



FCND DP No. 91

FCND DISCUSSION PAPER NO. 91

**COMPARING VILLAGE CHARACTERISTICS DERIVED FROM RAPID
APPRAISALS AND HOUSEHOLD SURVEYS:
A TALE FROM NORTHERN MALI**

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July 2000

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ABSTRACT

This paper investigates whether inferences drawn about a population are sensitive to the manner by which those data are obtained. It compares information obtained using participatory appraisal techniques with a survey of households randomly drawn from a locally administered census that had been carefully revised. The community map tends to include household members who do not, in fact, reside in the enumerated locality. By contrast, the revised official census is slightly more likely to exclude household members who actually lived in the surveyed area. Controlling for the survey technique, we find that the revised official census produces higher estimates of average household size and wealth but lower estimates of total village size or wealth, than the community map. Pairwise comparison of the survey techniques, holding the households constant, shows that the community map leads, on average, to higher estimates of household size and lower estimates of wealth.

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ACKNOWLEDGMENTS

This paper could not have been written without the outstanding research support provided by Sidi Guindo and Abohurhemone Maiga and the help and cooperation of our respondents. We thank participants at a conference held at Yale on “Imperfect Information and Fieldwork in Developing Countries,” especially Esther Duflo, for helpful remarks on an earlier version of this paper. We also thank two referees for particularly useful comments that improved the paper’s exposition. Funding for data collection and analysis of these data have been supported by the International Fund for Agricultural Development (TA Grant No. 301-IFPRI). We gratefully acknowledge this funding, but stress that ideas and opinions presented here are our responsibility and should, in no way, be attributed to IFAD.

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1. INTRODUCTION

Many empirical studies of household behavior in developing countries rely on probability sample surveys. The reasons for this are well understood. Sampling reduces survey costs while maintaining the ability to make valid inferences of the characteristics of the underlying population, provided that it is undertaken randomly. A prerequisite for the drawing of a random sample is a sampling frame, a list of the units in the population (or universe) from which the units that will be enumerated are selected. In practice, this is often an actual list, a set of index cards, a map, or data stored in a computer (Casley and Lury, 1987). But unlike sampling issues such as the choice of sample size or the mechanism for randomly selecting units,¹ the construction of the sample frame rarely receives much attention. This is unfortunate. For example, a sample frame that excludes the poorest households in a locality will lead to biased inferences regarding the incidence and severity of poverty in that community, irrespective of the quality of the data collection or the sophistication of the subsequent statistical analysis.

The starting point for constructing a sampling frame is often an administrative list. As Casley and Lury (1987) note, these extant lists are regularly flawed. They may include units that do not belong to the population of interest (overcoverage), exclude a unit that does belong to the population of interest (undercoverage), or list the same unit several times. Although these flaws can be rectified by careful cross-checking, doing so

¹ For an introduction to these issues, see Carletto (1999), Cochran (1977), Kish (1965) and Newbold (1988).

is not a trivial exercise. Moreover, the need to validate these lists increases the costs of undertaking household surveys.

Even if the sample frame is carefully constructed, there exists a view that the information collected subsequently will be unreliable. “Again and again...the experience has been that large-scale surveys with long questionnaires tended to be drawn out, tedious, a headache to administer, a nightmare to process and write up, inaccurate and unreliable in data obtained, leading to reports, if any, which were long, late, boring, misleading, difficult to use, and anyway ignored” (Chambers, 1994a, 956). Motivated by concerns such as these, the last several years has seen the development of new methods for obtaining information on the socioeconomic characteristics of communities. One such approach falls under the very broad rubric of “participatory rural appraisal” (PRA). PRA is “a family of approaches and methods to enable rural people to share, enhance, and analyze their knowledge of life and conditions, to plan and to act” (Chambers, 1994b). There are numerous attractions to PRA approaches. They are predicated on the notion that local people have a wealth of knowledge that they can articulate. Further: (a) they allow local people to generate and analyze information on their own living conditions; (b) such methods foster transparency and trust; and (c) they tend to “empower” respondents vis-à-vis outsiders. It is also claimed that a particular PRA method—participatory village mapping—can be used to obtain data on demographic characteristics and measures of well-being more accurately than standardized household surveys and at a fraction of the cost (Chambers, 1994b, 1994c).

This paper speaks to these issues. It investigates whether inferences drawn about a population are sensitive to the manner by which those data are obtained. Specifically, we started with a common sample unit (the household) and a common universe (villages in northern Mali). In these villages, we conducted two types of surveys. One was a household survey based on the random selection of respondents from a locally constructed administrative list that had been carefully checked. The second was the outcome of participatory activities—the construction of a detailed village map—in these communities. We sought to determine whether these two methods yielded comparable characterizations of these villages.

