A New Approach to Measuring Residence Status

Jennifer Childs, Elizabeth Nichols, Aref Dajani, Jennifer Rothgeb
Statistical Research Division, US Census Bureau, 4600 Silver Hill Road, Stop 9100, Washington, DC, 20233

Abstract

To determine where to count people who have multiple places to stay (e.g., second home, relatives' homes) on Census Day, Census coverage operations use a set of "cycle" questions that ask how often a person goes back and forth between places. These questions offer predefined patterns (e.g., going between places every week) and ask where the person spent most of the time during a specified time period (e.g., March and April). Because the cycle questions presume set patterns of living situations that may not reflect reality, we investigate an alternative method of assigning residency. The new "dates" method involves collecting dates of stays for each address and calculates where a person should be counted on census day given the dates. This paper presents findings from an experiment that evaluated the effectiveness of this alternative method of assigning residency.

KEY WORDS: coverage, residency, split-panel field test

1. Introduction

Determining where to count a person on Census Day (i.e., Census Day residence) is typically a straightforward task, since most people have only one home, and they stay at that home most of the time. However, for people who stay at multiple places, whether they have an official second home or they frequently stay informally with friends or family, where to count them in the census becomes more complicated. For the most part, the U.S. Census Bureau employs a *de jure* rule to count people at their usual residence, that is, where they are usually living and sleeping, on a given Census Day[1]. Past research has shown that respondents' understanding of where each person *lives* often differs from the Census Bureau's rule of usual residence (Gerber, 1994). This paper addresses how to operationalize the Census Bureau's concept of usual residence for people with more than one place to live and sleep.

2. Background

For the 2010 Census, two programs look closely at how residence status is classified: the Coverage Followup (CFU) and the Census Coverage Measurement (CCM) programs. The CFU program attempts to determine who should and should not be counted in a given household. The CFU interview is initiated based on unclear responses to the census, and results from the CFU are used to improve the census itself. The CCM surveys a sample of housing units to determine where people should have been counted in the census. The CCM sample and interviewing are conducted independently of any census operation. The CCM program is used to evaluate the coverage of the census.

The CFU is conducted using a computer-assisted telephone interview (CATI). The CCM has two components to its person coverage measurement: the initial enumeration uses a computer-assisted personal interview (CAPI), while a follow-up interview uses a paper-administered personal interview (PAPI) form.

Because respondents' own determination of usual residence differs in key ways from the Census Bureau's definition (Gerber, 1994), the Census Bureau implements a series of questions in both the CFU and CCM to assign the correct residence status according to the official rule. To determine Census Day residence status for people with more than one address, both the CFU and the initial enumeration for the CCM start with a question asking where the person spent most of the time around Census Day. If the person has two addresses, both surveys also use a set of questions asking about how often the person

This report is released to inform interested parties of research and to encourage discussion. Any views expressed on methodological issues are those of the authors and not necessarily those of the U.S. Census Bureau.

[1] There are a few important exceptions to this rule. The Census Bureau enumerates people who stay in group quarters (i.e., places that house groups of people such as college dormitories, nursing homes and jails) in the place where they stayed on Census Day. This particular rule is *de facto* rather than *de jure*. See the report from the National Research Council (2006) on the complexities of applying this rule.

goes back-and-forth between the places. These questions offer predefined patterns, for example cycling between places every week or every month, and ask the respondent to determine where the person spent most of the time during a specified time period (e.g., March and April). We refer to these as "Cycle" questions. Cycle questions were also used to evaluate coverage in Census 2000.

In 2006, the CCM surveys were field tested. A new approach to determine residence status was proposed for the CCM follow-up interview in 2006. It involves collecting dates of stays for each address mentioned by respondents during the interview, instead of asking about patterns of going back-and-forth. We refer to this method as asking "Dates" questions. The motivation behind this new approach comes from the fact that the Cycle questions presume set patterns of living situations that may or may not reflect the realities of peoples' lives (Martin, 2004). The Dates approach does not make this assumption about regular patterns. Additionally, the Cycle questions were intended to assign the correct Census Day residence for a person who has exactly two addresses. If the person has three or more addresses, it does not make sense to ask a structured question about how often the person goes back and forth between those three places. It would be nearly impossible to construct response categories that could match all possible combinations of back–and-forth patterns. The Dates questions do not have this limitation and can record dates of stay for any number of addresses.

