"The gift received has to be repaid:" Respondents, researchers and gifting.

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Introduction

This paper is a response to our fieldwork experiences. Although very different in terms of our respective research populations -- two of us worked in rural sub-Saharan Africa, the other in urban Philadelphia -- and in our methods -- two of us are social demographers with primary training in survey research and the third is primarily an urban ethnographer -- we each emerged with some of the same concerns about the relationships that researchers develop with their respondents or informants. Many of these concerns, we have come to realize, are related to questions about reciprocity: how much do I owe my respondents and how much do they owe me? Or, an alternative formulation: how much can I demand from my respondents and how much can they demand from me? These are, of course, ethical questions. However, based on our own experiences and on a survey of contemporary researchers' experiences -- 55% of the researchers had gifted -- we argue that they are also very important, but rarely addressed, methodological questions. Social science is constructed out of the social relationship that researchers develop with respondents. We show that there are good theoretical and empirical reasons for thinking that the quality of that relationship, which we argue is in large part an outcome of past or projected reciprocal exchanges, can affect all stages of the research process, from sampling to data quality to analysis. We contend that this means that researchers need to give more formal attention to the notion of reciprocity as a methodological issue.

The paper has two main sections. In the first section we introduce our theoretical arguments. We examine the special characteristics of the researcher-respondent relationship and we link it to what we call the 'norm of reciprocity.' Then, using Mauss's famous essay *The Gift* as a conceptual springboard, we argue that, in fieldwork, reciprocity is synonymous with 'gifting,' that is, compensating respondents for their participation in, and contribution to, the research process.

The second section is the empirical heart of the paper. Our main argument is that gifting is an important methodological ingredient in fieldwork. We show this with respect to the research projects that we have worked on. We begin with Stern's account of the role of gifting during her ethnographic fieldwork in urban Philadelphia. She explains how her gifting strategies affected her ethnographic sampling, her choice of informants, and her exit from the field. Next, Madhavan discusses some of the same issues with respect to her survey and qualitative research in Mali. A recurring theme in her account is that differences in local social conditions, including in respondents' expectations of benefit, affected her survey (and ethnographic) data. Finally, using data that Watkins and Weinreb collected in Kenya, Weinreb looks at specific effects of gifting on

survey data quality.

Section 1: The main arguments

The basic aim of social science is the construction of knowledge about people: about their motives and beliefs and behaviors and institutions. Distinct research methodologies have been developed to facilitate this aim in a number of disciplines. For the purposes of this paper we distinguish between two methodological schools. In the first, scholars derive all the data that they require from archival, historical, or archaeological sources. And in the second, more common in most social science disciplines, scholars rely on data that they or others have collected from a contemporary population. This paper aims to address concerns among members of this second school. Henceforth, they will be referred to as 'researchers,' the process of data collection as 'fieldwork,' and the objects of fieldwork -- the informants -- as 'respondents.'[1]

a) The instrumentality of the researcher-respondent relationship

Our basic premise is that all fieldwork, however qualitative or survey-oriented, shares two necessary characteristics. First, it entails some sort of interpersonal relationship between a researcher and a respondent. And second, it entails the transfer of information from the respondent to the researcher. Together, these mean that knowledge in social science is inherently *relational*. It is the product of a social relationship between a researcher and a respondent. And it can therefore be affected by the particular characteristics of that relationship.

This is clearly not a new observation. Critics of positivist approaches to social science have long been concerned with weaknesses in the underlying assumptions of fieldwork. Some of these are mainly related to questions of methodology (Denzin and Lincoln, 1994), but others are equally concerned with potential ethical problems (Patai 1991). Their starting-point has often been this idea of the social relationship between the two parties. For example, they have tried to show how social inequalities between researchers and respondents, including the question of the relative 'positionality' of the researcher *vis-a-vis* the respondents, affect all aspects of research, from the

[1]These terms are used as ideal types. We realize that they may fit some types of social science research better than others. For example, there may be less of a distinction between what we call a researcher and respondent where the researcher is more of an insider with respect to his/her respondents, and/or where the process of data collection derives solely from observation and informal conversation. However, because we think that the fundamental academic role of the 'researcher' -- to understand and then report about people -- is not substantively different across methodologies, and likewise the academic roles of 'respondents' -- the object of research -- and 'fieldwork' -- where/how the researcher enters the social space of the respondents -- we think that single functional terms for each is justified.

construction of research questions to methodology to final analysis. These types of issues are among the main substantive concerns in most of the contributions in Wolf's (1993) edited volume on feminist dilemmas in fieldwork. Similarly, with respect to ethics in fieldwork, some critics have also argued that such inequalities can lead to the exploitation of respondents for researchers' own needs (Patai, 1991).

In large part, however, these types of interrelated methodological and ethical concerns have been used in a rather limited way. While inspiring an array of epistemological, class, and gender-oriented critiques of particular research methodologies -- especially survey research -- they have rarely been used to develop a more extensive critique of fieldwork *per se*. Some exceptions are Stacey (1991) and Patai (1991). In other words, notwithstanding the considerable epistemological shifts in social science over the last few decades (see Denzin and Lincoln, 1994) we researchers still tend to assume that fieldwork is useful. Presumably, this is because we believe that the data it generates are meaningful.[2]

This belief in the utility of fieldwork does not mean that it is problem-free, however. In fact, we suggest that there may be an inherent problem with the social relationship that researchers and respondents are able to develop while playing their respective roles in the research process. We also suggest that this problem unites all field researchers across all methodologies; that it is what we refer to above as a problem with "fieldwork *per se*." Specifically, the social relationships which develop in fieldwork are much more instrumental than more 'authentic' social relationships like friendship. This instrumentality follows from the nature of the researcher's role. With the exception of relatively uncommon "participatory research" projects, researchers need respondents but that need is not reciprocated. Moreover, not only do researchers need the respondents, but they are under a professional obligation to use their social relationship with them in order to extract information. This is the goal of fieldwork, and it remains the goal whether the respondents are (or become) "friends" of the researcher or not.

We are not trying to claim that researchers develop social relationships with respondents out of a simple desire for career enhancement. They may do so because they love the research process, think that the pursuit of knowledge is a worthy end in itself, or even believe that their project will be used to develop better public policy and therefore directly improve people's lives. Our argument is simply that the researcher-respondent relationship is essentially instrumental.

[2] Indeed, these are beliefs that we are institutionally pre-disposed to have; otherwise we would not have chosen to be, or at least to remain, sociologists.

And that whatever the motives that researchers ascribe to themselves, the ultimate goods that emerge from the research process rarely touch the lives of those people who participated in its generation. Thus, researchers get data that become the building-blocks of academic careers. Any funding agencies that are involved derive satisfaction from knowing that their money financed projects that matched their funding agendas. In fact, the only party involved in research with no institutionalized way of benefiting is the respondent. Even friendships that develop during fieldwork rarely survive the researcher's return home, at least for more than a few years (Wolf, 1993).

Nor do we think that the degree of instrumentality is necessarily related to particular research methodologies, at least in the long term. That is, while researchers who use more intimate methodologies such as participant observation or ethnography are more likely to develop deeper relationships with respondents in the field than, say, survey researchers, they may also be more likely to betray those relationships and upset their closest informants once they have left the field (Reinharz, 1992; Stacey, 1991). Stacey's (1991: 114) warning about the relative potential for researchers' exploitation of respondents is clear. More intimate social relations expose subjects "..to far greater danger and exploitation than do more positivist, abstract, and 'masculinist' research methods. And the greater the intimacy ... the greater the danger."

b) The norm of reciprocity and gifting

Our main argument is that survey and ethnographic data are affected by gifting because gifting is a demonstration or at least a token of reciprocity. This argument derives directly from Marcel Mauss's essay *The Gift*, originally published in 1927 and probably the most famous and ambitious theory of gifts.[3]Finally, he even used his notions of gifting to support various public policies. He argued that state-funded pensions, for example, can be seen as the reciprocal exchange for years of productive contribution to the state's wealth.

Although Mauss (1990) is primarily concerned with what he calls a 'gift economy', which predates the 'market economy,' and although he considers the transition from the gift to the market economy to have diminished the importance of gifts, Mauss believed that the exchange of gifts is

[3] We think of his general theory as "ambitious" because he used its basic formulations to at least begin to develop much more sweeping historical theories. For example, he (1990: 17) refers to sacrifice as "contract sacrifice" where "..gifts ... to the gods also serve the purpose of buying peace" and "those gods who give and return gifts are there to give a considerable thing in the place of a small one." It is easy to see how this might become a theory about the origin of religion, not only the origin of specific ceremonies. Similarly, his theory of the origins of alms-giving (p.18): "Generosity is an obligation, because Nemesis avenges the poor and the gods for the superabundance of happiness and wealth of certain people who should rid themselves of it."

(p.4) "one of the human foundations on which our societies are built." His principal argument in support of this claim is that there are symbolic elements in the exchange of gifts that are at least as important as the value of the actual economic exchange. Thus (p.13), "To refuse to give, to fail to invite, just as to refuse to accept, is ... to reject the bond of alliance and commonality." The argument's Durkheimian texture is clear. Gifting can be seen as a normative social ceremony that gives meaning and substance to the notion of community. Thus, one of its primary functions is to construct and/or maintain a network of social relationships. A secondary function is to give each participant or group that is involved in the exchange a moral and, in some cases, economic incentive for maintaining the relationship. In apparent confirmation of Mauss's argument, both of these functions appear to have remained important ingredients in interpersonal relations even in today's supposedly less relational market economies (Schwartz, 1967; Green and Alden 1988).

In fact, the idea that gifts, as tokens of social relationships, can be used to construct and/or maintain those relationships fits with the notion of symbolic and other non-economic elements in economic exchanges. This has long been the principal premise in economic sociology (.....). Similarly, research in political economy has shown that market economies are not antithetical to the existence of a parallel or underground gift economy, especially, but not solely, in emerging markets (eg. Bates, 1981). Nepotism, to one degree or another, remains an important part of economic and political structures in most, if not all, areas of the world. And at least part of the motivation to be nepotistic may be to maintain or build a network based on the idea of reciprocity.