We began by considering coverage error. We examined how a sample frame, based on official census lists and revised in discussion with local people, compared with one derived from a participatory mapping approach. We found that the revised official census suffered from a slightly higher level of undercoverage than the participatory map. However, the mapping exercise tended to lead to larger errors of overcoverage. We then investigated if these errors led to different conclusions with respect to certain characteristics of the underlying population. We controlled for the survey instrument used, and found that households sampled from the revised official census appear, *on average*, to be larger and wealthier. If we characterized the villages in terms of *total* size or *total* wealth, we obtained larger estimates from the participatory village mapping because of the overcoverage associated with this technique. Finally, we examined if the characterization of these villages was sensitive to the survey technique used. In particular, we compared results obtained from the same households, but drawn from different survey

instruments. We found that the participatory village mapping, by which information on the households is obtained in a public setting, produced higher estimates of household size and lower estimates of household wealth than the household survey, in which households are surveyed in private.

2. DESCRIPTION OF SURVEY METHODS

The purpose of our study was to examine measures of well-being in the *Zone Lacustre* of Mali, with particular attention paid to the incidence, severity, and causes of household food security and child undernutrition. The survey site was centered around the town of Niafunké, approximately 200 kilometers southwest of Tombouctou. Since the mid-1980s, this area has been the scene of regular food aid interventions and increased development activity, driven largely by external donors, a feature we return to in our conclusion. In this region of Mali, people live in villages that take the form of nucleated settlements. Within the villages, individuals live in dwellings or huts grouped together in a compound surrounded by a wall. One further distinguishes families, who consist of several households linked by kinship. As the household forms the basic social unit in this locality, and hence the basic unit for our analysis, we needed to construct a list of all households actually living in each village to be surveyed. Following local practice, households were defined as consumption units, that is, collections of individuals eating from a common pot. These individuals recognized a common authority, the household head, and shared their incomes. Although household members usually lived together in

the same dwelling or hut, this was not always the case. For example, it was not uncommon for brothers to live together with their wives and children in separate dwellings within the same compound, while still sharing their meals with and acknowledging their father as household head. These individuals also composed one household. Households typically consisted of parents and their children, and sometimes grandparents, brothers or sisters, and adopted children. Following local practice, polygamous households were counted as separate if each constituted a separate consumption unit and the women did not share their income or live in the same dwelling.

After discussions with local authorities in which we explained the purpose of our survey and guaranteed the confidentiality of data obtained, we were granted access to an administrative listing of families residing in each village. These had been compiled in 1996, the year before the study began. They serve several purposes, including providing a basis for village taxation and the delivery of food aid. In theory, these are kept up-to-date by local government officials. When we began our survey work, we were aware that such lists might well be inaccurate and that it would be necessary to distinguish between families, who were identified on these lists, and households, which were not. Consequently, in each village surveyed, we took these lists to a meeting with the village head and a conclave of elders. Collectively, the list was reviewed to identify families that no longer resided in the village and, where necessary, distinguish households from families. This generated a list households and made it possible to eliminate names that were no longer resident. Households, resident in the village but not registered as such, were added. In other words, we revised an official listing that purported to enumerate

everyone within a prescribed locality (as opposed to working with a self-description of the community). Accordingly, we called the sample frame that resulted from this exercise the revised official census (ROC),² which was used to take a one-third sample of each village. Using a structured questionnaire that included questions on household composition, assets, income, expenditures, and anthropometry, enumerators resident in the villages being studied interviewed households four times over a one-year period.

Our second method draws from a technique popularly used in participatory rural appraisal. In the strictest sense of the word, we did not undertake PRA. A better description is that we undertook a rapid community appraisal, in that we included community members in the research and relied on participatory methods.³ We consulted with the village head in each village to ask permission to get the entire village together for an exercise of constructing a village map and to establish a convenient time and place that would ensure the maximum participation of all village members. At the appointed time, we began by outlining the purpose and methods of the exercise. The village head would begin by constructing his compound and his dwelling within it, using *banco* (the local material used to make dwellings), as well as the location of the village mosque. Another person was requested to identify the locations of the main tracks within the village and individuals were encouraged to locate their own dwellings and compounds. As the map neared completion, we discussed the concept of household with the participants, and starting from the dwellings on the map, we identified all households

² Our thanks to an anonymous referee for suggesting this term.

³ The phrase *rapid community assessment* was coined by Dan Maxwell.

residing in the village. Where the inhabitants of two or more dwellings composed only one household, this was so indicated. Local materials such as twigs and stones were used to represent the number of people resident in each household, the number of livestock, and the amount of land they operated. Typically, the entire exercise took one to two hours. In keeping with the spirit of PRA, the research team “handed over the stick” (Chambers, 1994a). That is, beyond facilitating participation at the beginning, we stood by while the map was constructed. As far as we could tell, residents in these villages greatly enjoyed this exercise. Once it was well underway, we began to systematically record the information being presented to us—constructing a listing of all households; recording the quantitative information being presented; and transferring to paper the three-dimensional map that had been created. We call this the RCA listing.

