans--Francis Ayuka, Theresa Akoth, Marcellus Ayoma, Phoebe Ogolla and Rena Otieno--who were interviewers in the first phase, and supervisors of teams of 20 interviewers in each site in the second. Not only were they superb interviewers and supervisors, but because they were smart and curious they came to understand the research questions well, and were thus invaluable in helping us to develop our questionnaires and to interpret our data. Every evening they reported about their day, and we collected

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<sup>2</sup> In addition, in April 1994 we conducted exploratory interviews, seven with women and two with men in and around Oyugis, and three in Magunga, near Owich.

<sup>3</sup> The first phase lasted from June 6-July 15, 1994; the second from December 9, 1994-January 31, 1995 (minus 9 days for Christmas). In addition, Watkins and Rutenberg were in Kenya for about a week in October 1993, Watkins and Green for about a week in December 1993, and Watkins and Rutenberg for 10 days in November 1994 for the pretest.

anecdotes: where and why respondents were suspicious of us, how they tracked down elusive respondents. Although our supervisors were raised in these areas they live in Nairobi, and their own understanding of our areas was limited: thus, we all also talked with various "community leaders"--chiefs and sub-chiefs, clinic health personnel, traditional healers--, with Community Based Distributors (volunteer family planning workers for GTZ/Ministry of Health). We talked with various people we met around: people in shops and markets, at the hairdressers, etc. And, perhaps most usefully, we talked with some of our interviewers to get their sense of what the respondents had in mind when they answered particular questions.

In what follows, we begin with a description of our research sites, since these influenced the procedures we followed (or had to abandon). We then discuss the first, qualitative stage of our research, describing our sampling, interviewing and translation procedures, and then the second, quantitative stage of our research, describing our sampling, the selection of supervisors and interviewers, and the data entry. We then evaluate the completeness of our coverage, and discuss what we learned in the field that helped us to interpret our data. This information, we think, is useful for evaluating the quality of our data and in interpreting our results.

In addition, we hope it will be useful for researchers planning and executing surveys in similar contexts.

In addition to this description of our methods, there are two major points we wish to make in this paper. The first is that the organization of our study--from choice of research sites to the sampling procedures to the conduct of the interviews themselves--was much less tidy than either our research proposal or most published accounts of surveys would suggest. (This is a point that experienced survey researchers no doubt understand well, but novices may find our discussions useful). There was a large gap between survey procedures as they are described in published accounts and our experience, and we suspect a similar gap in surveys done in similar settings. Much went wrong, or at least less smoothly than we think most survey researchers like to suggest. Some of what went wrong was, in the end, irrelevant to the quality of our data: the conditions under which we worked (including decrepit vehicles that broke down, a solar panel that failed and a borrowed generator that blew the chargers for our computer batteries). But other problems probably did affect our data: some respondents whom we think were taciturn because their sons and daughters had not been chosen as interviewers, others who suspected that we wanted to force the women to take family planning pills, yet others Christian fundamentalists who thought we were devil-worshippers.

Our second, and we think more important, conclusion regards the importance of "being there"--i.e. the presence of the principal investigators in the field-- for surveys done in settings like that of Nyanza Province. This is costly, especially in terms of time, and in our experience was often quite uncomfortable: sparse accommodations, monotonous, bland food, the possibility of malaria and typhoid, bats in the bedrooms. Nonetheless, we are convinced that it is worth it. In areas without a well-developed infrastructure to support survey research, being there is probably the only way to ensure that the data are of high quality, and that the areas of error or bias are identified.

Being there helped us to develop appropriate and feasible sampling procedures and to ensure that all those in our sample were reached (or could not be reached). By checking the results of the day's work every evening, we could see that the semi-structured interviews made sense, and point out areas to probe the next day; by checking the questionnaires from the household survey quickly, we were able to detect missing data or obvious inconsistencies in the responses while it was still possible to ask the interviewers to return to the respondent the next day. By supervising data entry in the field, we could resolve problems while the memories of the supervisors and interviewers were still fresh. Lastly, being there helped to maintain the morale of our supervisors upon whom data collection depended. As a result of being there, we have a fairly good sense of where our data are more nearly accurate and where they are not, and why not.

The knowledge we gained from being there has been crucial in another way--in giving us a "feeling for the organism" (the phrase is that of Barbara McClintock, in Keller, 1983). Less mystically, being there helped us to interpret our results. The data we collected, which seems so pristine when it is eventually summarized numerically in our computers, is a collision of understandings by participants who live in quite different contexts: what we had in mind when we

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<sup>4</sup> Even where there is an infrastructure to support survey research, as in Kenya, the experience has been gained largely on surveys to guide program efforts; we believe that most of those who commission these surveys and conduct them do not aim to achieve the levels of coverage and accuracy that are considered necessary in the U.S. academic community. Our supervisors told us that in the surveys with which they had had previous experience or heard about, the principal investigators usually did not go to the field, and that interviewers would sometimes simply sit under a tree and tick off plausible responses, or report call-back visits that had not been made. We are convinced that did not happen in our study: not only were our supervisors highly motivated, but they (and our local interviewers) were full of small details that could only have been obtained by their interaction with the respondents, and their description of efforts to find respondents were entirely convincing.

developed our questionnaire, what our interviewers understood when they asked the questions, and what our respondents understood when they answered the interviewer's questions. Despite our intensive qualitative phase, during the quantitative stage we were made painfully aware that these multiple understandings did not always coincide.

We are not referring here to the more elaborate versions of theories of the social construction of reality, but rather to something more mundane. For example, we asked about ownership of radios and bicycles as a measure of wealth in communities that are so close to subsistence that wealth is difficult to measure. Yet chatting with a group of interviewers at the end of one day, one told us of a respondent who wanted the interviewer to answer the question "Do you have a radio" with "No"--even though a radio was playing--because she thought we might be giving out radios at the end of the research; another interviewer added that a man said "Circle 'No Bicycle!'", and others later repeated this. Thus, two of our few economic measures appear to capture actual and desired wealth.

Or, to take another example, one respondent said she had begun using family planning about five years ago, but was not currently using it. Our questionnaire only asked current users about the method being used, but as it happened the respondent volunteered that she had had a tubal ligation five years ago, and the interviewer had written that in the margin of the questionnaire. It appears that, for this woman at least, a tubal ligation is a single rather than a continuing use of family planning--an interpretation that we had not even considered. A few misunderstandings like this can be treated as "noise"; many are more problematic. Although we think that being there has resulted in higher quality data than are usually available, being there has also made us somewhat squeamish about our own data--and, by implication, the data of others.

#### **RESEARCH SITES:**

We chose Kenya because it is English speaking, has a good climate, and has a sufficiently developed research infrastructure that we expected to be able to find local collaborators. In addition, Kenya has a reputation in the demographic community: although contraceptive use has been increasing steadily since at least the late 70s, and possibly earlier (see the data in Heisel, 1968; Dow, 1967; Anker and Knowles, 1982; World Fertility Survey, 1978), fertility in the mid-1980s was exceptionally high, and subsequently has fallen rapidly (Brass and Jolly, 1993; Cross et al, 1991). Our research question concerned social interaction and the diffusion of new ideas and of family planning techniques; we think diffusion

might be particularly important in the earliest stages of fertility transition (see, for example, Casterline and Rosero-Bixby, 1993).

Thus, we chose Nyanza Province, which the 1989 DHS showed to have relatively low contraceptive prevalence compared to the rest of Kenya.

In at least one of our specific research sites, we think we are close to the beginning of the fertility transition in these areas; a 1990 survey of an area in Owich shows a total fertility rate of 8.7, and a contraceptive prevalence rate of 5.5 (Ferguson, 1990).

Within Nyanza, we chose four sublocations, all within the former South Nyanza district, a relatively remote and underdeveloped part of Kenya. The choice of sublocations was made by a collaborator, Alan Ferguson, of GTZ/Ministry of Health. Our aim was to choose four sites that were as similar as possible except along two dimensions, the characteristics of their social networks and the presence/absence of Community Based Distribution programs (volunteer family planning programs). Two of them, Obisa (near the small town of Oyugis) and Kawadhgone (near the district capital of Homa Bay) were chosen because they were expected to have relatively heterogeneous networks and to be relatively open to outside influence; two, Owich and Wakula South (on Mfangano Island, in Lake Victoria) were chosen because they were expected to have relatively homogeneous networks and to be relatively closed to outside influences because of their remoteness. In each pair, one sublocation has a Community Based Distribution program of family planning run by GTZ/MOH (Obisa and Wakula South); Ferguson planned to introduce such programs in the other two, such that with a follow-up survey we could evaluate whether the characteristics of the networks influences the rapidity with which the effect of CBDs is evident. In other respects (e.g. levels of wealth/poverty, ethnicity) the areas were quite similar.

We begin with the similarities across our sites that struck us, and then describe the differences. What follows is based on our impressions, with some supporting evidence in footnotes.

### Economic development

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In the first phase of our research, we interviewed in two, not one, sublocations near the town of Oyugis and two on Mfangano Island because a single sublocation had recently been divided, and the chief of each wanted all members of the original sublocation to get the "benefits" of our research. In discussing this first phase, we will refer to the sublocations in which we interviewed in the second phase. In the first phase, we interviewed in the two sublocations of Kamuma and Obisa, but only in Obisa in the second phase; in the first phase we interviewed in Wakula North and Wakula South, but in the second phase only in Wakula South.



To reach Nyanza from Nairobi, we drove across the Rift Valley, which has many signs of development: gas stations, private cars, factories, billboards, power transformers, telephone lines, and permanent housing (cement with metal roofs). Beyond the Rift Valley, these virtually disappeared: it felt like we were in a different world. The terrible quality of the roads was experienced acutely in the long time it took to travel short distances. Travelling to the field in the first phase, one of our vehicles lost a wheel as we were nearing our first site: we had to wait quite a while by the side of the road for a matatu (rickety, crowded small buses with names such as "Japan's Boy"). Most of the sparse traffic consisted of these matatus and the occasional official vehicle (often with the logo of a foreign donor). There were few signs of industry or commerce--rarely did we see billboards along the roads, stores, or gas stations--and in none of our sites could we buy a postcard. Finding accommodations was difficult. In all our sites, our female interviewers had difficulty finding even pit latrines when they were out interviewing for the day. We were worried about getting sick--malaria is endemic in our sites (and there's a lot of HIV/AIDS, although that worried us less)--and local health facilities in our areas were small and poorly equipped; the 1993 DHS shows infant mortality (lqo) at 127.9 for the province, nearly twice as high as the national average and as the next highest area. The newspapers account for the underdevelopment of this area in political terms: Nyanza is Luo-land, and has been in opposition to the government of President Moi. Development funds and votes are explicitly linked by politicians.

Each of the four sites was more remote than the last. The first, Obisa, has frequent matatu communication with other parts of Kenya; here it was possible to buy a newspaper or make a local or an international telephone call. The second, Kawadghone, is about a

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<sup>5</sup> According to the Homa Bay District Development Plan, there were 4 licensed motor vehicle repair businesses in 1992 (HBDDP, p. 41, Table 2.11).

<sup>6</sup> In 1992, there were 4 licensed manufacturing businesses in Homa Bay District (Homa Bay District Development Plan, p. 41, Table 2.11).

<sup>7</sup> In Oyugis we stayed at the guest house of the Amani Christian Development Project; in Kawadghone at a tourist hotel (although we saw few tourists) 30 minutes from our sublocation because there simply was no accommodation in the sublocation itself; in Owich we stayed in the Italian-Kenyan Scout camp (no electricity or running water) in one phase and an unopened SDA health clinic in the second phase (also without electricity or running water) and in Wakula South in some deserted buildings of the health clinic (also no electricity or running water, but bats and cockroaches in the rooms).

<sup>8</sup> We could also make calls and buy newspapers in the second site, Kawadghone,

45 minute drive over a dirt road from the district capital of Homa Bay; the third, Owich, is a dry and rocky area on the shore of Lake Victoria, a 2.5 hour drive over dreadful roads from Homa Bay, and with only one matatu to Migori (and thence to Homa Bay) passing by in the morning and returning in the evening. The last site, Wakula South, is on island in Lake Victoria about 2 hours from the mainland, with direct boat service one day a week to the mainland and circuitous boat service three days a week: from the mainland port, it is another two-hour journey over dreadful roads to Homa Bay. As we approached the island, one of our team members asked if the population of the island voted. When we first arrived in Owich, one of our supervisors said "This is the back of beyond"; when we got to Wakula South, she said "My God, you wouldn't know you were in Kenya. You'd know you were in Africa, but you wouldn't know where."

