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**Strength of Attachment:  
Survey Coverage of People  
with Tenuous Ties to Residences**

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## **Strength of Attachment:**

### **Survey Coverage of People with Tenuous Ties to Residences**

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U. S. Census Bureau research on the attachment of individuals to households is driven by the requirement that people be enumerated at their “usual residence” in the decennial census so that the Constitutional reason for the census—apportionment of seats in the House of Representatives—is accomplished. However, the implications of this research extend far beyond arcane questions of census coverage to shed light on the nature, size, and dynamics of demographically and socially interesting population subgroups, such as fragile families, non-resident fathers, children in joint custody, those with special residences for work, and households of immigrants.

What unites these populations is that in each case their definition depends on the nature of the attachment of individuals to household groups. Similarly, the census notion of “usual residence” assumes that each person can be uniquely and unambiguously attached to a household where he or she “lives and sleeps most of the time.”<sup>1</sup> The validity of this assumption is seldom examined but may affect concepts of household and residence, their measurement and

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<sup>1</sup>The Census Act of March 1, 1790, authorized the first census of the United States and established three rules of residence: (1) inhabitants should be enumerated at their “usual place of abode”; (2) those without a “settled place of residence” should be enumerated “where he or she shall be” on Census Day; and (3) those who are “occasionally absent at the time of enumeration” should be enumerated “at the place in which he usually resides” (Clemence 1986). From time to time, additional rules are adopted to clarify the usual residence principle. In the 2000 census, 31 residence rules covered special circumstances: for example, people staying in institutional settings and most group quarters (e.g., dormitory, shelter, or nursing home) on April 1, 2000, were enumerated there even if they had a usual residence elsewhere. See National Research Council (2006) for history and critical review of census residence rules.

application in demographic research, and the coverage of the population in surveys and the census. This article draws on evidence from an exploratory survey of living situations to assess the validity of assumptions about residence and to consider the implications for survey coverage of under-represented groups, such as non-resident fathers. It suggests some methodological innovations that may achieve better coverage of people with tenuous attachment to households.

Residential ambiguity and measurement errors (with other factors not explored here, such as errors in address lists) contribute to differential census undercounts of males, young adults, babies, renters, blacks and Hispanics, poor people, and other segments of the population (Robinson et al. 1993, Robinson 2001). Some groups, such as college students and elderly women, are overcounted. As a consequence, census and survey estimates for important demographic subgroups may be distorted. For example, survey data appear to be suspect for nonresident fathers, in part because their lifestyles lead them to be missed in surveys. Garfinkel, McLanahan, and Hanson (1998) found that the National Survey of Families and Households missed, or failed to identify, over 40% of fathers (4 million) who had a child with a non-resident parent, noting that “household surveys typically undercount men who are only loosely attached to a particular household” (1998:41). Undercoverage was higher among low-income and black fathers as well as fathers who never married the child’s mother. Based on ethnographic research, Sullivan (1990:14-15) described how the attachment of poor urban males to the households in which they grew up weakens as they reach their late teens. For financial and emotional reasons, they remain partly attached to their households of origin but spend much of their time - days, weeks, or months - in the households of women whose children they have fathered. In these situations, their household of residence may be ambiguous or contested in their own and others’ views, leading to coverage errors.

Although ethnographic research illuminates the complex household and individual circumstances that lead to omissions, it does not point clearly to how survey methods might be improved to achieve more complete coverage of household members in censuses and sample surveys. This article analyzes the nature and correlates of residential attachment, and offers several methodological innovations to improve survey coverage of tenuously attached people who may be especially pertinent to surveys in which hard to enumerate groups are a focus.

### **SOURCES OF AMBIGUITY ABOUT RESIDENCE**

In this section I explore several sources of residential ambiguity: complex living situations, residential mobility, lack of knowledge by proxy respondents, inconsistent and counterintuitive rules, and errors in measurements.

#### **Complex Living Situations and Irregular Households**

Sweet and Alberti (1994) found that about 9% of persons in a 1993 survey had “complex living situations” that were vulnerable to misreporting by household respondents: that is, they had more than one residence, lived away at college, lived away from home to be closer to their jobs, or lived in an institution or were in the military. Coverage errors in the 1990 census were higher in households that contained unrelated or mobile individuals, people with ambiguous household membership, and two or more nuclear families, and in households that were formed for the sole purpose of sharing rent or living expenses (de la Puente 1993; see also Schwede, Blumberg, and Chan’s 2006 description of ethnic variations in complex households and their coverage implications).