In this paper we investigate the effectiveness of the two approaches, Cycle and Dates, using a split-panel design in a CATI random-digit-dial (RDD) sample survey independent from the census production environment. The primary objective of this experiment is to determine which set of questions is easier for the respondent to answer while providing more accurate data to code residency using an automated system. This paper presents findings from this experiment and proposes recommendations for assigning residency to people with more than one place to live or stay.

3. Method

3.1 Field Test

We conducted a split-panel experiment to test two different strategies to determine where a person should be counted according to our census residence rule assuming a Census Day of April 1st, 2006. One panel contained the Cycle questions and the other panel Dates questions, hence referred to as the Cycle panel and the Dates panel.

This experiment was included as part of the Census Bureau's Questionnaire Design Experimental Research Survey (QDERS) 2006. QDERS is a special survey developed by Census Bureau staff for conducting methodological experiments offline from the agency's ongoing production surveys. QDERS 2006 was a split-panel controlled experiment conducted between November 3 and November 21, 2006. It was conducted using a RDD sample via CATI from one of the Census Bureau's centralized calling centers. The sample was a nationally representative sample (excluding Alaska and Hawaii) with independent samples for each of the two treatments.

Twenty interviewers were provided classroom training on only one of the two panels. We attempted to balance the two groups of ten interviewers in terms of interviewer characteristics such as tenure, previous experience with the CFU instrument, skill level and gender. All interviewers had previously been trained on an RDD survey. Therefore, the classroom training focused only on the specific content of the QDERS instrument.

The total sample size for each panel was 2996. Using the response rate calculation standards established by the American Association for Public Opinion Research (AAPOR), excluding cases of ineligibility and unknown eligibility, the response rate for the Dates panel was 60.77 percent compared to 55.92 percent for the Cycle panel.[2] These were significantly different from one another ($p<.01$). The refusal rate was 26.20 percent for the Dates panel compared to 28.40 percent for the Cycle panel.[3]

This resulted in a total of 1870 completed interviews[4]. There were 982 completed interviews in the Dates panel and 888 in the Cycle panel.

3.2 Questionnaire Development

The QDERS questionnaire was adapted from the 2006 CCM initial enumeration interview and the

[2] Rates reflect the AAPOR RR6 definition (AAPOR, 2006).
[3] Rates reflect the AAPOR REF3 definition (AAPOR, 2006).
[4] We limited the substantive analyses to people who had a "complete" flag set in the instrument, which means the interviewer got to the end of the instrument for at least one person.

followup survey. Generally, in both panels of QDERS the instrument collected a roster of current occupants of the housing unit, demographics and the address of the sample phone number[5]. In both panels, the instrument then collected other addresses where a person reported living or staying during 2006. In order to determine residency according to the current Census Day residency rules, two sections in each panel differed.

In the Cycle panel we collected information on where the person spent most of the time around Census Day. If another address was reported, we collected whether they reported moving or cycling between the sample address and that address during 2006 (from January 1st until interview day), and if they cycled, how often they went back and forth between places. If two or more addresses were collected in addition to the sample address, we collected interviewer-keyed respondent's comments on how long the person stayed at each of the addresses during 2006.

The Dates panel collected the dates a person reported staying at each address mentioned during the interview. If dates were not reported (either because there was more than one set of dates of stay or because the respondent could not remember them), the interviewer could use checkboxes that listed common scenarios that attempted to quantify where most of the time was spent. For each address the interviewer could also enter respondent comments on when he/she stayed at the address.

At the end of both panels, after the interviewer had ended the telephone interview, the instrument included interviewer debriefing questions, asking interviewers to summarize each person's living situation, based both on how the respondent had answered the questions and on anything else the respondent may have mentioned during the interview.

3.3 Automated Assignment of Residence Status

We created a computer program to use data collected in each panel to assign each person in each interviewed household a residence status code. In this paper, we concentrate our analysis on three types of residence codes for both panels: Resident, Nonresident or Unresolved.

[5] From this point forward, we use the term *sample address* to refer to the address corresponding to the phone number that was selected through the RDD method.

Assignment of automated residence status codes was rather complex. Because of the way the questions in the Dates panel were asked, the automated coding in that panel was more sophisticated than that of the Cycle panel. In the Cycle panel, there was essentially a single basic path through the residence status questions. In the Dates panel, the interviewer had options to enter exact dates, estimated dates, or to use checkboxes quantifying where most of the time was spent. All of these had to be taken into account when assigning residence codes. Typed notes could not be taken into account during the automated residence status assignment.