### Administration

Administratively, our sites were all sublocations. The hierarchy is province (Nyanza), district (Homa Bay, with Homa Bay town as its capital), locations (perhaps best thought of as equivalent to states), sublocations (perhaps best thought of as equivalent to counties), and villages (descendants of a not-too-distant ancestor, and the sampling unit in the second stage).

All of our sublocations were within the former district of South Nyanza, which was recently re-organized. The largest of our sublocations was Owich, with a population of about 11,000 and a much larger area than the other sites, and the smallest Kawadghone, with a population of around 4,500. In Obisa and Kawadghone it would be possible to walk across the sublocation in less than an hour.

In Owich this takes 3 hours or more, and in Wakula South, although it is possible to walk across the settled areas near the lake in about two hours, it is a walk (climb) of 3 hours to reach the areas in the hills.

Provinces, locations, sublocations and villages are administrative areas, but they have some social meaning. By social meaning, we mean that social interaction--the topic of our study--is likely to be more intense within an area than outside of it, in part because of a common identification with the group that lives in that area, and in part because of geographic proximity. Nyanza Province

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but that is because we didn't stay there, but rather 45 minutes away in the tourist hotel in Homa Bay, because we couldn't arrange for accommodations in Kawadghone.

By the quantitative phase, we could no longer make international calls from Homa Bay, because the hotel had not paid its telephone bill.

<sup>9</sup> These figures are probably based on the 1989 census.

is predominantly Luo (except for some of the population of the few towns). The story is that all Luos are ultimately descended from the same ancestor, with different progeny of that ancestor settling in different parts of Nyanza (Ogot, 1967). Ethnic identification appears to be fairly strong in Kenya, and identity as a Luo--indicated by, among other signs, a home-place in Nyanza Province--rather important. There seems to be relatively little identification with locations or sublocations per se, but the locations are said by Evans-Pritchard to roughly correspond to "old tribal areas" (Evans-Pritchard, 1938). At least in principle, sublocations are a set of related clans, and (male) residents of a village are members of the same clan. Villages do sometimes evidently produce a strong sense of identification. Although little visibly distinguishes one village from another, in two cases, we encountered strong identification with a particular village. In both, members of the clan had bought land in another village, but insisted that they were still members of their original village (which complicated the logistics of interviewing).

Social interaction can be expected to be most intense in the smallest areas and least intense in the largest, not only because of the degree of relationship but because of geographical proximity.

Our household survey shows that conversational partners are most likely to be from the same village, next from the sublocation, and relatively rarely from outside the sublocation. Marriage partners do not come from the same village (the Luos have exogamy rules): an analysis of marriage distances shows that most are likely to come from within the same sublocation; if they do not, they are likely to come from sublocations that were within the former district of South Nyanza. Residents of neighboring sublocations meet each other at the larger markets, at periodic barazas of the chief, and, we think more rarely at places like the district hospital in Homa Bay.

The locations of Nyanza Province participate in an annual sports festival, which has been held for several decades in December, and which has activities for both men and women.

Locations are headed by chiefs and sublocations by assistant

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There are also a small proportion of Basuba in Wakula South sublocation. The Basuba apparently came from nearby Uganda early in the century, and are relatively assimilated. There has been considerable intermarriage: all understand Luo, and only the elderly do not speak it well. In a few cases, the interview for the household survey was in Basuba.

<sup>10</sup> A few of our female respondents said that they were born in the village in which they were surveyed. Our supervisors told us that this didn't violate the rules, since some ancestor of the woman would have immigrated to the village.

chiefs. Both are appointed by the central government, and receive a regular salary. The chiefs usually wore suits, although rarely ones that looked new; the subchiefs dressed more informally--although one was a natty dresser, with gray pin-striped flannel pants and a perfectly pressed cotton long-sleeved shirt. Their duties seemed to be primarily resolving disputes (e.g. over plot boundaries, or accusations of false weights by fishermen) and finding and punishing thieves and dealing with higher levels of government. They also call together the members of the sublocation (in the case of assistant chiefs) or the location (in the case of the chief) for periodic barazas, attended in principle by all the male residents of the area, but in practice, we understand, by about three-fourths of the men plus some women. The chiefs also help in implementing projects such as ours, and had to give permission for our research in the area. It was evident that chiefs and assistant chiefs are powerful figures. For example, the chief of the location in which Wakula South lay forbade anyone to enter the health center area where we stayed after 6:30 p.m., and this seems to have been effective; when we made arrangements through the sub-chief to rent a boat and the arrangements fell through, the sub-chief brought our money back. They were not always happy with us, however (see below, in the section on hiring interviewers). The miji-kumi's, or clan elders (one for each village), are the lowest level of administration. They are unpaid, and often elderly and illiterate. Their dress was indistinguishable from that of the other males in the community.

Barazas were held by the chief to announce our arrival and some version of what we would be doing, and to call for focus group participants (in the first phase) and applicants for interviewers (in the second). Although they had been told earlier both in person and in letter that we were simply doing research, we believe that they also were persuaded, and told their people, that we would benefit the area in terms of "development". Both chiefs and assistant chiefs knew the boundaries of the sublocation, and we relied on their information in mapping out our areas. We used the miji-kumi's to draw up lists of the households in the villages in our sublocation for sampling, and of the individuals in those households for our lists of eligible respondents. In addition, in two sites where the boundaries of villages were particularly ill-defined, the miji-kumi walked around with the supervisor of the interviewing team for that village. Occasionally, when a respondent was particularly suspicious, the miji-kumi helped: in two cases, the miji-kumi explained to our supervisor that the respondent had stolen some money and thought we were the police, and explained to the respondent that we were not interested in such things.

#### Government sponsorship

It clearly would be impossible to do field research in Nyanza without the permission of the government. We got formal permission from the Office of the President, which our study director then took around to the various District Officers and Chiefs. But the permission needed is more than formal. The chiefs are supposed to know about everything that goes on in their areas, and it would be very hard to do work in their area without their knowing it: social interaction here is very intense, word spreads quickly, and the presence of a team with vehicles, foreigners, people from Nairobi, and money, would not go unnoticed. Moreover, in all sites we encountered considerable suspicion on the part of some. Our interviewers were asked why their name was on a paper (the list the supervisors carried around of people to be interviewed), or how this interview would benefit either the community or themselves personally. In addition, some were suspicious of our motives. The support of the chiefs and the elders was important in allaying suspicions, although we believe that some shared our respondent's doubts about family planning. We think the chiefs and subchiefs gave us such support as they did in part because we were associated with the Ministry of Health, and as government officials they were supposed to help us; because it appeared to them possible that our research would benefit the community; and because they expected our presence to benefit them personally, either in the form of payment for their services, or by hiring their relatives as interviewers.

The support of the government has a potential down-side, however, familiar to those who do surveys in contemporary developing countries. Most governments at least formally support family planning, and the Kenyan government has one of the oldest family planning programs in sub-Saharan Africa, as well as a vigorous one. Thus, by being associated with the government (and the MOH in particular) we are assumed to be promoting family planning. In addition, assistant chiefs and miji-kumi's were sometimes quite present: in Kawadghone, the assistant chief hung around our vehicle, and in Owich miji-kumi's were guides. They did not sit in on interviews, and they stayed outside the compound, but they were visible. We made feeble efforts to keep them further away, but the supervisors were reluctant to antagonize them, and the efforts were sometimes unsuccessful.

One might expect that this would make people say that they are in favor of family planning when they are not. And indeed, many of the women's questionnaires, and some of the young men's (especially those who have had some secondary school) might appear to reflect this bias: many of these say that although they are not using now, they "intend to use" in the future, and many list network partners who "approve" of family planning. In informal conversations

it is very difficult to get people to say they don't approve of family planning: even a rather elderly miji-kumi in Owich said "people here approve of family planning", something that was patently not true; only with some difficulty did he finally admit that he knew a couple who were unhappy that their son and his wife appeared to be spacing their births.

Despite this presumed bias in favor of family planning, the government's reach would seem to be rather shallow in this respect--perhaps because the issue is too close to home for people to really care what the government thinks, but also because this is an area in political opposition. The chiefs, assistant chiefs and miji-kumi's with whom we talked are undoubtedly quite ambivalent about family planning themselves: to us they may say "people here approve of family planning", but it is doubtful that they speak that way when we are not around. We found considerable opposition to family planning--extending to, in some cases, chasing our interviewers away, or hiding when they come around-- and apparently little reluctance to admit disapproval of family planning in our interviews. The bias in favor of government programs should be greatest in the case of men, since government is an issue of men here: both the formal power structure (chiefs, assistant chiefs) and the informal power structure (the village elders) are men. Men could be presumed to be most influenced by the central government, yet they are the most likely to express complete opposition to family planning.

Thus, the support of the government was certainly necessary to do our field work at all, and it may have led some men (the younger, more educated ones) to exaggerate their interest in family planning; it does not, however, seem to have had such an outcome for the vast majority of the male respondents. If the men are resistant to government influence (of a subtle sort here) the women should be even more indifferent to it. Thus, we take seriously the suggestion from our interviews the response of many about "intend to use". We doubt that this means that all really do intend to use, but interpret it as a sign that they are intrigued by the possibility of family planning.

#### **QUALITATIVE PHASE: JUNE-JULY 1994**

In June and July of 1994 we interviewed 10 men and 10 women in each of our four sites, and conducted at least two focus groups

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<sup>11</sup> This section was drafted by Kevin White.

in each site (three in Obisa and Wakula South). The interviews were preceded by a week in Nairobi in which we interviewed potential interviewers, and then discussed our project and our questionnaire with those who were chosen. All of the interviewers were university graduates and Luo-speakers, and all were born and raised in South Nyanza. None, however, had any professional experience with family planning. We spent a week in each site.

We think these interviews and our observations during this stage were invaluable in giving us an understanding of our areas that informed the questionnaire and the sampling procedures for our quantitative survey; they also permitted us to investigate some topics--particularly the content of informal conversations about family planning and women's reproductive health--that could not have been adequately covered in a quantitative survey. This stage was also important in creating the team of interviewers that became our supervisors for the second stage.

Sampling for the semi-structured interviews: We had no list of inhabitants in our sites, to use for selecting couples to interview. The most recent sampling frame, that of the 1989 Census, was expected to be quite out of date (John Kekevole, personal communication). This describes the alternative procedures we developed on the ground. For the qualitative interviews, our aim was simply to be sufficiently systematic in selecting the couples that there would be no obvious significant biases. We developed our procedures in the first area in which we interviewed, Obisa, and then modified them subsequently.

We begin with a discussion of the procedures for selecting couples for the qualitative interviews then with a brief discussion of the selection of participants in the focus groups (2 in each site).

In principle, we began at the center of the location, and counted dwellings according in a pre-determined direction and for a pre-determined number of dwellings (this varied across the locations for topographical reasons: here we give the example of our first site). If, when we reached that dwelling it was empty or had no one who met our criteria (a woman of reproductive age, her husband or another man of an appropriate age) we went to the next dwelling. At regular and pre-determined intervals we returned to the road or to the intersection at the center of the sublocation, and then set off in a different direction, following the same procedures. We tried to make appointments to return for interviews, but sometimes we made interviews on the spot. Our study director introduced the interviewers and explained our aim, usually in terms of research on health and family planning.

Only houses within the boundaries of the sublocation were counted, and if the outer edge of the sublocation was reached before we got to the predetermined house, we would go back and take the other direction. (The sub-chiefs told us when we had reached the sublocation boundary, as this was not evident visually). Only structures which we could visually, from a distance, classify as a building in which people did not live were not counted; questionable cases were rare. If, in the process of going to the house, another house within 50 meters of the road but not visible from it which would have been between the counted last and the penultimate houses became visible, that one was taken instead.

If no one eligible was in that house, other people within the compound were questioned as to whether an eligible person would be back soon, and if possible, someone was sent out to look for them or a future interview was arranged with them through another family member. If a house did not qualify, or no one in it could be contacted, we proceeded to the next house, or houses, until an arrangement could be made.

After this first house (which was on one of the main roads of our intersection), we moved off the road. We went to the first path or road on the left of the main road we were following which did not lead directly to a single compound (i.e. a thoroughfare, not merely the entrance to a compound). We went down it, counted to the sixth house on the left, and, as before, either arranged an interview with the husband or wife, or went on. (If the path split, the path closer to straight was taken).

