Who Lives Here?
Survey Undercoverage and Household Roster Questions

Roger Tourangeau,[1] Gary Shapiro,[2] Anne Kearney[3] and Lawrence Ernst[4]

We carried out an experimental comparison of three versions of questions for enumerating the residents of a dwelling. One version took the approach that is used in many surveys: it began by asking respondents to name all persons living at the dwelling. The experimental versions began by asking how many persons had spent the previous night at the dwelling and used other probes to complete the roster. The two experimental versions differed only in that one version did not require persons to be listed by their full names, allowing respondents to use initials or nicknames instead. A total of 509 interviews were completed, about a third of them with each version of the questionnaire. The results indicated that both experimental versions of the roster questions yielded more persons per household than the standard version; however, only the version that did not require full names yielded more persons identified as usual residents of the dwelling. Additional analyses indicated that the same types of persons were listed on all three versions; only the number of persons listed differed.

Key words: Roster; undercoverage; usual residence.

1. Introduction

During the fall of 1992, NORC carried out a methodological experiment for the U.S. Bureau of the Census. The purpose of the study was to test new versions of the roster questions used to enumerate members of sample households in many surveys in an effort to improve coverage of household members. In addition, the new roster questions permitted comparison of different rules for linking persons to dwellings. In 1991, a test of differing rostering procedures had been conducted in conjunction with the American Housing Survey (Shapiro, Diffendal, and Cantor 1993). In the same year, a small-scale

experiment, funded by the Survey of Income and Program Participation (SIPP), compared two versions of the roster questions (Cantor and Edwards 1992; Shapiro et al. 1993). This study built on the results of these earlier efforts to improve the coverage of federal surveys.

Problems in enumerating household members are part of the larger problem of coverage errors in surveys and the decennial census. Coverage errors encompass problems resulting in overcounts (typically arising from counting individuals more than once) as well as those producing undercounts, although most research attention has focused on the issue of undercounts. The omission of members of households that are partially enumerated is thought to be a major source of undercounts. Shapiro et al. (1993) estimated that 60% or more of the persons missed in the Current Population Survey (CPS) are members of partially enumerated households – that is, both the dwelling unit and the household were included, but individual household members were nonetheless omitted from the survey. Similarly, using data from the 1986 Los Angeles test census, Fein and West (1988) estimated that roughly two-thirds of the persons missed by the test census were members of partially enumerated households. Results from the 1990 Post-Enumeration Survey carried out to estimate coverage in the 1990 census suggest that nearly a third of the omissions occurred within partially enumerated households.

Recently, several evaluations of coverage problems in the census (Fein 1990; Fein and West 1988; Hogan 1993; Robinson, Ahmed, Gupta, and Woodrow 1993) and in the CPS (Hainer, Hines, Martin, and Shapiro 1988; Fay 1989) have been reported. Much of the work described there and in earlier research on undercoverage problems (e.g., Valentine and Valentine 1971) suggests that within-household omissions arise through several distinct processes. For example, Brownrigg and Martin (1993) argue that undercoverage occurs for five main reasons: (1) mobility; (2) illiteracy and other language problems; (3) deliberate concealment; (4) irregular household structure or living arrangements; and (5) resistance as a strategy for dealing with the government and other persons from outside the community.

These causes can be grouped into two larger categories – those involving deliberate omissions and those involving difficulty in applying the residence rules underlying the roster questions to the living situations of the persons to be enumerated. As Hainer and his coworkers put it:

One reason people are missed is motivational. Black (and Hispanic) males are deliberately omitted from household rosters because of the potential loss of household income if the men were known to authorities. ... A second cause of undercoverage is the lack of fit between the census definitions of household and residency, and people's actual living situations.

Both processes are thought to contribute to the very high rates of undercoverage observed for young Black men (ages 20 to 24), one-third of whom may be missed in the CPS (Hainer et al. 1988; see also Shapiro et al. 1993). In Black households, respondents may believe that complete reporting of household membership may lead to such consequences as loss of welfare eligibility, eviction, or arrest. Within Hispanic households, the threat of deportation may be an additional concern.

Compounding these motivational problems are the definitional difficulties that can arise with the complex family structures apparent in many Black households. According to Hainer and his colleagues, Black households often comprise extended families whose

members reside in several nearby dwellings and whose composition can change quickly; in such families, key roles may be filled by persons unrelated to the head of household, and members of the household may disagree among themselves as to who is actually a member. Ethnographic research suggests that Hispanic households may also exhibit structures that do not readily fit the definitions embodied in most survey roster questions. McKay (1992) has noted a common arrangement within Salvadoran households that have recently immigrated to the United States, in which a core family shares a residence with one or more roomers. The roomers are particularly likely to be omitted from household rosters reported for surveys (Hainer et al. 1988; McKay 1992); according to McKay, they are simply not seen as ''residing'' with the household.

The study reported here attempted to address both categories of causes. In an effort to reduce deliberate omissions, we allowed some respondents not to report full names; in an effort to circumvent definitional difficulties, we developed an extensive set of probes.

There is evidence that undercoverage seriously biases estimates for a number of household characteristics. Moreover, a number of readily-measured household characteristics that may indicate a bad fit with the standard definitions are known to be related to undercoverage. These include household size (members of large households are more likely to be omitted; see Fein and West 1988) and household composition (household members who are not related to the head of household and members who are related but not members of the same nuclear family are more likely to go unreported; Fay 1989; Fein 1990; Fein and West 1988; Hainer et al. 1988). Race and ethnic differences in undercoverage may also result in part from differences in family structure and the resulting differences in the fit between respondents' living arrangements and the definitions underlying the enumeration procedure. Similarly, variables that are likely to be related to the need for concealment of household members (such as the presence of household members who are welfare recipients or recent immigrants) have also been shown to predict undercoverage (Fein and West 1988).

