UNITED STATES DEPARTMENT OF COMMERCE
Economics and Statistics Administration
U.S. Census Bureau
Washington, DC 20233-0001
OFFICE OF THE DIRECTOR

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MEMORANDUM FOR: Ditas Katague
Chair
National Advisory Committee

From: John H. Thompson
Director
U.S. Census Bureau

Subject: U.S. Census Bureau Responses to National Advisory Committee Recommendations

The U.S. Census Bureau thanks the National Advisory Committee on Racial, Ethnic and Other Populations (NAC) for its recommendations. Your expert advice is critical to identifying new strategies for improved census operations and survey and data collection methods, as well as determining cost-efficient ways to increase census participation.

We are responding to the Committee's recommendations submitted as a result of the meeting held on November 3-4, 2015.

Attachment

Official Recommendations from the National Advisory Committee on Racial, Ethnic and Other Populations

Meeting Dates: October 8-9, 2015
Submitted by: Ditas Katague, NAC Chair
Date Submitted: December 18, 2015

Dear Director Thompson -
The NAC has reviewed the summary recommendations and transcripts from the October 8-9, 2015 in person meetings. I want to thank your team for helping prepare these summary documents as we navigate the transition from the REAC process to the NAC process. Below you will find six Recommendations for new working groups and one recommendation regarding the existing Language Working Group. By unanimous vote, the NAC submits the following official recommendations:

New Working Groups:

1. Working group on undercount of children (per Jerlean Daniels)
2. Government to Government Relations (per Maile Taualii with Desi Rodriguez Lonebear, Randy Akee and Jake Fitisemanu)
3. How Persons with Disabilities are Counted (per Barry Steinhardt)
4. How Census Addresses the Homeless (per Meagan Maury)
5. Gender Identity and Sexual Orientation (per Meagan Maury)
6. Subcommittees that can zero in on HTC groups like persons with disabilities and same sex (per Hassan Jaber)

Census Response: The work of the Hard to Count Working Group included the suggested working group categories listed above referencing populations for children, persons with disabilities, homeless, gender identity and sexual orientation. The group focused on methods to enumerate hard-to-count population that are not easily reached through Administrative Records or the Internet. Specifically, this group reviewed the Census Bureau's research program and plans involving administrative records and third party data use and the internet in the 2020 Census.

The group reviewed interim findings from 2020 Research and Testing projects using administrative records and third party data and findings from research and testing using the internet as a mode for data collection. The Hard to Count Working Group reported during the Spring NAC meeting on May 26-27, 2016. The working group will provide a final report through the NAC within 30 days of the May 26-27 meeting.

We are not prepared to institute a working group on government-to-government relations at this time.

Existing Working Group:

7. Extended time for Language Working group to report during October 2016 meeting (per Carol Gore).

Census Response: The Language Working Group will no longer report during the Spring 2016 meeting. The new date set for the Language Working Group is reporting during the Fall 2016 NAC meeting. This will allow the Language Working Group reporting to be informed by the findings and recommendations presented by the Hard to Count Working Group that reported during the Spring NAC meeting on May 26-27, 2016 and will issue their final report through the NAC within 30 days after the May 26-27 meeting.

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Meeting Dates: October 8-9, 2015
Submitted by: Ditas Katague, NAC Chair
Date Submitted: December 18, 2015

Dear Director Thompson -
Below are unofficial topics submitted by individual NAC members. Included are 10 Requests/ Suggestions, and 5 Census Deliverables. Also, there are some minor changes/edits to the transcripts that are also provided as attachments and one additional statement to add to the record from a new member who could not attend but in support of the recommendation for a working group on Undercount of children.

I. 2020 Census Operational Plan

Individual Member Recommendation:

1. **NAC member recommends Hawaii or Island area as a location for a Census test.**

Census Response: We appreciate this recommendation. The Census Bureau currently has no plans for testing operations specifically in Hawaii but we are considering a test in one of the Island Areas at the end of Fiscal Year 2016. This test would be more technical in nature, and would be meant to investigate the telecommunication environment and feasibility of conducting automated field operations in the Island Areas.

Requests:

1. **NAC would like to provide recommendations for the requirements for the RFP.**

Census Response: On October 27, 2015, the Census Bureau notified the NAC of the tentative schedule for the 2020 Census Integrated Communications Contract. On December 11, 2015, the draft Request for Proposal (RFP) for the 2020 Census Integrated Communications Contract was released and posted as scheduled on the FedBizOpps website and the Business Opportunities website.

2. **NAC members would like to give feedback on the charter and scope of the developing working group.**

Census Response: The Census Bureau launched the Integrated Partnership and Communications Working Group on March 29, 2016, with a total of 12 advisory committee members. This group will assist with each part of the Decennial Census Integrated Partnership and Communication Program by advising research, providing subject matter expertise, and advising on ways to reduce cost. Working group members reviewed and approved the guidelines during the March 29, 2016, kick-off meeting.

3. **NAC would like to provide recommendations on the Tribal consultation handbook.**

Census Response: The National Advisory Committee received a briefing during and October 13, 2015, virtual NAC meeting on the Tribal Consultation Handbook and provided a NAC Official Recommendation and individual member comments on AIAN Enrollment Question Moderator Guide. The Census Bureau provided responses to the recommendation and individual comments on January 28, 2016.

4. **NAC would like an opportunity to give feedback on the testing of population groups such as homeless. This should be included in the HTC scope of the HTC Working Group.**

Census Response: We appreciate your comments and look forward to receiving the report and recommendations from the Hard to Count Working Group at the NAC Spring meeting on May 26-27, 2016. We look forward to the working group's final report submitted through the NAC within 30 days of the NAC Spring Meeting on May 26-27, 2016.