After arranging for these two interviews, we went back to the main road we had been travelling along, and followed the same procedure. We continued east, but this time we took first road or path on the right, and followed it down to the sixth house on the left, as before. Then we returned to the main intersection, headed in the opposite direction (i.e. west) for 16 houses, and repeated the procedure, except this time we only went on one side path, to the left. This procedure for selecting five houses was used in each of the two sublocations sampled in Obisa, and modified slightly elsewhere.

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<sup>12</sup>. The common types of these dwellings included small churches, school buildings, administrative buildings in chief's camps, shops, and small huts next to larger ones that were kitchens. Every building in which someone might live was counted as a house, and the questionable cases were rare. In trying to arrange an interview, we only hit a few huts which we had classified as houses but were actually used for storage or were particularly large and separated kitchens.



We were rarely refused an interview, but we were often unable to interview couples. Indeed, we often found either one or both members of an eligible couple absent, and went on to the next house.

This probably led to some underselection of the busy and socially active. During this phase, we realized that we had made several assumptions that were not correct.

1) Compounds vs. houses: In our areas, people live in extended-family compounds--usually containing a hut for the parents and huts for the married sons, their wives and children. For the first house chosen, we counted 5 compounds instead of 16 houses down the road, and took the next house. We then realized that this would tend to select against the people in larger compounds, and would give a random sample of compounds, not married women. Counting huts thus means assuming that married women are distributed evenly across visible houses -- not a perfect assumption, but better than assuming that they are distributed evenly across compounds. Areas with more married women per (visible) household will still be under-represented, and married couples in areas with a higher concentration of the elderly or unmarried children will be over-selected. Middle income compounds seemed to have the most houses per women.

2) Hidden houses: Sometimes it was hard to see the dwellings until we were very close to them. While going to a house hidden from the road (if it would have been the 16th) removes local bias against women in hidden houses, areas with lots of hidden huts within 50 meters of a road will still be under-selected.

3) On or off the road: A more obviously problematic assumption comes from how the households were selected in Obisa. Two-fifths were picked from decent tarmac or dirt roads, three fifths from paths or roads off these. This seemed to be an important difference -- almost all of the wealthier metal or tile roofed houses we saw were at least on a decent dirt road. The appropriate on/off road ratio should depend on the portion of houses in an area on the road. In Obisa, we at first suspected that more than three-fifths of the population lived on paths or roads more minor than those which make up the central intersections we started from. When, however, we mapped out the houses and roads for a small area, we found this ratio to be about right. This ratio will vary strongly by area and site.

In areas where more people live along roads, this problem would solve itself: The next path is apt to be a road, or a smaller path is apt to lead back to houses whose main access is a larger road.

Unfortunately, there were fewer and fewer roads in the subsequent sites, leading to a near total change in sampling procedures.

4) Distance from the sublocation center: We selected houses that were consistently a similar arbitrary number of houses from the sublocation's geographical center, as defined by the intersection. This is probably unimportant, as the sublocations boundaries in and of themselves are rather arbitrary and their geographical centers and boundaries have next to nothing to do with how their population is distributed -- both of the sublocations in Obisa were just regions containing residential houses and shambas around Obisa, while between them they split the town of Oyugis. But if there were any meaningful center or consistent internal structure to a sublocation, repeatedly selecting 16 houses away from that center might under-select the very center or extreme edges of the sublocation.

5) Initially we did not schedule interviews, concerned that respondents might prepare for the interview or spouses might concoct a joint story. But we decided that any fearfulness and preparation caused by prescheduling was probably quite limited, and in any case a less serious bias than the bias caused by the absence of couples when we arrived for an unscheduled interview. Thus, we began scheduling interviews.

In Owich and Wakula South, the attempt to base the structure of the interviews on roads fell apart. There were large areas of the place simply served by meandering paths, with no distinguishable road anywhere in the area. Instead, we divided the area up into subsections, estimated the proportion of the population in each subsection, and then designed an interviewing pattern which would get approximately the right proportion of people within each section, again in a prespecified arbitrary pattern.

Interestingly, despite the physical remoteness of some parts of our sites, news of our presence spread rapidly. Our fourth site, on an island, was relatively densely populated near the lake, but with a substantial proportion of the population living up the mountain. We arrived at the island on Sunday night; by Tuesday, when we began to interview up the mountain, two-thirds of the potential interviewees we met had heard what we were about. Apparently one particularly gregarious man had talked to a woman in Monday's focus group, and had spread the word to two families, a third woman had learned from her CBD husband, and another woman had gone down to the health center the day before and asked what the foreigners were doing around. The highlands may be physically

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Most CBD's are women.

isolated, but information can spread rapidly there -- the proportion who knew about us was similar to that on the coast.

Overall, the procedure for picking houses seemed sufficiently arbitrary to avoid many types of local selection bias -- we weren't consistently getting the high status, the sociable, or the rich, although we got some of these. Also, the rules devised seemed to get about the right proportions on and off major roads, and in the major geographical divisions of the sublocations -- coast or highlands in Owich and Wakula South. We followed the sampling rules strictly.

As everyone except one particularly busy woman in Kawadhgone agreed to the survey, that form of non-response bias wasn't a problem.

We have more concern, however, about absence from home. While fishermen away for weeks or family members working in distant cities could be considered only in the local social networks for the proportion of time they were actually there, and therefore interviewable, this argument cannot be used for those working in other parts of the sublocation just for the day.

Those for whom we could not schedule an interview because they were away from home were clearly not a random group. Farmers with distant shambas, fishermen, market women, and teachers, and members of small families (all of whom are absent so an interview can not be scheduled indirectly) might all have had unusual answers. If these absent folk are spending their time away from home at sociable place such as the market or school, they may actually be more influential than those hanging about at home.

#### Selection of Focus group participants:

We conducted two focus groups in each site, plus a focus group of CBD workers in Obisa and an (attempted) focus group of those who disapproved of family planning in Wakula South. The focus groups were conducted by two of our interviewers together; in principle they alternated as moderator and note-taker, but in practice both acted as moderators. Neither were trained as moderators, although all five interviewers attended a session on focus groups given by Dr. Tony Armstrong in Nairobi. The interviews were taped, and fully translated by the two interviewers. We understand that it is often

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<sup>13</sup> This caused one of the few arguments among the members of our team. The interviewers, Kevin White and Steve Green had made the arduous hike up the mountains on the island, and when they reached a house in which the inhabitants were to be interviewed found that they were visiting schoolteachers from Nairobi. The interviewers wanted to push on and get a local resident, but lost the argument.

the practice to translate only selected portions of a focus group, but we think this is a mistake: ours provided some quite rich material.

On the other hand, we made a number of mistakes with the focus groups.

Having them done by interviewers who had not been trained as focus group moderator was one, although it was compensated for partially by the familiarity of these interviewers with our project. Our most serious mistake was in the procedure for selecting the focus group respondents.

Participants for the focus groups were largely selected before we arrived. The study director asked the chief/subchief to do this. They said they did this by calling a baraza, and designating certain women to meet with us for the focus groups. The criteria set were that the women be married, with no more than a primary education (women who go to secondary school in these areas are relatively rare), and either age 20-29 (for one focus group) or 30-39 (for the second).

These criteria were transmitted by the study director to the chief when he visited to make arrangements, and then in a subsequent letter.

We should not have had the chiefs make the selection. It was difficult to find out just how they made the choices they did: we talked to the chiefs in Obisa, Owich and Wakula South, but could not get them to say more than that they followed our instructions.

We did learn, however, that they were concerned to have all the villages represented (presumably to receive equally any benefits of our visit). And a rough comparison of the women who came to the focus groups with those chosen systematically for the qualitative interviews, suggests that the chiefs either misunderstood our criteria or, more likely, selected their most "presentable" women from those that met our criteria, or deliberately altered our criteria. In addition, we think that they probably selected their relatives first and foremost, since this became an issue in some sublocations in the subsequent phase of our research when we rejected some of these relatives as interviewers.

In particular, the participants often had more than a primary education, and seemed to know a bit more English. In Obisa, Kawadhgone and Owich more women wanted to participate than could be accommodated; in contrast, on the island only about half the number expected showed up--and these included not only the sub-chief's wife, the wife of the former director of the clinic, a woman who was clearly

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<sup>14</sup> Although we did not pay the respondents in the interviews at this stage, we did pay focus group participants KSh100 (about \$US\$1.50 at the time), since they had to leave their homes and work for several hours. The chiefs did not know this at the time of the selection, however.

post-menopausal but was the head of her local women's development group, but also several women who had been trained as community-based family planning workers. On the island, we tried to arrange for a third focus group with people known to be opposed to family planning, but were completely unsuccessful: several contraceptive users attended (we held the focus group anyway).

We therefore think that the attitudes expressed in the focus group about family planning are likely to be disproportionately favorable. Still, however, we think some parts of the focus groups are illuminating. Occasionally the women seemed to forget where they were, and to become energetically involved in extended conversations among themselves--for example, when they were talking about the "secret use" of family planning, or relating stories about side effects (e.g. a woman who took pills but gave birth to a child with four eyes). In these episodes, which sometimes went on for extended periods without interjection by the moderator, we may have glimpses of the way these admittedly unrepresentative women may talk among themselves.

### Translation

The interviews in the first stage were taped, and lasted from 45 minutes to an hour and a half.

Individual interviews: For the Obisa interviews, the interviewers translated directly to a typist. This proved to be quite timeconsuming, so we switched to having the interviewer translate and handwrite, and then it was typed by Watkins, Rutenberg, Green or White. Translators were asked to translate literally. The typist cleaned up the grammar. After typing, the typist and the interviewer/translator went over the interview together. Where literal translation didn't make sense, or when local terms were used, the interviewer explained. In the transcripts, these are put in brackets or parenthesis, usually with the translator's initials.

In addition, the interviewer/translator often added information that was not part of the transcript, such as a description of the dwelling, what the respondent and children were wearing, whether the children looked malnourished or "plumpy"; this is included at the beginning of the interview.

The interviewers became much better over the course of the four weeks--although the first male interview was one of our best. But because we spent a lot of time going over the interviews, asking questions, suggesting areas that might be probed, they came to understand the topics of our research quite well. Some of the

interviews are better than others, however. In some case the respondent was not feeling well, or busy, or reluctant to elaborate.

In others the interviewer was not feeling well, or bored or inattentive. This is not surprising, as our days were long: all the interviews were translated (and typed) in the field, which meant very long days. By the end, on the island, all of us were tired (and two of the interviewers had malaria).

We had five interviews (one from each interviewer) transcribed and translated by someone not connected with the project. Although there were occasional differences in wording, the translations were very close.

Focus Groups: Translations for focus groups were done by the moderator and note-taker, who could often remember who said what, and fill in for physical gestures --e.g. in one focus group a participant refers to "things they put in your hands" but points to her upper arm, and later says Norplant. Or when they are talking about women's health issues, they point to the area of their abdomen just below their waist. The translators also evaluated some of the women, e.g. saying "so and so wanted to show that she was experienced."

### **Summary of the sites based on the fieldwork of June-July 1994**

It is worth describing what we learned about our sites, since this influenced the sampling procedure used in the second stage, and our questionnaire. Although the second stage provided larger numbers of respondents to confirm some of our impressions, we present here the description based on our field notes from the first stage, since rarely were these contradicted by our subsequent research (and we note where this is the case).

Because we were interested in the extent of social interaction between our sites and other parts of Kenya, we paid attention to transportation and communication. The people of our areas are linked to the rest of Kenya only with some difficulty. A few simple community-level measures confirmed our categorization of two sites as "relatively open", and two sites as "relatively closed". We noticed the presence or absence of electricity, TV aeriels and newspapers, and asked about matatu routes to and from our sublocations to other areas, about telephones, about the nearest markets, and about the level of health and family planning services available. Obisa and Kawadghone were expected to be more closely linked to the rest of Kenya than Owich and Wakula South, and they were. From Oyugis there is frequent matatu service to Kisumu, the

largest city in western Kenya, and thence to Nairobi, over fairly good roads (at least most of the way); Oyugis town has a large market and a district health centre, and the town (but not its hinterland, which included our sublocation) also has electricity, telephones and stores that sell Nairobi newspapers. Residents of our other sublocations could only reach Nairobi by going through Homa Bay, the district capital. Kawadghone was connected to Homa Bay by frequent matatu service, and thence to Kisumu and Nairobi; there was a telephone in the clinic in our sublocation, but no electricity.