The questionnaires used in this study were modelled on those used in the Survey of Income and Program Participation (SIPP). SIPP is a longitudinal survey of adults, conducted in person, that measures economic and demographic characteristics. Households in the SIPP sample are interviewed every four months for a total period of participation of two and a half years; households that move during that period are followed to their new addresses. The SIPP questionnaire embodies what is probably the most common approach for enumerating members of sample households in surveys. Virtually the first thing that new respondents are asked to do is to list the names of everyone who lives or stays at the address; later probes then attempt to capture babies, roomers, persons who are temporarily away, and others who may have been omitted from the initial roster of residents. (In addition, the longitudinal character of SIPP requires that the roster from one wave be updated in the next wave and that changes in household composition be recorded.)

As is true in many surveys, the SIPP counts a person as a resident of a particular dwelling if the person ''usually'' lives there. Under current U.S. Census Bureau definitions, a person's usual residence is the one where the person lives and sleeps most of the time; if a person spends time at several dwellings, the usual residence is the one where he or she stays most often. In principle, then, a plurality rule is to be used; in practice, however, respondents are left to their own devices in determining who to count as a usual

resident. Even if respondents were aware of the plurality rule, they may not have the information needed to implement it correctly; the respondent may not know where the person in question stayed when he or she was not staying at the sample residence. Other situations may also make it difficult for respondents to decide whether someone should be counted as a usual resident. It may be unclear, for example, whether to count a person who is staying in the respondent's home temporarily. The respondent may need an explicit reference period to help decide whether to include temporary residents. The study described here attempted to capture housing situations like these by using several alternative methods of developing household rosters.

2.   Method

Sample. The sample for the experiment consisted of 644 occupied dwelling units spread across a total of 49 blocks in three sites – the Chicago and Washington, D.C. metropolitan areas and the city of Baltimore. The Field Division of the U.S. Census Bureau selected and listed blocks that were predominantly Black and low income in composition; each block included at least 40 housing units. Twenty-five blocks were selected in the Chicago area, 18 of them located within the city limits and seven of them from two suburbs. Fifteen blocks were selected in the Washington metropolitan area, eight of them within the city limits and the remaining seven from nearby suburbs. All nine blocks in the Baltimore area were located within the city of Baltimore. Based on listings of each of the 49 blocks, NORC staff selected 15 housing units on each block using a systematic selection procedure; of the 735 dwellings selected in this way, 644 turned out to be occupied and thus eligible for interview.

NORC field staff completed interviews at a total of 509 of these dwellings, for a response rate of 79.0%. Unfortunately, other than their addresses, we have no information on the nonrespondents. Data were collected on a total of 1,949 individuals. Almost 92% of these – 1,791 persons – were Black.

The experiment compared three versions of the questionnaire. The interviewers arranged the questionnaires in a predetermined random sequence and then administered the questionnaires in this random order at each cooperating household. A total of 38 interviewers completed one or more interviews. The interviewing took place over a four-week period from November to December 1992.

Questionnaires. The three versions of the questionnaires embodied different approaches to enumerating the residents of the dwelling. The first, or standard, version of the questionnaire begins with items taken from the SIPP control card and used in many other household surveys. In Version 1, an initial roster of residents is compiled based on responses to the *standard roster questions* (''What are the names of all persons living or staying here? Have I missed any babies or small children? … Does (NAME) usually live here?''), and demographic information is collected for each person on the roster.

Next, a series of probes not included in standard rostering procedures were administered. The probes attempted to identify residents omitted from the initial roster. These *additional probes* ask:

- How many people besides those you've already listed stayed here last night?
- Is there anyone else who stayed at least one night during the past month?

- Is there anyone else who usually stays here but was not here during the last month?
- Is there anyone else who ate here at least once during the last week?
- Is there anyone else who usually eats here but who did not eat here during the last week?
- Is there anyone else who you consider to be a member of this household?
- Is there anyone else who considers himself or herself to be a member of this household?

Persons identified by these additional probes were added to the roster for the dwelling. The probes cover several reference periods (the prior night, the past week, and the past month) and, as a result, their exact yield may depend on both on time of the year and the day of the week on which they are administered.

Once the roster was complete, Version 1 continued with a series of questions designed to pinpoint the relation of each person listed to the sample dwelling. These questions concern a number of dimensions that respondents may consider in determining whether to count an individual as a usual resident (Gerber 1990):

- How many nights did NAME stay here during the last month?
- On how many days did NAME eat here at least once during the last week?
- Does NAME contribute money, food, or other help to this household?
- Do you consider NAME to be a member of this household?
- Was NAME here at all during the last day?
- Does NAME consider himself/herself a member of this household?
- Does NAME usually live here?
- Does NAME have other places where he/she frequently stays?

After these *follow-up items*, there was a series of questions on labor force participation. Version 1 concluded with several items regarding the respondent's reactions to some of the earlier questions.