That reciprocity is a basic building block of community and society can also be seen when one reads ethnographies from both non-western and western societies. In her foreward to the latest translation of Mauss's essay, Douglas (1990: xiv-xv), for example, describes the effects of Mauss's essay on Evans-Pritchard's famous ethnography of the Sudanese Nuer.

> "Evans-Pritchard, who promoted the original English translation and wrote a foreward to the edition that this one replaces, had Mauss's teaching very much at heart when he described the marriage dues of the Nuer as a strand in the total circulation of cattle, and wives, and children, and men: every single relationship had its substantiation in a gift."

Denzin's (1992: 122) description of the working-class neighborhood in urban America that is depicted in another ethnographic classic, Whyte's *Street Corner Society*, sounds remarkably similar.

> "Whyte's message was clear... There was order, honor, dignity, and pride here in Cornerville. A reciprocal system of exchange and obligation held this order together and served to maintain a daily system of

equilibrium."

In short, in both a pastoral Sudanese and urban American society -- and there are countless other examples -- norms of reciprocity underscore social relationships. We think that this is because reciprocity is an inherent characteristic or, in Mauss's language, "one of the foundations" of society. It embeds people in what Joseph (1993: 119) calls "webs of relationality." Given that, as discussed above, we think that social science knowledge is itself relational, we can expect the type of reciprocity in the researcher-respondent social relationship to affect the data that emerge from that social relationship.

c) The dangers of an instrumental relationship given the norm of reciprocity

Having established that the researcher-respondent relationship is instrumental and that there is a norm of reciprocity we can now address the next important questions: so what? Aren't all social relationships instrumental? And, if so, why should social scientists worry about it? There are two possible sets of answers to these questions. The first, and we think the weaker argument, is the solely ethical one. Its gist would be the standard ethical argument that to develop a relationship with someone in order to use them instrumentally or, in slightly more traditional ethical language, to treat people as means rather than ends, is wrong. This is the type of argument that Patai (1991) develops in her discussion of western academics' research on, or for, Third World women. We choose not to attempt it in this paper.[4]

Instead, we argue that there are simple methodological reasons for worrying about the degree of instrumentality. Given the norms of reciprocity, it affects what survey researchers call sampling and non-sampling error, fundamental measures of data quality, and what more qualitative researchers might call the "content" of their discussions. This occurs because when levels of instrumentality are skewed, the norm of reciprocity is threatened. In other words, if we make it too difficult for respondents to use us, or if we fail to compensate them for their participation in our research, they have little motive to willingly participate.

With respect to survey research, this unwillngness to participate may not necessarily mean that they will openly refuse to be interviewed. We shall argue below that in many areas of the developing world, for example, the structure of local authority may make refusals normatively difficult. In such circumstances, it may be easier for unwilling respondents to choose to "be busy",

[4] It should be clear from our use of emotive, value-laden expressions like "inequality" and "exploitation," however, that we sympathize with this argument.

to try to avoid interviewers or, even worse, to agree to be interviewed but then lie. These are types of non-response that Scott (1993) would call "weapons of the weak." Similarly, with respect to ethnographic research, compensating or gifting informants may be essential for the type of snowball sampling and personal introductions and references that are so important when entering a community (eg. Joseph, 1993). And they may also be crucial for developing a reputation of rootedness and trustworthiness in the community. Again, we will give examples of these below.

d) The content of the gift

By gifting respondents, we do two things. First, we overtly recognize the instrumentality that is inherent in the researcher-respondent relationship. And second, we attempt to counteract the imbalance in that instrumentality. This is our core argument. However, we do not want to argue that all gifting is good; nor that gifting can't give rise to undesireable and unintended consequences. Madhavan gives an empirical example of this in her section. A previous researcher in one of her sites was much more generous than Madhavan planned to be. This led to high levels of non-response in her project.

There are also theoretical reasons for expecting that gifting may sometimes have undesireable consequences. That is, in the Maussian framework, the symbolic value and social consequences of particular gifts are context-specific. This means that researchers need to pay attention to the *content* of the gift, that is, to the meaning that recipients will ascribe to it. As Schwartz (1967) argues, this meaning is not only related to how the recipients think of us. It is also related to the image that we want them to have of us and to the way that we see them. He calls gifting "an imposition of identity." In short, in terms of fieldwork decisions about gifting, we think that the overall meaning of particular gifts derive from the following questions:

- **What to give?** There are lots of options. Should you give material goods -- food, clothing, money, medical help, photos etc.. -- or non-material goods -- advice, volunteer-work, rides, copies of research papers etc.? Should you give the same thing to all your respondents? Should you try to fit with local systems of stratification by, say, giving higher status objects to older men, or different gifts to men and women? Should you give something that is easily available locally, or something that will allow the recipient to feel not only appreciated but also relatively distinguished?[5]

[5] In Appendix A we provide some basic data on the types of gifts that appear to be most common among contemporary researchers. The data are based on a survey of 32 social scientists, 29 of whom have done fieldwork.

- **Who to give to?** Should you gift the individual respondent or his/her clan or community (respectively **direct** *versus* **indirect** gifts)?

- **When to give?** At what stage of fieldwork should you gift? For most survey researchers this is basically a question about whether one should gift before or after a relatively short interview. With respect to the more intimate social relationships associated with ethnography, however, the decision is more complicated. Those relationships are ongoing and more "natural," at least in the short-term.

- **How to give?** Should the gift be made in public or private? Should the interviewer give the gift or, if it is a different person, should the project leader?

These types of questions are clearly related. For example, a researcher who decides to volunteer at a community center is making a decision to gift the community publicly rather than his/her individual informants privately. By contrast, a researcher who gives food to those who are participating in the project uses a direct and private gifting strategy. Each of these will interpreted differently. We will return to some of these points in the conclusion to the empirical section. We will also discuss one of the larger debates about gifting in the developing world: the question of "payment," that is, whether to use money as a gift.

Section 2: The empirical arguments

We now move on to empirical arguments. Stern and Madhavan take turns to examine how and when gifting became important during their fieldwork experiences. Then Weinreb looks at the effects of gifting on sampling and non-sampling error. First, however, we discuss contemporary researchers' attitudes to gifting.

a) Researchers' attitudes to gifting

We surveyed 32 social scientists, most of whom are sociologists and demographers. Of the 32, 16 are professors, 5 are post-doctoral researchers, and 11 are graduate students. Aside from asking about their own gifting behavior -- 29 had done fieldwork and 16 had gifted -- we asked them a set of general open-ended questions about their attitudes to gifting:

What role do you think "gifting" plays in the data collection process? Do you think it affects the

content or the ‘quality’ of the data? If so, how?

The following discussion is based on their answer to this set of questions.

The most general theme that emerged from their responses was the ambivalence in attitudes to gifting. Only four researchers were uncompromizing supporters of gifting and only two were uncompromizing opponents. Most of the researchers (n=26) fell somewhere between these extremes. While being relatively suspicious of gifting -- presumably because it challenges assumptions about the “authenticity” of the researcher-respondent relationship -- they seemed to acknowledge that it was an imperfect solution to some of the inevitable problems of fieldwork. Indeed, most of them had gifted (16 out of 29), and most of the research projects that they’d been involved in had gifted (34 out of 53).

Two types of arguments were used in favor of gifting. The first and most common was a straightforward methodological argument: gifting improves sampling and data quality. Thus, it can encourage participation in a project, boost the sample population and, for longer-term projects, ensure compliance or minimize attrition. A few researchers also suggested that gifting can affect respondents’ motivation, especially where a long questionnaire is being administered or some other extensive time commitment is required.

The second type of argument in favor of gifting was related to the types of issues that we discussed in the first section of this paper, that is, the characteristics of the researcher-respondent relationship. Thus, one researcher argued that it can be a good way to reduce local people’s suspicion if you are an outsider. Another, that once in the community “it is essential to contribute as a privileged community member should.” A third, that is is “imperative” to gift if that is part of the “local culture of politeness.”

In terms of the actual interview process, several gifting supporters adopted some type of equity argument. Thus, one argued that gifting “tends to make the interview process seem a bit more equitable.” Another -- one of the few uncompromizing supporters of gifting -- thought that it makes the researcher-respondent relationship more realistic (because it explicitly acknowledges the nature of the exchange). A third took a monetarist perspective, that it is appropriate to reimburse someone for the time that they spent doing the interview, especially if it is a long interview. And a fourth argued that respondents should be seen as research assistants and should therefore be compensated in the same way that normal research assistants are compensated.

There were also two types of arguments used in opposition to gifting. First, there were some opposing sampling and data quality arguments. In certain situations, the promise of renumeration can bias a sample if it draws too many unrepresentative people into the sample population. Similarly, several researchers argued that gifting is not a guarantee that the data will improve. In fact, the opposite may be true. Gifting may make respondents more likely to tell you what they think you want to hear.

The second type of argument against gifting, mentioned by only a few researchers, is related to possible unintended consequences. One researcher worried that gifting can create conflicts between respondents and non-respondents if the latter are not interviewed and therefore do not derive any direct benefit. This may be much more likely to occur if the research is being conducted in an impoverished population. Two others researchers argued that gifting can raise expectations among respondents *vis-a-vis* other projects. Not only might this be a problem for the respondents. It may also be a problem for researchers or research projects that come to the area later, but which have smaller budgets. They may not be able to afford the sorts of gifts that respondents have come to expect.

Other issues were also raised. Some researchers distinguished between the types of research where they think that gifting is appropriate. This is related to the levels of sampling error that are typical of certain societies. Weinreb will return to this issue in the final empirical section. And there was also some discussion about different types of gifts, first about the relative merits of individual *versus* community gifts, and second, about using money as a gift. We will return to these issues in the conclusion. We now move on to each author's discrete empirical sections.