Owich and Wakula South were far more isolated. Owich is connected to Homa Bay by one daily matatu to and from the town of Migori, and from thence to Homa Bay: the trip from Owich to Homa Bay directly took us two hours over terrible roads that are impassable in the rainy season. To reach Homa Bay from Wakula South requires a 2 hour boat trip to the mainland, and then a 2 hour trip over terrible roads.

Transport vehicles from Wakula South to the mainland, and from the mainland to Homa Bay, are few. Neither Owich nor Wakula South has electricity or a telephone; nor are newspapers available. Wakula South has a health center, but residents of Owich have to travel (walk, usually) 9 kms to a Catholic Mission dispensary, or a further 5 kms to a government dispensary.

There are also two, more indirect, measures of exposure to the outside world. In Obisa and Kawadghone, the sight of foreigners wandering around was not very surprising. Even in Kawadghone, however, when our team got separated at one point, we were re-united by passers-by who knew not only where the whites were, but where the Nairobi Kenyans were. In Obisa while we were waiting at the vehicle for an interview to finish, people looked at the car when walking by, but didn't stop. In Owich, wherever our car stopped crowds gathered--of women and children around most places, of men in the "shopping center". People in Wakula South appeared to take us more in stride than in Owich (although a few babies cried when they saw the whites, and we were frequently asked what we thought of the island).

A second indirect measure of exposure to outside influences, and one that probably colors our impressions most, is the extent to which we were unable to locate couples at home for an interview.

In the sublocation of Obisa, people were often absent during the day--attracted by the town of Oyugis--but were eventually contacted.

In Kawadghone, those who were not home when we arrived were usually further away, often because the the husband was working outside the area. In Owich, although the walking was particularly difficult (a very large sublocation, with high hills) the people were usually there when we arrived: as one of our supervisors said, "The men are just there, chatting with their [several] wives." In Wakula South,

women were likely to be at home at least for some part of the day, but many men were fishermen, difficult to interview because they left for days at a time.

Despite the geographic isolation of Nyanza, the people of the area interact with those in other parts of Kenya. The main reasons to travel long distances are migration for work (primarily male), family visits, and funerals. Many men in the households where we interviewed currently work, or have worked, elsewhere in the country, not infrequently in Nairobi or Mombasa (on the eastern coast). The minimum required visiting (for husbands as well as other relatives) appears to be once a year, at Christmas, but many of our respondents saw their relatives from closer places far more often than that, either receiving or making visits. Funerals are a major stimulus to congregation: it is virtually a cultural requirement to return home when a relative dies, and funeral ceremonies last a week or two for adults. In addition, for funerals on the affine's side, one brings "friends" to demonstrate one's social standing. We found that at least one member of many families would speak some English, and many claimed to speak at least some Kiswahili.

Short-distance travel appears to be primarily to larger markets, and to hospitals/clinics. We were particularly interested in larger markets as a place where men and women from different areas of the sublocation might congregate and "gossip", as well as an indication of commercial development. Oyugis has had a large market since pre-colonial times, and has a government health center and private clinics. Kawadghone has 3 markets on its boundary, though getting there would be about a 45 minute walk from many of the homes in the sublocation. Neither Owich nor Wakula South had anything that could be counted as a market. In Owich, a few women were selling things outside the posho mill near us (where women came to grind grain for Ugali, a staple of the diet): a basket of tomatoes, a basket of "small fish", a few onions, matches, a bit of salt, a bit of tumeric. In Wakula South, in the evenings some women sold a bit of food. Each of these places had "shopping centers", a collection of rickety huts, a few of which sold soft drinks, beer, cigarettes, matches, soap, salt, and where young men hung out. To buy other things, the people of Owich had to go to the twice-weekly market at Magunga, about a half-hour ride away (although most probably walked), and those of Wakula South went to the thrice-weekly market at Sindo on the mainland, an 1.5 hour boat-ride away.

Given the lack of transport in these areas, one would expect

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<sup>15</sup> Thus, somewhat perversely, AIDS may stimulate social interaction.



people to meet each other when they are walking around, and they do. All areas are criss-crossed by paths. In Kawadghone and Owich, these paths were somewhat like driveways, connecting a single compound to a public thoroughfare (i.e. dirt road). In Wakula South, particularly up in the mountains, the paths went directly through people's compounds, and people expected passers-through to sit and chat. The central gathering places are the market (in Oyugis) and the beaches (in Owich and Wakula South). We found no evident gathering place in Kawadghone. Social life is quite gender-specific, and men and women tend to gather informally in different locations. For example, at the Oyugis market, goods sold by men (e.g. hardware) were arranged together, and goods sold by women (e.g. food) were together. There are "men's areas" on the beaches of Wakula South and Owich and "women's areas": Men bathe in some areas, women (and children) in others.

The main places where women meet informally on a regular basis appear to be the market and the posho mill and the rivers or beaches where they collect water. In Obisa and Kawadghone the market is probably the most important central place: our sense is that women go to the Oyugis market almost daily. Kawadghone itself doesn't have a market, but there are three modest-sized markets on the boundaries of the sublocation, within walking distance. In Owich the local market is at the posho mills: the one near our accommodation was tiny (3 or 4 women selling a few things to other women as they come to grind their grain), and in Wakula South the "market" seems to be a few women selling a few vegetables in the evening at the "shopping center". In Wakula South and Owich the lake is particularly important; again, most women go there daily to collect water. In addition, we saw women at the "shopping centers", but there were usually far more men than women there. Women also meet, but more irregularly, at church and at school meetings.

The men's equivalent of interaction at the market or the beaches seems to be lounging around the tea-houses or "hotels" of the "shopping center". In Oyugis, the shops are next to market; men also sell in the market. In Kawadghone, it's not clear where the men gather; perhaps while quarrying stone. In Owich and Wakula South, men gather at the "shopping centers" and on the beach. In addition, men collect less regularly at church and school meetings, and at barazas called by the chief or the assistant chief: we understand that typically the assistant chief has baraza about every other week, whereas the chief's baraza occurs more rarely, e.g. when there is a major outbreak of malaria, immunization program, visitors from outside, etc.

We turn now to two other topics of interest to us, cultural

and economic heterogeneity and homogeneity; we assume that both cultural and economic differences are probably a hindrance to social interaction. Our areas seem to be quite homogeneous in both respects.

In terms of culture, the entire province is predominantly Luo, and certainly there is a belief that Luo customs are pretty much the same across "Luo-land", a term used by our interviewers and respondents. In response to our questions about conversations on health, wealth flows, and family planning, few of our respondents said they had had conversations with people who weren't Luo, and these were usually men who had worked outside the area.

Marriage also links Luoland together. Marriage across ethnic lines is rare among our respondents. Marriage, however, is geographically exogamous. Most wives have married into each specific site from other areas; these, however, were areas that were not only Luo but also likely to be in S. Nyanza. Housing patterns are also similar. The area is one of the more polygamous in Kenya, and most people live in extended-family compounds in which each adult woman has a hut. In terms of religion, the areas are primarily Seventh-Day Adventist or Catholic, although there were substantial minorities in other Protestant sects (e.g. Roho, a Pentacostal group). Our sense was that church participation is quite important in our areas, although we think that the Vatican's stand on family planning either has not penetrated these areas very successfully or is not taken very seriously.

The economic base in our four sites is largely subsistence farming (many respondents said they had no source of income, which was confirmed in the household survey) and very small scale trading, what our respondents described as "something small"--selling a few vegetables or firewood in the market, or buying, drying, and selling "small fish" (sardines) in Owich and Wakula South. In Kawadhgone, one common attempt to generate local non-agricultural income was through quarrying and cutting stones in Kawadhgone, and in Owich and Wakula South there were quite a few fishermen. All of the areas, except possibly Obisa, have had at least several years with "too much sunshine", i.e. drought, and many respondents talked about hunger. Few respondents--and virtually no women--have a regular source of income; those who did were predominantly teachers or worked outside the area. There are a few cash crops.. All compounds owned

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<sup>16</sup> The production of maize, sorghum and rice declined between 1989 and 1992; the district is a net importer of these staple foodstuffs. Maize production declined by nearly 40%. (Homa Bay District Development Plan, p. 310.

<sup>17</sup> We did see some cotton growing in Kawadhgone, and pineapples are a cash crop around Obisa. Cash crops (e.g. seed sorghum, cotton, tobacco, simsin and sunflower) are grown by 8% of the households in the district, although this varies

some land which provided some basic foodstuffs for the family to eat (primarily corn, millet and sorghum) and perhaps a bit left over to sell. We think most compounds also owned some chickens and some grazing animals (primarily cows, but also goats). The grazing animals are primarily used to pay bride-price, although they may sometimes be sold to pay school-fees. None of our respondents mentioned getting any income from selling milk, although we were told that women do sell milk to neighbors; nor did they mention selling eggs.

Differences in poverty/wealth across the four sites and across the individuals within them appeared to us to be small, at least compared to the variation in Kenya. In all four, poverty was evident in the houses. These were small--the average about 15' x 10'-- most of which were made of mud (mud-walls and mud-floors) with peaked, grass-thatched roofs. There was a sprinkling of tin roofs and some stone houses and buildings (particularly in Kawadghone and Owich, both of which are quite stony); concrete houses and tile roofs are quite rare. Furniture was almost invariably sparse--a few chairs and a table, a bed visible behind a screen--although we did interview in a few places with a "sofa set", and one respondent had a rather elaborate stereo set that ran off a car battery. The walls were either bare, or decorated with a calendar(s) (usually out-of-date), pictures (e.g. of birds or trucks) cut out of magazines, and an occasional political poster for the opposition party; the most substantial houses sometimes displayed family photographs of important occasions (weddings, graduations). There were few signs of consumer goods in the houses: these were mainly plastic utensils for carrying water or food. Clothing was invariably "western" in style, and probably second-hand; we rarely saw blue jeans, and almost never on women (although they appear to be considered very fashionable).

Even within areas that appeared to us largely economically

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by Division; in the division of Gwasi, for example, the percent of households with "high value cash crops" is "negligible" (Homa Bay District Development Plan, p. 30 and p., 32).

<sup>18</sup> The average size of land under small holder cultivation is 3 hectares (Homa Bay District Development Plan, p. 19). Corn is the main crop (508,520 bags in 1992), sorghum next (193, 695 bags in 1992) (Homa Bay District Development Plan, p.1 30, Table 2.1).

<sup>19</sup> The Homa Bay District Development Plan shows that out of a total of approximately, 150 million Kenyan shillings in livestock products sold in 1992, about 59 million KSH was Eggs, 36 million KSH was milk, and 41.5 million KSH beef (p. 69, Table 2.30).

homogeneous, however, our respondents made distinctions. In the second, quantitative stage of our research we asked respondents whether their network partners were "better off", "worse off", or "about the same". We didn't ask for their criteria, but our interviewers told us what they mentioned as they mused about the answer: "so-and-so is better off because his son has a job with a regular income"; "so-and-so has a bicycle so I guess he is better off than I am"; "so-and-so has a sofa set, so she would be better off". We think a son with a steady job, land and cattle, many children (especially daughters, for whom the family can expect cows as bride price), tin roofs and a few other consumer goods like watches, bicycles, and a sofa set appear to be the main considerations in our respondents' system of economic stratification.

## **HOUSEHOLD SURVEY**

### Pretest

The questionnaire for the second phase of our study was developed in the U.S., and pre-tested in November, 1994. The five interviewers from the first stage interviewed a handful of people in each of our four sites. In retrospect, our pre-test was too skimpy. It would have been useful to interview more, and also to attempt to train one or two local interviewers to administer the pre-test. Our interviewers were so experienced that they handled the problems of interpretation, of coding, of the order of the questions that surfaced later (see below).

### Sampling for the household survey

We knew from our qualitative phase that a significant proportion of our sublocation populations were likely to be out of the village at any given time: both men and women would be away for funerals, women would be visiting relatives, men would be working out of the village. We thus decided to begin with sample sizes of 250 women and 250 men, so that the final number of completed interviews would approximate 200 men and 200 women in each of our four sublocations.

Our sample was chosen to represent well only our four sublocations, although we think it also probably represents Nyanza Province reasonably well. As noted earlier, cultural patterns are said to be quite similar across Luo-land, and our impression is that the economic and social life of most of Nyanza Province is much like that in our areas, with the exception of the few towns.

In choosing a method of sample selection, we were mindful of of the chasm between sampling theory and the facts encountered on

the ground. One of these facts was our budget, which would have been badly strained had we tried to list all the members of the sublocation and then sample from that list. Another is the supervision of the interviewers. While sampling theory might suggest that we would be better served by taking a smaller fraction of the residents of a larger number of villages in each sublocation, we were limited practically by the need to supervise the interviewers, coordinate logistics and call-backs, and to respond with flexibility to difficulties that arise in the field.