Below is a brief description of how assignments were made. Because of the richer set of data in the Dates panel, we could automate more complex rules in that panel. One asterisk (*) next to the description means that the rule was automated in the Dates panel, but not in the Cycle panel. Two asterisks (**) mean the rule was automated in the Cycle panel, but not the Dates panel. No asterisk means the rule was automated in both panels.

3.3.1 Rules for residence codes for Cycle and Dates panel

For this experiment, Census Day was defined as April 1, 2006. Residence status is reckoned to Census Day.

<u>Resident</u>:

- a person who reported living only at the sample address during 2006
- a person who reported a single other address, but said he/she moved from that address to the sample address on or before Census Day, April 1st
- a person who reported going back-and-forth between two addresses during 2006 (i.e., cycles between them), and said he/she spent most of the time at the sample address
- a person who reported going back-and-forth between places during 2006 and said he/she spent half of the time and spent Census Day, April 1st, at the sample address*
- a person who reported three or more addresses during 2006 and said he/she spent most of the time around Census Day at the sample address**
- a person who reported spending more than half of the time, including the time around Census Day, at the sample address*

- a person who reported spending the spring semester of 2006 (which includes April 1st) at the sample address*

Nonresident:
- a baby born after April 1st
- a person who was in a group quarters[6] on April 1st
- a person who reported being at the sample address only daytimes, but not spending the nights*
- a person who reported moving to the sample address from another housing unit (not a group quarters) where he/she lived on April 1st
- a person who reported moving back-and forth-between two addresses during 2006 (i.e., cycles between them), but said he/she spent most of the time at an address other than the sample address **
- a person who reported going back-and-forth between addresses during 2006 (i.e., cycles between them), but said he/she spent less than half the time, or half of the time, but was not there on April 1st*
- a person who reported three or more addresses during 2006 and said he/she spent most of the time around Census Day at an address other than the sample address**
- a person who reported spending the spring semester of 2006 (which includes April 1st) at a college address that was not a group quarters and not the sample address*

Unresolved:
- a person who answered "don't know" or "refused" to key questions used to define Census Day residency

3.4 Analyst Assignment of Residence Status

In both panels, an independent analyst[7] looked at the data collected for each person including any notes associated with the case, and coded each person as a resident, nonresident or unresolved. This is similar to our standard practice in production of training clerks to code residency. These clerks, like our analyst, are permitted to ask experts questions about specific residence situations. The analyst also assessed whether the data captured in the questionnaire seemed to match the notes. This allowed us to get a feel for how an analyst would code each case given all the data, if we had resources for each case to be reviewed by an analyst.

3.5 Analytic Questions

We compare these two panels on a number of measures including response burden, ability to automate coding, and data accuracy.

Two criteria are used to determine response burden. We examine how long it takes to administer each questionnaire, and we look at how many respondents break off during the interview. A longer interview and an increase in break-offs would suggest more respondent burden.

To measure the difference in our ability to automate residence coding given each set of data, we compare the percentage of people with an unresolved residence status after an automated residence coding. More people with unresolved residence status implies that greater clerical resources, hence cost, would need to be devoted to that panel to try to resolve the case by reading notes.

To measure the accuracy of the data, we examine two different criteria. First, we look at an independent assignment of residence status by an analyst review compared to the automated residence code. A higher mismatch between the two sets of codes for a specific panel would suggest that panel was less accurate in collecting the data needed to accurately assign a residence code using the scripted questions. This assumes the notes written in open text fields are a more accurate reflection of the true residence status than the data collected through scripted questions. Secondly, we look at the number of people with unresolved residence status after the analyst review in addition to the automated review. If one panel had a higher rate of unresolved residence status, that would imply that the data collected (both from the scripted questions and from the notes) was not sufficient, and therefore, not accurate enough for Census Day residence determination.

4. Limitations

Since each interviewer was assigned to only a single panel, we attempt to control for interviewer effects by including in our statistical models a fixed effect of panel and a random effect of interviewer on the intercept. The difference in response rates between

[6] The Census Bureau takes a special count of all people who live in a group quarters (e.g., college dorms, jails, nursing homes). For more information, see National Research Council (2006).