3. COVERAGE ERROR IN THE REVISED OFFICIAL CENSUS AND THE RAPID COMMUNITY APPRAISAL

Our first step was to compare the names of the households appearing on the ROC and the RCA listing. This initial comparison revealed that there were many households present on one list but absent from the other. To determine whether these discrepancies were real (as opposed to resulting from mundane factors such as misspellings of names), we arranged meetings with the village authorities. We went through the ROC and reconciled it to both the RCA list and the village map. Households that could not be identified were noted separately. We then went through the RCA list, locating on the village map those households not appearing on the ROC. As a final check, we tried again

to locate households that appeared on the ROC but not on the RCA listing. These reconciliations were lengthy and somewhat exhausting. In one of the larger villages, it took four researchers, the village head, and five of his advisors nearly five hours to reconcile the lists. However, it proved to be time extremely well spent. In particular, two sources of discrepancies were identified. First, in some cases the lists had been produced several months apart. (The ROCs were done in August and September, 1997 while the RCA listings were compiled over from August, 1997 to February, 1998.) In and out migration, deaths of household heads, and household reformation accounted for some of the differences between the lists. A more important source of discrepancy was confusion over the names of household heads. There were people with completely different names on different lists and people with just slightly different names. For example, in one village Abdoulaye Traoré was also known as Hamedi Traoré,⁴ and Hamadou Yattara and Amadou Yattara also proved to be the same person. But more complicated cases also arose. For example, in one village Ali Baba Touré appeared on the ROC and Baba Ali Touré was on the RCA list. They turned out to be different people: the former being the father who passed away between the construction of the two lists and the latter being his son who took his place as head of household. Also, despite our best attempts to use consistently a local definition of the household, confusion continued to arise. For example, a single young man who had his own compound, was described as being the

⁴ These names are pseudonyms. We use them to convey a sense of the challenges that we encountered when reconciling these lists.

head of a household, but slept in one compound (where he appeared on the ROC) while regularly eating in another (his sister's, who appeared on the RCA list).

This reconciliation exercise greatly reduced the number of discrepancies between the two lists. We again distinguished three groups: households appearing on both lists and those only appearing on the ROC or the RCA list. Table 1 presents the number in each group for the villages in which we undertook this comparative exercise. Roughly three quarters (72 percent) of all households listed appeared on both lists. Less than 5 percent appeared on only the ROC list, while 23 percent appeared on only the RCA list. A detailed case-by-case analysis showed that most of the households missed by the community mapping exercise should have been included. In some cases, these were households that appeared on the participatory map but whose names had been mistakenly omitted when this information was put onto a list. Furthermore, members of certain ethnic groups, the Tamasheq, Peulh, and Bozos tended to be excluded from the RCA lists. The Tamasheq and Peulh live a semi-nomadic life, the Bozos are fishers. Members of these groups were often absent on the day that the mapping exercise took place and their houses were not listed by their neighbors or other community members. In other words, the RCA list had an undercoverage rate of 5 percent—it omitted 22 out of a total of 430 households (368 + 22 + 40) that should have appeared on both lists.

By contrast, approximately two-thirds of the households appearing only on the RCA list did not in fact meet the criteria for being a household in this village. The principal reason for this was that during the physical construction of the community map, a number of dwellings were modeled that belonged to individuals who had out-migrated

and who no longer resided in the village. Furthermore, some households were counted twice, as they occupied separate houses in the same village. In the case of Tomba, the community map inadvertently included part of a neighboring settlement. In the confusion surrounding the physical construction of the map (at one point, more than 100 people were building the model of Tomba) this error went unnoticed. The community mapping exercise thus led to an overcoverage error of 19 percent, i.e., it included 80 households more than the 430 households that should have been included in both lists. Finally, 40 households appearing on the RCA list, should also have been picked up by the revised official census—resulting in an undercoverage error of 9 percent for the ROC (40 households out of 430).

To conclude, both approaches produced relatively small undercoverage errors, with undercoverage slightly more pronounced in case of the ROC. The participatory village map, however, also produced a significant error of overcoverage. We surmise that in the case of the administrative lists that we used to construct the ROC, there are both advantages (gaining access to food aid) and disadvantages (paying taxes) associated with being on the list. Finally, before proceeding with our household survey, we undertook several additional steps to check the lists, and this too may have reduced under and overcoverage. By contrast, the communities being studied were confident that they would suffer no adverse affect from appearing on the participatory map. Indeed, they perceived that benefits, such as food aid or development interventions, might be made available to them—despite the efforts of the research team to emphasize that this would not be the

case—and so had an incentive to list empty dwellings as resident households as well as duplicate households.

4. DO COVERAGE ERRORS REALLY MATTER?

We have shown that both the ROC and the RCA generated different listings of households within the same villages, although the real differences were much smaller than the apparent ones. Do these differences matter? Recall that the purpose of our survey work was to describe and analyze measures of well-being in these villages. In the case of the RCAs, such indicators were obtained using various participatory techniques.⁵ The ROC was used as a sampling frame, from which a random selection of households was drawn. Data collected from this sample were then used to draw inferences about the underlying population or about causal relations such as the relationship between food consumption, wealth, and household demographic characteristics. In either case, a discrepancy between the “true population” and that enumerated by the RCA list or the ROC could lead to biased estimates of these characteristics. The magnitude of this bias forms one basis for assessing these two methods. The other important criterion is cost effectiveness, as higher validity might come at the price of higher survey costs.