5. **NAC would like time to review and provide recommendations on the advertising campaign especially in regards to recruitment and advertising.**

Census Response: The Integrated Partnership and Communications Working Group launched on March 29, 2016. This NAC working group will assist with each part of the Decennial Census Integrated Partnership and Communications Program and we look forward to the recommendations that emerge from this effort.

Census Deliverables:

1. **NAC member requested a report about the characteristics of the nonrespondents.**

Census Response: The Census Bureau acknowledges and appreciates the recommendation from a NAC member requesting a report about the characteristics of nonrespondents. We are currently documenting the analysis, measures, and metrics that we will produce associated with the 2016 Census Test; we will add analysis of the characteristics of nonrespondents to the scope.

As we plan and conduct subsequent tests, we will plan to include this analysis as a matter of course.

For more information about the interviews conducted during the 2010 Census with Nonresponse Followup respondents, including the language in which the interviews were conducted, the number of contacts that enumerators made to enumerate a housing unit, and the type of respondents who completed Nonresponse Followup interview, click on the link below for the 2010 Census Nonresponse Followup Operations Assessment Report:
http://www.census.gov/2010census/pdf/2010_Census_NRFU_Operations_Assessment.pdf

The American Community Survey has a high response rate (96.7 percent in 2014) and has not conducted a survey of nonrespondents. We have looked at the characteristics of respondents by mode based on the 2005 American Community Survey Data found at the following link:

(http://www.census.gov/content/dam/Census/library/working-papers/2008/acs/2008_Joshipura_01.pdf). This report shows the differences in who responds by mode.

Also included in the appendices are several reports and papers that provide further detail on Census efforts and studies on characteristics of nonrespondents, which describe characteristics of hard to survey populations for Census Bureau demographic surveys and decennial Censuses (see Attachments 1-4):

- *The Last Five Percent: What Can We Learn From Late/Difficult Interviews?* (Nancy Bates and Kathleen Creighton)
- *Hard-to-Survey Populations*, Chapter 1: Defining hard-to-survey populations (Roger Tourangeau)
- *Nonresponse in Household Interview Surveys*: Wiley Series in Probability and Statistics Survey Methodology Section (Robert M. Groves and Mick P. Couper)
- *Hard-to-Survey Populations*, Chapter 3: Measuring undercounts for hard-to-survey groups (Mary Mulry)

2. Census will develop a milestone schedule with touch points. This milestone schedule would be finalized in the working group that is in the process of being developed.

Census Response: The Census Bureau shared a preliminary version of the milestone schedule with touch points document during the NAC Fall Meeting on November 3-4, 2015. The document entitled, *National Advisory Committee Fiscal Year 2016 Engagement Plan*, was provided during the meeting and posted along with other meeting materials on the NAC website: https://www2.census.gov/cac/nac/meetings/2015-10/2016-engagement-plan.pdf.

An updated milestone schedule with touch points document was again briefed to the NAC in a virtual Public meeting on January 13, 2016, and the document posted to the NAC website at https://www.census.gov/about/cac/nac/meetings/2016-01-meeting.html.

The Integrated Partnership and Communications Working Group on launched March 29, 2016, to assist with each part of the Decennial Census Integrated Partnership and Communication Program by advising research, providing subject matter expertise, and advising on ways to reduce cost. Working group members reviewed the milestone schedule with touch points during the first meeting on March 29. The working group reviewed the milestone schedule with touch points document entitled, *NAC 2020 IPC Working Group Check Points* (see Attachment 5), during its second meeting held by Integrated Partnership and Communications Working Group Convener Megan Maury on April 28, 2016. Working group members are aware that the schedule may be modified throughout the seven-year period of the document that spans 2015-2021.

3. Census will provide a spreadsheet of administrative records and third party data and process of negotiation for the acquisition of the files.

Census Response: Attachment 8 is a spreadsheet listing the inventory of administrative records and third party data that are available as of April 25, 2016. Below is our process of acquiring population and housing files from federal, state, and third party entities:

Federal:

- The Census Bureau is working on modifying/renewing existing agreements in order to increase frequency of federal data delivery in order to support 2020 decennial activities.
- The Census Bureau is working with different Federal agencies for new data sharing agreements.
- The Census Bureau is working with congressional staff to explore legislative changes to acquire new and improved data sources.

States:

- The Census Bureau is engaged in communication with all 50 states and the District of Columbia with a primary focus on:
  - Supplemental Nutritional Assistance Program (SNAP)
  - Temporary Assistance for Needy Families (TANF)
  - Women, Infants and Children Program (WIC)
- Communication with states is at various stages
  - The Census Bureau has data sharing agreements from 15 states
  - The Census Bureau has received data from 10 states
- The Census Bureau is expanding outreach efforts to include personal visits to states to encourage them to enter into data sharing agreements.

Third Party data:

The process for acquiring third party data begins with market research, followed by submitting Requests for Proposals (RFP) or Requests for Information (RFI). After receiving data from a vendor that won the contract, more research is conducted to determine the fitness for use of the data.

- The Census Bureau is currently engaged in establishing multi-year agreements with vendors.
- The Census Bureau is engaged in increasing the frequency of delivery when possible to support 2020 decennial activities.