[7] Dawn Norris, an intern for the Statistical Research Division, was trained in the residence rules and served as our objective analyst.

panels (cited previously) as well as observations made during the training sessions suggest that the Dates panel might have had more skilled interviewers than those who worked on the Cycle panel. The inclusion of a random effect is aimed at controlling for this.

The QDERS interviews were conducted using a CATI instrument in an RDD sample universe. Households with no landline telephones and households in Alaska and Hawaii were not eligible for this study. Additionally, a telephone interview differs in some ways from a personal visit interview. However, with the lack of face-to-face props (e.g., calendar, flashcards), we argue that a telephone interview is a more stringent test of the new Dates approach.

The interview was limited to English speakers. We did not allow interviews to be conducted in other languages. Thus, the results are not necessarily generalizable to non-English speaking households.

The analyst who independently assigned residence status codes was a single person trained for this project in residence rules. She did not have the benefit of years of experience, as many of our production analysts do. She coded each case independently but asked questions about residence rules as needed. The limitation is that we do not know how experienced production analysts would have coded these cases. For this paper, we assume our analyst coded residence status in a similar manner to the production analysts.

Additionally, the statistical tests in this report were performed as if the data were collected in a simple random sample, with replacement. However, these data were not collected in this manner, but rather through an RDD sample without replacement. We make the assumption that results would be similar and present the results accordingly.

5. Results

5.1 Respondent burden

5.1.1 Timing

For this analysis, we measure respondent burden by the time it took to conduct the interview in the two panels. Instrument time is the time the interviewer was on the phone with a respondent, from introduction to closing, not including the interviewer debriefing at the end of each interview. In our set of 1866 complete (not including sufficient partial) cases, mean Instrument Time for the Dates panel was 9.65 minutes versus 9.33 minutes in the Cycle panel[8].

We examine whether this difference in time is significant, controlling for other covariates. We conducted a regression using number of addresses reported at a household level (No. of addresses), number of people in the household (Size of HH), and respondent race as covariates, as well as including the interviewer effects and sought to predict instrument time by panel[9]. Panel was not a significant predictor of instrument time above and beyond effects of the other variables (see Table 1).

Not surprisingly, the covariates were significant predictors of Instrument Time. Longer interviews resulted when more addresses were collected for the household and for households with more people. Interestingly, the race of the respondent was a significant predictor of interview length given the other variables in the model. Respondents who self-identified as nonwhite, multiple races, or did not provide a race took longer to complete the interview than respondents who were white.

Table 1. Linear regression model of instrument time (in seconds).

Predictor	Parameter Estimate	Standard Error
Intercept	117.72**	21.84
No. of addresses per HH	218.18 **	6.27
Size of HH	59.24 **	2.93
Race of the respondent		
Black	46.23 **	15.03
DK/Ref	67.24 *	26.14
Multiple races	62.78 *	27.43
Other race	49.20*	25.09
White (Control group)		
Panel		
Cycle	-29.38	26.96
Dates (Control group)		

N=1866
* $p \leq .05$
** $p < .01$

[8] These are pure means, not accounting for covariates.
[9] These covariates were included either because the two experimental panels differed in these characteristics or because the variable was expected to impact the amount of interview time.

We conclude that there was not a significant difference by panel in time to complete the interview and therefore, the Dates panel imposes no additional burden over the Cycle panel.

5.1.2 Break-offs

Another respondent burden indicator is the number of times a respondent starts an interview and then gives up or breaks off before its completion. In both panels, we recorded the number of break-offs and where in the interview the respondents ended the interview. A higher break-off rate for one panel over another would indicate increased sensitivity or difficulty of one panel.

Because our interest lies in the comparison between panels, we looked at break-offs starting where the panels differed. In both panels, there were identical introduction, roster, demographic, and sample address sections. Following that, the residency section differed between panels. We examined break-offs starting during the residency sections. This only yielded 13 total break-offs, nine in the Dates panel and four in the Cycle panel.[10] Because there were so few break-offs, we will not draw any conclusions about respondent burden from them.

5.2 Automation: Ability to resolve residence status

We examined the number of resolved and unresolved people for each panel based on automated coding, co-varying number of addresses, and race of respondent, as well as including a random effect for interviewer behavior. If one panel had more people where the residence status code was unresolved after automated coding, that panel would require more analyst review, and thus cost more.