The manner in which both over- and undercoverage error introduces bias in a statistic depends on the form of the statistic itself (Groves, 1989). Here, we consider both

⁵ In addition to the community map, we held focus group discussions on local perceptions of the concepts of food security and causes of hunger, constructed time lines and classified households using pile sorts.

village *total* and village *mean* demographic and wealth characteristics as well as the distribution of household wealth.⁶ In this region of Mali, the village totals are especially relevant because the provision of food aid is often based on measures of village size, such as total village population or total number of female-headed households. Consequently, biased estimates of these sums could lead to a misallocation of scarce resources.

In the appendix to this paper, we show that for linear statistics such as totals or means, the bias induced by coverage error consists of two components: an undercoverage and an overcoverage error term. These have opposite signs. Each term consists of the relative importance of the error, as measured by their proportion with respect to the total population, and the difference in the survey statistic between those appearing in the (ROC or RCA) list and those who are wrongly omitted or included. The key insights are the following.

First, even if a large proportion of the population is erroneously excluded, this does not necessarily induce large biases as long as the characteristics of the population excluded closely resemble those of the population covered. On the other hand, even a small undercoverage error might well lead to substantial bias if those excluded have very different characteristics compared to the households on a given list. The effect of overcoverage can be interpreted in an analogous fashion. Second, the joint effect of undercoverage and overcoverage depends on the magnitudes of the differences—they can either exacerbate or offset each other. Third, the bias induced by coverage error will not

⁶ An introductory discussion of the effect of coverage error on analytical statistics such as measures of the (causal) relationship between variables, such as regression coefficients is found in Groves (1989).

necessarily be constant across different variables. Undercoverage of pastoralists (such as Peulh and Tamasheq people) by the village mapping for example, might lead to greater bias with respect to average livestock ownership without affecting average household size. Fourth, in the case of sums, the bias consists of an undercoverage and an overcoverage term, opposite in sign, and each a product of their respective number and mean value. Large differences in the numbers of households wrongly included and excluded, can thus lead to substantial overestimates or underestimates of the sum total of interest, even if the mean values of the overcovered and undercovered populations are alike.

We now examine whether coverage error actually does change our inferences. Ideally, we would estimate the *absolute* magnitude of the coverage error bias for each method by comparing computed statistics against their “true” value. However, because the “true” value is unknown, we could not do this. However, we could examine their performance relative to each other by comparing statistics generated by both methods. In the appendix, we show that the difference in these statistics equals the difference in their coverage bias.

Before undertaking this evaluation, we need to be cognizant of two additional factors. Characterizations derived from households randomly drawn from the ROC and those obtained from the RCA list will also be affected by the fact that these data were collected in different settings. The randomly selected households were administered a structured, largely quantitative questionnaire in a relatively private setting—the household courtyard—by an enumerator who could validate the information being

provided by direct observation (cross-checks were also built into the questionnaire). By contrast, the community map was built in a highly public setting. Validation of information provided here came through comments and corrections made by other members of the community. In section 5, we further consider the impact of these settings on the data we collected. Here, in order to focus solely on the impact of differences in coverage, we need to use data collected in exactly the same manner. Accordingly, Tables 2, 3 and 4 use data derived only from the community map. With these data, we compare the characteristics of those households drawn randomly from ROC to all households appearing on the map.⁷

Second, since our statistics about the population enumerated in the ROC are obtained from a randomly selected sample, we need to construct confidence intervals for these estimates. Suppose that the value of the statistic drawn from the RCA falls outside this confidence interval. If this is the case, we can conclude that at least some of the differences in these statistics is due to coverage error. The further away the RCA statistic is from the confidence interval, the larger the bias induced by the choice of sampling frame. If the RCA statistic falls within the confidence interval, differences between the statistics obtained from the ROC and the RCA could be attributed to randomness induced by sampling. Although coverage error may also be playing some role, we cannot discern an effect that is separate from randomness in the estimated statistic.

⁷ This is possible, since all households sampled for the detailed household questionnaire (but one) also appear on the list resulting from the community map.

From Table 2, it can be seen that average household size across all villages is larger in the sample randomly drawn from the ROC than from the RCA, the difference being statistically significant at the 5 percent confidence level. The same result holds for each village separately, though due to relatively small sample sizes, the differences are not statistically significant. Sampling from the ROC leads to overestimates of household size relative to the RCA. As the original administrative list—the basis for the ROC—only reflected registered families, this suggests that small households within the family, often only consisting of a few older (sometimes sick or handicapped) people, were underreported as separate households during the revision. Similarly, all unregistered small households might not have been included.

One might expect that these erroneously omitted households will tend to be poorer. This hypothesis can be tested, either by comparing average household wealth or by looking at the distribution of wealth. Table 2 shows that almost all indicators of average household wealth are higher for the sample drawn from the ROC. Yet, we cannot conclude that this is a statistically significant difference, as the values for the RCA listing fall within sample confidence intervals. Further analysis of the distributions of household wealth however, shows that the households on the ROC list are richer. In particular, we examined whether the distribution of household wealth based on our sample from the ROC lies unambiguously to the right of the distribution of all households listed on the

community map. We did so using first-order stochastic dominance analysis.⁸ Specifically, we calculate a Kolmogorov test statistic, which measures the maximum vertical distance between the cumulative density function of the sample drawn from the ROC ($S(x)$) and the values obtained from the RCA ($C(x)$) (Conover, 1980). The test statistics reported in Table 3 indicate that the household wealth distribution of the sample from the ROC first-order stochastically dominates the household wealth distribution of the community map in virtually all villages.⁹