II. 2015 Census Test-Savannah Site Digital Advertising

Individual Member Recommendations:

1. **NAC member recommends the following:**

   - **People's ages should be recorded, and ideally date of birth and date of data collection should be recorded so that age in years and days should be calculable.**

Census Response: Age, date of birth, and data collection information are recorded. Age data are derived from a two-part question. The first part asks for the age of the person and the second part asks for the date of birth. The question is designed in two parts in order to maximize both the accuracy and the number of people responding to this item. In the decennial census respondents are asked for their age as of Census Day and that age is compared to the age calculated using their date of birth. Please see the 2010 Census Brief, *Age and Sex*

Composition: 2010 for a reproduction of the age and date of birth question in the 2010 Census https://www.census.gov/prod/cen2010/briefs/c2010br-03.pdf.

- **When providing results, if age groups are provided, these should be given in 5 year (or less) increments all the way to the maximum age recorded in the Country. There are major differences for example between people living to 85 versus 95 versus 105 (eg. rarity, clinical homogeneity, survival and other features) and if at all possible ages should not be grouped in greater than 5 year increments.**

Census Response: A variety of tables are available showing both 2010 Census data and American Community Survey data in five-year or less age group increments. For instance, *2010 Census Summary File 1,* Table PCT12: Age and Sex shows age data for singles years of age by sex down to the tract level of geography (see Attachment 6). Data are provided in single years of age 0-99 followed by ages 100-104, 105-109, and 110 and over.

Additionally, 2010 Census data are available down to the block level of geography in five-year increments for ages 0-84 and 85 and over with smaller increments for ages 15-17, 18-19, 20, 21, 22-24, 60-61, 62-64, 65-66 and 67-69. These data are available from the *2010 Census Summary File 1*, Table P12: Age and Sex. Similar data are available for places with population 65,000 and over using the American Community Survey, 2014, Table B01001: Age and Sex.

- **Results should be portrayed with and without segregating by sex for these groups as well.**

Census Response: The age and sex tables indicated above show data for males and females that can be aggregated to show age data for the total population. Additionally, tables show information by age for both sexes. For instance, see the *2010 Census Summary File 1*, Table DP1: Profile of General Population and Housing Characteristics: 2010 (see Attachment 7). Other tables include *2010 Census Summary File 1*, Table QT-P1: Age Group and Sex: 2010 and American Community Survey, 2014 Subject Table S01001: Age and Sex.

2. **NAC recommends sharing questions for the Census Test focus group. The HTC group will include this in their recommendations to the NAC.**

Census Response: The Hard to Count Working Group has as-needed access to Census Bureau subject matter experts and requested information as they prepare for their recommendations to be presented to the NAC during the public meeting on May 27, 2016.

Census Deliverable:

1. **Census Bureau will provide NAC more analysis and examples of ads.**

Census Response: We are on schedule for the upcoming NAC meeting to provide the additional analysis requested. At that time, we will also make additional advertising examples available.

2. **Census will provide list of 12 languages for the TQA telephone lines**

Census Response: During the 2015 Census Test TQA operation, the Census Bureau provided support in 10 languages – English, Spanish, Chinese (Mandarin, Cantonese), Vietnamese, Korean, Arabic, Tagalog, French, and German. We are on schedule for the upcoming NAC meeting on May 26-27, 2016. Census will provide list of 12 languages for the TQA telephone lines.

III. Committee Discussion

Individual Member Recommendations:

1. **Request to include/invite NAC members in local Census public discussions so that we can be aware and possibly attend.**

Census Response: The Census Bureau will notify NAC members of meetings involving public discussion, whenever possible.

NAC Request:

1. **2016 Test – NAC would like to review the focus group plan or focus group questions before Census Day of April 1, 2016. This could be done in a call in meeting. Lisa Blumerman can give a timeframe to set the virtual meeting.**

Census Response: The Census Bureau regrets no prior call-in meeting was possible. The NAC Spring Meeting on May 26-27 will include a Decennial Directorate presentation on *2020 Census Program Overview* to include an update on the April 1, 2016, Census Day and a site test in parts of Harris County, TX and Los Angeles County, CA.

Request for information:

1. **Third Party/Administrative Record data– Evan Moffet asked about Administrative and third party data that can be used. Ditas/Carol request that NAC member send in Third Party and/or Administrative Record data.**

Census Response: Attached is a spreadsheet listing the inventory of administrative records and third party data that are available as of April 25, 2016 (see Attachment 8). Below is our process of acquiring population and housing files from federal, state, and third party entities:

Federal:

- The Census Bureau is working on modifying/renewing existing agreements in order to increase frequency of federal data delivery in order to support 2020 decennial activities.
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- The Census Bureau is working with congressional staff to explore legislative changes to acquire new and improved data sources.

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- The Census Bureau is engaged in communication with all 50 states and the District of

Columbia with a primary focus on:
  - Supplemental Nutritional Assistance Program (SNAP)
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Third Party data:
The process for acquiring third party data begins with market research, followed by submitting Requests for Proposals (RFP) or Requests for Information (RFI). After receiving data from a vendor that won the contract, more research is conducted to determine the fitness for use of the data.

- The Census Bureau is currently engaged in establishing multi-year agreements with vendors.
- The Census Bureau is engaged in increasing the frequency of delivery when possible to support 2020 decennial activities.

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 1

The Last Five Percent:

What Can We Learn from Late/Difficult Interviews?

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[1]This paper reports the results of research and analysis undertaken by Census Bureau staff. It has undergone a more limited review than official Census Bureau publications. This report is released to inform interested parties of research and to encourage discussion. The authors wish to acknowledge Adelle Berlinger, Greg Weyland, Marilyn Monahan, Dave Watt, Tracy Mattingly, Patricia Bowles, Chris Laskey, Antoinette Lubich, Susan Gajewski, Ive Barreiros , Nancy Cioffi, and Nancy Hunter for administrative and technical assistance with this paper.