The other two versions of the questionnaire contain essentially the same items as Version 1 but administered them in a different order. In Version 1, the standard roster questions come first, then the additional probes, and finally the follow-up items; in Versions 2 and 3, the additional probes were administered first, then the follow-up items, and finally the standard roster questions. (Both the additional probes and the standard items were reworded slightly to fit their new context.) Like Version 1, Versions 2 and 3 concluded with the labor force questions and the items assessing reactions to some of the earlier questions. Versions 2 and 3 differ from each other in only one respect: Version 3 required respondents to identify household members by their full names, whereas Version 2 did not; Version 2 allowed respondents to use initials, nicknames, or other means of identifying individuals instead. In this version, the introduction to the roster questions read:

We're going to ask some questions about each of these people. To help us keep track of which persons we're talking about, can you identify each one with a first name, a nickname, a set of initials, or by some other means? It is not necessary to give their full names.

At the conclusion of Version 2, respondents were asked to provide full names for all persons on the roster to permit any follow-up interviewing. In both Version 1 and Version 3, full names had been requested from the outset.

3.   Results

The analysis examined five main issues: (1) Did the household rosters compiled under the three versions of the rostering questions include different numbers of persons? (2) Did the rosters compiled under the three versions include persons with different relations to the residence? (3) What criteria are associated with household informants' decisions on whether to classify someone as a usual resident? (4) Do alternatives to the standard procedures for linking persons to dwellings improve coverage? and (5) Were respondents to Version 2 ultimately willing to provide full names for the persons they had listed on the household roster? In most of the analyses reported here, we examine persons listed on the roster for Version 1 prior to the administration of the additional probes. Thus, when Version 1 is compared to the other versions, the comparison assesses the effect of the additional probes on number of persons reported. Similarly, when we compare Version 2 to Version 3, the comparison reflects the effect of requiring full names in Version 3. A major issue for the analysis is whether asking the additional probes and relaxing the requirement that full names be used increased the number of usual household members reported.

The data from the experiment are clustered both by segment and by interviewer. Although neither the segments nor the interviewers constitute random selections from a larger population, we sought to capture the clustering in the data by treating them as if they were from a two-stage probability sample with segments as the first-stage unit and households as the second. We then reanalyzed the results, treating interviewers as the first-stage units. We used SUDAAN to carry out both sets of analyses. The two sets of results are quite similar, yielding the same conclusions in every case. This convergence is not surprising since interviewer assignments generally encompassed only one or two segments and each segment was worked only by one or two interviewers. As a result, we report only the significance tests from the analyses that treat the interviewers as the first-stage unit by which the data are clustered. For the more exploratory analyses that examine person-level data, we ignore the clustering of the observations.

There are several ways of comparing the number of persons rostered under the three versions of the questionnaire. One is to compare the average number of *total* persons listed per housing unit across versions. We would certainly expect to get larger averages when the additional probe questions are included since the probes would add persons with casual connections to the dwelling unit. Another method is to compare averages of *usual residents* across versions. That is, we only count persons for whom we get an affirmative answer to the question ''Does NAME usually live here?'' This is a simple way of deciding who we want to count as belonging to the unit, consistent with the practice of most federal surveys. In Section 3.4, we examine several alternative rules for deciding who should be linked to a given dwelling.

3.1.   Persons rostered by version

Table 1 shows the average number of total persons and usual residents listed on each roster by version of the questionnaire. For Version 1, the table displays the results only for those persons listed in response to the standard roster questions (which were administered first) and excludes those added by the later probes.

Total persons listed. Versions 2 and 3, with their numerous experimental probes, netted

Table 1. Average number of total persons and usual residents rostered by version

	Version 1 (n = 173)		Version 2 (n = 177)		Version 3 (n = 159)	
	Total persons	Usual residents	Total persons	Usual residents	Total persons	Usual residents
Total	2.90	2.83	3.94	3.13	3.87	2.99
All males	1.28	1.25	1.86	1.47	1.70	1.25
All females	1.62	1.58	1.99	1.63	2.10	1.69
Black males	1.12	1.09	1.80	1.45	1.55	1.18
Black females	1.48	1.45	1.93	1.60	1.98	1.60

Note: A few persons were missing sex data; as a result, column entries may not sum to column totals. Figures for Version 1 reflect only persons listed using the standard rostering questions.

more total persons that Version 1. On the average, respondents listed about one additional person per dwelling on these two versions (a mean of 2.90 for the standard questions administered on Version 1 versus means of 3.94 and 3.87 for Versions 2 and 3). An analysis of variance confirmed that these differences by version were statistically significant – $F(2, 37) = 15.46$, $p < .001$. Additional analyses indicate that the differences across versions in the total number of males ($F(2, 37) = 8.09$, $p < .001$), females ($F(2, 37) = 6.04$, $p < .01$), Black males ($F(2, 37) = 13.04$, $p < .001$), and Black females ($F(2, 37) = 6.84$, $p < .01$) were all statistically significant as well. Post hoc comparisons indicated that, in each case, Versions 2 and 3 differed significantly from Version 1, but not from each other.

The differences across versions in the total number of persons rostered are greatly reduced if persons added by the later probes are included in the figures for Version 1. The total number of persons per household rises to 3.67; the total number of males to 1.65 and the total number of females to 1.99 when these persons are included. Furthermore, the differences across versions in the total number of persons per household are no longer significant. This suggests that the order in which the special probes were administered (either before the standard roster questions, as in Versions 2 and 3, or after them, as in Version 1) had little effect on their ultimate yield.