Taken together, Tables 2 and 3 suggest that the village maps constructed as part of the RCA also capture smaller and poorer households. Consequently, estimates of mean values of household size and wealth tend to be smaller when we compare the RCA data with those obtained from households drawn from the ROC. This may be related to the fact that the village map visualizes all housing units, also those of poorer and smaller households, thereby increasing their probability of appearing on the list. But recall that we are also interested in the summation of village characteristics. In the case of RCA, we added up the quantitative information provided to us by each household. Totals for the ROC are obtained by multiplying the means from Table 2 by the total number of households on the ROC.

⁸ Formally, first-order stochastic dominance of a distribution S over a distribution C is equivalent to the condition that the cumulative density function $S(x)$ of distribution S is always equal to or smaller than the cumulative density function $C(x)$ of distribution C , with $S(x) < C(x)$ for some x and $x \in X$, the wealth range of both distributions.

⁹ In Table 3 we only compare the distributions of cattle and sheep/goat ownership because the ownership of ploughs, carts and proahs, and draft animals is generally limited to at most one per household. Percentages of equipment and draft animal ownership are reported in Table 2.

Table 4 shows that with respect to total population, almost all RCA demographic variables lie outside the 95 percent confidence interval, being significantly larger than the ROC sample estimates. The one exception, total residential population, lies just on the boundary of the confidence interval. These results are due to the large overcoverage error on the RCA list. The participatory mapping exercise produced an estimate of village population about 10 percent higher than the ROC sample. Although average household size derived from the community map is smaller than in the sample, the overcoverage in the RCA list more than compensates for this. Given that food aid is mostly related to the total population, this is not an innocuous finding. Further note that the reported number of migrant members is about 33 percent higher in the RCA list. This result, driven by overcoverage, is further reinforced by the particular nature of the overcoverage error. Several dwellings modeled were no longer inhabited, especially in Tomba, and their former residents were listed twice, as household members and as migrant members, increasing the average number of migrant members per household amongst the wrongly included households.

5. DATA COLLECTION IN PUBLIC AND PRIVATE SETTINGS

We now turn to a third question associated with these two methods, whether the mode of data collection—in a public or a private setting—might also affect the results we obtained. We took the households that appeared on both lists and compared the characteristics reported in the household survey (obtained by enumerators resident in the

village) with those obtained via the community mapping, where either a household member or a well-informed neighbor or resource person provided the information. The results of this comparison, summarized across all villages, are reported in Table 5.

We find that the estimates of household size are significantly higher in the community mapping and reported levels of asset ownership (with the exception of draft animals) significantly lower. Differences in other demographic variables were not statistically significant. The first finding is consistent with the larger overcoverage error reported for the community maps. As it was perceived that only benefits could be acquired (potentially through food aid or development projects) from appearing on the participatory mapping (despite our repeated efforts to explain that this was not the case), villagers had incentives to maximize the number of households in the village. Similar motivations would lead to reporting higher household sizes. These influences might be more easily overcome during the personal interview with a structured household questionnaire, where group pressure is absent and more triangulation opportunities are available.

It is notoriously difficult to obtain credible data on assets, especially livestock ownership. Our experience during the several household survey rounds, with several opportunities to verify the numbers reported, suggested that the household survey numbers are lower bounds. Consequently, the RCA figures must be underestimates. It may be that in these very poor communities, individuals are reluctant to reveal their asset holdings in public. Furthermore, reporting high asset ownership might be perceived as detrimental to receiving potential future aid.

6. CONCLUSIONS

In this paper, we compared information on five villages derived from a sample drawn from an ROC and from a participatory village mapping exercise. We found that both methods generate a small amount of undercoverage, but the community mapping work also leads to significant overcoverage. When we controlled for the survey instrument used (by relying only on data obtained from the construction of the village maps), we found that households sampled from the ROC appear, *on average*, to be larger and wealthier. If we characterize these villages in terms of *total* size or *total* wealth, we obtained larger estimates from the RCA list because of the overcoverage associated with this technique. When comparisons are restricted to households that appear in both the random sample drawn from the ROC and the community map—that is, comparing results from different survey instruments controlling for the sample frame—we find that the latter produced higher estimates of household size and lower estimates of wealth.

Before concluding, we note two caveats. First, we must be careful not to over-generalize from these findings. They pertain to a particular region. Whether they are replicable elsewhere is an empirical question. Second, even if outsiders and participants agree on concepts and definitions, there can still remain differences in interpretation and application. Although we adopted local definitions of households, there will always be borderline cases. In the context of household surveys, our enumerators had criteria by which they could adjudicate, for example, the definition of a “migrant.” By contrast, in

the participatory mapping exercise, respondents undertook this adjudication. This is not to say that one set of criteria was more valid, but rather that the same criteria can be used in different ways by different actors. Similar considerations can be attributed to the notion of a village. We followed local administrative conventions that defined villages in geographical terms. By contrast, village boundaries and village membership did not always conform to administrative boundaries. This can be seen in those instances where certain ethnic minorities did not appear for the participatory activities (because they formed a separate group and therefore did not “live” within the village) or where local boundaries differed from administrative ones.