Abstract

Statistical government agencies expend a great deal of resources to keep survey nonresponse to a minimum. To explore whether the additional time and resources are worth the effort, we focus specifically on the characteristics of late or 'difficult' cases that comprise the last few percentage points of survey response rates. To address this topic, we examine several characteristics of late/difficult cases from the May 1999 Current Population Survey (CPS) and two quarters of 1999 data for the National Crime Victimization Survey (NCVS). First, we explore whether the household and person-level demographic characteristics of late cases differ from earlier interviews. Next, we check to see if critical survey estimates would be different without late cases by dropping them from the data and re-running the estimates. We found evidence from both surveys that a portion of late cases are similar in some respects to nonrespondents. We also found that while the magnitude of difference between estimates with and without these cases is usually small, in many cases, the estimates are significantly different without the late cases. We recommend that the time and effort required to obtain late/difficult interviews is worth the added costs since they are critical to producing unbiased estimates and minimizing nonsampling error. We conclude with a discussion of how these results can help survey managers and analysts better understand the relative contribution that late cases make to the two surveys examined and how further increases in nonresponse may contribute toward bias.

1. Introduction

At the 1999 International Conference on Survey Nonresponse, researchers from around the world presented evidence that survey organizations are expending more resources than ever just to keep survey response rates at their current levels. Over the past two decades, government agencies have witnessed a gradual but steady decrease in survey and census participation rates (Atrostic, et al, 1999; de Heer 1999; Groves and Couper, 1998; Bryant and Dunn 1995; Fay, Bates and Moore, 1991). Some theories behind this decrease include social isolation, growing privacy concerns, mistrust of government, increased time pressures, and proliferation in telemarketing. To combat the decline, organizations have implemented a variety of techniques including respondent incentives, interviewer incentives, enhanced interviewer training, changes to field procedures, and experimentation with alternate modes of response (see Singer, Van Hoewyk and Maher, 2000; Olson, Srinath, Burich 2000, Singer et al, 1999; Lauria, Smith and Scott, 1999; Groves and McGonagle, 1998; Schaefer and Dillman, 1998; Dillman, West and Clark, 1994).

Assuming these strategies are successful, is the extra effort to keep response rates from declining a few percentage points worth the added time and money? According to the "continuum of resistance" theory, the answer is yes. This model of nonresponse postulates that people who require the most calls or contacts before participating are also the persons most resistant to the interview (Fitzgerald and Fuller, 1982; Lin and Schaeffer, 1995; Filion 1976). If we take the theory one step further, we arrive at the assumption behind it, that is, the more

difficult it is to gain survey participation by an individual, the more he or she resembles individuals who actually refuse. From a field perspective, we might also think of late interviews reflecting nonrespondents simply because if the field period is shortened, such cases become nonrespondents. If this thinking is accurate, then late cases are very important to reduce nonsampling bias in the estimates.

Alternatively, several studies cast doubt on the assumption that difficult interviews are necessarily indicative of nonrespondents (particularly refusers). For example, Fitzgerald and Fuller (1982) rejected the notion when they failed to find many similarities between cases requiring a great number of callbacks and those who ultimately refused the survey. Similarly, Lin and Schaeffer (1995) report the continuum model was not successful in reducing the true degree of nonparticipation bias in estimates of child support. However, Fitzgerald and Fuller did find evidence of demographic differences between the sample at various callback periods and the full sample. Additionally, they report that reluctant respondents had significant effects on the relationships between variables in their study of community characteristics and social networks.

A handful of other studies have focused on nonresponse and the impact it has on survey estimates (Harris-Kojetin and Robison, 1998; Tucker and Harris-Kojetin, 1998; Kennickell, 1999; Groves and Couper, 1998). Some of these studies suggest the potential for small yet significant levels of nonresponse bias as a result of differential characteristics between responders and nonresponders (e.g., labor force participation, socioeconomic status, presence of children). Similarly, a few studies have focused specifically on late interviews that comprise the

last few percentage points of response (Cohen, Machlin and Branscome, 2000; Kennickell, 1999; Voigt, Koepsell and Daling, 1999; Stoop and Louwen, 1999; Stapulonis, Kovac and Fraker, 1999; and Krenzke and Griffin, 1997). These studies report varying degrees of differences between demographic characteristics of early versus late respondents, (sometimes referred to as the degree of *interim distributional bias* in a survey, see Hawkins, 1977).

The Krenzke and Griffin study finds that demographic characteristics of the "last 10 percent" in the 1990 Census are significantly different from the population as a whole only when truncated at the national level -- when examined from the tract level, characteristics of the first 90 percent are similar to the last 10 percent. A study of late respondents in the Netherlands Facility Use Survey revealed that early and late respondents differed both demographically and in reported facility use; however, these differences disappeared for the most part once weighting adjustments are applied. Cohen, Machlin and Branscome report that differences between medical expenditure survey estimates with and without reluctant respondents were mostly negligible. In their study of a welfare population, Stapulonis et al. report surprisingly small deviations in demographic characteristics of both a 40 percent and 60 percent response rate from those of all survey respondents (76 percent response rate) and the full sample. Both Voight et al. and Kennickell find that late respondents tend to be significantly younger than earlier respondents but neither found strong evidence to suggest that late respondents are necessarily similar to nonrespondents (as measured by initial refusals). Based on these few findings, it is difficult to draw conclusions whether the absence of late interviews contributes to nonsampling bias that cannot be adjusted for using traditional weighting methods.