We decided, therefore, to do complete censuses of randomly chosen villages in each sublocation, interviewing all eligible women and their husbands living there (eligibility is discussed below).

Our experience with the first phase of the research suggests that little variability exists from one village to another within a sublocation, so the inclusion of more villages from the same sublocation would be unlikely to provide us with much new data, while greatly increasing our expenses and complicating the tasks of supervision and coordination. We can make a plausible argument that these complete censuses of villages within a sublocation are providing the same quality of data as would partial samples of all of the villages within the sublocation. Since the villages can be randomly sampled, we will have village means with no sampling error.

If these means can be plausibly generalized to the rest of the sublocation, as we are arguing they can, then we have an extremely precise picture of what is occurring in these sublocations. In addition, this design permits close supervision (e.g., oversight of work done, potential for responding to questions from interviewers), facility of call-backs to absent potential respondents, and greatly reduced logistical demands (e.g., transporting and feeding interviewers, supplying questionnaires and pens) without sacrificing data quality. Because of the homogeneity of our areas, we were not greatly concerned to stratify.

The villages in our areas are based on clans, and residence is patrilocal; thus, in principle all the men in a village are related to each other and, at least distantly, to men in the other villages.

In Owich and in Wakula South, there were a few fishing villages, inhabited largely by men from other areas; we excluded these from consideration, since we understood that the inhabitants were primarily temporary, and usually men who were either unmarried or staying without their wives.

Many of the inhabitants know the clan boundaries pretty well--as we found out when we were in the field--but the geographical boundaries of villages are not marked, and social interaction appears

to flow easily across them. Because of the homogeneity of our areas, we were not greatly concerned to stratify our sample. In Obisa and Kawadghone, which are rather flat and with fairly evenly distributed compounds, the main difference among villages appeared to be the number of compounds; In Owich and Wakula South, there were distinct patterns of geographical settlement within the sublocation, with part of each population living near the lake and part in the hills. Since we expected this to influence social interaction, we were concerned to represent both areas proportionately.

As noted earlier, there was no list of villages and their inhabitants, nor, indeed, of the individuals in our sublocations. We were assured by our study director and interviewers that the miji-kumi's --each of whom is responsible for a clan village that in principle consists of 10 households (kumi means 10, in Kiswahili)--know everyone in their village: "They know them." In a preliminary phase in November, 1994 (while we were conducting the pretest, and afterward) we asked the chief to call together the miji kumi's. Using one of our interviewers as a secretary, we asked them to list the households in their village, along with the names of the adult members of the household, specifying the household head, and noting if someone was outside the area (e.g. working in Nairobi) or if a wife was over reproductive age. Typically, several miji kumi's worked together, and would remind each other "you forgot so-and-so".

We were aware that selecting the villages was a public relations as well as a statistical issue: we had learned that the village powers expected our research to bring benefits to their community, and that they were concerned about just which villages would benefit. Therefore, once the list was made, we asked the miji-kumi's to draw slips representing the village from a hat until we reached enough villages to give us 250 women and their husbands. In Obisa and Kawadghone, we had two hats, one for the smaller villages and one for the larger, and selected from them alternately; although in principle each village is supposed to contain 10 households, in fact few were as small as 10, and some contained 50 or even 60 households. In Owich, we selected alternately from "lake" and "hills" until we had what we thought, based on our experience in the qualitative phase, represented the proportion living in the "lake" and the "hills" in the sublocation (about 50-50). In Obisa, our final sample included

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<sup>20</sup> The blurred boundaries of Kenyan villages is also noted by Kok (1986).

<sup>21</sup> The exception to this was Owich, where the selection was made in Nairobi.

12 out of the 18 villages in the sublocation; in Kawadghone, 10 out of the 16 villages in the sublocation; in Owich, 10 out of the 21 villages in the sublocation (half from the lake, half from the hills).

In Wakula South, there were only 5 villages, and all were included: thus, for this sublocation we have a population census rather than a sample.

Once the village was selected, we worked with the miji-kumi's to refine the lists. There is a basic contradiction between the miji-kumi's criteria for eligibility and ours. The miji-kumi's lists were based on men, because it is the male's lineage that determines his residence in a village. Marriage is exogamous: ideally, daughters leave the village upon marriage and do not return, whereas other women enter the village by marriage and leave after separation or divorce. In contrast, we followed standard demographic practice and focussed on women of childbearing age. Women were eligible if they were of reproductive age, ever-married, and living in the village; men were eligible only as the spouse of an eligible woman. Thus, a widow is eligible, but a widower is not.

The lists were a mixture of de jure and de facto. They were de jure because they listed everyone who should be in the village (i.e. male members of the lineage and their wives) but they excluded adult women who had married out of the lineage, although the woman may have divorced and returned to her home village. They were de facto because the list noted who was presently living in the compound and who was working away. We tracked down respondents who were working nearby--for example, we interviewed respondents in the town of Oyugis, and on fishing islands near Wakula South--but did not follow respondents to Nairobi or other distant locations. In addition, we asked the miji-kumi's to note which women were not of childbearing age, i.e. "old". Although our collaborators assured us frequently that the miji-kumi's would know everyone in their village, we were prepared to find that the lists were not fully accurate.

We found immediately that when we were told that the miji-kumi's "know everyone", what was meant was that they know the males. The lists included compounds headed by long-deceased males, whereas women were often listed only as "wife 1" or "wife 2", or by a name representing her place of origin, e.g. "Nyar Kano", or "Daughter of Kano". Because we were concerned about the accuracy of the miji-kumi's memory, from the beginning we asked the supervisors to make intensive efforts to find if anyone had been omitted. Omissions were rare, but there were some (see below).

But it was only in the last days of the fieldwork for our second

phase that we discovered other omissions that were due to a gap between the ideal residents of the village and the actual residents of the village. In Wakula South, we interviewed everyone in the sublocation, and thus in principle we would be able to see which individuals were central to sublocation networks and which individuals appeared to be isolated. We thus asked interviewers to get the full names of those with whom their respondents chatted, not just the first names as we had been doing previously. While in the field, we began to try to make linkages: thus, if a respondent said she talked with Esther Achieng who lived in the village, we looked for Esther Achieng's questionnaire and respondent number.

In doing this, we discovered that there were individuals named as living in the village but who were not on the miji-kumi's list. Unravelling this, we discovered, much to our surprise, that there were occasionally unrelated individuals living in the village--some of whom had been living there for many years--and we discovered a divorced daughter of a compound head who had returned to live in her natal village. The supervisors had known about these types of village residents from the beginning, but had assumed that they were not eligible because since they were not members of the lineage, they were not really residents of the village and thus not eligible for interviews. Thus, just as our lists were a mixture of de jure and de facto, so was our sample: de jure because we began with the people who resided in the village on the basis of their lineage (omitting the unrelated individuals), and de facto because we only interviewed those who were in the village at the time (i.e. we did not trace men who were working and thus living elsewhere in order to interview them).

### Completeness of Enumeration

Evaluation of the completeness of enumeration depends on the quality of the lists with which we started. As noted earlier, these lists were developed by clan elders, usually two or three working together to make a list of households in their village and of the eligible people in them. We did not expect these lists to be completely accurate. Although some of the miji-kumi's were middle-aged and literate, others were quite elderly, infirm, and illiterate. Simply looking at the original lists suggested ways in which they might be inaccurate. Most importantly--since the basis for our sample was women of child-bearing age who were residents of the village (and then their spouse)--the miji-kumi's lists showed a bias towards males. This is not surprising. The miji-kumi is a clan elder, and it is males who are important in the lineages that they represent, and in the formation of new households (and, eventually, new lineages: see Ocholla-Ayayo, 1976; Blount, 1975; Cohen and Atieno, 1989; Parkin,



1978). In addition, social life in our areas was largely gender-specific: our survey showed that the network partners of women were overwhelmingly other women, and the network partners of men were overwhelmingly other men. Thus, the clan elders almost certainly knew less about the comings and goings of women than of men. On the lists women, and especially younger wives, were sometimes only noted as "wife 1" or "wife 2", or referred to by a nickname that represented her place of birth (e.g. Nyarkano, "daughter of Kano"). In addition, we thought it unlikely that the miji-kumi would accurately know whether a woman was of child-bearing age.

We also had to be concerned about possible omissions from the list of eligible respondents, especially since those omitted might be less socially active than those that were remembered--they might be those (probably men) who either were away a lot, or who perhaps were unsociable. In addition, the patriarchal nature of the society suggested that we might be particularly likely to miss compounds headed by women (widows). The patterns of geographical settlement and the patterns of social interaction helped us locate these people, as did the use of local interviewers. With the exception of Owich, paths criss-crossed the area, and supervisors would enter any compound they saw that appeared to be in the village and ask who lived there. As our areas were rather densely populated, the compounds were visible and hard to miss; this was the case even in Owich, the largest and most dispersed of our sublocations. Compounds were less visible in Wakula South, where the underbrush--especially in the hill area--was quite thick; still, we discovered omitted compounds even there. In addition, the use of local interviewers helped us.

We did find some compounds that had been omitted. There may have been more that we simply never found. In each location, we discovered between 5 and 10 compounds this way (out of a total of about 100 compounds in each location). In an project to map all the compounds in Wakula South, conducted by Kevin White in June and July 1995, a further 10 or so compounds were discovered. Usually the "found" compounds were small compounds that were headed by an elderly widow, or had recently been formed when a married son moved outside of his father's compound to set up his own just outside the gates. In one case, the head of a "discovered" compound thought it had been omitted by the miji-kumi because the miji-kumi didn't want the compound to get any of the benefits that our work might bring. On the other hand, in Wakula South we found an omitted

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<sup>22</sup> In Obisa, we found 3 new households (i.e. couples). In Owich, we found 8 new households (one with three wives, one with two).

compound that was right next to the miji-kumi's compound. The compounds discovered in the summer of 1995 were primarily in a very remote part of the sublocation: the subchief said that although the children in those compounds attend the school of the village, they also consult the miji-kumi in a neighboring village in another sublocation, and thus might have been identified by our interviewers as belonging to that other village. In addition, we think that some of the compounds we "discovered" were in fact ineligible: thus, interviews were done in several homesteads where we think that the respondent lied about being in a particular village--again, our supervisors speculated that these respondents wanted to be interviewed because they thought it would bring them benefits. Some support for this view comes from the fact that we found 13 duplicate interviews in Obisa--people who sat through the interview again without telling the interviewer that they had already been through it (in only one case do we have a note from an interviewer that a woman refused to be interviewed because she had already been interviewed). We found some men who claimed to live in a village, although they were not on the miji-kumi's list, and whose wives we did not interview: since we did not know that the wives were eligible, we did not consider the men to be eligible either. There is the possibility, however, that we simply missed these wives. We also found several women in each site who were interviewed but who gave the names of compound heads and spouses who were not on the miji-kumi's list. Although they may be additional wives to men we had interviewed, it is also possible that we should have interviewed their husbands but didn't. Since we know they are eligible (living in the village and of child-bearing age), we have included them in the data set.

In addition to looking for omitted compounds, we were also alert to the possibility that individuals within a compound might be missed.

When the supervisors or interviewers first made contact with a compound, they asked who was staying there: in this way, we sometimes added extra people (usually additional wives or people who had returned from working elsewhere). The compounds are small, with the huts of closely related people (the household head and his wife/wives, his adult sons, and their wives and children) typically arranged in a circle on a plot of land no bigger than a substantial lot in a middle-class suburb in Philadelphia. People within a

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<sup>23</sup> In Obisa, 4 new men (including one in a village in which we only interviewed men, so the wife was irrelevant); in Owich, 3 new men.

<sup>24</sup> In Obisa, we interviewed four wives who could not be attached to a man in the village, in Owich one.

compound live within eyeshot and earshot of each other: although they may not speak frequently, as our survey showed, it is highly unlikely that they do not know who is living in the compound. We also found out that some women were too old to be eligible for the survey, or that women whom the miji-kumi had categorized as beyond childbearing age were in fact still giving birth. We interviewed, but do not categorize as "found" women whose age was younger than the miji-kumi had thought, or men or women who had returned from working elsewhere. We also found individuals who claimed to be in a particular sampled village but were not. For example, we interviewed two women and their husbands who claimed to be in one of the villages in Obisa, but both the miji-kumi who made the list and another one who was passing by later insisted that these families did not live in this village. In Obisa people seemed rather more eager to be interviewed than elsewhere (perhaps because they thought the interview would bring them benefits), so perhaps they were lying. As another check, our supervisors also asked respondents who their miji-kumi was. Although in general people knew who their miji-kumi was (indeed, most people knew in what m-k's village a particular compound was located), we occasionally found people who didn't.