Mindful of these caveats, these results can be read in a number of ways. They can be used to support the claim that participatory mapping is *more* accurate than a sample of households randomly drawn from an ROC because it is less likely to exclude poorer, smaller households. Conversely, participatory mapping could be regarded as *less* accurate due to the overcoverage we observe and the apparent underreporting of assets.

Our interpretation is somewhat different. We surmise that these results are principally driven by the particular dynamics of these different activities. Despite our best efforts to remain “invisible” during the participatory mapping exercise, we suspect that even our minimal presence was sufficient to induce households to alter their responses. In an environment where everyone is aware that most outsiders are associated with financial resources that are to be disbursed, data may be as much the outcome of social interactions

as they are immutable “facts.”¹⁰ Thus, for example, the “number of people resident in a household” is not just a figure to be measured, but also possibly part of a negotiation with a respondent, who perceives that financial gain may come from proposing a higher figure than is actually the case. A different set of social interactions affected our household survey. As discussed in Section 5, here, there were repeated measurements of these data conducted in a private, rather than public gathering, and often interviewing was supplemented with direct observation and triangulation with other information in the questionnaire.

If our supposition is correct—that different survey techniques generate different social dynamics between research teams and their respondents—then it is incorrect to claim the “superiority” of one method over another. Instead, it is important to carefully examine and acknowledge the biases that may result from the particular method being used. It also points to the importance of triangulating, or cross-checking, information that is obtained. We further stress that our use of both techniques was not driven so much by a desire to determine the “right method,” but rather by our desire to enrich our understanding of these villages. The participatory appraisal techniques allowed us to interact with certain groups, such as women, that was simply infeasible when visiting individual households. They also allowed us to observe the dynamics of these villages literally “at work,” and led to a more nuanced understanding of dynamics within these villages (such as relations between different ethnic groups) as well as their relationships

¹⁰ These issues are discussed further in Lockwood (1992).

with outsiders such as ourselves. Our quantitative surveys enabled us to complement these understandings with a more detailed, in-depth look at a wide variety of measures of deprivation.

APPENDIX

DERIVING ESTIMATES OF BIAS DUE TO UNDERCOVERAGE AND OVERCOVERAGE

Below, we derive the formulae for estimating biases due to undercoverage and overcoverage. In what follows below, N_j will refer to numbers of households in category j , and S_j will refer to the statistic (for example, mean household size) calculated for households in the j th category. Recall that we distinguished between households on the ROC and RCA lists, households that were covered on both lists, and households that were either overcovered or undercovered on either of the lists. So we have:

Description	Symbol
Households common to the ROC and the RCA list, i.e., that appear on both lists.	N_c
Households that correctly appear on the ROC, but not on the RCA list, i.e., an undercoverage error of the RCA list.	$N_{u,rca}$
Households that wrongly appear on the ROC, but not on the RCA list, i.e., an overcoverage error on the ROC.	$N_{o,roc}$
Households that correctly appear on the RCA list, but not on the ROC, i.e., an undercoverage error on the ROC.	$N_{u,roc}$
Households that wrongly appear on the RCA list, but not on the ROC, i.e., an overcoverage error on the RCA list.	$N_{o,rca}$
Total number of households on the ROC.	N_{roc}
Total number of households on the RCA list.	N_{rca}
Total number of households.	N

Similarly, S_c refers to the statistic for households that appear on both the ROC and the RCA list, $S_{u,rca}$ refers to the statistic for households that correctly appear on the ROC list but not on the RCA list, and so on. Note that we assume here that the “true population” is the sum of households appearing on both the ROC and RCA lists as well as those that were undercovered on one list, but not the other ($N = N_c + N_{u,roc} + N_{u,rca}$). By doing so, we can express any linear survey statistic about the “true population” as the sum of three components:

$$S = (N_c / N) \cdot S_c + (N_{u,rca} / N) \cdot S_{u,rca} + (N_{u,roc} / N) \cdot S_{u,roc} \quad (1)$$

$$S_{roc} = (N_c / N_{roc}) \cdot S_c + (N_{o,roc} / N_{roc}) \cdot S_{o,roc} + (N_{u,rca} / N_{roc}) \cdot S_{u,rca} \quad (2)$$

$$S_{rca} = (N_c / N_{rca}) \cdot S_c + (N_{o,rca} / N_{rca}) \cdot S_{o,rca} + (N_{u,roc} / N_{rca}) \cdot S_{u,roc} \quad (3)$$

To illustrate the nature of the bias caused by coverage error, multiply equation (2) by (N_{roc} / N) and add $(- (N_{o,roc} / N) \cdot S_{o,roc} + (N_{u,roc} / N) \cdot S_{u,roc})$ to yield

$$S = (N_{roc} / N) \cdot S_{roc} - (N_{o,roc} / N) \cdot S_{o,roc} + (N_{u,roc} / N) \cdot S_{u,roc} . \quad (4)$$