Case Study #1: Field Experiences from Milton, Philadelphia
(Patricia Stern's section)

In my dissertation research, I am examining how the restructuring of the economy, urban blight, and demographic change are affecting the lives of individuals and families in Milton, a white working-class and poor neighborhood of Philadelphia. In particular, I am interested in people's experiences of rootedness and dislocation, loyalty to and ambivalence about leaving the neighborhood and working-class milieu in a time of rapid economic and social change. My research also looks at how social organization around location operates in this white neighborhood in ways that both foster social and employment networks enhanced by geographic proximity and perpetuate geographic and social isolation (Sennett & Cobb, 1972; Susser, 1982; Stack, 1974).

My objective in this research has been to understand the impact of macro structural forces on the lives of residents in New Milton. Many ethnographic studies of urban neighborhoods have accomplished this through examinations of social interaction and, more specifically, collective action and community politics in local institutions such as worksites, churches, schools and recreation areas (Suttles, 1968; Kornblum, 1974; Susser, 1982; Horowitz, 1985; Rieder, 1985; Anderson, 1990). When I decided to do a community study, I wanted to explore and represent a more interior view of social life. In addition to being a participant observer in neighborhood "anchor institutions," I also sought out more micro-levels of social interaction in more intimate "places"--inside the house, out on the stoop, or in the street.

Thus, from the very start of this project, I assumed that "getting in" or gaining access to interior social worlds would require "gifting" strategies. That is, I would have to exchange my time, skills, and friendship in order to observe daily life in these more intimate places. By definition, ethnographic research requires close contact and trust that comes from spending regular and intense periods of time with informants. What I have learned through my fieldwork thus far is that I would constantly have to alter or augment these "gifting" strategies with each new situation and set of people. In addition, I discovered that expectations for my contribution as well as the nature of gifts would change as I became more accepted in the community.

"Getting In"--Data and Sampling

In my first year of graduate school, I decided to investigate the precarious position of white-working class people and increasing downward mobility and poverty among whites living in the inner-city. I also wanted to do an ethnographic study of an urban neighborhood. Given these interests and my location in Philadelphia, the Milton neighborhood was a logical place to conduct my research. Traditionally a thriving white working-class community supported by textile and shipping industries, Milton is now an increasingly isolated neighborhood in industrial decline. To most outsiders, the name "Milton" signals danger and decay. The fact is that Milton is an extremely private and closed neighborhood. Residents on the street and staff in local institutions are wary of outsiders. Through suspicious looks or more direct comments, Miltonians alert you that "you are not from around here."

Common to all ethnographers is the concern about how to enter the field. Problems researchers encounter and the frequently ad-hoc or innovative solutions they employ reveal interesting and important information about how a community defines and maintains its membership boundaries. In light of the intimate and guarded nature of this white working-class and poor neighborhood, I knew from the beginning of my research in Milton that I would need to

find various and locally appropriate forms of exchange to gain access. In addition, I believed that these "gifting" strategies could also serve as data for the study. Through experiencing first hand people's resistance as well as invitations into their world, I could observe mechanisms of social control and solidarity operating in local communities.

After selecting the neighborhood, I used an inductive approach to sampling common to ethnographic methodologies. My initial investigation was intended to explore and discover themes that were important to local residents. I understood that the process of selecting field sites would depend on where I could "get in" and "stay in" as a participant observer. In order to reach more interior places, I would have to embrace the organic and improvisational aspects of this research design. In a sense, my ability and success in "gifting" would directly affect my research design in how I defined my case study and established relations with informants.

Having worked as a community organizer in the past, my first thought was to contact local organizations to see if they would be interested in having me do some research. I presented this as an offer to "exchange" my time and research skills in return for introductions to people and to neighborhood issues. My first involvement of this kind was to assist one particular organization with a survey of small businesses.

Perhaps the most fortuitous break I had that summer was meeting the executive director of the New Milton Community Development Corporation (CDC). Because of my continued involvement with this organization both as an employee and a volunteer, I have developed and maintained a network of contacts in community institutions. In addition, my familiarity with this neighborhood and social networks has provided me with access to social life that has been vital to my ethnographic research. The executive director has made available to me a variety of data sources on the New Milton neighborhood population, housing patterns, business and job base, as well as clientele profiles. Thus, I have selected New Milton's service area within the broader Milton area as the primary geographic boundaries of my dissertation research. For several years, it has been my base of operation.

In the summer of 1994, New Milton CDC's executive director hired me as an intern to organize a community strategic planning process. In this position, I learned about the structure of neighborhood institutions and was introduced to local leaders from a variety of schools, churches, hospitals, and businesses. In addition, I facilitated the recreation committee which aimed to provide more activities and support for local youths. Unlike other committees that were made up of professionals, the recreation group consisted largely of neighborhood residents involved with running local playgrounds. Through our conversations, these people shared invaluable

information with me about how these playgrounds function as "intimate places" and how to read playgrounds as markers of distinct social groupings or "territories" in New Milton.

After the planning process ended, I continued working with this recreation group through the next year. Although the committee eventually disbanded, I maintained a relationship with one particular family that identified strongly with a playground nestled in a residential area around the corner from the CDC. Pop's Playground is a small recreation facility which hosts a free Recreation Department day camp for six weeks over the summer. The wife, her husband and daughter, and her sister had all joined the recreation committee because of their lifelong attachment to and involvement with Pop's Playground as neighbors, campers, and staff members. In the summer of 1996, I contacted this family again and arranged to volunteer as a counselor at the summer camp.

This was another turning point in my research. It was my first intensive immersion in the field and experience as a participant observer in a central and "intimate place" within a section of New Milton. The playground provided a wonderful venue to observe the social organization of one little neighborhood and to experience its rhythms of daily life. My role as counselor allowed me to interact with children and adults in a "naturalistic" setting. In particular, playing with children and hanging around with counselors exposed me to cultural norms and forms of storytelling which offered a more inside view of neighborhood life than I had seen as a community organizer. After six weeks of regular participation, I became what I would call an accepted outsider. At moments like when I listened to staff members gossip together or talk with parents about their children, I felt like a tolerated presence. Other times, such as the when the staff invited me on the camp trip to an amusement park and treated me to a free ticket, I enjoyed my status as an appreciated guest. The camp budget was so tight that they could not really spare the $15 for non-staff adult chaperons. I took this gesture as a sincere and generous expression of the staff's appreciation of my time and interest in the kids.

After taking a break from the field for the academic year, I prepared to take the next step in my research: full immersion in neighborhood life. Becoming a resident would be the most effective and legitimate way to have regular access to the most intimate of places and local institutions, the house and block. My search for a proper housing situation proved to be challenging. I had determined that I would prefer living with an elderly woman who could also offer some social connections. After making my pitch unsuccessfully at several senior centers in the Milton area, I finally found a woman who was interested in having a boarder for money and company.

For three months in the summer of 1997, I lived with this elderly woman in her rowhouse

which is located in a relatively well-off and stable neighborhood in New Milton. The house I lived in was across the street from a Catholic school and behind a large recreation center about six blocks from Pop's Playground. I found myself situated in an extraordinarily advantageous position to observe neighborhood life. At any time day or night, I could watch local youth congregate on the school steps, in the playing field at the rec center, and on different corners of the block. In addition, living in a rowhouse offered opportunities for daily contact due to close proximity with neighbors.

My residence on this street and my close relationship with my landlady, a practicing Catholic and parishioner of the local Catholic church for fifty years, and her network of friends and acquaintances directed my attention to life in this private parish neighborhood. During that period, I also began volunteering at Chances, a lottery fundraiser for the Catholic church. Once a week, I checked tickets and socialized with a group of women parishioners many of whom have spent their whole lives in the area. I also resumed working part-time as a community organizer for New Milton CDC. However, the most significant part of my research that summer took place talking at the kitchen table, sitting out on the step, walking around the blocks, and sharing a beer at the local taproom with my landlady. Getting to know her well sensitized me to the most interior of experiences--nostalgia for the past, current strains, and anxieties about the future that are characteristic of residents in this white working-class neighborhood.

Since the summer, I have continued to visit with my former landlady and other neighborhood contacts I have met throughout my fieldwork in New Milton. In part, I do so to keep my "place" in the neighborhood which allows me to return for more data as needed. In addition, I enjoy friendships with and offer occasional assistance to those few "sponsors" who took me into their lives and their social settings. Finally, my ethnographic research also takes me into other neighborhood institutions for more limited observation. I have also been conducting interviews with neighbors, religious people, employers, school and playground staff, and youths.

"Getting Along or Staying In:" Gifting Strategies

The norm of reciprocity and exchange is central to all ethnographic research. I present my experience of "getting in" to show how each situation of exchange has led to more contacts and how one door opened the next. Attention also needs to be paid to "staying in." My work with the CDC has been and continues to be a reciprocal arrangement on a professional level, though my relations with the staff remain friendly. While I am no longer a paid employee at New Milton CDC, I am still involved with the organization volunteering on the board and participate in other seasonal events and activities there. This fall I helped out covering the phones during a busy

period with an energy program. The staff and managers in this department surprised me with their show of appreciation for not only offering my assistance, but also for showing my interest as a board member in understanding the challenges in their work.

The executive director and other staff members have expressed desire and expectation to read my dissertation about the neighborhood. For the academic, the written product of one's research is an extremely personal gift. As Mauss observes, “to make a gift of something is to make a present of some part of 'oneself’” (Mauss, 1990, xx). Knowing that the CDC staff would be one of my audiences has had a significant effect on some of my writing choices. In particular, I decided not to give an ethnographic rendering of the CDC as a local worksite to avoid analyzing my colleagues and friends there in ways that might be interpreted as personal critiques.

In the case of New Milton CDC, "the gift" of my research findings to the executive director and interested staff conditioned the design of my study. One negative consequence of this decision has been that I gave up the opportunity to use my extended experience in and observations of this influential neighborhood institution. However, through my work and connections at the CDC, I discovered more intimate institutions such as playgrounds or parish activities that are integral to neighborhood life. As a result, I will focus my writing on more local places where I have been able to observe and talk with people about experiences of rootedness and dislocation during these times of economic and social change. Thus, my decision to *not* feature New Milton CDC enables me to devote more time and space in my dissertation to developing the central themes of my research.