In summary, the lists appear to have been largely complete with respect to members of the lineage and their wives. The names were often somewhat inaccurate, because the miji-kumi's tended to use nicknames--the names they knew these people by as they were "walking around"--and, in the case of women, often didn't know the name at all. Some women were incorrectly thought to be "old", others who were thought to be of childbearing age were not. But we think few de jure residents living in the village were entirely omitted.

Other errors in the list were easier to discover and rectify. The main omissions were women, who appear to be of relatively little interest to the miji-kumi's, especially second or third wives. More minor errors had to do with the age of the wife. In addition, during the time between our listing and our survey demographic processes continued: some people died, some women married into the villages and some marriages dissolved, some men (or couples) who had been working outside the area at the time the listing was made had returned by the time of our field work while others had left.

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<sup>25</sup> Our study director, Charles Onoko, explained this by saying that the miji-kumi's don't even notice young boys; it's only when the boys become youngish men that they start to ask "who is that", and is then likely to be given a nickname. The elders pay much less attention to women.

<sup>26</sup> Because of the importance of funerals, we expected the distinction between dead and alive to be particularly accurate. Most of the deaths appear to have occurred between the time of listing and our survey, although we did discover

### Completeness of Coverage:

Whether or not coverage of respondents who are meant to be covered is achieved is fairly important in any survey, but seems particularly important in our attempt to study social interaction.

It is likely that those not interviewed are different from those who are; in our case, those not interviewed are likely to be different in ways that are directly relevant to the topic of our study. We expected to be able to find more easily those people whose activities, and presumably their social networks, were rather limited, whereas those whose activities took them hither and yon, and whose social networks would presumably be larger and/or more diverse, could be expected to be more difficult to find.

For this reason, we were particularly energetic with call-backs.

We made a first visit and at least two call-backs; in the first two sites, Obisa and Owich, the call backs had to be on different days, but in the second two this was modified--if the person was visited in the morning and we were told that he or she would be back in the evening, we returned then and counted it as a separate visit.

Relatives and neighbors were often fairly knowledgeable about where the person was--"at the market", "at a funeral", but also often did not have a good sense of just when the person would return--in part because there does not seem to be a very precise schedule to daily life in these areas (e.g. walking to the market may take longer than one estimates, and in any case few people have watches). At all sites we know from comments at the end of the day that interviewers often made more than three visits to find the person, some of which involved a lot of going back and forth, or a lot of waiting around.

Some of these efforts were rather heroic. In Kawadghone, we learned that one respondent was working in a quarry in the sublocation, but not which quarry: he was tracked and finally located in the third quarry. In another case, an interviewer happened to see a matatu going by, driven by one of the respondents he was searching for: he commandeered our vehicle, and followed him to the town of Oyugis, where he was interviewed successfully (this anecdote also illustrates the value of local interviewers). Although our completion rate was high, we did miss some. Some of the most active people (e.g. going back and forth from Nairobi) happened to be home when we visited, but others--a hawker of second-hand clothes, a travelling businessman--we never did reach.

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a case where the miji-kumi thought the person was alive but he had died some years ago.

A second, and important, source of incomplete coverage was the dynamism of life in our areas. Although our listing was done only a month or two before our survey, time had passed, and people's lives had changed. The sites in which we worked were out of the mainstream, but there were still a lot of entries into and exits from our populations. Some of the people on the miji-kumi's list had subsequently died, and some of the relicts had remarried; some people had moved away and others who were working elsewhere had returned; some marriages had fallen apart and the women had left, other men had taken additional wives--which in some cases made men who had been ineligible for inclusion in our survey eligible (e.g. a man with elderly wives who added a wife of child-bearing age).

A third, and we think minor, source of error is the eligibility of women by child-bearing status. Not only could the miji-kumi not be expected to know this, the supervisors (occasionally an interviewer) sometimes made the determination that a woman was "too old" or "still of childbearing age". When the woman herself did not know her age, which sometimes happened, this was determined either on the basis of appearance, or by asking whether the woman had had a child recently. Although there was often an indication on the cover of the questionnaire about the age of the woman--e.g. "this woman is 70", some women were excluded because they appeared old--though just what this means to a young interviewer is not clear--and had not had a child "recently", or the woman was "very very old". The determination was probably not influenced by the extent of social interaction of the woman, and so would not offer an obvious bias; however, some women might have been excluded on a combination of age and recency of childbearing who looked younger and had not had a child recently because they were using birth control. women may have been birth control users. We also found women whom the miji-kumi had thought were beyond child-bearing age who were younger (sometimes much younger) and we did interview them.

For all four sites, but less so for the first, Obisa, we came very close to covering everyone on the miji-kumi's list: we either had completed interviews for them, or had information that explained why not, provided by relatives in the compound or, in the case of compounds that consisted of a single couple or where everyone was out, by neighbors. In Obisa, we apparently failed to contact 5 people who were on our list. Two of these are miji-kumi's who appear on our lists as having elderly wives, but for whom we had women's questionnaires listing them as spouse. It seems unlikely that the miji-kumi who made the list would have forgotten a wife, even a very young one, so we think there is a possibility that the women's questionnaires are wrong, and these two miji-kumi's may not have been eligible. The other three appear to have been genuinely overlooked: there is no indication that they work elsewhere, or any

other reason for not being available for an interview. Of these, one was--according to a supervisor, whom we queried when we found the interview was missing--was a person likely to have particularly active networks: the dealer who travelled buying second-hand clothes.

In addition, we failed to contact 5 women who were not on the miji-kumi's list but who appeared as extra spouses on male questionnaires. We think there is some likelihood that these are former wives, and were not living in the village at the time: in one case (with two unlisted wives), the supervisor recalled that the respondent in fact said they were former wives. And we lost three questionnaires for which we have a record that the visits were made but data were either not entered or lost: one of these was definitely interviewed (the supervisor recalled it) and the other is married to a man living in Eldoret, so she may in fact have not been present in the village. We are also not counting as eligible several men who were not on the miji-kumi's list but who were interviewed, listing spouses whom we didn't interview: we believe these may have been men who did not actually belong in our sampled villages, but for one reason or another agreed to be interviewed.

In our second site, Owich, we interviewed all of the women and all the men on the miji-kumi's list. We "found" one woman who was not on the miji-kumi's list, but we did not interview her husband, at least not under the name she gave for her husband (which may vary considerably from the name on the miji-kumi's list). She may be a "new wife" for someone on the list under a different name, or one member of a "new household" for which we failed to find other members.

As in Obisa, we also discovered men (6 in Owich) who were either "found" (i.e. not on the miji-kumi's list) or thought to be living elsewhere with their wives (and thus not eligible, as they did not have a wife of childbearing age resident in the village). There was no indication that the wives had returned with these men, or that if they had they were of childbearing age; thus, we considered these men (and their wives) ineligible. We "lost" two questionnaires in Owich--and one was not accidental. When we awoke at dawn one morning, we were confronted by a man who came to our compound with a club, a panga and a Bible, demanding his questionnaire back: he feared that our work was part of a Satanic cult, and that because we had information about him he would die. He had spent a sleepless night, and when he came for his questionnaire he said that he had survived these many years and wanted to survive more. The data on his questionnaire had already been entered, so it was painless to give it back to him--another argument for data entry in the field.

In summary, the major reasons for failing to interview an eligible respondent was simply the amount of activity in their lives.

Just as this activity helped us to locate those who had been omitted from the sampling lists, it also made it difficult for us to find them. Probably the most important reason we could not contact respondents was that they had moved, usually to work or to look for work, between the time that the listing was made and our arrival.

These people may be among the more widely networked (giving them opportunities to learn about job possibilities: see, e.g., Granovetter for another context). Second, and also particularly important for our study, were people who were living in the village but whom we could not locate in three (often more) visits: it is these failures, in particular, that suggest that the most active people may be underrepresented in our survey. One woman, for example, was at the market when we first visited, and when we returned she had gone to visit her family in another part of South Nyanza. Some of those we failed to contact may have been avoiding us, although our supervisors and interviewers were quite persistent. A third important reason, again related to network contacts outside the area, was that the person had left temporarily, typically for a funeral but also (for women, primarily) to visit family in another area, either affines who lived in her place of origin or a spouse working elsewhere, usually a major city. A fourth important reason was marital separation. Although we had expected marriages to be stable in Luo-land (see Potash, 1978, who argues that they are stable because there are few alternatives for women if they leave their husband) in fact we found a fair amount of marital disruption, although this was not always acknowledged by the husband: in several cases the husband insisted his wife was away on a trip and would be back soon, but other relatives said the marriage had ended and they doubted the woman would return (in one case where the man insisted his wife would return, his sister-in-law said the wife had been dead for ten years). This is less likely related to the characteristics of the individual's networks.

We expected that the four sites would differ in the extent to which we would not be able to interview eligible respondents. Obisa and Kawadghone were chosen precisely because we thought the networks of the villagers there would be wider. In the event, however, there was little difference. It is true that the people in Obisa and Kawadghone seemed busier: the supervisors complained that people were "here and there, never still", and told of going to a compound, being told that the person was at the market, going to the market, being told that the person was at home, and dashing back to the home to find the person still not there. Men were particularly difficult to contact in Kawadghone, because many worked in the quarries. In contrast, in Owich one of our supervisors said, with some sarcasm, "The men are just there, chatting with their wives". On the other hand, in Owich distances were long and public transport extremely

limited: thus, although the activities were not apparently so varied, the journeys were probably longer. From Owich, a trip to the nearest dispensary (9 kms away) could take several hours each way, plus a wait at the dispensary, and the nearest market, in Magunga (which also had a dispensary and the nearest post-office), was even further away (13 kms). And while we were there several posho mills were not functioning, which meant that the women who used those posho mills had to walk even longer distances at least several times a week.

### **QUESTIONNAIRE:**

The first stage of long interviews was extremely helpful in designing the questionnaire for the second, sample-survey, stage. We learned what questions produced no variation (i.e. virtually everyone approved of family planning--although subsequently raised doubts about it). We got a sense of the major occupational categories for coding; and where people were likely to be if they were working outside the area. We satisfied ourselves that the DHS finding that religion didn't discriminate in terms of family planning was probably right, and didn't ask religion (although we did ask whether a respondent's network partner went to the same church, since church is a major social activity in our areas). We learned the complexity and vagueness of family relationships

Nonetheless, there were still surprises in the field. Some of these were discovered in the course of checking the questionnaires before data entry and asking the supervisors, others by talking with some of the interviewers. Checking the questionnaire before data entry not only for missing data, but also as if it were the story of a life (albeit in very limited terms), looking for things that appeared to us as inconsistent. We found many of these--e.g. four fathers, a 27 year old man with a grandson who had been to secondary school, respondents who said they talked with a network partner "every day" although the network partner lived outside the sublocation. When we talked about these with the supervisors (who in turn sometimes asked the interviewers), they usually turned out to be consistent in terms of Luo life and culture. Thus, the extra fathers are the respondent's father's brothers; the "grandson" was an uncle's grandson; the respondents who live apart but talk every day mean that when they are together (e.g. on vacation, or at some time in the past) they talk every day, even though they don't talk frequently now. We also interviewed some of the interviewers, going through the questionnaire and asking them what they thought the respondents understood by the question. Thus, we discovered that "membership in a church group", by which we had in mind something like a woman's



auxiliary (which we knew existed) was interpreted simply as membership in a church. It is possible that some of these mis-matches between our questions and the respondents' understanding of the question could have been picked up with a longer pretest. But our pretest was rather short, and in addition was done by our supervisors, who knew what we were looking for; it is probably better to have the pretest done by interviewers more like those that will be ultimately hired for the interview.

Since we think the importance of being there during fieldwork is particularly well demonstrated by these inconsistencies, we give further examples below:

.The early interviews in Obisa turned up a rather surprising number of "professionals". We first realized this when one of the cooks in the guest house where we were staying said her husband was a "professional", he was the clerk of the stores at the guest house; another cook said her husband was a "professional", he worked for the forestry department. We discovered that the common understanding was that anyone with a wage job is considered a professional. We instructed the interviewers that professionals are those with extra training for their job, like teachers or nurses, and to write in what the profession is (so we can check).