In irregular circumstances, disagreements within a household about whether a person lives there (Gerber 1994; Hainer 1987) may result in inconsistent reporting. Schwede and Ellis

(1994) found mismatches in the subjective assessments of household membership by household respondents and other individuals, especially for young adults aged 18 to 29. Coley and Morris (2002) found (in a sample of low-income children from Boston, Chicago, and San Antonio) disagreements between matched mothers' and fathers' reports about the father's residence: only 43% of the mothers, compared with 54% of the fathers, reported that the father lived in the mother's household. Fay (1989) and Ellis (1994, 1995) found that non-relatives (e.g., boarders) were more likely than relatives of the respondent to be left off a census or survey roster. Unrelated individuals may be omitted because they are not regarded as "family" or part of the core household (Rodriguez and Hagan 1991).

### **Residential Mobility**

People who move from one residence to another around the time of the census are at risk of being included at both locations, or omitted from both, depending on the timing of the move and nonresponse follow-up attempts. About 2.1 million in-movers were enumerated in the 1990 census at the address to which they moved after April 1, accounting for 20% of estimated erroneous enumerations (Moriarity 1993).

Respondents interviewed months after April 1<sup>st</sup> may find it difficult to recall accurately when a move occurred. Respondents or interviewers sometimes ignore the April 1 census reference date, reconstructing a general time frame instead of recalling a specific day (Wellens and Gerber 1996). Determining where people should be enumerated may be difficult when they move among different places, any of which might be considered residences for the purpose of census enumeration. Bates and Gerber (1998) found that the *pattern* as well as *amount of time* spent away influenced household respondents' residence determinations. Repeated visits to the same place created ambiguity about where a person lived. People who are frequently absent may

be assumed falsely to have another residence (Gerber 1990) and erroneously excluded from rosters by household respondents (Martin 1999).

### **Proxy Reporting**

Census enumerators (and follow-up interviewers) often must rely on reports from landlords and other proxy respondents who may lack knowledge of a person's living arrangements. In follow-up interviews conducted as part of the Accuracy and Coverage Evaluation survey after the 2000 census, over 20% of proxy respondents could not answer a question about a person's other residences, compared with fewer than 1% of non-proxies. Non-household proxies produce higher coverage error rates than household respondents (Griffin and Moriarity 1992).

### **Inconsistent and Counterintuitive Rules**

The determination of census residence is made more difficult by rules that do not conform to people's own understandings of where they live. Respondents often disregard counterintuitive instructions to count college students at their dorms, for example (Gerber, Wellens, and Keeley 1996). Gerber (1990) found that respondents rely on various criteria to determine residency, including a person's intentions and agreements, where a person keeps belongings or receives mail, financial contributions, permanence of attachment, and other criteria that may conflict with official rules. Hainer (1987) argued that household membership is performance-based, and adults who help with chores and make house rules are more likely to be reported as usual residents (Schwede and Ellis 1994).

### **Errors in Measurement**

The Accuracy and Coverage Evaluation (A.C.E.) survey was conducted after the 2000 census to estimate net errors in census counts. Subsequently, a reinterview study was conducted to identify A.C.E. measurement errors, and a matching study using name and date of birth

identified duplicate enumerations in the census. These two studies found significant errors in the A.C.E. measurements of erroneous enumerations, leading the bureau to recommend against using A.C.E. estimates to adjust census counts (U.S. Census Bureau 2001). Subsequent comparisons of A.C.E. and reinterview questionnaire responses demonstrated high levels of unreliability in reports about moves in and out of households, second residences, and stays in group quarters (Martin, Fay, and Krejsa 2002). Many census duplications were not identified by survey questions designed to identify multiple residences (Feldpausch 2001). Measurement errors were pronounced for people in unconventional living situations, such as group quarters (especially college dormitories); other residences for work, school, or vacation; children in joint custody; and movers (Adams and Krejsa 2002).

Errors in measurement may arise from many sources, including deliberate concealment (Tourangeau et al. 1997; Valentine and Valentine 1971), lack of knowledge or failure to recall relevant facts, and confusing terminology. For example, a question about “another residence” sometimes elicited “official” or permanent residences where a person spent almost no time, or failed to elicit legitimate second residences if respondents interpreted “residence” too strictly (Wellens and Gerber 1996).

### **THE LIVING SITUATION SURVEY**

The Living Situation Survey (LSS) was a special pilot survey developed by U. S. Census Bureau researchers and conducted by Research Triangle Institute in May through October of 1993 to test new methods for improving coverage of people tenuously attached to households. It collected detailed data about objective and subjective correlates of residence and is well suited to explore individuals’ attachments to the households in which they were encountered at the time of the survey.