Usual residents. It is not surprising that the probes increased the average number of persons listed on the rosters, since the expanded rosters included dinner guests, overnight visitors, and others with only tenuous connections to the household. The key issue is whether the number of persons identified as usual residents of the household differs by version. All three versions of the questionnaire contained an item to distinguish usual residents from others on the roster. In each version, respondents were asked ''Does NAME usually live here?'' for each person they had listed on the roster. Responses to this item were used to identify the usual residents. Again, with Version 1, we focus on persons identified as usual residents in response to the standard roster questions.[5]

Overall, there is no evidence of differences among the three versions in the number of

[5] For the Version 1 households, only five additional usual residents were identified in response to the later probes, yielding only .03 more usual residents per household. None of the conclusions are altered when these additional usual residents are included in the figures for Version 1.

usual residents. Version 1 yielded an average of 2.83 per household if just the standard items are considered, Version 2 an average of 3.13, and Version 3 an average of 2.99. An analysis of variance indicated that the overall differences by version were not significant – $F(2, 37) = 1.38$ ($p > .25$). However, when we focus on the Black males – the group known to have the highest rates of undercoverage – the differences are more striking. Version 2 produced the highest mean number of Black males listed as usual residents – 1.45 per household; the corresponding figures for Versions 1 and 3 are 1.09 and 1.18, respectively. Version 2, thus, produced an increase of 33% more Black males reported as usual residents than did the standard rostering questions in Version 1. The increase for Version 3 over Version 1 is only 8%. Across the three versions, the differences in the mean number of Black male usual residents reported was significant – $F(2, 37) = 5.01$ ($p < .01$). A post hoc comparison indicates that the results for Version 2 differed from those for Version 1.

These differences across versions in the number of usual residents reported did not extend to Black females. The means are 1.45, 1.60, and 1.60 for the three versions (see Table 1); an analysis of variance confirms that these differences were not statistically significant – $F(2, 37) < 1$.

Additional analyses. The analyses reported so far examine the average counts of different classes of persons by version. As is common with counts, the distribution of the data was highly skewed, and it is possible that the analyses reported earlier gave misleading results because of the nonnormal distribution of the observations. There are a number of methods available for dealing with the skewness of the data. With counts, transformations are sometimes applied. Winer (1971, p. 400), for example, recommends taking a log transformation; .5 or 1 is often added to the raw counts first to permit the transformation of cases with counts of zero. We reanalyzed the data, using the natural logarithm of the counts (plus .5) instead of the raw counts.

There were no differences between the results from the analyses of the raw and the transformed counts. For example, the analysis of the log number of Black males reported as usual residents showed a significant effect by version ($F(2, 37) = 4.32$, $p < .05$). The results of post hoc comparisons were also unchanged when the log transformed counts were analyzed in place of the raw counts.

The comparisons reported above examine total persons and usual residents. It is also possible to compare the three versions of the questionnaire under different rules for linking persons to dwellings; Section 3.4 reports some comparisons along these lines.

3.2.   *Characteristics of persons listed on the rosters*

It is possible that the different versions of the roster questions netted not only different numbers of persons but different types as well. Version 2, for instance, may capture a higher proportion of persons with loose or unconventional ties to the sample dwelling. We explored this possibility by comparing the characteristics of the usual residents listed under each version, according to responses to the follow-up items. These items asked how many nights each person stayed at the sample dwelling during the previous month, how many days he or she ate there during the past week, whether the respondent considered the person to be a member of the household, whether the person considered himself or

Table 2. Selected characteristics of usual residents by version

	Total	Version 1	Version 2	Version 3
Mean nights stayed at dwelling	29.5 (1,500)	29.3 (486)	29.5 (542)	29.5 (472)
Mean days ate at dwelling	6.78 (1,455)	6.60 (471)	6.79 (540)	6.79 (453)
Proportion contributing money, other help	59.0% (1,498)	61.7% (483)	56.4% (546)	59.3% (469)
Proportion staying other places	3.5% (1,492)	2.7% (482)	3.1% (550)	4.8% (460)
Proportion there yesterday	98.0% (1,517)	97.6% (489)	98.6% (553)	97.7% (475)
Proportion considering self a household member	99.7% (1,519)	99.2% (488)	100.0% (553)	100.0% (488)
Proportion considered a household member	99.5% (1,506)	98.8% (488)	99.8% (543)	100.0% (475)

Note: Sample sizes given in parentheses; *n*'s fluctuate from item to item due to missing and out-of-range values. Figures for Version 1 reflect only persons listed using the standard rostering questions.

herself a household member, whether the person had other places where he or she stayed frequently, whether he or she had been at the residence at all during the last day, and whether he or she contributed money or other help to the household. Table 2 displays the results of these comparisons.

The usual residents listed on the three versions do not appear to differ much. On all three versions, the usual residents were persons who slept at the sample dwelling almost every night, who ate there almost every day, and who were there some time during the last day. Nearly all considered themselves household members and were in turn considered household members by the respondent. The majority of those identified as usual residents (59.0%) also contributed money or some other form of help to the household. The usual residents listed on the three versions of the questionnaire thus seem to have similar relationships to the sample dwellings. (Additional analyses showed that this same pattern held when the Black males listed as usual residents were examined separately.) The same sorts of persons were reported as usual residents on all three versions of the questionnaires; the only difference was that on Version 2 more of them were reported.