In this article, we explore the impact of interim distributional bias by examining the characteristics of late interviews for two continuing demographic surveys conducted by the U.S. Census Bureau. These are the Current Population Survey (CPS) and the National Crime Victimization Survey (NCVS). Like other surveys, both the CPS and the NCVS have documented incremental increases in nonresponse since the beginning of the decade. In 1990, the initial nonreponse rate[2] for the CPS was 5.7 percent but by 1998, it was 8.8 percent. Similarly, the initial contact nonresponse for NCVS in 1990 was 4.3 percent but increased to 6.1 percent by 1998 (Atrostic, et al, 1999). By private-sector standards, these levels of nonresponse may seem trivial. But, to survey sponsors, field data collection managers, and data users alike, the increase is still cause for concern. The concern is twofold: first, since both surveys provide national indicators of critical social and economic value, a continual increase in nonresponse could translate into increased bias in the estimates. Second, declining budgets for statistical programs make it more difficult to justify the added costs and resources to keep nonresponse at these relatively low levels.

In the sections that follow, we first describe major design aspects of the two surveys. This description is followed by our operational definitions for a late/difficult interview. We then use logistic regression to see which variables are best at predicting these interviews and present tables of selected survey estimates from both surveys (labor participation rates, unemployment

[2] Sum of the eligible units that were not interviewed during the first round of interviewing because of language problems, refusals, no one home, temporary absence, or other reasons, divided by the number of eligible units multiplied by 100. See Atrostic et al. (1999) for detailed initial contact response rate definitions and formulas.

rates and victimization rates). These tables contain two sets of estimates: those produced with the late/difficult cases and those without them. Based on these findings, we conclude with a discussion of the relative importance of obtaining the "last 5 percent," that is, the late/difficult cases.

2. Methodology

2.1 National Crime Victimization Survey (NCVS) Design

The NCVS has operated continuously since 1972 and is one of two Justice Department annual measures of crime in the United States (the other being the Uniform Crime Reports) and the only one that measures crimes not reported to the police. The survey has a national sample of about 58,000 designated addresses and uses a "rotating panel" design, in which 1/6 of the total sample is interviewed by telephone or personal visit in a given month and again at 6-month intervals. Sample addresses are interviewed a total of seven times over a 3-year period before rotating out of the sample. The first month an address comes into sample, a Census Bureau interviewer conducts a personal visit interview using a paper and pencil questionnaire. In the subsequent six survey periods, the vast majority of interviews are conducted by both centralized computer-assisted telephone interviewing (CATI) from two telephone centers and decentralized telephone methods. On average, an NCVS interview takes approximately 25-30 minutes. This estimate varies widely depending on the number of eligible household members and whether the sample person reports a crime. The NCVS also occasionally includes supplements to the basic

interview which may extend the interview by 10 minutes.

Following the first month of contact, interviews are virtually all conducted by telephone, unless the respondent specifically requests a personal visit. Only persons age 12 and over are eligible for interview and each interview must be conducted with the sample person him/herself. Proxies are taken only if the sample person's physical or mental disability precludes the self-response, if a sample person is temporarily absent and will not return until after the end of the survey period, or if the parent of a 12 or 13 year old insists on responding for the child. If none of these conditions is met and the interviewer is unable to contact the sample person for an interview, the case becomes a person noninterview. Most CATI interviewing is conducted from the first through the fifteenth of each month, with occasional extensions for holidays or natural disasters -- personal visits continue until around the 23rd of each month. The reference period for the crime interview is the six months prior to the interview date, and the initial interview for a sample household is used to "bound" the interviews and is not used in the annual estimates. This bounding interview establishes a time frame to avoid duplication of crimes on a subsequent interview.

The NCVS weighting procedure adjusts for two types of noninterviews: household noninterview and person noninterview. The initial contact household nonresponse now exceeds 6 percent of occupied households, up from around 4 percent at the beginning of the decade, while person noninterview now exceeds 10 percent of the persons eligible in an otherwise interviewed unit, up from 6 percent since 1992 (Atrostic, et. al, 1999).

For this analysis, we used data from the 1st and 2nd quarter of 1999 (January - June). This enabled us to analyze reports and construct estimates from the full annual sample of NCVS households which is divided into six equal parts and interviewed over a six month period. The total number of interviewed households in Q1 and Q2 was 43,141; the number of person-level interviews was 78,640.

2.2 Current Population Survey (CPS) Design

The CPS has operated continuously since 1940 and provides monthly estimates of employment and unemployment. In addition to the monthly labor force estimates, the Census Bureau funds a supplemental CPS interview that collects annual data on work experience, income, poverty, and migration. The survey has a national monthly sample of approximately 60,000 designated addresses composed of eight panels that rotate on a schedule of 4 months in, 8 months out, 4 months in, so that only 25 percent of the households differ between consecutive months. The first month a unit comes into sample, interviewers conduct a personal visit interview using an automated instrument on a laptop computer. In the subsequent 7 survey periods, interviewers conduct both centralized and decentralized interviews by telephone. On average, the recurring portion of the CPS interview takes about 10 minutes, although supplemental items are included in most months of the year and can add 5 - 25 minutes to the interview length.

Following the first month of contact, most interviewing is by telephone, although the

interviewer is required to attempt a personal visit in the 5th month of interview, following the 8-month resting period. A household respondent is interviewed for the labor force portion of the interview and an interview is completed for each household member age 15 and over. Unlike the NCVS, the CPS procedures for the core labor force section permit a single household member to respond for everyone else, thus requiring only household-level nonresponse weighting adjustments. Most interviewing takes place during a one-week period (Sunday to Saturday) during the week containing the 19th of the month and refers to labor market activities for the prior Sunday to Saturday period. Initial contact household nonresponse is now close to 9 percent of occupied households, up from around 6 percent in 1990 (Atrostic, et al, 1999). Since the survey produces monthly labor force estimates, we selected one month of data (May 1999) for the analysis.[3] This included 47,613 interviewed households which yielded information for 92,899 persons.