The LSS was designed to cast a broad net, using extensive roster questions and probes to capture people with any presence in or attachment to sample households, no matter how slight. After they were identified, screening questions were asked to identify nonresidents. The roster began with a probe for the names of people who stayed there the previous night and continued with 25 additional probes to identify all the people who had lived or stayed in the households at any time during the previous two full calendar months, up to and including the day of the interview (the reference period). Cues were targeted to undercounted groups, such as live-in employees, boarders, and foster children. Probes were developed based on evidence about undercounts and cognitive and ethnographic research on how people think about residency issues (Gerber 1990, 1994). The probes were deliberately more inclusive than those used in the decennial census.

The sample was designed to represent the U. S. population in housing units. It oversampled census tracts with high concentrations of minorities and renters, which are groups that historically have higher rates of undercoverage in the census. Initial interviews were conducted with household respondents in 999 households (representing a 79.5% response rate). A total of 3,537 people were listed on household rosters. Individual follow up interviews were conducted with a subsample of 1,451 of the rostered people (response rate of 95%). Follow-up interviews collected additional detailed information, including all the places where the sample person had stayed, and the duration of the stays, during the reference period.

The experimental roster probes produced an average of one extra person per household, representing a 38% increase in average household size, 3.62 people per household in the LSS. Restricting the comparison to usual residents still produced significantly more residents per household in the LSS (2.76 compared with 2.63 in the 1990 census) and more Hispanics (4.12



residents per household versus 3.28 in the 1990 census) (Sweet 1994). The probes were especially effective at finding more young minority males—the group with the highest rate of census undercoverage—who were less likely to be mentioned in response to standard probes (Sweet 1994; Sweet and Alberti 1994). These results have led to the adoption of some of these probes in the coverage measurement interview and in the decennial census itself.

### **ASSESSING THE “USUAL RESIDENCE” CONSTRUCT**

The LSS provides a rich set of data with which to examine questions about residence and individuals' attachments to households: (1) its roster procedures were inclusive, and captured many people only tenuously attached to sample households; (2) it collected a great deal of auxiliary information about movements into and out of sample households; (3) it asked about attachments to other places; and (4) it gathered subjective measures of residence and household membership.

Despite population changes since 1993, the LSS data are still relevant to understanding how people apply Census Bureau concepts to report their residence as well as to characterizing living situations that are not adequately covered by standard survey methods for identifying household residents. The relatively small sample limits the precision of estimates available from these data.

The initial LSS interview with household respondents asked the following two questions about each person listed on the household roster: (1) "Do you consider this address to be (your/NAME's) usual residence, that is the place where (you/NAME) live(s) and sleep(s) most of the time?" and (2) "Do you/Does NAME have a usual residence somewhere else?" A definition of usual residence was provided to respondents as part of the first question, but the specific residence rules were not. The second question was asked regardless of the answer given to the

first. Note that this permits a person to be reported as having one or two usual residences, or none. Of the 3,537 people listed on household rosters, 76.2% were reported as usual residents “here”, in response to the first question; and 24.3% were reported having a usual residence elsewhere, in response to the second question. But these percentages are the marginals of the cross tabulation of the two “usual residence” questions. The internal cells form the basis of an examination of the uniqueness and ubiquity of “usual residence.”

### **Uniqueness and Ubiquity of Usual Residence**

The usual residence principle is intended to link each individual to a housing unit where he or she can be enumerated in the census, on the assumption that everyone has one and only one place where he or she “lives and stays most of the time.” (Special rules apply to people found in group quarters and institutional settings.) If the assumption is correct, the answer should be “yes” to either item 1 or item 2, but not to both. A usual residence typology created from the cross classification of these two items is presented in Table 1. Standard errors are shown in parentheses. Significance tests and standard errors are calculated with VPLX (Fay 1998), using jackknifed replication methods that take into account the complex sample design and are appropriately applied to weighted data to estimate variances and calculate test statistics (see Fay, 1985; Skinner, Holt, and Smith 1989).

[Table 1 here]

As shown in Panel 1 of Table 1, the assumption that each person had one and only one usual residence was met for 98% (rows 1 plus 2) of the people listed on LSS rosters: 75% reported the sample household as their usual residence, and 23% reported a usual residence somewhere else.

The assumption fails for an estimated 3.8 million people with two usual residences (row

3). The 90% confidence interval suggests that between 1.3 and 6.4 million people nationally were at risk of double counting in the census due to multiple usual residences.