At Pop's Playground, the most significant part of my contribution was my regular visits to play with the children. Initially, I was motivated by my research to attend day camp in rain or shine. However, it did not take long before I began to care about the children, the staff, and the institution of Pop's Playground as a safe and desirable "place" in the neighborhood. One of the first things I did was to memorize all the children's names. This meant a great deal to them and helped me gain their trust. Soon after, I discovered that what the children wanted was for me to "be there" with them as they played. An example of this was our trip to an amusement park. I chaperoned kids ages seven and under. I spent a long day watching them on rides and making sure they got food and took rest breaks. These children talked about this day for the rest of camp and probably remember me most from it.

After the first couple of weeks, I could see that the children and the counselors appreciated my level of commitment. The "caring" I demonstrated through my regular attendance and active participation with the children was my "gift" that allowed me entrance to the interior of

this social world. Nevertheless, they constantly reminded me that I was an outsider. In particular, the staff was usually polite and even shy towards me. They rarely ever asked me to do anything for them. At the time, I sometimes felt ignored in these situations. I now interpret this behavior as deference to an outsider, particularly someone with advanced education. However, in exchange for my helping out with the kids, they allowed me hang out and listen to their stories and would answer my questions about the neighborhood. I got the sense at times that the other counselors liked having me around as listener or audience. This was definitely the case with the playground manager who is also a Catholic School teacher. He would often talk to me about the past and his frustrations with how things have changed with younger generation in Milton and in his school.

The manager and the director of the camp (the daughter from the recreation committee family) thanked me specially for my efforts with the campers. These two also helped me most directly with my research. Both of them gave me their official support to do research at Pop's Playground. In addition, I interviewed them at the end of that summer. When I was working as an organizer at New Milton CDC again in the spring of 1997, I had the opportunity to use my professional skills to help get more financial support for this special local institution. I wrote a grant application with the playground manager requesting special funds from the city for new sports and crafts equipment, uniforms, and money for fieldtrips to cultural institutions around Philadelphia. While I do not know the outcome of this "gifting" effort, I felt a particular satisfaction in deploying my grant writing skills in the service of keeping this place viable for neighborhood residents.

As my research progressed, I discovered more local terms of exchange, or "gifting," that are required to gain membership in the community in Milton. The closer I got to more intimate places and levels of social interaction, the more residents demanded of me in terms of my ongoing physical presence in their lives. Once I lived in the parish neighborhood and got involved in church activities, I realized that I was supposed to express my loyalty through regular, almost ritualistic participation. After a few weeks checking tickets at Chances, I observed that people always sat at the same table even in the same seat. If you miss a week, everybody notices and asks about your absence. It is a lively bunch, always chatting, swapping of news, bringing cakes and candies and pictures to share. This exchange at Chances has been both personally pleasurable and lucrative for my research. Through my work for the church, I have gotten to know the pastor who has offered me many contacts in this particular parish and the Archdiocese in Philadelphia.

"Gifting" to gain residence has been more complicated. As a boarder, I paid my landlady an agreed upon weekly amount in cash. Payment for room and board, this financial renumeration

was very important and desirable to an elderly woman on a fixed-income and an ethically sound practice for a researcher in this situation. My presence also offered her some prestige in the neighborhood in terms of having something and someone to talk about. This prestige was part of the array of concrete and in-kind and non-material resources I could provide. Routinely, I would do favors for her such as driving her to the pharmacy or bank and assisting her with grocery shopping. On a number of occasions, I took her out to visit her dear friend, a 92 year old nun living in a retirement home for nuns outside of Philadelphia.

Perhaps the most important "gift" I offered was my company and companionship. She took me in as a member of her household, someone with whom to share occasions and experiences. This membership included sharing with me her life story, introducing me to her friends and acquaintances, and gossiping about neighbors and neighborhood affairs. My close relationship with my landlady allowed me to observe and understand her relations with other residents while in public and the cultural norms and meanings embedded in daily interactions. Finally, living with her allowed me a legitimacy on the block. I did my best observation and felt a pleasurable sense of belonging in New Milton while sitting out with her in front of her house or taking walks together around the neighborhood. This is the biggest gift of all to the ethnographer who needs to demonstrate his/her credibility as having "been there" both among informants and also with peers in the academy.

However, as Schwartz (1967) points out, gifts can and often do involve a measure of social control, hostility and unfriendliness. As it was my most intimate of contacts, I experienced this dimension of "gifting" most acutely in my relationship with my former landlady. Throughout the summer, I felt an undercurrent of manipulation and guilt in her attitude toward me which exceeded our contractual arrangement. While she acknowledged that I had a busy schedule, she communicated through her tone, gestures, and sometimes verbal complaints an imperative to stay with her and around the house. For example, she would often ask me why I did not work more on my dissertation when I was I was in the house. At the same time, she would inevitably interrupt me when I was reading a book, grading papers, or typing on my laptop computer.

Cultural norms around loyalty heightened the emotional cost of my entrance and exit from membership as a resident in Milton. As discussed above, loyalty in this culture is expressed through ongoing physical presence and participation in family or neighborhood life. In addition, children, even adult children, are seen as family resources. Thus, I found that the most challenging part of "staying in" this most interior and intimate place was understanding and negotiating with my landlady about her expectations of my loyalty and my expectations of my autonomy. When I moved out of her house and of Milton, she went through a mourning period which entailed her loss

of sleep, weight, and ability to function. Though we have found new forms of exchange and continue our friendship, my former landlady still blames me for "leaving her." Interestingly, other more casual acquaintances from the block expressed similar, though less intense, sentiments of loss and even hurt when they realized I had moved. This experience more than any offered a valuable insight for my research as to why people might be ambivalent about leaving the neighborhood.

Conclusion: "Getting Along While Getting Out or Exiting the Field"

Mauss observes that in fundamental social relations among self-interested parties in archaic as well as in contemporary societies, "the gift given must be repaid." On some level, there must always be a debt on one end to ensure continual social interaction. How, then, does the outsider of a given culture complete and then get out of the cycle of exchange? Ethnographers grapple with a similar problem in their research process when they exit the field. This separation inevitably raises questions about gifting and relationships based on the exchange of favors, information, and trust.

Ethnographers look for "natural" breaks as moments to leave their research sites. For me, this has been the end of summer camp, the conclusion of a community organizing project, or the need to return to school. Sometimes leaving the field is more arbitrary, or can feel that way to people in the communities the researcher leaves behind. When I left my residence in Milton at the end of last summer, my former landlady still felt hurt and left behind by my departure despite our contractual arrangement.

If an ethnographer needs to maintain relations in her field site to collect further data, it may be necessary to change "gifting strategies." In my experience, this has meant constituting relationships through less frequent contact but a greater level of trust in the friendship. With my former landlady, my exit required her to engage in new forms of communication and emotional awareness and me to accept my loss of intimate contact with neighborhood life. Since I moved out, both of us have had to rise to the demands on our friendship. Perhaps our solutions and innovations to problems of cultural difference have been a gift of better understanding and possibilities for closeness with others that can come from this kind of exchange.

It is not always possible to change relationships to adapt to different levels of involvement in the field. Figuring out appropriate gifting strategies--emotional, favor trading, or continued participation--can be difficult. For ethnographic research, however, I believe it to be necessary, at times rewarding, often uneasy, and certainly constantly negotiated.

Case Study #2: Field Experiences from Mali
(Sangeetha Madhavan's section)

The West African country of Mali ranks as one of the poorest countries in the world. Funded by the National Science Foundation, our comparative study investigated the effect of women's social networks on infant and maternal health in the Fulani and Bambara communities of Mali (1 -footnote insert - This project was headed up by Alayne Adams of Columbia University and Sarah Castle of the London School of Hygiene and Tropical Medicine; the fieldwork was conducted between March 1996 and May 1997). The research team included 20 male and female interviewer. My involvement in the project came about as a result of needing data for my doctoral dissertation in demography and sociology.

When we designed this project, there was little question about whether to gift or not. However, we were very concerned about developing a "natural" as opposed to an "artificial" relationship with the respondents. I am using "natural" to describe the conditions that exists between friends and "artificial" to refer to relationships based on an external agent such as a "gift." We decided with little debate that we should give some small token of our appreciation for the respondent's valuable time and willingness to answer our questions. We also managed to assure ourselves that such a ritual would not compromise the "natural" aspects of the researcher respondent relationship. I highly doubt that we were even remotely concerned with the ethics and consequences of gifting at the time. The issues brought up here all developed during the course of fieldwork. The decision as to what to gift also was not heavily debated. We simply asked our Malian colleagues for their suggestions and accepted them as irrefutable wisdom. In hindsight, this lack of attention seems peculiar if we are to draw anything from Mauss's emphasis on the content of the gift. As discussed earlier in this paper about Mauss, it is of utmost importance to insure adherence to cultural norms which in effect condition the intensity and meaning of the relationship itself. For us, the gift symbolized little more than a token of gratitude but for our respondents, as we found out later, it meant something much more. Through trial and error in our field work, we learned several important lessons about the norms of gift content, i.e. type, quantity, and its effects on our capacity as effective researchers.

Data and Sampling

This study is divided into two components: quantitative (use of survey) and qualitative (use of in-depth interviews and a longitudinal follow-up). The survey sample includes 500 ever-married women ages 15-49 from each ethnic group giving a total of 1000 women. The longitudinal one is made up of 24 women from each community. The objectives of the study

called for a sampling methodology that turned out to be more problematic when operationalized than we anticipated. Given that the study focused on maternal and child health, we restricted our sample to women of reproductive age. The villagers, naturally enough, were curious as to why were there no men or old people included in the sample. In an effort to respect sampling theory, we sampled only one eligible woman from a household. This was met with concerns about why only one woman was chosen from a household and not the others. One of the two ethnic groups in our study has two castes within it. We chose to look at only caste in order to avoid mixing up two potentially different cultural patterns. While this may satisfy social scientists, the villagers interpreted it as a sign of favoritism made even worse by the fact that we chose the higher status caste. Finally, it proved to be a task to convince people that we did not choose the women for our qualitative study because we liked them more but because they met a set of criteria.