Note that $N_{roc} = N_c + N_{o,roc} + N_{u,rca}$. By adding $(N_{u,roc} / N) \cdot (S_{roc})$ to both sides and rearranging terms, equation (4) can be expressed as

$$S_{roc} = S + [(N_{u,roc} / N) \cdot (S_{roc} - S_{u,roc}) - (N_{o,roc} / N) \cdot (S_{roc} - S_{o,roc})]. \quad (5)$$

A similar manipulation of equation (3) yields:

$$S_{rca} = S + [(N_{u,rca} / N) \cdot (S_{rca} - S_{u,rca}) - (N_{o,rca} / N) \cdot (S_{rca} - S_{o,rca})] \quad (6)$$

Equations (5) and (6) show that the bias induced by coverage error consists of two components: an undercoverage and an overcoverage error term. Both terms have opposite signs and are each the product of their relative importance, as expressed by their proportion with respect to the total population of interest, and the difference in the survey statistic between those covered in the frame population and those who were not covered or wrongly covered.

Sum totals are calculated by multiplying mean values by the number of household listed under both methods. The effect of coverage error on sum totals can be expressed as the sum of two components:

$$N_{roc} \cdot \bar{S}_{roc} = N \cdot \bar{S} - N_{u,roc} \cdot \bar{S}_{u,roc} + N_{o,roc} \cdot \bar{S}_{o,roc} \quad (7)$$

and

$$N_{rca} \cdot \bar{S}_{rca} = N \cdot \bar{S} - N_{u,rca} \cdot \bar{S}_{u,rca} + N_{o,rca} \cdot \bar{S}_{o,rca} \quad (8)$$

where \bar{S} is the mean value of variable S of the population of interest and \bar{S}_j is the mean value of variable S of group j , with the subscripts j taking the same meaning as above.

Finally, note that by respectively differencing equations (5) and (6) and (7) and (8), the difference between the statistics obtained from each method equals the difference in their coverage bias. That is,

$$D_{roc,rca} = S_{roc} - S_{rca} = ((N_{u,roc} / N) \cdot (S_{roc} - S_{u,roc}) - (N_{o,roc} / N) \cdot (S_{roc} - S_{o,roc}))$$

$$(S_{rca} - S_{u,rca}) - (N_{o,rca} / N) \cdot (S_{rca} - S_{o,rca}) \quad (9)$$

and

$$DT_{roc,rca} = N_{roc} \cdot \bar{S}_{roc} - N_{rca} \cdot \bar{S}_{rca} = (-N_{u,roc} \cdot \bar{S}_{u,roc} + N_{o,roc} \cdot \bar{S}_{o,roc}) \\ - (-N_{u,rca} \cdot \bar{S}_{u,rca} + N_{o,rca} \cdot \bar{S}_{o,rca}) \quad (10)$$

where $D_{roc,rca}$ is the difference between the values of linear statistic obtained from the ROC and RCA lists and $DT_{roc,rca}$ is the difference between the values of the sum total obtained from the ROC and RCA lists.

TABLES

Table 1—Number of households (post reconciliation) on the revised official census and the rapid community appraisal list

Number of households (post-reconciliation) by village	On both lists	Only on the revised official census list	Only on the rapid community appraisal list
Tomi	33	2	0
Goundam Touskel	36	5	5
Anguira	65	7	17
Tomba	114	3	60
N'goro	120	10	38
Total = 515	368	27	120

Table 2—A comparison of average household demographics and wealth based on a random sample from the revised official census (ROC) and a rapid community appraisal (RCA)

	Tomi		Goundam Touskel		Anguira		Tomba		N'goro		Total from all five villages	
	Sample from ROC	RCA	Sample from ROC	RCA								
Household Demographics												
Household size (residential)	6.82 (±1.78)	6.24	7.43 (± 3.07)	6.00	5.13 (±1.32)	5.05	4.72 (± 0.77)	4.46	6.53 (± 1)	5.83	5.94 (± 0.57)	5.25
Percentage of households with migrants	46 (± 34)	42	36 (± 29)	34	42 (± 22)	41	44 (± 17)	49	47 (± 13)	41	44 (± 8)	44
Percentage of Sonraï households	73 (± 31)	85	50 (± 30)	44	17 (± 17)	20	100 (± 1)	100	80 (± 11)	79	71 (± 8)	74
Percentage of male headed households	91 (± 21)	85	79 (± 25)	78	96 (± 11)	98	92 (± 10)	94	93 (± 7)	95	92 (± 5)	93
Household Wealth												
Percentage of households with carts, ploughs or <i>proahs</i>	18 (± 27)	15	29 (± 27)	15	25 (± 19)	15	3 (± 7)	1	5 (± 6)	3	23 ± 7)	20
Percentage of households with draft animals	18 (± 27)	12	29 ± 27)	15	25 (± 19)	15	5 (± 8)	1	2 (± 4)	1	22 (± 7)	19
Number of cattle per household	0.73 (± 1.14)	0.76	2.14 (± 1.02)	2.17	25 (± 19)	19	3 (± 7)	2	0.38 (± 0.22)	0.26	0.72 (± 0.28)	0.67
Number of sheep and goats per household	5.45 (±0.14)	3.12	4.43 (± 2.85)	4.63	37.5 (± 21)	36.7	46 (± 17)	43	1.25 (± 0.68)	0.80	2.32 (± 1.33)	1.80
Number of observations	11	33	14	41	24	79	39	170	60	158	148	481