The assumption also fails for almost 2.1 million people reported to have no usual residence (row 4). They might literally be homeless, or they might have multiple places where they stay—none of them “most of the time.” Together, rows 3 and 4 account for almost 2% of rostered people nationally ( $SE = 0.56$ ).

To assess the impact of the failure of the assumption of a unique usual residence on census population estimates, each row’s total is adjusted to reflect eligibility for census enumeration in the sample household. Row 1 should be weighted 1.0 because it represents usual residents exclusively of sample households. Row 2 should be weighted 0.0 because these people should be enumerated at their residences elsewhere. People represented in Row 3 were at risk of duplication and should be weighted 0.5 to reflect their potential inclusion at two places. Finally, row 4 should be weighted 1.0, because census rules specify that people without a usual residence should be enumerated in the household where they are found. These weights yield an estimated U. S. household population of 253,339,083 in 1993. Although this estimate has a very large standard error (about 20%), it is (perhaps surprisingly) close to the estimated civilian, non-institutionalized population of 253,580,000 on October 1, 1993 (U.S. Census Bureau 2000). Thus, the LSS strategy of inclusive rostering, combined with screening questions and an adjustment for eligibility to be enumerated as a “usual resident,” produces plausible population totals compared with population benchmarks.

Sampling errors on national estimates are large because the LSS sample is small and oversamples minority and renter populations. When data are nationally weighted, the 328 cases in the stratum representing areas with less than 40% minority or renter population have very

large weights that unacceptably increase standard errors. These cases are eliminated from subsequent analyses. Thus, the following tables represent areas in which the population is at least 40% black or Hispanic, or renters, and not the U.S. population as a whole. Although these strata account for only about one-quarter of the U. S. household population, they are disproportionately affected by coverage errors, and may represent as much as half of the population with two usual residences.

Panel 2 of Table 1 shows that the percentage distribution for the high minority/renter sample strata is similar to the national distribution, with smaller standard errors. A larger fraction (2.3%) had two usual residences, with a 90% confidence interval of 1.6% to 3.0%.

### **Consistency of Reporting**

Reporting consistency may be assessed by comparing usual residence reports given in initial and followup interviews, using the typology presented in Table 1. Three procedural differences may muddy the comparison of the two interviews. First, although both interviews referred to the same time period, the follow-up interview was often conducted several days or weeks later and asked about usual residence on the date of the initial interview. Thus the follow up interview was subject to recall error and distortions due to events (e.g., a person moving out) occurring after the first interview. Second, the initial interview was given by a household respondent reporting for all people in a household, but the individual him- or herself was interviewed in the follow up.<sup>2</sup> Thus the follow-up response was provided by the individual who was most

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<sup>2</sup> Proxy interviews for adults were allowed later in the field period, and follow-up interviews for children aged 12 and younger were conducted with knowledgeable proxy respondents. Of 1,296 people who were the subjects of follow up interviews, 1,037 were personally interviewed and 259 were proxy interviews. Most proxy interviews were conducted with the household respondent from the initial interview. Martin (1999) found that consistency between initial and follow-up reports was greater when the follow-up was with the person him- or herself.

knowledgeable about his or her own living situation. Third, questioning procedures differed. Questions about usual residence were asked early in the initial interview. In the follow-up, before identifying their usual residence, respondents reviewed a calendar and recounted all the places they had stayed and the duration of their stays during the reference period<sup>3</sup>. This review would substantially increase the amount of relevant information available in working memory to respondents. It alters the cognitive task from a quick appraisal to a more considered calculation based on an extended review of recent history. This procedural difference would make the follow up response more well-informed than the more superficial initial response.

Table 2 cross tabulates usual residence reports given in initial and follow-up interviews for the same people. (Because of small numbers, responses of multiple usual residences and no usual residence are combined. Both responses are taken to indicate that there is no one place where a person lives most of the time.)

Although 95% of the sample was reported consistently in both interviews, people initially said to live only in the sample household (row 1) were reported far more consistently than people in other living situations (rows 2-3): 99% of the people in row 1 reported consistently, whereas 22% in row 2 and 64% in row 3 contradicted the initial report by claiming in the follow-up that the sample household was their sole residence. (See Martin 1999 for analysis of some correlates of this inconsistency.)

[Table 2 here]

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<sup>3</sup> The questions asked in the follow up interview were as follows: (1) "Of the places we have listed (MENTION EACH PLACE BY NAME), which did you consider to be your/NAME's usual residence, that is, the place where (you/he/she) lived and slept most of the time as of [END DATE OF REFERENCE PERIOD]?" and (2) "Is there any other place that you (also) consider to be your/NAME's usual residence, where (you/he/she) lived and slept most of the time as of [END DATE OF REFERENCE PERIOD]?"