There is nothing particularly surprising about any of the questions that the villagers posed to us. In environments of scarcity where an outsider is seen as a potential channel of upward mobility, people tend to exhibit feelings of envy about the rewards that they perceive as being given to some and not to others. As researchers, we tend to forget that what we consider to be a scientifically pure sampling procedure is interpreted as something very different by people living there. What is essential to grasp is that there are different agendas and motivations operating for the researcher and the respondent. It is in such a context that decisions about if, to whom, how and what to gift are made.

The Context of Gifting

a) Entrance Into A Community

As in most African settings, the first thing to recognize and appreciate in seeking entrance into a community in Mali is that it is linked to other contextual factors. One important one is the history of "intervention" in the area. The two most common types of intervention are development aid and research. I use the term intervention as a synonym for entrance here because most respondents see little difference between a westerner doing aid work and one who conducts research. The southern site for our study, Kolondieba, is situated in an area which has received both. Save the Children US has been running a demographic surveillance system for the past 12 years with the objective of collecting data and has been facilitating the development of primary schools and providing contraception and other health related services. People in this area are, not surprisingly, very used to “toubabs”(white person) showing up to ask extensive questions. For the most part they are quite receptive. The Northern site of Douentza, on the other hand, is in an area where there has been heavy emphasis on aid work, mainly as a result of the serious droughts in the region. People here are more used to toubabs showing up to give them “help” (wells, cows, millet)

rather than to ask questions. I argue that the difference in respondent receptivity in the two areas comes from the level of trust.

A long term project such as the population surveillance system in Kolondieba usually allows for relationships to develop between respondent and researcher unlike short term aid work which permits respondents to get no more than an ephemeral contact with development personnel. Such a dynamic would lead us to believe that there is a simple correlation between the level of outside intervention and non-response rates. However, this is not so. People residing in an area that has had a lot of "research" intervention tend to suffer from "research fatigue." This could cause them to give more overt and/or covert non-responses (discussed in Weinreb's section). On the other hand, it could lead them to be far less suspicious of foreigners which would in theory decrease non-response. People living in an areas such as Kolondieba, which has a history of "services rendered to the population" such as family planning, vaccination, clean water supply, etc., tend to far more sympathetic to the researcher's needs. In the same manner, respondents in areas with short spurts of aid intervention may be either more suspicious leading to higher non-response rates or simply more curious prompting lower non-response.

The choice of villages for a demographic or health related study usually attempts to follow some statistical sampling theory. However, in sub-Saharan Africa, the decision often ends up being dependent on logistical feasability, i.e. transport, and in our case specific site criteria that had positive and negative implications for the respondent researcher relationship. The selection of villages in the South was based on the success of Save's programs in the villages. In other words, we wanted a representation of villages where literacy and vaccination campaigns have had success as well as those in which there appears to be very little effect. It is argued that the availability and use of social capital in the village is crucial in determining the success of development projects. If this is true, it stands to reason that this very same social capital would also affect the respondent researcher relationship. Put differently, respondents in a village with successful programs would have a different perception of research teams (most likely positive) and would probably have a different set of expectations from those in villages with unsuccessful programs. Those having reaped the benefits of a successful program do not feel as if they have been cheated of rewards as do their counterparts in the unsuccessful villages. This quite often means that the latter will need a greater assurance of some repayment for any future work, i.e. research.

b) On the Ground

The success of a study depends to a large extent on the types of relationship that the interviewer has with respondents. Furthermore, the content of these relationships condition both

the expectations of respondents and the obligations of researchers. In Mali, as in most African settings, access to the respondent is through a local team of interviewers. These people are fluent in the local language and have important kin and non kin relationships with the respondents that facilitates the interview process. Their roles are crucial to the researcher's success - a fact that is well known to both the researcher and the interviewer. While we managed to maintain fairly amicable relationships with members of the interview team throughout our stay in Mali, there were instances during which we were forced to confront and concede power plays. I offer the following two examples:

EX 1: one very savvy interviewer who was well aware of her power purposefully mistranslated responses because she was not happy with her salary;

EX 2: we had a mutiny by the team and nearly lost access to a village after firing two interviewers who were not performing well; one of the women was a close kin of the village chief which means that she was instrumental in securing us an invitation; we ended up hiring them both back;

While the interviewers ultimately determine the success of each interview, it is the local leader, i.e. village chief, who functions as the gate keeper to the "respondents." A strict protocol usually involving gifting is followed to gain access to them. Aside from material gain, they stand to gain status attainment for themselves and the village which would increase the chances of future work in the village. Quite often, they will try to use their positions to influence sample selection. Finally, there are the respondents themselves who has to give his or her permission to be interviewed. This is certainly not guaranteed for a variety of reasons including bad health, bad mood, lack of time, fear, suspicion, or simply lack of interest. As discussed above, there may or may not be an expectation of a payment. For some, the chance to talk, complain and possibly learn is a gift in itself.

Gifting Strategies

We decided to give two meat boullion cubes to each woman in the survey at every sitting. We justified this choice by its convenience, relatively low cost and its popularity in Malian cuisine. We made an assumption that our respondents would value this item and would see it as adequate compensation for their time and energy and so we purchased cartons of the Cube Maggi and doled it out liberally. While the women in our sample gladly accepted it, they certainly did not attach as much the value to it as what we had hoped for. Simply put, most Malians, even in the villages, can afford to buy a few cubes a week whereby diminishing both the novelty and value of the gift. Furthermore, it was pointed out by other researchers that there is something ironic about a health related project promoting the use of a product that is nothing but salt and chemicals. I should mention, however, that despite this, word spread very fast around the villages in which we had

women coming to us wanting to be interviewed because of it. I attribute this less to the actual gift and more to the opportunity to get something for free. We did experiment with other gifts such as soap and water filters but they too had limited efficacy.

There are two forms of gifting that have special significance: food and money. Food items have a lot more value in an environment of food scarcity which characterizes nearly all the countries of West Africa. One example of this is the vast amounts of time spent making sure that food is divided up equally. This happened regularly amongst our research team as well as with respondents. A second very telling experience is an argument that I had with two of my interviewers. They became angry with me because I did not include them in the gifting of left over food that I had made to the cook. There are two reasons for this reaction. The first is that their inclusion in the process would have increased their clout in that village which clearly has much more of a utilitarian value to them than for me. The second is the fact that it was food that was given in an area vulnerable to frequent food shortages. By not including them in the gifting process, I deprived them of notoriety that they would have surely received for such a gesture.

An important and controversial type of gift and gifting strategy is the payment of respondents with money. For the Malian context I argue that money is the taboo gift in field work and is used as a last resort measure. It was used once by my colleague who decided that she really had no way to justify why she could not give it at that time. Herein lies the problem. Despite the fact that we try so hard to come up with a convincing explanation to our respondents as to why we do not give the monetary equivalent of gifts, we rarely succeed. We do, however, have a fairly obvious answer for ourselves: we do not want to treat the process of data collection as a commodity that can be bought. This is evident in our preference to use terms such as "token of appreciation" instead of "repayment." What gets us into trouble with this type of reasoning is that we use money all the time in most western countries for at least survey research. Why is it that we consider a respondent's expenditure of time in the States as "services provided" that merits monetary compensation but not so in the African context. I believe part of the answer lies in what appear to be a conception of monetization which at best lacks any proof and at worst comes dangerously close to paternalism. The use of money as a form of payment is perhaps a more recent phenomenon in Africa but it is quickly becoming the preferred means of transaction. There is no getting away from this by miring ourselves in romantic notions of a more simplistic system of the exchange of goods and services. While I am not convinced that money is a viable or necessarily desirable solution for the African case, I do believe that our reasons for not using it are weakly grounded and quite outdated.

There are different perspectives on gifting in long term and qualitative data collection where the researcher respondent dynamic is distinctively different from that of the survey research. For our longitudinal component, we selected twenty women from each site and literally followed them for three months. It should be fairly obvious how intrusive this can become and, consequently, how self-conscious (bordering on guilt) researchers become knowing this. This type of situation forces us to question our roles as both inside and outside agents. It is expected that we immerse ourselves in the respondent's life and attempt to gain their respect as a friend. We strive to make the relationship as natural as possible by not using any material goods as an incentive for a friendship. Assuming that we do achieve this fairly lofty goal, we face another dilemma - if and how to compensate the respondents who are friends? In other words, at what point does "gifting", in its formal sense, become replaced by culturally defined norms of friendship which may or may not include the exchange of gifts?

As the Philadelphia experience in this paper illustrates, friendship and trust established over extended periods of time are in themselves simply non material gifts that happen to carry more weight in the ethnographic environment. In fact, this is precisely the argument against material "gifting" that is made by long term intervention projects in Africa such as Save the Children in Mali. The staff of these programs worry that material gifting practices by projects such as ours contaminates the "natural" relationship that has been so carefully nurtured over time. The "artificial" relationships that are created through material gifting set up expectations amongst the people that similar "gifts" will follow from then on. It is fairly obvious how this could threaten the success of resource poor development projects. Such a fear echoes concerns brought up by Sherry et al. (1993) in their discussion of the disruptive potential of the gift. This in fact is a common complaint of Peace Corps volunteers in the area who are there to promote self-sufficiency not to buy people's interest.

In our longitudinal study, we gave plastic buckets to each of the 24 women in the sample and sacks of salt to the village as a whole for their hospitality. While we can certainly question to what extent our relationships were "natural", I do not believe that this gift or any exchange of gifts changed any dynamics with the women. Having personally experienced the hospitality that was given to our research team, I can honestly say that at least some of the relationships that were made were indeed genuine. Not only were the signs of a "natural" relationship present - i.e. gossip, advice, arguments, and mutual help, but there was also reverse gifting in which the research team constantly received gifts of fruit, tea and other foods. Finally none of the women in the in-depth study were aware of the gift that they would be receiving. While I hesitate to say that all cultural barriers came down, I can go so far as to say that I felt very much at home during this phase of the

work. This sense of ease is rarely if ever felt in survey work.