3.3. Distinguishing usual residents from others listed

Altogether, the household rosters listed 1,949 persons, of whom 1,528 were identified by the household informant as usual residents of the dwelling. (For ten persons, the usual residence variable was missing; these ten are dropped from Table 3 below.) What characteristics are associated with respondents' decisions to classify persons listed on the roster as usual residents? To address this question, we examined responses to the item ''Does NAME usually live here'' as a function of responses to the follow-up items. We fit logistic regression models to determine which of the follow-up items were significantly related to responses to the usual residence item. Because these analyses are somewhat more exploratory in nature, we have not tried to take into account the clustering of these person-level data by household and by interviewer or segment.

We tested an additive model that included six of the follow-up items as predictors.[6]

---

[6] We dropped the follow-up item that asked whether the person considered himself or herself a household member, because that item agreed more than 98% of the time with the item that asked whether the respondent considered the person a household member.

Analyses of the data separately by version of the questionnaire yielded results similar to those given in the combined analysis; as a result, we present only the latter here. This analysis examined *all* persons rostered under Version 1, including those first reported in response to the special probes administered after the standard roster questions. The additive model gave a good fit to the data, with a likelihood ratio chi-square value of 1,580.4 (with 6 degrees of freedom).

Five of the six variables emerged from this analysis as significant predictors of whether the household respondent classified a person as a usual resident – the number of nights the person had stayed in the last month (the logistic regression coefficient for this variable was 0.192, with a standard error of 0.025); whether the person contributed money or other help to the household (logistic regression coefficient of 1.72, with a standard error of 0.501); whether the person had other places where he or she stayed frequently (logistic regression coefficient of -2.20, with a standard error of 0.448); whether the respondent considered the person a member of the household (logistic regression coefficient of 3.67 with a standard error of 0.661); and whether the person had been at the dwelling in the last day (logistic regression coefficient of 1.19, with a standard error of .529). Only the number of days the person had eaten at the sample dwelling in the past week failed to make a

Table 3.   Persons listed as usual residents by selected characteristics

Variable	% Identified as usual residents	*n*
Total	78.8	1,939
Nights at dwelling in last month		
Fewer than 7	6.8	368
7–29	78.2	119
30–31	99.5	1,398
Days ate at dwelling in last week		
Fewer than 7	17.2	429
7	97.2	1,430
Contributes money or other help?		
Yes	98.0	905
No	61.5	1,008
Considered a household member?		
Yes	94.4	1,594
No	2.7	329
Considers self a household member?		
Yes	94.7	1,520
No	2.7	331
There during last day?		
Yes	91.4	1,634
No	11.0	301
Stays frequently at other places?		
Yes	13.3	437
No	97.8	1,475

Note: Because of missing and out-of-range values, row entries sum to different totals. Responses to two of the items (the number of nights the person stayed at the residence in the last month and the number of days the person ate at the residence in the last week) have been grouped into categories.

significant contribution to the classification of persons as usual residents, despite a strong bivariate relation. The results were very similar when we restricted this analysis to Black males; the same five variables again emerged as significant predictors of whether the Black male was classified by the respondent as a usual resident of the dwelling. We stress that these results are merely descriptive and do not necessarily represent the key variables in the underlying cognitive process of the respondents.

Across all three versions of the questionnaire, people who spend the night at the dwelling more often, who had no other place where they frequently stayed, who contributed money or other support to the household, who considered themselves household members, and who had been at the dwelling within the last day were likely to be labelled usual residents. Their counterparts who had slept there fewer nights, frequently stayed elsewhere, did not contribute money, did not consider themselves household members, or did not visit the dwelling in the last day were much less likely to be identified as usual residents. Table 3 shows the percentage of persons identified as usual residents conditional on values of the seven follow-up variables.[7] All seven show a strong relationship to whether the respondent classified the person in question as a usual resident.

We did two further analyses concerning the classification of persons as usual residents. The first analysis determined the number of persons who met a series of common sense tests of residence. We determined the number of persons who spent at least 21 nights out of the last 30 at the residence, ate at the residence every day in the last week, contributed money or other help to the household, had been at the residence some time during the last day, considered themselves household members, and had no other place where they stayed frequently. (We dropped the item asking whether the respondent considered the person a household member since it was largely redundant with the one asking whether the person in question considered himself or herself a member.) We counted how many of these six tests each person met. Nearly 97% of those listed as usual residents – 1,476 out of the 1,528 usual residents – met at least four of these six criteria. In fact, 91% met at least five of these criteria. By contrast, the vast majority of those who were not classified as usual residents – 383 out of 411, or 93.2% – met two or fewer of these criteria. Thus, there seemed to be little room for confusion about how to classify almost everyone who was listed on the rosters.

Our final analysis attempted to provide a more direct estimate of the number of persons in the sample whose living arrangements did not readily map on to the concept of a usual residence. We classified persons on the rosters by the number of other places they stayed frequently and by the number of nights they had spent at the sample dwelling during the prior month. Table 4 shows the results. The top panel displays persons classified by the household informant as usual residents of the sample dwelling. The bottom panel shows the persons listed on the roster but not classified as usual residents; persons in this bottom

---

[7] By contrast, the figures in Table 2 are conditional on a person's being classified as a usual resident. For example, fewer than 60 of the 1,528 persons listed as usual residents of sample dwellings were reported to stay elsewhere frequently. These persons represent only 3.5% of the usual residents (Table 2). These same persons classified as usual residents make up 13.3% of the 437 persons listed on a roster who were reported to stay somewhere else frequently. The majority of these 437 persons were overnight visitors, dinner guests, or others with a limited connection to the sample dwelling who were added to the rosters in response to one of the special probes.