3 Operational Definitions of Late/Difficult Interviews

3.1 Late Interviews for the NCVS

Our decision to concentrate on late interviews is driven by the underlying assumption that late interviews reflect cases most difficult to obtain and may be indicative of future noninterview

[3] In addition to the core CPS survey, the 1999 May CPS had a supplemental questionnaire on tobacco use for all persons aged 15 and over. For the supplement, each eligible person self-responds. Thus, the amount of time required to close-out a case was longer than usual for households with a large number of eligible respondents.

levels. Presumably, the last few cases are cases that require repeated contacts, refusal-conversion, multiple callbacks and the like. However, without access to contact and scheduling information, the association between the timing of the interview and degree of difficulty is only an assumption. When constructing our definition of late cases for this analysis, we hoped to use ancillary contact information to validate using date of interview as an indicator of difficulty. The NCVS routinely collects information on the record of personal visits and telephone calls that interviewers make for each case. However, this contact information is kept separate from the electronically stored questionnaire data. Consequently, we were limited to the date of interview variable stored on the questionnaire data file.

To decide the cut-off date for late cases, we examined a chart of date of interview by month. A fairly clear pattern emerged for each month whereby the number of interviews dropped off and became flat typically beginning around the 17th of the month. Interviews completed from this date until regional office closeout comprise between 5-6 percent of the total person-level interviews for each month. When analyzing data at the household level, we defined a case as late if one or more interviews within a household was conducted after the cutoff date. When analyzing at the person-level we simply used date of interview.

To validate our assumption that the date of interview correlates to some extent with number of contacts, (the later the interview date, the more contacts) we generated a random

sample of late cases and nonlate cases from two regional offices (Boston and Atlanta)[4]. For selected cases, clerks photocopied the contact information from the control cards and sent the information to headquarters. Based on the sample with contact information, we found the mean number of contacts (personal visit and telephone combined) to be 7.1 for late cases compared to 3.5 for nonlate cases[5]. We took this as evidence that date of interview is an indicator, to some degree, of interview difficulty.

3.2 Late/Difficult Interviews for the CPS

When constructing the operational definition of late interviews for CPS, we had access to more information than just date of interview. The CAPI data file also contains a counter of the number of actual and attempted person contacts. However, there are several limitations to using this variable. First, in 1999 the CPS instrument recorded contacts only for personal visit interviews. This applies to all cases requiring personal visits (month-in-sample one and month-in-sample five). The information is also presumably captured for personal visits conducted in other months (where personal visits are not required but may be necessary if the interview is not obtained by phone, usually after three days).

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[4]From our original sample of approximately 300 cases, just over 250 were located and photocopied. However, close to 100 of these did not have contact information recorded on the control card for the month in-scope. In some cases the contact information was simply left blank while in other cases, the household for the month of interest had moved and a replacement control card for the new household had superseded the old contact information. This yielded approximately 160 usable cases from which contact information could be tallied.

[5]Nonlate cases had a slightly higher incidence of personal visits and telephone calls made before 5 p.m.; for late cases, personal visit contacts were made with about the same frequency both before and after 5 p.m., but more telephone contacts were conducted after 5 p.m.

The second, and perhaps more serious caveat to this indicator is the degree of between-interviewer variability with which it is recorded. For the counter to increase, the interviewer must access and open the case from the laptop, thus recording each personal visit made to the address. However, we have no way to confirm how often interviewers fail to actually open a case from the computer when visiting an address; that is, the interviewer could visit an address on several occasions, find no one home, but never activate the case prior to attempting contact. Such cases will understate the number of contacts.

Similar to the NCVS, we constructed a chart to illustrate the distribution of CPS interviews by date. We decided to define late cases as those completed during the last two days of the data collection period (Tuesday or Wednesday of the second week). We also included in our definition cases that were personal visit and required four or more contacts, yet may have been interviewed before the late cutoff. Using this criteria, our late cases comprised 4.6 percent of the May, 1999 interviews. As expected, we found a positive relationship between day of interview and the mean number of contacts required in personal visit cases. Personal visit late cases required an average of 3.0 visits while nonlate personal visit cases averaged 1.2 visits.

4. Results

4.1 Characteristics of Late Interviews - NCVS

To understand the person and household characteristics that predict late/difficult interviews and control for covariation between them, we ran a series of logistic regression models where the response variable was defined as Late = 1, Non-late = 0. We ran two

separate models predicting late interviews at the person level and again at the household level. At the person level, our predictor variables included: proxy/non-proxy, mode of interview, number of crimes reported, age, education, relation to householder, sex, race and Hispanic origin. We found that, holding the other variables constant, proxy status, age of respondent, education, race, relation to householder, mode of interview, and number of crimes reported were all significant predictors of being a late interview (see table 1).

Table 1 about here

Specifically, we found that proxy interviews were over twice as likely to be late interviews compared to non-proxies. This is common of late interviews as NCVS procedures allow proxies only as a last resort to complete a case. Telephone interviews were only about half as likely to be late interviews compared to personal visits. Since CATI cases that are not completed early in the interview cycle are automatically recycled to the field, this is expected. As the number of reported personal crimes increased, the likelihood of being a late interview decreased (by about 10 percent). As age increases, the odds of being a late interview decrease (with each one step increase in the age category, the odds of being a late interview decrease by about 32 percent). Interviews conducted with a spouse, child, or other relative of the householder are all less likely to be late interviews compared to those conducted with the householder. Finally, compared to white respondents, both Blacks and Asian/Pacific Islanders were more likely to be late interviews (26 percent and 40 percent more likely than whites, respectively).