Effects on Data Quality

While my preoccupation with the ethical dilemmas of the researcher respondent relationship took up a substantial amount of my waking hour over the past year, I found myself speculating on how this was affecting the data that we were working so hard to collect. The most glaring problem occurred in one village in the North where we had to deal with the effects of not gifting. This village had the highest non-response rate and the highest number of women refusing to be weighed and measured. Part of the reason was our refusal to conduct research in the same manner as one particular western researcher had done in the past. The women basically would not get on the scales or sometimes even answer unless this person was there. On one hand, I found myself in awe of my predecessor. She had spent extensive periods of time in this area, developing both her expertise in the local language as well as her social relationships with people. This after all is the "natural relationship" that we all strive for. However, I began to learn what constituted this "natural relationship and felt frustrated with what I found. Part of the grievances was our refusal to give medical care or provide transport on demand as apparently this researcher had. They could not accept our reasoning that the project had only agreed to pay for the medical expenses of acutely ill women and children in any of the villages that we worked in. The expectation of a payment was so strong that we simply could not have been able to administer a survey without a promise in the beginning. It would be very difficult to call this anything else but the purchase of responses with little guarantee that they are truthful.

As explained earlier in this paper, researchers worry that gift expectation changes the way people answer. In one of our Northern villages, which has the distinction of having the most unhygienic water supply of all our villages, we noticed an unusually high number of women reporting that their children are sick. Within hours of our arrival in the village, the word spread that women who participated in the survey would be given soap and beef bouillon cubes. While neither of these two products can cure guinea worm, a water borne disease, or cholera, they did create the expectation that more assistance would come if the need were there. One way to demonstrate a collective need is for each woman to say that her kids are sick. I am not suggesting here that this is indeed what happened nor am I implying that these women were lying. In fact, I would go so far as to say that it is a perfectly sensible and potentially beneficial strategy. However, if our goal is accurate data, then we have reason to worry because we are likely to come up with overestimates of child morbidity.

Interpretation by Respondents

The expectation of some short term benefit, either material or non, is ultimately what motivates people to participate. This, in turn is linked to several macro level factors, some germane to social science research in general and some specific to the African setting. Working in Africa or in any developing country as a foreigner automatically forces one to tackle issues of neo-colonialism and paternalism (Riedmann 1993). History in this context not only lives on but is highly effective in creating a condition in which both researcher and respondent are extremely aware of their relative positions to one another. In Mali, westerners are called “toubabs” which interestingly enough, is an Arabic word that means white doctor. Toubabs, researchers included, are seen as rich and capable of finding solutions to anything from typhoid to getting to the United States. I remember well the puzzled looks I received when I tried to explain that in our interviews, the respondent should see herself/himself as the teacher and myself as the student.

In understanding the instrumental nature of the researcher respondent relationship, we need to recognize that guilt is an ugly yet very real component of any field work situation. The African context is a particularly good example because of the extreme poverty that is present in many sub-Saharan African countries. Researchers including myself, have a difficult time extricating themselves from feelings of guilt which explains to a large extent the use of material gifts. Furthermore, respondents who are well aware of their indispensibility to the researcher, are quite skilled at making you feel guilty about your status. This is one of the rare occasions in which the respondent uses the instrumental relationship to his/her advantage. The experience of an ethnographer in Kenya illustrates this - " Watoro's extreme poverty caused her to make material demands on me that I found difficult to meet over time given my ownlimited resources; her demands left me with a certain sense of guilt and resentment" (Davison 1996: 109). Let me quickly point out that I find nothing particularly surprising nor reprehensible in this. It is simply a condition that is created in an environment where most people are illiterate and poor. The researcher, definitely myself, attempts to constantly re-assure her/himself that the research is of value to the people but eventually comes to the realization that the "natural relationship" is not just possible but perhaps not even desirable. If for nothing else but to ease the researcher consciousness, the gift of the Maggi cube becomes a necessity. By acknowledging this, we are in essence recognizing the inherently instrumental nature of the researcher respondent relationship and attempting to balance the scale more in the respondent's favor.

Conclusion: Putting Mauss into Work

I would like to suggest some possible solutions to the ethical and practical dilemmas presented here. I do, ultimately, agree with Mauss that a "gift given must be repaid." I believe that

researchers do have an obligation to give something back and that we must treat respondents as people not as “data”. That being said, it is time to give thought to the what, to whom, when and how parts of the gift. The use of material gifts such as soap, tea or even beef boullion cubes is perfectly justified given the time expense of each woman as long as we accept them as "payments" instead of "token gifts." More than just a semantic change, this shift would force us to be more honest about our roles and more realistic about what the research can bring about. For Mauss, the gift does symbolize a form of payment whereas for western researchers in Africa, it is a gesture of "good will" and "appreciation." Mauss quite correctly recognizes that the gift serves to meet the self-interests of both giver and recipient. I strongly feel that researchers and research would benefit from a literal application of Mauss.

In addressing the when and to whom parts of the debate, I propose that researchers follow a strict protocol of gifting once again being true to Mauss. From the Malian experience, I can say that researchers tend to get sloppy with the rules without thinking about the consequences. At best, such approaches could make life rather difficult for the researcher. At worst, they can cause long term problems for other research and development work in the area. To refuse a piece of soap to a woman who has not participated in the survey might appear to be miserly but it is a rule worth following because it demonstrates the researcher's adherence to a formal contract of exchange. In my experience, I found that it was relatively easy for villagers to understand why a woman who gave us her time got some soap and not another who had done nothing. This type of discipline requires that we pull ourselves out of the western mindset of viewing a piece of soap as nothing and put ourselves in the Malian reality of scarcity. The what and how of gifting should be determined in consultation with members of the local research team, other organizations working there and the respondents themselves. For example, in Kolondieba, we were requested by Save the Children US to get their OK on gifts so that we would not inadvertently set a precedence that most African researchers, development organizations cannot possibly live up to. In other words, we would be ruining it for others who would follow. In Mali, research projects funded by huge western foundations are accused of setting up dependency relationships.

Gift standardization is not only ethically sound but may also, ironically enough, help facilitate the "natural relationship" that we strive for if we can pull ourselves away from dichotomizing artificial and natural relationships. Such a framework is restrictive because it classifies natural as being based on that which is innately human, i.e. commonalities in experience, mutual reciprocity, the ability to criticize, etc. and artificial as that which is created by a material good. Based on the Malian research, I do not believe that there is a value difference between the gift of soap or money and the gift of friendship or advice. Both serve a purpose and their use is

determined by the research setting and the respondent. I also think that the concept of a "natural relationship" is vague at best for the African setting and perhaps for any fieldwork situation. Even the anthropologist who has spent ten years in a village speaking the language with perfect fluency would probably agree that the melding of identities is not possible. Needless to say for survey research, this is a fairly futile endeavor. Instead of focusing on how to make the relationship "pure", why not accept the existing conditions and maximize the material and emotional utility for both respondent and researcher. Furthermore, we may be pleasantly surprised by the additional benefit of better quality data in the process.

Case Study #3: Sampling and non-sampling error in a sub-Saharan African survey (Alex Weinreb's section)

Introduction

Gifting increases response rates -- and therefore reduces sampling error -- across all types of survey methodology (ie. mail, phone, face-to-face, medical examinations, expenditure diaries -- Ferber and Sudman, 1974; Whitmore, 1976; McDaniel and Rao, 1980; Berk et al., 1987; Willimack et al., 1995). We should recall that this was the most common argument in support of gifting among the contemporary researchers that we polled (see p.8 above). In fact, minimizing sampling error is the reason that gifting is such an established part of survey methodology in the U.S., usually in the form of direct monetary compensation, which appears to be more effective than non-monetary compensation (Goodstadt et al., 1977). Without it, response rates would be too low, and the bias associated with selectivity of response too high.

The acceptability and frequency with which respondents are compensated in the developed world does not appear to have carried over to the developing world, however. This is at least true of survey research. Survey researchers who work in the developing world tend to be much more suspicious of gifting. One of the senior researchers that we polled urged that gifts "be used with caution." Another, who agreed that gifting can be used "to ensure cooperation or compliance," went on to claim that it is "more appropriate to MDCs than LDCs." Because of this frequent suspicion, direct gifts to individual respondents are relatively uncommon. A good example is the Demographic and Health Surveys, an ongoing, multi-country, multi-wave study of fertility and child health. This is touted as the largest social science project in history, a flagship of survey research, and it prohibits gifting of any sort, whether to individual respondents or to communities (Nyblade, 1998: personal communication).

Even while rejecting many tenets of positivist social science, then, most of these researchers appear to fall into a positivist trap. On one hand they do not want to appear to be biasing their data by buying answers. But on the other they do not want to bias their data by allowing response rates to fall below an "acceptable" level. The implicit positivist premises from which these methodological attitudes derive are:

1. Data are not biased when response rates are high; and
2. Data are not biased when we don't gift.

While there may clearly be some contextual differences in the relative validity of these premises I challenge them as general rules of sociological practice. As I argued in the theory section, the norm of reciprocity that gifting represents is a feature of most societies. If researchers really were to base their gifting decisions on what one of our academic respondents called the "usual culture", then they would almost always gift because *that is* the usual culture. In fact, the context-specific decisions should be less over whether to gift than over what, when, how and to whom to give.

This appears to be at least implicitly recognized by many researchers involved in long-term projects. For example, the Matlab project in Bangladesh, a long term population laboratory -- ie. charged with collecting reliable population data in areas without much reliable data -- does not give direct gifts to its respondent population. But it provides free and relatively high quality healthcare. Similarly, researchers involved in the 40-year anthropological project on social change in the Gwembe valley in Zambia give small direct gifts, though not money, to participants (Cliggett 1998: personal communication).[6]

a) Why the positivist premises that data are not biased when (i) response rates are high and (ii) we don't gift, are flawed

The most common method that researchers currently use to guage whether gifting is necessary or not, and hence the main reason that it is an apparent non-issue in survey research in the developing world, is to look at response rates. In LDCs they are almost always very high, typically more than 95%. People rarely refuse to be interviewed and this lack of refusal is assumed to say something about their relative willingness to be interviewed and hence about the quality of

[6] In an informal discussion about gifting, Cligget -- a social anthropologist who works in the Gwembe -- was asked what the original P.I.s on the project (Theodore Scudder and Elizabeth Colson) had thought about gifting. She said: "Ted and Elizabeth said right from the beginning 'we do not pay our respondents'." She was then asked: "So do you have problems?" She smiled and replied: "No, but we give gifts."

their responses, at least in terms of bias.