People were significantly more likely to be reported as usual residents of the sample households in follow-up interviews than in initial interviews. Follow-up interviews produced a million *more* usual residents of sample households, and nearly 1.3 million *fewer* people who lived elsewhere, increasing the population estimate for the high-minority/renter strata by 1 million. Reviewing all the places where a person stayed during the reference period may have corrected some household respondents' false inferences that a person lived somewhere else, or may have revealed a stronger claim to residence in the sample household than had been initially supposed. This shift in reporting about the same people demonstrates that different methods of questioning influence determinations of residence and should temper anyone's confidence in the results of almost any survey.

Table 2 sums the weighted population estimates in the off-diagonal (inconsistent) cells of each row or column. Residence was reported inconsistently for an estimated 2.3 million people initially said to be residents (or non-residents) of sample households (rows 1 and 2 in Table 2). Another 2.4 million were initially said to have multiple usual residences or no usual residence (rows 3 and 4 in Table 1). People whose residence status is ambiguous or uncertain are at risk of being omitted from or incorrectly included in demographic surveys and the census. Groups that are defined in terms of their attachment or lack of attachment to households (e.g., non-resident fathers, children in joint custody) may be misidentified and poorly measured because of ambiguity about where they live.

### **Residential Attachment: An Alternative Measurement Approach**

The evidence suggests that an estimated 4.6 million people in the high minority/renter strata cannot be readily or reliably characterized as "resident" or "non-resident" in a given household. An alternative approach is to consider attachment to a residence as a matter of degree, rather

than a strict dichotomy, to allow for intermediate and ambiguous attachment. I examine this approach by analyzing responses to three measures of residence asked in the initial interview. *Usual residence* is measured with the question, "Do you consider this address to be your/NAME's usual residence, that is the place where you/ NAME live(s) and sleep(s) most of the time?" (76.2% answered "yes.") Second, *household membership* is measured with the question, "Do you consider yourself/ NAME to be a member of this household?" (78.3% answered "yes.") Third, *Living or visiting* is measured with the question, "Please think about all the time (you/ NAME) actually spent here since (DATE). Were you/Was NAME living here, staying here, visiting here or something else?" (76.3% said "living," 1.2% said "staying," 1.9% said "visiting," and 20.5% were not asked the question.<sup>4</sup>

Although many researchers would proceed to simply count the number of "positive" responses to these items as if they indicated the position of the subject on a scale or latent dimension, this procedure may not be justified in the terms of a strict measurement model for the particular items in hand. In many cases, no such empirical justification is attempted. In the present case, the items have been analyzed in terms of the Rasch measurement model (Rasch 1960/1980)<sup>5</sup>. The analysis supports an interpretation of the three items as a unidimensional scale

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<sup>4</sup> The item was not asked about "casual visitors" who were reported to live elsewhere and to have spent seven or fewer nights in the sample household during the reference period. "Living" and "staying" are combined, and casual visitors are classified as "visiting."

<sup>5</sup> The Rasch measurement model (Rasch [1960]/1980) postulates that the items are indirect measures of a single underlying latent variable (residential attachment) that entirely accounts for associations among responses to them. When the Rasch model fits, then (conditional on the value of the unobserved latent attachment variable for a given respondent) responses to the items are statistically independent. That is, the unobserved latent variable entirely accounts for responses to the items, and the items may be considered to form a scale measuring latent residential attachment. RASCHPLX (Fay and Turner 1989) was used to calculate expected frequencies for the cross classification of the three measures under the Rasch model and compare them to observed frequencies using a chi-square test statistic. (These methods were developed by Duncan 1984; Fay and Turner 1989; Goodman 1978; and others, and are applicable to weighted categorical data from complex samples.) The Rasch model fits the data extremely well (jackknifed Pearson  $\chi^2 = -0.53$  on 1 degree of freedom,  $p > .50$ ), implying that the data are consistent

of Residential Attachment (RA). An individual's scale score equals the sum of the number of "attached" responses (0, 1, 2, or 3) that he or she gave to the three items (Duncan 1984). As shown in row 1 of Table 3, 75% of the rostered people had a *strong attachment* to sample households, because they were said to be usual residents, to be household members, and to live there (RA= 3). Two percent gave responses of "attached" to any two items (RA = 2) and are labeled as having *ambiguous attachment*. (An example of someone with an ambiguous attachment is one who is physically present in a household but not considered a household member, such as a live-in domestic, or one who is considered to be a household member although he has another residence where he spends most of his time.) Another 2% had a *tenuous attachment*, in that they gave an "attached" answer to only one item (RA = 1). Finally, 21% did not give an "attached" answer to any item (RA = 0) and are labeled as having *no attachment* to the sample household.