.Many respondents were coded as "doing nothing" to earn money. When we asked the local interviewers about this, it appears that these respondents live largely from their plots (mostly maize, millet, sorghum and some cassava), with money for cash purchases (cooking oil, matches, soap figure prominently) from a relative with a job, or from an occasional day's labor (e.g. working on someone else's plot) or when the women go the market, buy some bananas, and resell them for a few more shillings.

.Although in the qualitative interviews the possibility that children might die was often given as a reason for not restricting family size, our question about this showed that it did not seem to be so important in decisions about family size. Our supervisors say that the question is simply too complicated, and neither the respondents nor the interviewers understood it well.

.Approval of family planning: Some of our respondents report that network partners who are said by the respondent to disapprove of family planning are using it themselves. This may be interviewer error, but our supervisors think that what

is going on is that although the network partner approves of family planning in general, she or he disapproves of the respondent using it--perhaps because the respondent doesn't have enough children, or has had health problems with it.

Yet further errors arose only in the process of data entry--despite what we thought was rigorous checking. Thus, for example, a data entry person noticed that "Nyakach" had been incorrectly coded as a location outside of S. Nyanza.

### Interviewer Selection, Training and Performance

We selected teams of about 20 interviewers in each site. We decided to get interviewers who were graduates of secondary school, since we expected some of the training to be in English (in Kenya, secondary school is in English), and our questionnaire was rather complicated, with loops for the networks and a matrix of names to fill in. We had also determined that the interview should be in Luo: although after about the 3rd grade school is at least supposedly in English, we had found in the first phase of our research that even respondents who had been to secondary school were much more comfortable speaking Luo (see also Heine, 19xx). The applicants for interviewers were given an aptitude test; in the first two sites a brief interview was given to those with borderline scores, and in the other two all were briefly interviewed.

The aptitude test was adopted as a screening device on its own merits, but also to protect us against charges of favoritism. In this it was only partly successful, but far better than nothing.

To the extent that the chiefs, sub-chiefs and miji-kumi's cooperated with us, they did so because they thought our research would bring "benefits" or "development" to the community. By this they seemed to understand what we had told them--a vague possibility that health or family planning services might someday improve--but they also understood something far more specific: immediate, albeit temporary, employment. There were many unemployed secondary school graduates in our areas, and the wages we were paying, while not handsome, were, for some, better than the available alternatives. This meant that individuals wanted their relatives to be employed--indeed, our project director thinks that one or two chiefs took bribes, promising to get so-and-so's son or daughter a job as an interviewer.

In every site our selection aroused bitterness and accusations of unfairness. Although our interviewers were paid little, in our areas there were few other sources of employment, even temporary,

for secondary school graduates, so our 10 day job was quite desirable. Generally, this took the form of complaints that we had hired people from "out of the area"--i.e. either interviewers who did not come from the specific villages in the sample, or from the sublocation, and complaints that we hired people who already had jobs. We were also accused of favoritism: in Owich, a sublocation in which one of our supervisors lived, of hiring his relatives and friends (and indeed, we did hire his nephew), as well as hiring "devil-worshippers" like ourselves. But they were also quite personal. One chief wrote our study director a note begging us to reverse our decision not to hire his second wife, who failed our aptitude test: "Are you truly a Luo?", he said, "Do you not understand that a man's power lies with his younger wife? I will not be able to eat breakfast in her house [i.e. sleep with her]." Another chief whose son did not make the grade told us that his wife reproached him bitterly for not having written us a note saying we should take his son. We explained our criteria to him, and he explained his criteria to us: "In Africa the family is very important, and a man must be able to do things for his sons".

Although we think that there is so much favoritism in hiring in Kenya that it was quite natural to think that we practiced it too, the notion of a test as a criterion for employment was also understood. Thus, our study director overheard some village people upbraiding a bunch of miji-kumi's for only telling their own relatives about the jobs, saying that since their own relatives were so stupid that they couldn't pass the test, we had to hire outsiders.

And one chief remained quiet when we turned down his wife, his daughter and his brother--although he did ask for payment for his services. Given the effectiveness of our test in helping to quiet resentments, we were reluctant to add personal interviews. We found, however, that some people who passed the test made lousy interviewers, and the supervisors--who depended heavily on the interviewers--insisted on the personal interview.

We had two days for interviewer selection and training; selection took about three hours. The first day of training emphasized interviewing techniques (building rapport, confidentiality, not leading, etc) and introduced skip patterns and our network loops. On the second day the supervisors went through the questionnaire question by question, the interviewers practiced

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<sup>27</sup> In Obisa, they were primarily living at home; in Owich and Kawadghone, it seemed to me that a relatively large proportion, although from the area, happened to be visiting from somewhere else where they had a job, and took this as a way of making a spot of money.

on each other, and then on "real" respondents. In the first two sites we paid the practice respondents, since they had to come to where we were for several hours. In Owich, however, this caused us trouble, since the news that we were paying respondents spread very rapidly, and some of our "real" respondents thus wanted to be paid. So in our next sites we went to the market and collared some people, interviewing them on the spot. This was no problem: people volunteered to be interviewed. One supervisor found one of her interviewers practicing with a large crowd around her: children, men, women, and both interviewer and respondent happily moving through the questions on family planning.

The decision to hire new teams rather than train one team and move it around was made on budgetary and logistical grounds: taking interviewers out of their area would require paying per diem, and in two of our sites it was very hard to find accommodations for ourselves, and it would have been impossible to find accommodations for the interviewers (although no doubt some of them would have had relatives in the area). This practice had the distinct disadvantage of requiring repeated training, and in each site the interviewers improved dramatically in the field. The first day there were many respondents who claimed they talked to no one, i.e. zero networks.

By the third day zero networks were much rarer. This is undoubtedly due to the greater confidence and experience of the interviewers.

Because networks were at the heart of our research, the decision to have new teams in each place was costly. On the other hand, the interviewers did not get bored with our repetitive questionnaire.

There were both disadvantages and disadvantages to having local interviewers. The greatest concern is confidentiality. Based on the qualitative interviews, we suspected a considerable amount of secret use of family planning in our areas: some of the women we interviewed in the first phase were secret users themselves, and many knew women who used secretly. Our sense was that although the penalties for being discovered seemed to be rather harsh (a beating from the husband) they were not so harsh as to keep women from telling others--sometimes their best friends, but also sometimes more broadly. We were somewhat reassured that respondents were frank by a note such as this on the questionnaire: in Luo, but translated into English by the supervisor, it said the woman was using injection secretly, "she didn't discuss it with the husband or even the co-wife, not even a friend. TOP SECRET: Only the nurse!". In an improptu conversation with one of our respondents, she said she had been interviewed, and told the interviewer she used family planning but her husband didn't know. We asked whether she felt uncomfortable telling this to an interviewer from this area, but she said no, she

didn't, that the interviewer had said everything would be confidential so she told her. We asked whether she had told even her best friend she was using, she said no, no-one knew but the nurse (and now the interviewer). Still, we remain concerned that respondents in the second phase would be reluctant to tell an interviewer from the area about her use of family planning if she were using secretly. We certainly did get some reports of use without the husband's knowledge, but we have no way of estimating either the true extent of this, or whether disclosure would have been greater to a Luo-speaking interviewer who was not from the area.

There were great advantages to having local interviewers. Not only did they demonstrate that we were bringing "development" (i.e. money) to the area, but the supervisors often said that an uncooperative respondent would agree to be interviewed by "so and so's son": one man, who had refused to be interviewed, capitulated but insisted on choosing his own interviewer from our team, taking someone he knew. In addition, the interviewers knew the area, and were very useful in locating people that were hard to find, as well as in finding people that the miji-kumi had omitted from our list of people to be interviewed. Our conclusion is that the advantages of using local interviewers outweigh the disadvantages, but that the training should be longer.

Our greatest concern about the interviewers is that we are

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In the summer of 1995, we tried to evaluate the extent of the underreporting of secret use of contraception. Using clinic records of those who had obtained contraception in the clinic at Wakula South between November and January (i.e. the three months before the household survey), we searched for those women among the respondents on our household survey. Out of the 33 women who were on the clinic records, we were able to match 13 firmly, and probably another two. (Some women go to clinics outside their home area to avoid detection, or simply because they happen to be visiting when it is time to renew supplies; in other cases, we may have failed to match because of the multiple names that respondents use.)

Of those 15, five reported on our survey (correctly) that they were injection users. Four other women who are using either pills or Depoprovera according to clinic records did not report this on the survey; a fifth woman reported secret condom use on the survey, but is actually using Depo. Thus, about 5/33 are secret users who don't appear as such on our survey, which thus causes us to underestimate both secret use and overall use. This is a minimum estimate, since it is possible that the other 18 clinic users who we could not link with a questionnaire are secret users.

<sup>28</sup> A similar conclusion was reached by Heisel (1968), who discusses local vs. nonlocal enumerators: nonlocal might confront suspicion and reserve, but local might risk loss of reassurance or anonymity. They decided to accept the latter risk. Enumerators reported greater resistance when interviewed at distance from their own area or group, with noticeably greater difficulty encountered in dealing with respondents who were not members of their own clan.

fairly sure that there is a substantial amount of variability across interviewers. We are certain that the interviewers did not stick to the questionnaire, as standard survey practice requires (see, for example, various articles in Tanur, 1992). When we sat in on a few interviews, we could hear that the Luo of the interviewer went on much longer than the Luo of the printed questionnaire, and we could see exchanges that did not appear to be the simple question-response that standard survey technique calls for. After the interview, the interviewers were often able to answer questions about a particular respondent that would not have been elicited from questions on the questionnaire. And they sometimes wrote notes on the questionnaire that turned out to be helpful.

Although the importance of sticking strictly to the questionnaire was emphasized to the supervisors who were training the interviewers, we do not think either the supervisors or the interviewers considered it desirable or possible. Our respondents were not at all familiar with surveys, and some of them were deeply suspicious of the research. They thus needed a longer introduction than we had provided, and we are not sure what was said in that introduction. But leaving aside the introduction, we think it would not have been possible to proceed verbatim through the questionnaire.

In part, this is because both our interviewers and our respondents expected the half-hour of the survey to be a conversation rather than an interrogation. Even when the interviewer may have stayed closely with the wording of the question, the respondent didn't always give short answers, but sometimes explained. For example, the interviewer of a man in Kawadghone (M646) noted on the questionnaire that the man said he didn't talk to anybody about children supporting their parents "because he had never been helped by his children", and that he had never talked to anyone about family planning "because he is a Christian". It is from these sorts of comments that we learned that a respondent asked the interviewer to say that he should "Circle No Bicycle!", or a woman explained that she was not using family planning any more because she had had a tubal ligation.

A second reason that the interviewers did not always stick to the questionnaire was that, despite the extent to which our qualitative interviews shaped the questionnaire, we still had concepts or terms that were confusing to the respondents (as well as to the interviewers, initially), and that needed to be clarified.

This may be more likely to happen when survey research is done in unfamiliar contexts, but as Suchman and Jordan (1992) point out, it also happens elsewhere. Survey data, they say, may be invalid because of what Fienberg (1990, cited in Suchman and Jordan) calls "errors of the third kind": that although the questions are, on their

face, the same across respondents, their meaning is not stable across respondents, and thus may do not measure the concepts of interest to the researcher. We are persuaded by our experience that Suchman and Jordan have an important point when they argue that it is preferable to permit more local control by interviewers in order to have stability of meaning across respondents .

The result is that we undoubtedly have considerable variation across interviewers; sticking closely to the questionnaire, however, would have given us considerable variation across respondents in their understanding of the question. In any case, we think a distant, antiphonal style simply would not have worked in this setting---and may not work well in others, as Suchman and Jordan suggest.

A disproportionate number of our interviewers were male: males in our areas were more likely to go to secondary school than females. This meant that males sometimes interviewed females; in addition, for logistical reasons females sometimes interviewed males. Our preliminary checks on this in our first site, Obisa, suggested that there was little difference in the outcome, and so we continued with this practice. A later and more careful examination of possible interviewer gender bias, using data from Wakula South, however, indicated that there is some gender bias. Using the number of network partners named by the respondent (a number that varied for women from 0 to 35) we compared the proportion of 0-networks (those in which the respondent claimed not to have talked to anyone about family planning) when the interviewer and respondent were matched by gender and when they weren't. This did not show statistically significant differences. It was the case, however, that for those who named any network partners, women interviewed by female interviewers were more likely (statistically significant) to have larger networks than those women interviewed by male interviewers.