This assumption may be flawed. High response rates do not automatically mean, and should not be assumed to mean, that people are willing to be interviewed. High response rates can simply mean that people are less willing to openly refuse to be interviewed than to agree to be interviewed. In many areas of sub-Saharan Africa, for example, we can imagine that this can occur because of the structure of local authority. At national, regional and local levels people tend to be part of relatively rigid patron-client hierarchies on which, to a large extent, their local status, and political and economic opportunities are dependent. Because most surveys legitimate themselves with the local power elites before or as soon as they enter the field, it may be difficult for respondents, especially the relatively disempowered like younger men and women, to openly refuse to be interviewed. However, their low motivation can persist. So, they can either try to use locally acceptable excuses to avoid being interviewed, or else they can lie at random points during the interview.[7]

In short, I suggest that there are three types of non-response, not just one, that researchers should use when trying to guage whether they need to to be gifting respondents or not. They are:

Overt non-response

- The traditional type of non-response, an overt refusal to be interviewed. It affects sampling error.

Covert non-response #1

- The avoidance of interviewers or claims like "I'm too busy". This also affects sampling error.

Covert non-response #2

- Giving inaccurate answers. This affects non-sampling error.

I will now provide examples for both types of covert non-response. The relationship between gifting and overt non-response is already established so there is no need to examine it here.

[7] There are many other areas were this may also be true. In Indonesia, for example, political authority is based on a formal patron-client hierarchy which extends from village leaders up to the President. The system is called *Bapakism* (leaders at each level of authority are called *bapaks*). Hull (1998) asserts that there is 100% survey compliance in Indonesia because of *Bapakism*.

b) The context

I use survey data collected in rural Nyanza Province, Kenya, one of the least developed and heavily populated areas in the country. The data were collected in two rounds between 1994 and 1997. I was a field manager during the second round and spent about two and a half months in the field. The survey focused on the relationship between social networks and fertility attitudes and behavior. Both women and men were interviewed though only women's data are used in this paper. No direct gifts to respondents were given in either round of the survey although small, indirect gifts like rides and children's books to a local school were made in 2 of the 4 sites. We also hired and trained local interviewers in all 4 sites, thereby contributing relatively large amounts to the local economy for the duration of our fieldwork.

From the very first day of fieldwork in the second round of the survey -- in all 4 sites -- we heard reports from our interviewers that some respondents were complaining that "nothing had changed" since our last survey. We soon learned that what was meant by this was that we had not brought any "development" to the area, that although we hired and trained local interviewers, there were no long-term projects or employment opportunities associated with us, no tangible benefits identified with our last visit.

What emerged, then, was that, like in Madhavan's Malian experience, there were differences in perception and in relative expectations between us, the research team, and the local community that included the respondents. In both rounds of the survey, we went to the field as researchers. But many respondents did not really see us as researchers because (i) the notion of research is little understood, (ii) it is difficult to distinguish a researcher from an aid worker or NGO representative, and (iii) we were "family planning people" because we were asking questions about contraception and family size. In short, local residents by and large appeared to see us as wealthy outsiders and associated us with "development," one of the ultimate goods in Kenyan political discourse.

In the first survey, our association with development potential, together with local people's curiosity about us and our research, appears to have created a relatively high motivation to be interviewed, notwithstanding the fact that, in this relatively high fertility and low contraceptive use population, we were simultaneously associated with the morally ambiguous goals of the national family planning programs. This motivation was apparent in a number of ways. First, true to the developing world norm, we got an almost 100% overt response rate. Second, the expected benefits that local people associated with us affected our initial sampling list. This was a list of local households compiled with the help of the chief. During the first round of the survey we discovered

that some compounds were omited from the list because, as Rutenberg and Watkins (1997) write: "he [the chief] did not want them [the families in those compounds] to benefit from our research." And third, the expected benefit associated with us also affected responses to some of our questions. Rutenberg and Watkins (1997) write: "..we learned that some who owned radios asked the interviewers to say that they did not, explaining that the project might distribute radios."

By the time that we returned to the field for the second round, however, perceptions of us had changed. In the eyes of a significant number of people we were now outsiders who had failed to live up to local expectations. I think that at least part of this changed perception is related to our failure to behave in accordance with the norm of reciprocity. And I will now show how it appears to have led to higher levels of both types of covert non-response.

c) Covert non-response

These data were not designed to test the hypothesis that we are investigating. To do that, we would need to do the same survey or project in similar populations while gifting one population but not the other. The plan here, then, is to look for signs in the data that we can interpret as saying something about the relative motivation of the respondents in the presence and absence of reciprocity. I identify three such hints.

i) Avoiding the interview

In each wave of the survey we hired and trained local interviewers in each of the four sites (some interviewers worked on both rounds of the survey). These interviewers then made up to three visits to each targetted person, targetted according to the sampling list constructed with the help of local clan leaders. The outcome of each visit was recorded on the cover sheet of the questionnaire. Table 1 presents these data for all women successfully interviewed by the third visit in each round of the survey.

Table 1 shows that there are clear differences in the proportion of women who were eventually interviewed who were interviewed in the first visit. In the first round, 75.5% were successfully interviewed in the first visit. But in the second round, only 63.7% were interviewed. It is also clear that this lower success rate in the first visit is not caused by much higher rates of overt non-response. Of the women eventually interviewed in the first round, only 0.65% had initially refused to be interviewed. The equivalent refusals in the second round amounted to 1.32%.

Table 1: The outcome of interviewers' visits to respondent's compounds, by survey round and interviewer visit. Note that the second round sample includes only those women eventually interviewed.

Outcome of visit	**First Visit**		**Second Visit**		**Third Visit**	
	N	***%***	***N***	***%***	***N***	***%***
First round						
Completed interview	700	75.51	142	15.32	83	8.95
Not home/other	221	23.84	78	8.41	....	....
Refused	6	0.65	5	0.54		
Total	*927*	*100.00*	*225*	*99.78***	*83*	*99.78***
Second round						
Completed interview	386	63.70	133	21.95	89	14.69
Not home/other*	212	34.98	85	14.03	....	....
Refused	8	1.32	4	0.66		
Total	*606*	*100.00*	*222*	*100.34***	*89*	*100.34***

* Where "other" was specified, it most commonly referred to "busy/working in local area" or "at a funeral". ** Sums the percentage column + percentage of completed interviews in previous visits. Does not sum to 100.00 due to rounding.

In short, the largest difference appears to be in the "not home/other" category. 23.8% of the first round women fell into this category in the first round, but 35.0% of the second round women were in this category. Note that the two rounds of the survey were separated by only two years, that they were conducted at the same time of year, and that there were no obvious infrastructural changes that could account for this increased non-compliance or covert non-response among respondents. Indeed, by limiting the second round sample to those women who were eventually interviewed I am, if anything, underestimating the degree of non-compliance. For I have excluded not only traditional sources of survey attrition -- the 93 women (10.74% of the target sample) who died, divorced, separated, or migrated in the interim -- but also a large number of temporary absences from the area. For example, 80 women were in some other Luo area at the time of the second round. Presumably many of these were working but might have been tempted back by the promise of a gift. So, the differences in the visit-success rate appear to be related to what I have

called motivational problems which I link to the failure of our project to live up to local people's expectations of benefit or reciprocity. But rather than manifest themselves in overt refusals to be interviewed, they take the less empowered route of delaying or avoiding the interview.

ii) Persuading the less motivated to be interviewed

The second indication of the effect of reciprocity on data can be seen if we look at the characteristics of the interviewers who successfully interviewed relatively unwilling respondents. In the second round of the survey interviewers were asked, in relation to every interview, how well they knew the families of the respondents. This was done in order to test whether there was a relationship between the interviewer's relative insider-outsider status -- an aspect of their "positionality" -- and the reported behaviors and attitudes of the respondents.[8] However, it also gives us relatively strong leverage for looking at the effects of reciprocity on survey compliance. This is because even though our survey did not gift respondents, we can assume that those interviewers who know the families of their respondents very well are more likely to have gifted, or to be from families who have gifted the respondent's family, in the past.

Again, the data fit with our expectation. First, Table 2 presents the number of completed interviews associated with an interviewer's familiarity with the respondent's family. We see that although interviewers who know their respondents families very well accounted for only 4.93% of the completed first visit interviews, they account for 15.71% of the completed third visit interviews. That is an extremely large proportionate increase.

[8] There is an assumption throughout survey research that outsiders are able to elicit the most accurate information. But like attitudes to gifting, this appears to be more a question of disciplinary norm than empirically rooted practice. In fact, the only published study that I have found that has tried to test this relationship empirically is Tixier y Vigil and Elsasser (1976). Interviewing the same Chicana women, Tixier y Vigil, a Chicana researcher, and Elsasser, an Anglo researcher, found that the respondents were more willing to talk about sex and bodily functions with Elsasser but about issues like discrimination with Tixier y Vigil. This implies that there appear to be both advantages and disadvantages to being (or using) either insider or outsider interviewers.

Table 2: The number and percentage of completed interviews in the second round, by the interviewer's familiarity with the respondent's family, and the visit.

How well do you know the respondent's family?	**First Visit**		**Second Visit**		**Third Visit**	
	N	**%**	**N**	**%**	**N**	**%**
Not at all	135	47.54	50	52.08	31	44.29
By name only	103	36.27	29	30.21	20	28.57
Quite well	32	11.27	11	11.46	8	11.43
Very well	14	4.93	6	6.25	11	15.71
Total	284	100.00	96	100.00	70	100.00

Second, relatively unwilling or unavailable respondents were much more likely to be interviewed by interviewers who knew their families very well. Table 3 presents the number of completed interviews among respondents who had intially refused or been otherwise unavailable. That is, 168 respondents who were unwilling or unavailable to be interviewed at the time of the interviewer's first visit were eventually interviewed.[9] Table 3 shows that interviewers who know families of respondents very well were able to interview 10.1% (17 out of 168) of those who had initially been unavailable. This is more than double the 4.9% of respondents who they successfully interviewed in their first visit (see Table 2).