[Table 3 here]

The lower the RA score (i.e., the weaker the attachment), the more probing it took before a person was mentioned during the process of compiling the household roster (row 3). On average, only 1.1 roster probes were needed before respondents mentioned people with a strong attachment, but it took 2.0 probes for people with an ambiguous attachment, 3.8 probes for tenuously attached people, and 4.7 cues for people with no attachment to be mentioned. With less probing, as occurs in virtually all household surveys, people with weak residential attachment would not have been mentioned at all.

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with the hypothesis that a latent residential attachment dimension accounts for responses to the three items. See Duncan (1984) for illustrative applications of the Rasch model to survey data.



The fraction of people who were related to the household respondent increases monotonically with residential attachment (row 4). Undoubtedly, many nonrelatives were visitors (e.g., kids spending the night for a pajama party). However, this result is also consistent with the well-documented census undercoverage of unrelated people in a household (Ellis 1994; Fay 1989), perhaps because tenuous residents are less likely to be mentioned as living there. It is also possible that kinship increases residential attachment; that is, one way a person is “attached” to a household is through kinship.

As attachment attenuates, the fraction reported by household respondents to be usual residents elsewhere increases (row 5).<sup>6</sup> People with tenuous or no attachment (RA=1 or 0) to the sample household were almost uniformly reported to have usual residences elsewhere. However, more than half of people with ambiguous attachment (RA=2) and 2% of those with strong attachment (RA= 3) also were reported to be usual residents of another place. Thus, many people with strong attachments to sample households also report residences elsewhere, suggesting that strong attachment does not preclude multiple attachments. The combination would seem to create a high risk of duplication in the census.

### **Residential Attachment and Social Characteristics**

The correlation between Residential Attachment and other measures of social and economic participation in households is consistent with research showing that residence is predictive of a wide range of demographic variables and outcomes. For example, Carlson and McLanahan (2004) found that co-residence<sup>7</sup> at the time of a child’s birth is an important predictor of non-

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<sup>6</sup>Despite a strong negative correlation, this item does not form a scale with the other three. Fitting a Rasch model to all four items yields an unacceptable fit.

<sup>7</sup>Their measure combines aspects of coresidence and emotional involvement, with four categories: “cohabiting”; “visiting,” defined as romantically involved but living separately; “just friends”; and “not in any

resident fathers' subsequent involvement with their children, concluding that "coresidence provides an important initial context for highly involved fathering" (p. 264).

Rows 6 and 7 of Table 3 show that Residential Attachment is correlated with individuals' roles in and economic contributions to the sample households, as reported in follow-up interviews. Half of the people with a strong attachment but only 8% of those with no attachment had children of their own also staying in the sample household. (This difference is statistically significant, as is the difference between 19% for RA= 2 and 50% for RA= 3.) Similarly, the fraction contributing money for food, rent, or bills declines from 89% of the most attached residents to 34% of the least. (Differences between RA = 0 and 3, 0 and 2, and 1 and 3 are statistically significant.) The number of cases at RA= 0, 1, or 2 is small; combining them produces estimates of 18% ( $SE = 6.4$ ) of people with relatively weak attachment who had children of their own in the sample household and 54% ( $s.e.=5.6$ ) who contributed money. These rates are significantly lower than corresponding rates for strongly attached residents but also suggest that a substantial portion of people with weak residential attachment had significant roles in the household: many had children there<sup>8</sup>, and most contributed financially to the household. Weakly attached residents (including, perhaps, some "non-resident" fathers) stand a good chance of being left off household rosters compiled through standard survey procedures, with the result that their household roles and contributions are underrepresented.

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relationship," determined by the mother's report that she and the father "never" or "hardly ever" talk (Carlson and McLanahan, 2004: 248-9).

<sup>8</sup> Note that a variety of situations (e.g., a parent and child together visiting another household where they do not usually live) might give rise to this pattern.