At the household level, we regressed the likelihood of being late on: presence of person-level nonresponse in the household, household income, household tenure, interview outcome from the previous month, number of eligible respondents, and number of personal/property crimes reported (see bottom of Table 1). Renter households were found to be 41 percent more likely to be late interviews compared to owner households. Also, for every additional eligible person in a household, the likelihood of being a late interview increases by around nine percent. Both medium and high income households were more likely to be late interviews compared to low income households; households missing income information were almost twice as likely as low income households to be late interviews. Finally, we found that as the number of reported crimes for a household increased (combined personal and property), the odds of being a late interview decreased (by about 13 percent).

We found that interview status from the previous month was very telling – replacement households in sample for the first time (to replace movers) were over three times more likely to be late interviews compared to households interviewed during the previous cycle. Presumably this is because they require extra time to discover the original occupants have moved and then to subsequently establish fresh contact and interview schedules for the new occupants. Compared to households that were interviewed during the last month, noninterview households from the previous interview cycle were 2.7 times more likely to be late interviews in the current month. Similarly, households that contained eligible persons who were never interviewed (some degree of person-level nonresponse) were twice as likely to contain late interviews compared to households where all eligible respondents were interviewed. The relationships between late

interviews and person-level nonresponse and prior-interview unit nonresponse lend some support to the notion that late cases resemble nonrespondents.

4.2 Estimates with and without Late Interviews - NCVS

In our last step of the NCVS analysis, we re-ran a selected set of crime statistics without the late cases and compared these to the estimates based upon all observations. This was done by converting the late cases to nonresponse cases and then re-weighting the data. We believe such an exercise can simulate the crime estimates under the conditions of slightly increased nonresponse (from approximately 6 percent to 11 percent), but several caveats are important to mention. First, our data represent only two quarters of data (January-June), not the entire year for 1999. Second, our estimates are at the national level, and third, crime rates in 1999 were at relatively low levels having steadily declined since 1993 (U.S. Department of Justice, 1998). Any of these factors could mask differences between estimates with and without late cases. For example, if we completed this exercise at a lower level of geography (e.g., a large metropolitan city), we might see crime rates with greater variability, more late interviews, and consequently, more significant differences between estimates with and without late cases.

The results are shown in Tables 2 and 3. Tests for significant differences between the two sets are calculated using the standard error for the late cases only since the overlapping cases from the two columns add nothing to the variance. Since the overlap of cases used to

produce both sets is very large (approximately 95 percent), the variance for estimates is extremely small. Survey design effects of the NCVS were taken into account when testing for significant differences.

Tables 2 and 3 about here

The magnitude of absolute difference in the crime victimization rates is small between those calculated with all cases and those that exclude the late cases. This is somewhat expected since both sets of estimates contain approximately 95 percent overlap. Nonetheless, over one-third of the comparisons yield a significant difference in the rate of victimizations per 1,000 persons. From Table 2, significant differences were found for total property crime rates and theft (the difference between the violent crime rates was just significant at the .10 level). In Table 4, significant differences were found for violent crime rates among age categories 16-19 and 25-34; for 'other' races; by ethnicity, for income categories of $15,000-24,999 and $50,000-75,999, and for suburban and rural residents.

In almost every case where differences are detected, the crime rates produced without the late cases are higher than those based on all cases. This seems logical considering we found the number of reported crimes to be inversely related to timing of interview, that is, households interviewed late in the cycle had fewer reported crimes. Of course, this finding could be due to something other than a true differential in victimization rates between early and late interviews.

Instead, the tendency for later interviews to have less crimes reported could be a data quality issue. We know that late interviews are more likely to be proxy, to have person-level nonresponse, to have been unit nonrespondents in the past, and have larger numbers of people to interview per household. These factors, and the fact that they are 'close-out' cases by definition, may all contribute toward a rushed interview that discourages reporting of crimes (which prolongs the interview). To check for a relationship between proxies and the number of crimes reported, we tested the interaction term PROXY*CRIMENO. This term was not significant so we found no evidence that the number of crimes reported in late interviews is conditional upon whether the interview is proxy or self-response. Aside from this, however, we lack substantive field evidence (or other evidence) to either support or refute this data quality hypothesis. Nevertheless, the detection of significant differences even with a 95 percent overlap in cases suggests that the absence of late interview households may cause an upward bias in certain crime estimates that are not adequately addressed using the existing weighting class adjustments for nonresponse.

4.3 Characteristics of Late Interviews - CPS

Similar to the NCVS analysis, we began our exploration of late cases for CPS by comparing person and household-level characteristics of late interviews to nonlate interviews. Unlike the NCVS, a knowledgeable proxy provides the majority of person-level information in the CPS. Consequently, most information in Table 4 is generated from one respondent per household who answers for him/herself as well as the other household members.

To summarize the predictive power of the various person and household-based characteristics, we ran logit models using the CPS data. Again, the response variable was coded=1 if the household was defined as late/difficult, otherwise it was coded= 0. Since date of interview is the same for all persons in the household, the response variable was the same for both person and household-level analysis. At the person level, we included race, relationship to householder, sex, age, education, Hispanic origin, and labor force status as explanatory variables (see Table 4).

Table 4 about here

We found that interviews about nonrelatives in the household were more likely to be late interviews and that late interviews are less likely to contain interviews with/about a spouse. This suggests that households with married couples are less likely to be late but households containing unrelated individuals are more likely to be late. We also found that, all other things held constant, if the interview was with/about a person in an older age category, the likelihood of being a late interview decreased (with each increase in an age category, the odds of being a late interview decrease by around 14 percent).