Table 4.   Usual residents and other persons by other places and nights at sample dwelling

	Nights at sample dwelling in last month					Total
	7 or fewer	8–15	16–23	24–31	missing	
Usual residents						
No other places	21	8	17	1,370	17	1,433
One or more other places	17	6	9	26	1	59
Number of other places missing	4	4	1	26	1	36
Not usual residents						
No other places	25	1	–	3	4	33
One other place	234	12	2	3	13	264
Two or more other places	12	1	–	–	–	13
Number of other places missing	76	3	3	1	18	101

group are at risk of undercoverage under the standard procedures for linking persons to dwellings. Among the persons not counted as usual residents, at least three distinct groups – encompassing a total of 63 persons – seem to us potentially at risk of exclusion: (1) the 33 persons who were reported not to stay at any place frequently; (2) the 17 persons who stayed at the sample dwelling eight or more nights in the prior month and who had one other place they were said to stay frequently; and (3) the 13 persons said to stay at two or more other places frequently. Members of these three groups either have no clear link to any dwelling or links to multiple dwellings. Still, most of those not counted as usual residents do not appear very problematic. For example, there are 234 persons who had stayed at the sample dwelling for a week or less and who had just one other place where they stayed frequently; it seems reasonable to assume that this other place is their home. Among those grouped in the top panel of the table, 59 were said to stay frequently at at least one other dwelling. It is possible that such persons also have unconventional living arrangements; however, because these persons were counted as usual residents of the sample dwelling they are at risk not of being excluded but of being linked to more than one dwelling.

3.4.   Other rules for linking persons to dwellings

If there are substantial numbers of persons who split their time among several dwellings, the ''usual resident'' question may not be the best way to determine which people should be linked to which dwelling. It is possible that the plurality rule (linking persons to the dwelling where they stay most often) is still generally workable, but that it could be operationalized more accurately with other questions than with the usual resident question. For example, more specific questions (such as the probes we used) might bring in persons who meet the plurality rule but who are not considered usual residents by the household informant. More specific questions might also improve coverage when informants are reluctant to acknowledge someone as a usual resident but would be willing to divulge enough information to allow the persons to be linked to the dwelling. It is also possible that the plurality rule simply does not cover a range of living situations and that alternative rules need to be developed for linking persons to dwellings.

We examined two new methods for operationalizing the plurality rule, as well as two alternatives to that rule. In an earlier version of this article (Kearney, Tourangeau, Shapiro, and Ernst 1993), we also examined a more complex procedure for implementing the plurality rule. That analysis was hampered because the questionnaire did not include all the items needed to classify each sample person under that procedure. In this article, we focus on relatively simple methods for classifying persons that can be carried out using the items at hand. Aside from the ''usual resident'' question, we examined two other plausible methods for operationalizing the plurality rule. One method counted as a resident anyone who had stayed at the sample dwelling at least 16 nights during the past month; persons meeting this test had spent more nights at the sample dwelling than any place else during the month. The other method liberalized the first test somewhat, counting both those who stayed at the sample dwelling for 16 or more nights and those who had no other place they stayed frequently. In addition, we examined two rules conceived as alternatives to the plurality rule. The first counted all persons who stayed at the dwelling the previous night, a rule sometimes referred to as the *de facto* rule. (The items needed to assess this rule were included only in Versions 2 and 3 of the questionnaire.) The other alternative rule linked to the dwelling all persons who had eaten there at least four days out of the last seven. Each of these procedures has potential problems. For example, although the *de facto* rule is quite simple, it will exclude persons who happened not to have spent the night anywhere (such as the homeless or persons in transit), and it is poorly suited to many analytical purposes, because it links people to dwellings to which they have only a temporary connection. Our analysis examines only the number of persons captured by each method and the extent that the various alternative methods give results that agree with the classifications under standard method based on the ''usual resident'' item.

Table 5 shows the mean number of persons linked to each dwelling under the different

Table 5.   Mean number of persons per dwelling by rule and version

	Version 1 (*n* = 173)	Version 2 (*n* = 177)	Version 3 (*n* = 159)	Total (*n* = 509)
All persons				
Plurality rule				
Usually lives here	2.83	3.13	2.99	2.98
16 or more nights	2.83	2.99	2.93	2.95
No other place or 16+ nights	3.01	3.34	3.30	3.22
Stayed last night	–	3.08	3.05	3.07
				(*n* = 336)
Ate 4 or more days in last week	2.86	3.16	2.98	3.01
Males only				
Plurality rule				
Usually lives here	1.28	1.47	1.25	1.34
16 or more nights	1.23	1.43	1.20	1.29
No other place or 16+ nights	1.32	1.55	1.39	1.42
Stayed last night	–	1.47	1.25	1.37
				(*n* = 336)
Ate 4 days in last week	1.26	1.50	1.26	1.35

methods. The top panel displays the results for all persons and the bottom panel displays the results for males only (who are at greater risk of undercoverage under the standard procedure). Only the relatively liberal method which assigned to the sample dwelling anyone who stayed there at least 16 nights during the past month or who had no place else where they stayed frequently consistently captures more people than the standard classification; overall, the liberal method includes about 8% more people than the standard method (an average of 3.22 per household versus 2.98). The *de facto* rule is the second most inclusive, linking an average of 3.05 persons to each sample dwelling. In general, though, the results do not differ dramatically by method and, regardless of the linking procedure used, Version 2 consistently netted more persons than the other two versions. In addition, the alternative procedures tend to classify individuals in the same way as the standard method. The rate of agreement with the classification under the standard method ranged from a low of 93.9% for the rule based on the number of days the person ate at the sample dwelling to a high 96.4% for the most inclusive procedure. The high level of agreement among the classification procedures is consistent with our earlier finding that the vast majority of persons counted by the informants as usual residents met a variety of common sense tests of residency and that most persons not counted as usual residents fail most of these same tests.