There was not a significant effect of panel beyond what was controlled for by the other variables (See Table 2). Number of addresses for the person was the only significant predictor of whether or not that person would be resolved by automation alone. As number of addresses increases, there is a decrease in the odds of a person being resolved. This is expected because in most cases, if a person only has one address, it is the sample address, and that person is automatically resolved. Overall, only 3.65 percent of people were unresolved after automated coding.

Table 2. Logistic regression model of resolved status after automated coding (1=resolved; 0= unresolved).

Predictor	Parameter Estimate	Standard Error	Odds Ratio
Intercept	6.76**	0.31	
No. of addresses per person	-2.28 **	0.12	0.10
Race of the respondent			
Black	0.33	0.36	1.39
DK/Ref	0.25	0.60	1.28
Multiple races	0.01	0.61	1.01
Other race	-0.02	0.54	0.98
White (Control group)			
Panel			
Cycle	0.16	0.29	1.17
Dates (Control group)			

N=4898
** $p<.01$

Thus, there is no difference in ability to use an automated coding scheme between panels. We conclude the costs for manual analyst coding of the residence codes would not increase for the new Dates approach as compared to the traditional Cycle approach.

5.3 Data Accuracy

5.3.1 Quality of Automated Codes

We were interested in whether an analyst would code a resident or nonresident differently than the automated coding, given access to the notes available as well as to the keyed data (but not the automated residence status code itself). We were not interested in looking at how an analyst would recode unresolved cases resulting from automated coding, figuring that most of these cases would become resolved after an analyst review. Also, some of the people coded as nonresidents during the automated process were nonresidents because they were defined as out-of-scope by the instrument using very clearly defined criteria[11]. Because the very first assessment is whether the person is in-scope or out-of-scope, we let the automated program determine this and removed out-of-scope people from further analysis.

This left 2385 residents or nonresidents in the Dates panel and 2209 residents or nonresidents in the Cycle

[10] Of the 13 cases, four were complete enough to be used in the analysis presented later in the report.

[11] Out-of-scope people include babies born after April 1st, people in a group quarter on April 1st, and people who reported being at the sample address only daytimes, but not spending the night.

panel. In a vast majority of cases (99.6% of Dates cases and in 99.1% of Cycle cases), the analyst code agreed with the automated code. In fact, there were only 10 cases in the Dates panel and 21 in the Cycle panel where the analyst disagreed with the resident or nonresident automated codes. Although these numbers are very small, we conducted a logistic regression on whether or not the analyst agreed with a resident or nonresident code co-varying the number of addresses a person had as well as including the random interviewer effect.[12] We found the effect of panel was not significant (see Table 3). After considering the other variables, we found no difference between panels on the proportion of cases where the automated code of "resident" or "nonresident" differed from the analyst code. The number of cases used for this analysis is very small, however.

Table 3. Logistic regression model of agreement between the analyst and automated resident/ nonresident codes (1=codes agreed; 0=codes did not agree)

Predictor	Parameter Estimate	Standard Error	Odds Ratio
Intercept	8.44 **	0.62	
No. of addresses per person	-1.94 **	0.21	0.14
Panel			
Cycle	-0.63	0.55	0.53
Dates (Control group)			

N=4623
** $p<.01$

Overall, in over 99 percent of cases, the analyst would not code the situation differently than would the automated coding program. Thus, not only did our automated coding work well in both panels, the Dates panel would require no more additional analyst review of cases coded through automation than would the Cycle panel.

5.3.2 Completeness of Data Collected

We examined the number of persons whose residence status remained unresolved after the automated coding and the analyst review. In each panel there were roughly 90 people (less than 4% of each panel's people) who were unresolved after the automated coding process. After considering the analyst codes as well as the automated codes (making an assumption that an analyst would only review casts that were unresolved by the automated system), in the Dates panel, 29 people, or just over 1 percent of all in-scope people remained unresolved. In the Cycle panel, only 6 (about ¼ of a percent of all in-scope people) remained unresolved. Again, despite the small numbers here, we conducted a logistic regression on whether the person's residence status was resolved or remained unresolved, co-varying the number of addresses a person reported as well as including a random interviewer effect.

We found the effect of panel to be significant in determining whether or not the person's residence status was resolved (see Table 4). After considering both the automated and the analyst code, the Dates panel had significantly more unresolved cases than did the Cycle panel. However, the numbers we are comparing are very small. In both panels almost 99 percent of in-scope people were resolved.