## CONCLUSIONS

The same groups that are affected by coverage errors in the census also are affected in demographic surveys conducted by the U. S. Census Bureau and other organizations. These groups often are of great demographic interest. For example, non-resident fathers make up a growing proportion of all fathers, and their emotional and financial involvement with their children is of great interest to demographers (see, e.g., Casper and Bianchi 2002; Hofferth et al. 2002). Coverage research conducted by the Census Bureau, and experiments conducted to improve census coverage, may suggest ways to improve the representation of tenuously attached people in demographic surveys. Three innovations of the LSS may prove broadly useful: expanded roster probes, review of places stayed during the reference period, and the Residential Attachment scale.

First, standard survey procedures used to compile household rosters may not be adequate to ensure good coverage of tenuously attached people. Demographers have recognized this, and have adopted innovative strategies to survey underrepresented groups. For example, the Fragile Families and Child Wellbeing Study (Reichman et al. 2001) sampled birth records and conducted interviews in hospitals at the time of a child's birth to identify unwed fathers. The LSS roster methods may achieve better coverage of this and other undercounted groups in more generalizable samples. Expanded roster cues and probes, such as those used in the LSS, cast a wider net and thereby increase the number of people listed on household rosters, including many who are tenuously attached. Some of the non-resident fathers who are missing (or misidentified) at high rates in demographic surveys may not be truly "non-resident" in the households where their children live, but rather may be ambiguous or tenuous residents. This inference is suggested by Coley and Morris's (2002) finding that 11% of fathers who were reported by the

mother of their child not to live with her, reported in separate interviews that they did. Presumably, if they had been interviewed through standard survey procedures, these women would have omitted the fathers (residents by their account) from household rosters. Although it requires more time to administer an extensive set of roster probes and cues, doing so may be productive in surveys that require coverage of segments of the population with tenuous residential attachment.

Second, evidence from the LSS follow-up interviews strongly suggests that an objective review of all the places where a person stayed the previous few months may improve the accuracy of residence reports for people with tenuous attachments to sample households. Although this sort of review is time-consuming and would not be practical in the census or many surveys, it is feasible in demographic surveys for which good representation of tenuously attached people is critical.

Third, the Residential Attachment scale may prove useful in demographic analyses for several reasons. It allows recognition of intermediate degrees of residential attachment, which may be particularly useful in segments of the population whose living situations are ambiguous and complex, such as non-resident fathers. As suggested by results in Table 3 and other research, Residential Attachment may predict other forms of social and economic participation in households. The Residential Attachment scale provides a clean measure of residential attachment. Because it does not conflate residential and emotional or other forms of attachment, it may permit analysts to separate the influence of different forms or dimensions of attachment upon demographic outcomes. The items form a scale, thus meeting standards of measurement. The items that it comprises are simple to administer and to answer, with low rates of missing data. When included in repeated or longitudinal surveys, the scale would permit observation of

variations in the degree of residential attachment over time, and would support analyses of the process of forming or dissolving such attachment.

It would be useful to replicate and extend the Residential Attachment scale, using larger samples. Additional measures might better discriminate degrees of residential attachment at the low ends of the scale. Follow-up reports produced more residents than expected among people with tenuous or no attachment. Adding items to which it is easier to give “attached” answers might help discriminate among people at the low end of the residential attachment continuum.

Does each person in the United States have a unique and unambiguous usual residence? This question is relevant for understanding how well the decennial census and survey methods in general are adapted to the living situations of the U. S. population. Empirical examination of the measurement properties of “usual residence” yields mixed results. *A usual residence typology* based on answers to two simple questions was able to classify 98% of the population as usual residents, or not, of the LSS sample units. For them, the answer may be, “yes”. This result supports reliance upon the usual residence concept to link individuals to households for census enumeration. However, for about 2% nationally (3% in the high-minority/renter strata), the assumption of a unique usual residence failed. It seems necessary to allow measurement to acknowledge that some people have more than one usual residence, and that some people may have none. (Some of these reports may also result from measurement errors.)

The typology of usual residence relaxed the assumption of a unique usual residence, while still assuming that each person could be characterized as a resident, or not, in a given household. The more nuanced, three-item *Residential Attachment scale* relaxed the latter assumption. It suggested that about 4% of people (in the high-minority/renter strata) had ambiguous residence status characterized by intermediate attachment to sample households.

Other analyses have found higher levels of ambiguity (Bates and Gerber 1998), and many people whose residence status appeared clear in initial interviews were not reported consistently in follow up interviews. The inconsistencies do not appear to result from simple unreliability, because the follow up interviews produced a net increase in residents of sample households.