3.5. *Obtaining the full names of persons listed on Version 2*

Version 2 may yield more complete reporting of Black males than the other two versions but it does so at a price. In Version 2, residents were not necessarily identified by their full names on the roster. At the end of the interview, however, respondents were asked to provide full names for everyone listed on the roster in case it was necessary to recontact them. Respondents provided full names for 521 of the 697 persons listed on the Version 2 rosters (74.7%) and for 448 of the 554 usual residents (80.9%).

Most respondents to the Version 2 questionnaire reported names for everyone on the roster. Full names were provided for everyone on the roster in 72.2% of the questionnaires. Full names were given for at least one of the persons listed in an additional 13.1% of the cases. The corresponding figures for usual residents are somewhat higher; respondents provided a full name for every usual resident listed on 79.0% of questionnaires and for at least some of the usual residents on another 6.2%. However, 14.8% of the Version 2 respondents provided full names for none of the persons listed. In total, 114 persons (85 of them usual residents) were listed on rosters on which no full names were provided. The omission of all names within a residence would make it difficult to track members of the sample over time.

It may be possible to obtain full names for a higher proportion of the sample while still achieving improved coverage. The Version 2 wording was: ''To help us keep track of which persons we're talking about, can you identify each one with a first name, a nickname, a set of initials, or by some other means? It is not necessary to give us full names.'' Other wording could result in more full names: ''To help us keep track of which persons we're talking about, can you give us the full name of each person? If you would rather not give us full names, you can identify each one with a first name, a nickname, a set of initials, or by some other means.'' This type of wording, of course, might not achieve the same

coverage gains. Another way of obtaining full names for a higher proportion of the sample would be to offer the option of less than full name in only those geographic areas or types of households where coverage is believed to be a particular problem.

4.   Conclusions

The anonymous rostering procedure certainly showed promise. Version 2, which did not require full names, yielded 33% more Black males listed as usual residents than the standard questions in Version 1 did. The magnitude of this difference seems reasonable given published estimates of the level of undercoverage in surveys and the decennial census. For example, Hogan (1993) estimates undercoverage of approximately 5% among Blacks of both sexes in the 1990 census; Hainer and his colleagues (Hainer et al., 1988) estimate that an additional 17% of Black males are omitted from the Current Population Survey. The areas used in our study were specifically chosen because they were likely to exhibit high rates of undercoverage.

   The apparent improvement in the coverage of Black males does not seem to result from the extra probes for additional household members that were included in both Versions 2 and 3. These probes do indeed add persons to the rosters – on average about 1 person per dwelling (see Table 1). But few of the persons added by the new probes are classified by the household respondent as usual residents of the dwelling. What does make a difference in the reported number of usual residents is an anonymous questionnaire, which was a feature of Version 2 only. However, since Version 2 included the extra probes as well, we cannot be sure that anonymity by itself would achieve significant coverage gains. The results involving the number of usual residents do not seem to be statistical artifacts due to the nonnormal distribution of the data or the clustering of the observations by segment and interviewer. It is, of course, possible that the results would not generalize beyond this purposively selected sample; but, within a sample of areas chosen to be representative of areas where coverage problems are most serious, rosters compiled without requiring full names appear to improve coverage substantially.

   At the outset, we distinguished two broad hypotheses about the sources of coverage error within households. Researchers have argued that respondents may be unclear about who should be included on the household roster and that they may deliberately conceal household members to avoid legal or financial repercussions. The confusion about whether a given person should be included is thought to reflect living arrangements – particularly those of minority families in inner city areas – in which persons spend time at several dwellings. Our results support an account based on concealment rather than confusion. Work by Singer, Mathiowetz, and Couper (1993) suggests that the confidentiality concerns of potential survey respondents are distinct from their privacy concerns; not requiring the use of full names would appear to address both sets of concerns (see also Kulka, Holt, Carter, and Dowd 1991).

   The results also suggest that decisions about who to count as a usual resident may be based on relatively simple criteria. The respondents' classification of the persons on the roster as usual residents could be predicted from five variables – how often the person had spent the night during the past month, whether he or she had been at the dwelling in the last day, whether there were other places the person stayed frequently,

whether the person contributed money or other support to the household, and whether the respondent thought of the person as a member of the household. These variables correlated highly with each other and with the classification of a person as a usual resident.

More generally, we can identify only a few persons with unconventional living arrangements that might engender omissions from the roster. Of the more than 1,500 persons listed as usual residents, only 59 – less than 4% of the total – were reported to stay frequently at some dwelling other than the sample dwelling; of these 59 persons, 49 were said to stay often at just one other residence. This same pattern is apparent for the persons listed on the rosters but not classified as usual residents of the sample dwelling; the vast majority of these persons were said to stay in only one other residence (see Table 4). Adding in other potentially problematic cases brings the estimated total at risk for undercoverage to 63. It is not clear that any of these relatively ambiguous cases were misclassified by the respondents with respect to their usual residences. Even if all 63 of these persons should have been counted as usual residents of the sample dwelling (according to the plurality rule), it would have increased the mean number of residents per dwelling only by 5%. The results are similar if we restrict the analysis to Black males – counting all the problematic cases would increase the total number of Black male residents per dwelling by slightly more than 5%. By contrast, anonymous rostering added about 30% more Black male residents per dwelling (see Table 1). These results lend further support to the hypothesis that concealment is a major cause of within-household undercoverage. The results in Table 5 nonetheless support the notion that new rules for linking persons to dwellings based on more specific questions might also improve coverage.