Notes: Confidence intervals are in parentheses. These are based on t-values for averages and continuity corrected z-values for proportions. For Anguira and Tomba, data on cattle, sheep, and goats were recorded as 1 if households possessed any of these animals, zero otherwise. Consequently, numbers for Anguira and Tomba are not considered in the figures found in the last column. Figures in italics for these two villages are percentages of households reporting ownership. For 7 households on the RCA list in Anguira and Tomba, information was missing or incomplete. As their number is negligible compared to the total population for these two villages, their potential impact on the census averages has been ignored.

Table 3—Testing for first-order stochastic dominance of household wealth distributions drawn from a sample of the ROC and the RCA list

Value of Kolmogorov test statistic T (=sup(S(x)-C(x)))	Tomi	Goundam Touskel	Anguira	Tomba	N'goro	Total from villages
Distribution of Household Wealth						
Number of cattle per household	0.03	0.17	-0.06	-0.02	-0.04	0.02
Number of sheep and goats per household	0.12	0.05	-0.08	0.02	-0.03	0.00
Critical value for T at 5% significance. level	0.35	0.31	0.24	0.19	0.15	0.13
Number of observations	12	14	24	39	60	85

Notes: We do not reject the null hypothesis that the household wealth distribution from the sample of the ROC first-order stochastically dominates the household wealth distribution from the RCA in cases where the computed Kolmogorov test statistic is less than the critical value. For Anguira and Tomba, data on animals were collected in a qualitative manner. Consequently, both villages have been excluded in the calculation of the test statistics for the total population reported in the last column. The reported critical values for the Kolmogorov test statistic are only exact when the reference cumulative density function (the RCA distribution in our case) is continuous. Otherwise the critical values tend to be conservative. The exact critical values for discrete reference distributions are often only one-third of their continuous counterparts. Exact critical values for discrete reference distributions, however, are not tabulated and their calculation is not straightforward (Conover, 1980). We therefore start by taking as critical value, one-third of the exact critical values for the corresponding continuous distributions. If the Kolmogorov statistic is well below this value, we can be confident not to reject the null-hypothesis.

Table 4—Village size and wealth based on a sample from the ROC and the RCA list

	Tomi		Goundam Touskel		Anguira		Tomba		N'goro		Total from all five villages	
	Sample from ROC	RCA	Sample from ROC	RCA								
Village Demographics												
Residential population	239 (±62)	206	305 (±126)	246	369 (±95)	414	552 (±90)	776	849 (±130)	921	2346 (±225)	2563
Number of migrants	32 (±30)	27	29 (±33)	25	42 (±25)	47	81 (±36)	197	117 (±47)	104	308 (±77)	405
Number of Sonraï households	26 (±11)	28	21 (±13)	18	12 (±12)	16	117 (±1)	174	104 (±14)	125	280 (±32)	361
Number of male-headed Households	32 (±7)	28	32 (±9)	32	68 (±8)	80	108 (±12)	164	121 (±9)	150	363 (±20)	454
Number of observations	35	33	41	41	72	82	117	174	130	158	395	488
Village Wealth												
Number of carts, ploughs and <i>proahs</i>	9 (±15)	6	18 (±20)	8					9 (±10)	6	31 (±22)	20
Number of draft animals	9 (±15)	7	23 (±22)	11					4 (±8)	2	31 (±23)	20
Number of cattle per household	26 (±40)	25	88 (±42)	89					49 (±29)	41	148 (±58)	155
Number of sheep and goats per household	191 (±355)	103	182 (±117)	190					163 (±88)	126	478 (±274)	418
Number of observations	35	33	41	41					130	158	206	232

Notes: Confidence intervals are in parentheses. For Anguira and Tomba data on material possessions (carts, ploughs and *proahs*) and animals were collected in a qualitative manner. For these two villages indicators on asset ownership have not been reported and the totals in the last column reflect asset ownership for the total population in the remaining villages.

Table 5—Household demographic and wealth characteristics obtained from a household survey and a RCA on a sample drawn from the ROC

	Household averages across five villages		
	Household Survey	RCA	t- or z-value
Household Demographics			
Household size (residential)	5.62	5.94	- 2.66 *
Number of migrants per household	0.70	0.78	0.67
Percentage Sonraï households	68	72	1.32
Percentage male-headed households	96	92	1.33
Number of observations	148	148	
Household Wealth			
Number of carts, ploughs and <i>proahs</i> per household	0.26	0.15	2.82 *
Number of draft animals per household	0.15	0.15	0
Number of draft animals per household	2.61	0.72	5.43*
Number of sheep and goats per household	8.27	2.32	6.47 *
Number of observations	85	85	

Notes: Data on household wealth exclude Anguira and Tomba; see Table 2 for explanation.

* statistically significant at 5 percent level; paired sampled t-value for comparison of means, paired sample z-value for comparison of proportions. See Newbold (1988) for derivation of these test statistics.

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