Our last point concerning interviewers is that we think our period of training was too short. We devoted two days to training, which included about 3 hours for interviewer selection. Our questionnaire was fairly complicated (skips, network loops, a matrix to fill in), and most of the training time was devoted to that.

In effect, we did training in the field. Especially on the first day in the field, the supervisors sat in on interviews, and went over the completed questionnaires closely with the interviewers while they were still in the field. The interviewers learned quickly, and by the second day in the field they made few errors. We think it would have been preferable to extend the training by at least a day, ideally two, of interviews in the field. In addition, we think that more time should be spent in explaining the purpose of the questions. It is only if the interviewer understands the purpose

of the questions that he or she can notice inconsistencies, and query the respondent. We did notice these in the checking of the data in the field, but this meant sending the interviewers back to resolve them, which was not a good use of their time.

#### DATA ENTRY

If all the questionnaires were in perfect order--checked, names and respondent numbers accurate, no missing data, no inconsistencies due to interviewer error--data entry would not need to be done in the field. In our experience, however, the questionnaires were not checked perfectly. It is hard for us to imagine that others will not also discover errors only when the data are actually entered and preliminary tabulations made. Some of these errors can be rectified if field work is still going on; others cannot, but the researcher can gain a better understanding of what the errors are.

We found that data entry in the field was troublesome. The size of our team was expanded by the two data entry people, who had to be paid per diem in addition to salary, and had to be transported, housed, fed. We also had to keep batteries charged for their computers, which was a problem in two sites in which we didn't have electricity. Our solar panel failed (the shadow of a passing cow would result in the loss of the charge); when we borrowed a generator from the Kenyan Italian Scout Camp (a 45 minute drive away) it turned out to be defective and blew two battery chargers. We then had to resort to charging the batteries with the engine of one of our vehicles: since neither vehicle had a functioning cigarette lighter, this had to be done directly from the car battery. Since one of the two vehicles was always malfunctioning, it was available; however, this used a lot of petrol--and the nearest place to buy petrol was a 3 hour round trip over terrible roads, a trip which consumed nearly half a tank of petrol.

Nonetheless, we think data entry in the field was worth the trouble, improving both the coverage and the quality of the data. We could keep track of the interviews done and those left to do, so that we could be convinced that we were getting the people we were supposed to get. We also think that accuracy was improved.

A particular example of this was matching spouses. Our sampling lists had the names of the spouses; however, these often differed from the names on the questionnaires. The miji-kumi's often used nicknames for men, but our respondents gave their "official" names to our interviewers, and matching was sometimes difficult; then we had to turn to the supervisors or the interviewers. In addition, we sometimes had to give new numbers, for example, to people who



were on our list as living out of the village (e.g. working in Nairobi) but had returned. Giving one person a new number also meant giving a new number to the spouse; we sometimes made mistakes, which could be rectified in the field when the supervisor would know whose spouse was whom. We also think that data entry in the field helped keep the concern of our supervisors for accuracy high. At several points along the way we did tabulations for them, so that they could see the tables that the data produced, and could realize that the table might look different if they failed to find 20 women, or if the respondent's number of children was inaccurate.

The data from this study were entered twice, and then checked using the Epi-Info verify procedure. We did not intend to enter the data twice and verify it. It was only at the beginning of the second site, when we began to evaluate the completeness of enumeration in Obisa, that we discovered errors. One of our first checks was to look for duplicates (two respondents with the same number) and we found them. We tried to reconcile them on the basis of the data alone--as would be necessary if the researcher only receives a data tape--and in doing so found that one of our data entry people had made a lot of mistakes.

We then sent someone to Nairobi to retrieve the questionnaires from Obisa, and began a thorough checking, which led us to re-enter the data. With the questionnaires in hand, we found that some of the duplicates were due to typographical errors, some were due to mistakenly giving two respondents the same numbers, and some turned out to be due to having interviewed the same respondent twice (in one case, a respondent was interviewed three times). We found a few women linked to the wrong husband (and vice versa). We found more wives listed as spouses on the husband's questionnaire than we had on our listing (or than we had interviewed). We also found we had lost a few questionnaires: the log showed that the interview had been done and checked, but there was no indication that the data had been entered, and the questionnaire no longer existed. In a few of these cases, the log showed that the data had been entered, so although we had lost the questionnaire, we still had the data on file (although we no longer had the ability to check for typographical errors).

In this stage, we took advantage of the supervisors' close knowledge of the respondents. Because the interviews in Obisa were still fairly fresh in the memory of the supervisors, we were able to determine which of the missing data was due to questionnaire loss (presumably random with respect to the social interaction of our respondents) and which due to failure to interview the person (presumably not random with respect to social interaction, since

the busiest people were the hardest to find). We went over all the missing people with the supervisor responsible for that village. In some cases they didn't remember the person; in other cases, they remembered quite well either that they had interviewed the individual, or they had not: for example, one supervisor recalled that a woman was a second-hand clothes dealer who travelled around buying the clothes and then brought them to her co-wives in the village to sell. We had interviewed (and still had the questionnaires for) the two co-wives, but had not interviewed the dealer.

Fortunately, we had revised our system for keeping track of the data at the beginning of our second site, and once these data were entered (by the beginning of the third site) we found that we had done very well at keeping track of who was interviewed and of the questionnaires. Under this system, each evening when the supervisors returned from the field the work of the day was logged; by the time they went to bed, they each had a list of all the people in the area for which each was responsible, with an indication of whether the person had been visited, how often, and whether the interview had been completed. This then served as a guide for the next day.

#### Where are the errors?

We have discussed above errors in our sampling lists and in our coverage. Our checks for missing information have been very thorough--the supervisor, a checker, and eventually the data entry program, which will not accept missing data. We thus have little missing data. To check for errors in data entry, we entered all the data twice, comparing files and then resolving discrepancies by returning to the individual respondent's questionnaire.

But what about errors in the responses themselves, made by the interviewers or by the respondents, which in the event will be largely indistinguishable? These are the errors that are potentially most serious, and we found them to be of two types. One type are categorizations that are less crisp in the minds of the respondents than they were in ours when we coded the responses for the questions we asked, and the second type are apparent inconsistencies that exposed a clash of understandings between what we had in mind when we posed the questions and what the respondents seem to have in mind when they answered them. In both cases, being there helped us to detect these errors. By the time they were detected it was too late to change the questionnaire--and there is no assurance that other coding categories, or other questions, would have worked better--but their detection does aid us in interpreting the responses.

One check was to examine interviewer errors. Our questionnaire had two network sections, where respondents were asked a long series of questions about those with whom they had talked about two different topics. If the respondent, when asked for the names of those with whom she talked about the second topic (family planning), the interviewer was instructed not to repeat the series of questions, in order not to bore the respondent. In the few cases where an interviewer did in fact repeat questions in the second network loop about someone named in the first network loop, we find an occasional difference. On one network loop, all the answers were the same except that for the age of the network partner (in one, an age mate, in the other, older); on another, all the answers were the same except for the degree of intimacy (on one, the network partner was a confidante, on the other, "just a friend"); on one [M984], the network partner was "better off", on the other "the same as me".

We also, serendipitously, conducted a handful of duplicate interviews, which provided another check on the quality of our data. In reviewing the data for Obisa, we were surprised to find 13 cases of the same respondent being interviewed two times (in one case, three times, and in another, twice on the same day by the same interviewer). We are at a loss to explain why respondents would agree to a re-interview. One supervisor, who is characteristically a bit dismissive of rural folk, said "Oh, those people, they think there might be something to be gained from the interview, so they want more"; she recalled one man in Obisa whom she knew they had interviewed, but who insisted he had not been. In addition, occasionally we found that an interviewer had asked the same questions about the characteristics of the respondent's network partner twice. (Our questionnaire had two network loops, one for the respondent's conversational partners on one topic, and one for the conversational partners on another. If the person named on the second loop was one of those named on the first, the questionnaire instructed the interviewer to note that, and to skip the remaining questions about the characteristics of that network partner).

These duplicate interviews made us painfully aware of the softness of the responses to our questions. When we compared duplicate questionnaires, they were close, but by no means identical. For example, one woman's age differed by two years; she said on one interview that she had 10 children, two of whom had died, and on the other that she had eight, two of whom had died; on one she said no to "intend to use family planning in the future", on the other (and later) one she said "uncertain". In one interview she named two conversational partners, in the other one only one. Fortunately, it was the same woman, Dorca: all the information for Dorca was the same except "attend the same church" (yes in one

interview, no in the other). In another case, the respondent's place of birth differed slightly and the year she moved to the village; her occupation was nursery school teacher in one, small business in the other; in one she said she sold at the market, in the other that she didn't; in one she said her husband worked in S. Nyanza, in the other he didn't; in one she said she had lived outside South Nyanza for six months or more, in the other she hadn't. Her sister Mary, mentioned as a network partner on both interviews, once had primary education, and once secondary, and her economic status differed.

We are pretty sure that when we had duplicate interviews, the interviews were of the same people: the code numbers were the same, the name of the head of the compound and the spouse, as well as the spouse's name. Variation may be due to the diligence of the interviewer: one interviewer may have emphasized that we only wanted to know about stints outside of South Nyanza that lasted six months or more, while the other interviewer may have emphasized simply living outside of South Nyanza.

That interviewer differences are unlikely to be the whole of the story, however, is suggested by the case where the same interviewer interviewed a man twice on the same day. The background information for this man showed no variation: he was a secondary school graduate, a tax collector, who was consistent with respect to his dates of birth, marriage, number of children. His network partners had the same names, and many of the same characteristics across the interviewers. On one, however, he said he talked to Sakaria "less than once a month", on the other "at least once a month"; on one, Sakaria was a "confidant", on the other an "acquaintance"; on one, Olenye had been to primary school, on the other secondary school; on one, all three lived outside of S. Nyanza, on the other they lived in the same sublocation (and two in the same village); on one, Grace was an "age mate", on the other "much younger than me"; on one, Grace used injection as a method of family planning, in the other Grace used family planning, but method was "don't know"; in both, Grace advised the respondent to also use family planning, but in one he advised her not to use it and in the other he reported "no advice given". In one interview the respondent said he intended to use family planning in the future, in the other that he was uncertain.

We think that some of this variation may be due to our coding categories. For example, we had only three age categories for the network partners (much older, age mate and much younger), and an interviewer might waffle over categorizing someone the respondent says is 5-6 years older. But we think that the variation across the same respondent probably primarily represents genuine lack of

precise knowledge about their own characteristics or that of their network partners, as well as genuine difficulty in categorizing their network partners. Few of our questions concerned attitudes or intentions, which we might expect to be labile, even within the same questionnaire: with the exception of "intend to use family planning", the questions had to do with characteristics such as education, behavior such as where the respondent had lived, or characteristics of their network partners. And some variation probably reflects genuine complexity--e.g. one respondent probably both taught nursery school and did a bit of selling on the side, mentioning one the first time, the other the second; the sister Mary may have had only one year of secondary school--is this "primary" or "secondary"? And relative economic status may be difficult to judge when individuals are fairly similar in this respect. In addition, some variation probably reflects genuine ambiguity--on one day, or even one time of day, a respondent may intend to use family planning some time in the future, but at another time he may feel more uncertain: after all, since the question was posed in terms of the future, there was no need to be decisive. About other differences we cannot even speculate: why, for example, would a respondent say on one interview that Grace uses injection as a method of family planning, but on the other "don't know"?

**CONCLUSIONS :**

Some of the problems we encountered in this research could have been avoided could have been avoided with a larger budget: in particular, we would have spent more money on vehicles and brought redundant equipment, we would have had a longer pretest, and we would have spent more time on interviewer training. Some could have been avoided with more experience: it was not until the second of our sites, after 10 days of research, that we developed a good system for tracking our progress, and it was not until then that we started checking the questionnaires every evening--or at least the next day--in order to find errors that could be fixed by returning to the respondent.

But some of the problems, we think, are largely unavoidable, and these have largely to do with the respondent's understanding of our questions--"Circle no radio"--and their genuine ambivalence.

We have no doubt that inconsistencies remain in the responses, but we think these are "real inconsistency", rather than inconsistencies due to errors by the interviewers. These problems surely plague all survey research, and may be particularly serious when there is a substantial gap between the understandings of the researchers and those of the respondents. We believe that these cannot be avoided, but they can, at least, to some extent, be identified. Our final conclusion, then, is the importance of being there, not only for ensuring that the data are of the highest possible quality, but also for identifying where the data are not.