The excellent fit of the Rasch measurement model supports the notion of residence as a continuum instead of a dichotomy. This raises a question: at what point on that continuum should a person be considered sufficiently attached to a sample household to be included in a survey? Evidence from demographic and qualitative research on census coverage suggests that current survey procedures are too exclusive. Further experimentation using larger samples would be needed to evaluate how inclusive survey procedures should be to adequately represent segments of the population with tenuous attachment to residences. (Such research might evaluate the use of the Residential Attachment scale to set a threshold for eligibility for inclusion in a survey.) Additional methodological research using larger samples might build on the innovations tested in the LSS to develop roster procedures to include such people, as well as to more accurately and reliably characterize their residence than current survey methods do. Such research may lead to improved survey measurements of important demographic subgroups that are currently poorly represented and measured in surveys and the census.

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**Table 1. Usual Residence of All People Listed on Household Rosters in the LSS**

Typology of Usual Residence	National Sample (1)			High-Minority/ Renter Strata (2)		
	Estimated Population	%	Eligibility Adjustment	Eligibility- Adjusted Population Estimate	Estimated Population	%
1. Lives at sample unit, no usual residence elsewhere ("yes" to item 1, "no" to 2)	249,350,995	75.1 (2.9)	1.0	249,350,995	63,008,089	73.5 (1.7)
2. Lives at another place ("no" to item 1, "yes" to 2)	76,761,348	23.1 (3.1)	0.0	0	20,348,761	23.7 (1.7)
3. Usual residence at both sample unit and another place ("yes" to items 1 and 2)	3,834,804	1.2 (0.3)	0.5	1,917,402	1,964,093	2.3 (0.4)
4. No usual residence ("no" to items 1 and 2)	2,070,686	0.6 (0.5)	1.0	2,070,686	421,133	0.5 (0.2)
Total	332,017,833	100.0		253,339,083	85,742,077	100.0
Unweighted number of people on rosters	3,537				3,209	

*Notes:* Of the 3,537 cases, 999 were with household respondents reporting for themselves, and the remainder were given by household respondents reporting about others in the sample households. High minority strata include census tracts with a population that is 40% or more black, Hispanic, or renters. Standard errors are shown in parentheses. Missing data are dropped; less than 0.1% of cases were missing for each item.

**Table 2. Consistency of Residence Reporting in Initial and Follow-up Interviews: High-Minority/Renter Strata**

Usual Residence Reported in Initial Interview	Usual Residence Reported in Follow-Up Interview					
	Sample Household Only (1)	Another Place (2)	Sample Household and Another Place; or No Usual Residence (3)	Total (%)	Unweighted <i>N</i>	Estimated Population (in 1,000s) in Off-Diagonal Cells
(1) Sample Household Only (%)	99 (0.4)	— <sup>a</sup> (0.1)	1 (0.4)	100	1,146	844 (227)
(2) Another Place (%)	22 (5.2)	49 (7.3)	28 (7.0)	100 <sup>b</sup>	100	1,487 (324)
(3) Sample Household and Another Place; or No Usual Residence (%)	64 (7.1)	6 (3.1)	30 (8.2)	100	59	1,324 (342)
Estimated Population (in 1,000s) in Off-Diagonal Cells	1,865 (368)	226 (89)	1,563 (336)			

<sup>a</sup>Less than 0.5%.<sup>b</sup>Row does not sum exactly to 100 because of rounding.

**Table 3. Characteristics of Rostered People, by Degree of Residential Attachment: High-Minority/Renter Strata**

	Residential Attachment			
	3 = Strong	2 = Ambiguous	1 = Tenuous	0 = None
1. Percentage Distribution	75	2	2	21
2. Estimated Number of People	63,855,712	1,772,383	1,937,405	17,750,626
3. Mean Number of Probes to List Person	1.1	2.0	3.8	4.7
4. % Related to Household Respondent (household respondents are excluded)	90 (1.7)	80 (9.4)	68 (7.6)	61 (4.2)
5. % Reported in Initial Interviews to Be Usual Residents Elsewhere	2 (0.5)	57 (9.9)	97 (2.3)	99 (0.4)
Unweighted number of rostered people	2,378	69	82	661
6. % of Adults ( $\geq$ 13 years old) Whose Own Children Stayed in the Sample Household	50 (3.5)	19 (10.2)	24 (17.8)	8 (5.8)
7. % of Adults ( $\geq$ 13 years old) Who Contributed Money for Rent, Food, or Bills	89 (3.8)	73 (9.7)	47 (13.0)	34 (11.6)
8. % Claiming in Follow-up Interview to Be Usual Residents in Sample Household	100 (0.13)	82 (7.3)	36 (13.1)	40 (10.8)
Unweighted number of follow-up cases	1,192	38	38	39