Table 4. Logistic regression model of resolved residence codes after considering both the automated and analyst coding (1=resolved residence code; 0=unresolved)

Predictor	Parameter Estimate	Standard Error	Odds Ratio
Intercept	7.09**	0.46	
No. of addresses per person	-1.71**	0.20	0.18
Panel			
Cycle	1.50**	0.50	4.48
Dates (Control group)			

N=4898
** $p<.01$

6. Conclusions

From this study, it seems the alternative approach to measuring residence status (i.e., the Dates method) performed comparably to the traditional approach (i.e., the Cycle method). Concerns had been raised that there would be more of a need for analyst coding using the Dates method; this was not the case in our experiment. Additionally, there was concern that people would not be able to give "dates of stay" or that it would be too burdensome a task. As evidenced by the 96 percent resolution rate in both panels, we were able to code residence in an equal proportion of cases when using the Dates questions as when using

[12] Due to the small amount of variance in the dependent variable, a model including race of the respondent was not run.

the Cycle questions. Additionally, we did not find a difference in respondent burden between the panels.

From the authors' perspective, specifying, programming, and testing the Dates panel was easier than the Cycle panel. In the Dates panel, the same questions were asked for each alternate address given, making the specification writing and testing straightforward. In the Cycle panel, when alternate addresses were given, there was a lot of conditional branching, depending upon the number of alternate addresses given and at what question they were offered. For example, if a person lived at another place on April 1st and moved to the sample address in May, we considered that person to be an "inmover" and did not ask the cycle questions. But, if another person stayed often at another address, we did ask the cycle questions.

We consider this telephone-only operation a strong test of the Dates approach because it does not permit the interviewer to provide a calendar as a respondent aid. Additionally, this was our first time programming the Dates approach, and we learned a great deal about how we would design the screens differently next time to avoid confusion about which dates apply to which address. Though this was not a prominent problem in the current experiment, we did see evidence of this confusion when examining data from the few unresolved cases. We think that the resolution rate in the Dates approach would be even higher if the data collection screens were improved through usability testing.

We should note that automated coding resolution rate in this experiment (96%) seems very high compared to the rate expected in a Census production environment. Only 4 percent of these experimental cases would have necessitated clerical review, which would be considered a very low rate in a production setting. This could be due to the dramatic difference in response rates between the experiment (55-61%) and production (high 90%), and the types of people who are likely to respond or not respond to a survey. Thus our conclusion that the costs for clerical coding of the residence codes would not increase for the new Dates approach as compared to the traditional Cycle approach in a production setting assumes that QDERS nonrespondents did not differ in ways that would affect our ability to resolve the residence status across panels.

On a subjective level, the authors believe that the Dates approach provided a much richer dataset from which to learn about living situations and code residency. In the QDERS experiment, the Dates panel data provided as accurate a picture of the Census Day residence as did the traditional method, as measured by our analyst review. Anecdotal analysts' comments from the 2006 CCM Census Test, where the Cycle questions were used in the initial enumeration and the Dates questions were used in the followup, suggest the rich dataset resulting from the Dates approach was superior to the dataset resulting from the Cycle approach for defining residence status using analyst coding. Thus, we conclude that the new Dates approach provides data that are as good, if not better, than the data provided by the traditional Cycle approach for assigning residence status to people with more than one place to live or stay.

Acknowledgments

The authors thank Dawn Norris, Rolando Rodriguez, and Bob Fay for their assistance with the statistical and data analysis. We also thank our reviewers, Terry DeMaio, Betty Murphy, Jeff Moore, Eli Krejsa and Dave Sheppard.

References

American Association for Public Opinion Research (2006). Standard Definitions: Final Dispositions of Case Codes and Outcome Rates for Surveys. 4th edition. Lenexa, Kansas: AAPOR.

Gerber, E. (1994). "The Language of Residence: Respondent Understandings and Census Rules." Final Report of the Cognitive Study of Living Situations. U.S. Bureau of the Census, Center for Survey Methods Research.

Martin, E. (2004) "Some Evidence on Patterns of Reporting Multiple Residences and Cycles of Staying in Them in EFU," Internal Census Bureau Memorandum prepared June 1, 2004.

National Research Council (2006). *Once, Only Once, and in the Right Place: Residence Rules in the Decennial Census.* Panel on Residence Rules in the Decennial Census. Daniel L. Cork and Paul R. Voss (Eds.) Committee on National Statistics, Division of Behavioral and Social Sciences and Education. Washington, DC: The National Academies Press.