[9] Of these, 115 were not at home, 25 were too busy or working, 9 refused, 9 were at a funeral, and 10 others fall into the unspecified "other" category.

Table 3: Number and percentage of second round completed interviews among women who initially refused or were unavaliable for interview, but who were later interviewed.

	How well do you know the respondent's family?				Total
	Not at all	By name only	Quite well	Very well	
N	83	45	23	17	168
%	49.40	26.79	13.69	10.12	100.00

The proportion of initially unwilling or unavailable respondents eventually interviewed by interviewers who know them quite well is also elevated. 13.69% of them were successfully interviewed by this type of interviewer, as opposed to the 11.27% at the time of the first visit (see Table 2). The fact that it is a more modest increase also fits with the notion of reciprocity: that one is likely to feel that one owes more to a family one knows very well than to a family one only knows quite well.

iii) Lying or giving inconsistent responses

Thus far I have only examined indications that reciprocity is related to data through sampling error. Now I briefly examine how it can affect actual responses. Once again, I use our measure of interviewer positionality, that is, how well they know the family of their respondents.

I argued above that in the second round of the survey there was less respondent motivation to participate, and that this reduced motivation was related to respondents' perception that we had not done anything for them since the first round. This argument provides us with strong grounds for believing that relatively neutral types of questions were answered more accurately in the first round than in the second. By neutral I mean questions about the person that is already public knowledge.

If we accept this argument, we can look for variation in motivation by looking at the consistency of reporting across the two surveys. The hypothesis is as follows. First, there is lower motivation to be interviewed. This is a general trend which occurs across all groups of respondents. But second, there are differences in how that lowered motivation manifests itself in actual responses. These differences are related, at least in part, to the social identity of the local interviewers. In particular, it is much harder for a respondent to give an inconsistent response to an

interviewer who knows her family well, than to one who does not know the family at all, at least in relation to more neutral types of questions.[10]

Table 4 presents odds ratios that appear to support our hypothesis. They were estimated in four bivariate logistic regressions. The dependent variables are inconsistency in reporting whether you (a) have lived somewhere else for more than 6 months, (b) have lived in Mombasa for more than 6 months, and (c) can speak English, and (d) can speak Basuba, a language spoken by members of a small ethnic group in this area of Kenya. These are all neutral, publicly-known pieces of information.

[10] There are two possible reasons. Either respondents are aware that interviewers who know their families well are more likely to know the "true" answer to such questions, so there isn't any point in lying. Or, respondents may feel that they owe closer interviewers more accurate answers, at least in relation to these types of relatively neutral questions.

Table 4: The relative odds of getting an inconsistent response over both rounds of the survey, by dependent variable.

Bivariate Logistic Regression		**Odds Ratio (S.E.)**	**N**	**Pseudo R^2**
Dependent variable Have you ever lived somewhere else for more than 6 months?			376	.0123
Explanatory variable	Not at all	1.00		
	By name only	.5983 (.214)		
	Quite/very well	.5189 (.198)*		
Dependent variable Ever lived in Mombasa for more than 6 months?			99	.1190
Explanatory variable	Not at all	1.00		
	By name only	1.2632 (1.491)		
	Quite/very well	.1316 (.1102)**		
Dependent variable Can you speak English?			375	.0489
Explanatory variable	Not at all	1.00		
	By name only	.2304 (.162)**		
	Quite/very well	.2387 (.177)**		
Dependent variable Can you speak Basuba?			375	.0426
Explanatory variable	Not at all	1.00		
	By name only	.4158 (.170)**		
	Quite/very well	.2669 (.109)**		

* significant at the $p \leq .10$ level; ** significant at the $p \leq .05$ level;

The respective odds ratios (for knowing the family well compared to not knowing them at all) are

all significantly less than one. All but the first are at least significant at the $p \leq .05$ level. This shows that insider interviewers were much less likely to be lied to on these types of neutral questions.

d) Conclusion

The principal argument throughout this section has been that people's motivation to be respondents is affected by their attitudes to the project or the researchers who run the project. This, in turn, is affected by the way they interpret their relationship with the project. In this particular survey, the research team failed to meet the expectations of local respondents. We did nothing for them. We did not think that that was part of our role as researchers.

From the perspective of a relatively large proportion of the research population, however, we may have broken the implicit contract that the norm of reciprocity represents. The respondents' motivation was lower. We have several types of indirect evidence that indicate this. First, they told us and members of our team that we had done nothing for them. Second, they made it much harder for us to track them down. Third, in many cases they were only persuaded to be interviewed because of their *existing* obligations to interviewers who know their families well. And finally, when all else failed and they could not avoid the interview, they lied, except where they could not because they guessed that the interviewer already knew the answer to the question.

We are not arguing that every respondent acted in these ways, just that a substantial number -- a large minority -- did. This is not, therefore, merely statistical noise. Lowered motivation constitutes a potential source of bias, both in terms of sampling and non-sampling error. And gifting may minimize that lowered motivation. It is time, therefore, to begin to question the normative antipathy among researchers to institutionalized gifting in the developing world. In this particular sub-Saharan African context there are strong indications that by ignoring the norm of reciprocity and not directly gifting individuals or

communities, projects affect overall response rates and actual responses.

References

Anderson, Elijah. 1990. *Streetwise*. Chicago: University of Chicago Press.

Bates, Robert H. 1981. *Markets and States in Tropical Africa*. Berkeley: University of California Press.

Berk, Marc L., Nancy A. Mathiowetz, Edward P. Ward, and Andrew A White. 1987. "The Effect of Prepaid and Promised Incentives: Results of a Controlled Experiment." *Journal of Official Statistics* 3(4): 449-57.

Denzin, Norman K. "Whose Cornerville is it, Anyway?" *Journal of Contemporary Ethnography* 21(1): 120-133.

Denzin, Norman K. and Yvonne Lincoln (eds.). 1994. *Handbook of Qualitative Research*. Sage Publications.

Douglas, Mary. 1990. "Foreword," in Marcel Mauss' *The Gift: The Form and Reason for Exchange in Archaic Societies*. New York: W.W. Norton. pp. vii-xviii

Ferber, Robert, and Seymour Sudman. 1974. "Effects of Compensation in Consumer Expenditure Studies." *Annals of Economic and Social Measurement* 3(2): 319-31.

Green, Robert T. and Dana L. Alden. 1988. "Functional Equivalence in Cross-Cultural Consumer Behavior: Gift-Giving in Japan and the United States," *Psychology and Marketing* 5(2): 155-168.

Goodstadt, Michael S., Linda Chung, Reena Kronitz, and Gaynoll Cook. 1977. "Mail Survey Response Rates: Their Manipulation and Impact." *Journal of Marketing Research* 14: 391-95.

Horowitz, Ruth. 1983. *Honor and the American Dream*. New Brunswick, NJ: Rutgers University Press.

Hull, Terence. 1998. "....." Paper presented at Population Studies Center, University of Pennsylvania, January 22, 1998.

Joseph, Suad. 1993. "Relationality and Ethnographic Subjectivity: Key Informants and the Construction of

Personhood in Fieldwork," in Diane L. Wolf (ed.) *Feminist Dilemmas in Fieldwork*. Boulder, CO: Westview Press. pp. 107-121.

Kornblum, William. 1974. *Blue Collar Community*. Chicago: University of Chicago Press.

Mauss, Marcel. 1990. *The Gift: The Form and Reason for Exchange in Archaic Societies*. New York: W.W. Norton.

McDaniel, Stephen W. and C.P. Rao. 1980. "The Effect of Monetary Inducement on Mailed Questionnaire Response Quality." *Journal of Marketing Research* 17: 265-68.

Patai, Daphne. 1991. "U.S. Academics and Third World Women: Is Ethical Research Possible?" in Sherna Berger Gluck and Daphne Patai (eds.) *Women's Words: The Feminist Practice of Oral History*. New York: Routledge. pp. 137-153.

Rieder, Jonathan. 1985. *Canarsie: The Jews and Italians of Brooklyn Against Liberation*. Cambridge, MA: Harvard University Press.

Riedman, Agnes. 1993. *Science that Colonizes: A Critique of Fertility Studies in Africa.* Philadelphia: Temple University Press.

Rutenberg, Naomi, and Susan C. Watkins. 1997. "The Buzz Outside the Clinics: Conversations and Contraception in Nyanza Province, Kenya." *Studies in Family Planning*.

Schwartz, Barry. 1967. "The Social Psychology of the Gift," *American Journal of Sociology* 73(1): 1-11.

Scott, James C. 1990. *Domination and the Arts of Resistance: Hidden Transcripts*. New Haven: Yale University Press.

Sherry, John F., Mary Ann McGrath and Sidney J. Levy. 1993. "The Dark Side of the Gift," *Journal of Business Research* 28: 225-244.

Stacey, Judith. 1991. "Can There Be a Feminist Ethnography?" in Sherna Berger Gluck and Daphne Patai (eds.) *Women's Words: The Feminist Practice of Oral History*. New York: Routledge. pp. 111-119.

Susser, Ida. 1982. *Norman Street*. New York: Oxford University Press.

Suttles, Gerald. 1968. *The Social Order of the Slum*. Chicago: University of Chicago Press.

Whitmore, William J. 1976. "Mail Survey Premiums and Response Bias." *Journal of Marketing Research* 13: 46-50.

Willimack, Diane K., Howard Schuman, Beth-Ellen Pennell, and James M. Lepkowski. 1995. "Effects of a Prepaid Nonmonetary Incentive on Response Rates and Response Quality in Face-to-Face Survey." *Public Opinion Quarterly* 59(1): 78-93.

Wolf, Diane L. 1993. "Situating Feminist Dilemmas in Fieldwork," in Diane L. Wolf (ed.) *Feminist Dilemmas in Fieldwork*. Boulder, CO: Westview Press. pp. 1-55.