As a practical matter, it may seem silly to contemplate using anonymous rostering in many survey applications, especially ones involving longitudinal data collection. For several reasons, we believe that our results still have important practical implications. First, they demonstrate that roster questions – which have been thought to be relatively innocuous – can in fact be taken as threatening by some respondents. Thus, procedures used to reduce threat with other sensitive questions may be useful with roster questions as well. In addition, efforts to improve coverage that focus solely on the clarity of the standard of residence may fail, because they ignore a major source of within-household undercoverage. Second, many surveys do not in fact require the full names of the respondents; for example, in telephone surveys, individual respondents are often selected through methods that do not require listing every household member by name. Clearly, the anonymous rostering procedure described here may be quite applicable in cross-sectional face to face surveys. Third, even in the context of a longitudinal design, there may be advantages to obtaining full names at the end of the initial interview rather than at the beginning. It is customary to delay asking sensitive questions until the end of the questionnaire – after the interviewer has had a chance to develop rapport with the respondent and to demonstrate the legitimacy of the survey. We know of no surveys that begin the interview by asking about household income or sexual practices. For some households, the full names of the household members may be an equally sensitive subject.

5.　References

Brownrigg, L. and Martin, E. (1993). Proposed Study Plan for Ethnographic Evaluation of the Behavioral Causes of Undercount. Paper prepared for the Census Advisory Committee on Population Statistics at the Joint Advisory Committee Meeting, April 13–14, Alexandria, VA.

Cantor, D. and Edwards, C. (1992). Testing Alternative Household Roster Questions for the Survey of Income and Program Participation. Unpublished report submitted to the U.S. Bureau of the Census.

Fay, R.E. (1989). An Analysis of Within-household Undercoverage in the Current Population Survey. Proceedings of the U.S. Bureau of the Census Annual Research Conference, Washington, DC: U.S. Bureau of the Census, 156–175.

Fein, D.J. (1990). Racial and Ethnic Differences in U.S. Census Omission Rates. Demography, 27, 285–302.

Fein, D.J. and West, K.K. (1988). Towards a Theory of Coverage Error: An Exploratory Assessment of Data from the 1986 Los Angeles Test Census. Proceedings of the U.S. Bureau of the Census Annual Research Conference, Washington, DC: U.S. Bureau of the Census, 540–562.

Gerber, E.R. (1990). Calculating Residence: A Cognitive Approach to Household Membership Judgments Among Low Income Blacks. Unpublished report submitted to the U.S. Bureau of the Census.

Hainer, P., Hines, C., Martin, E., and Shapiro, G. (1988). Research on Improving Coverage in Household Surveys. Proceedings of the U.S. Bureau of the Census Annual Research Conference, Washington, DC: U.S. Bureau of the Census, 513–539.

Hogan, H. (1993). The 1990 Post-Enumeration Survey: Operations and Results. Journal of the American Statistical Association, 88, 1047–1060.

Kearney, A., Tourangeau, R., Shapiro, G., and Ernst, L. (1993). Coverage Improvement from Experimental Roster Questions. Proceedings of the Section on Survey Research Methods, American Statistical Association, 162–167.

Kulka, R., Holt, N., Carter, W., and Dowd, K. (1991). Self Reports of Time Pressures, Concerns for Privacy and Participation in the 1990 Mail Census. Proceedings of the U.S. Bureau of the Census Annual Research Conference, Washington, DC: U.S. Bureau of the Census.

Martin, E. and Polivka, A.E. (1992). The Effect of Questionnaire Redesign on Conceptual Problems in the Current Population Survey. Proceedings of the Section on Survey Research Methods, American Statistical Association: 655–660.

McKay, R.B. (1992). Cultural Factors Affecting Within Household Coverage and Proxy Reporting in Hispanic (Salvadoran) Households. A Pilot Study. Proceedings of the Section of Survey Research Methods, American Statistical Association, 614–618.

Robinson, J.G., Ahmed, B., Gupta, P.D., and Woodrow, K. (1993). Estimation of Population Coverage in the 1990 United States Census Based on Demographic Analysis. Journal of the American Statistical Association, 88, 1061–1071.

Shapiro, G., Diffendal, G., and Cantor, D. (1993). Survey Undercoverage: Major Causes and New Estimates of Magnitude. Proceedings of the U.S. Bureau of the Census Annual Research Conference, Washington, DC: U.S. Bureau of the Census.

Singer, E., Mathiowetz, N., and Couper, M. (1993). Privacy, Confidentiality, and the 1990 U.S. Census. Public Opinion Quarterly, 57, 465–483.

Valentine, C.A. and Valentine, B.L. (1971). Missing Men: A Comparative Methodological Study of Underenumeration and Related Problems. Contract No. 1510. Washington, DC: U.S. Bureau of the Census.

Winer, B. (1971). Statistical Principles in Experimental Design. 2nd Edition. New York: McGraw Hill.