Once other characteristics are held constant, Blacks were found more likely to be interviewed late in the field period compared to whites (64 percent more likely). Compared to non-Hispanics, interviews for persons reporting a Spanish origin were almost 30 percent more likely to have been obtained during a late interview. Finally, we found interesting associations between labor force status and the likelihood of being a late interview. Compared to those who

were employed and currently at work, persons who were not in the labor force for 'other' reasons or who were employed but currently absent were both more likely to be late interviews. Conversely, retirees and those who are disabled are significantly less likely to be late, compared to those currently working (50 percent and 39 percent less likely, respectively). This follows since retirees and disabled workers should theoretically be easier to find at home and thus overrepresent interviews completed during the early stages of data collection. Neither sex nor education are significant predictors of late interviews.

At the household level we found significant relationships between interview mode, tenure, income, region, number of household members, and interview outcome from the previous interview cycle. Late interviews were less likely to be telephone (not unexpected since unresolved CATI cases are automatically recycled to the field and often require a personal visit) and more likely to reflect renting households than owners. Compared to the lowest household income category, all other categories (medium, high and income missing) were more likely to be late interviews. This is particularly true for households for whom an income value is missing (94 percent more likely to come from a late interview compared to low income households). The larger the number of people in the household, the more likely the interview is to be late -- this is likely due to the self response supplement on tobacco use administered during May 1999. Compared to the Midwest, interviews from all other regions are more likely to be late. The model revealed no significant association between level of urbanization and likelihood of being late once the other variables are controlled for. Finally, compared to households that were interviewed during the previous interview wave, households that were previously noninterviews

were more likely to be late interviews in the current month. Previously noninterview households were close to 3 times more likely to be late (2.8 times more likely). Again, like in the NCVS, these findings suggest similarities between a portion of the very last interviews and the nonrespondent subpopulation.

4.4 Estimates with and without Late Interviews - CPS

Similar to the NCVS, we arranged to have a selected set of labor force estimates re-run for the 1999 May CPS excluding the cases we defined as late/difficuilt. These cases were redefined as nonresponse households and the datafile was re-edited and re-weighted accordingly. When conducting tests for significant differences between CPS estimates with and without late cases, the survey design effects were considered in the parameters of the generalized variance functions. Table 5 contains labor force participation rates by age, sex, Black, and Hispanic while Table 6 contains unemployment rates.

Table 5 and 6 about here

From the above tables we see the absolute difference between labor force estimates with and without late interviews is small in most cases (less than 1 percent). However, it is important to note that differences in the unemployment rate are reported to one-tenth of one percent, thus seemingly minor differences still have significant impact on policy-making. A consistent pattern emerges where these differences are significant – labor force participation rates tend to be

slightly lower when late cases are excluded while the unemployment rates are slightly higher without them. Again, several factors could be influencing the degree of differences seen in Tables 5 and 6. First, labor force participation and unemployment rates were very stable during 1999. There was very little variation in these rates before and after the cross-section of data examined here, even among the age, race, sex and ethnicity subgroups shown (U.S. Bureau of Labor Statistics, 1999). Had labor force participation and unemployment rates been more volatile at the time, we might have expected a larger number of significant differences. Similarly, these rates reflect national totals – had we examined smaller geographic areas such as states, we might have seen greater discrepancies between estimates with and without late cases. Despite these potentially suppressing factors, we still found about one-third of the estimates to be statistically different without the representation of late interviews.

5. Conclusion and Discussion

Our general research question of interest is whether the extra time and resources currently being devoted to late interviews is worth the added cost. This extra effort is often defended on a generally held assumption that the last few interviews serve as approximations for survey nonrespondents. This assumption purports that late interviews reflect reluctant households and therefore are similar to households that ultimately refuse to participate in surveys. This premise is difficult to explore because we lack information about the characteristics of nonresponding households. Consequently, we approach it somewhat indirectly by seeing to what degree the characteristics of late interviews differ from early interviews and by comparing the characteristics

of late/difficult interviews to characteristics of nonrespondents from previous studies.

We found that across the two surveys analyzed, the late interviews shared some common differences from interviews completed earlier on. At the person-level, the odds of being a late interview increase significantly if the interview is with/about a younger person, someone who reports their race as Black, or is a proxy (in the NCVS). For NCVS, late households also tend to report fewer crimes than earlier interviews; in the CPS, late interviews are less likely to be retired or disabled compared to early interviews. At a household-level, the odds of being a late interview are significantly higher for renter households, larger households, households with medium or high incomes or those missing data for income, and households that were nonrespondents during the previous interviewing cycle (either person-level nonreponse or unit-level nonresponse).

These findings offer some support for the continuum of resistance assumption that late interviews resemble nonrespondents. First, late cases in NCVS contained more proxy interviews which, in this survey, mostly result from within-household person noncontacts (as opposed to refusals). Since noncontacts are a component of nonresponse, this finding supports the theory. Second, the relationship between late interviews in the current month and noninterviews in the previous survey period suggests that some percentage of late interviews (around 8-9 percent) are very similar to nonrespondents (in fact, these cases previously *were* nonrespondents). Third, we found in both surveys that households that refused to provide information about income (or for whom it was missing for other reasons) were much more likely to be late interviews. In their

study of nonrespondents to the 1996 Medical Expenditure Panel Survey, Cohen, Machlin and Branscome (2000) found a very similar trait among nonresponding households. Finally, in a Census match study of nonrespondents to six demographic surveys (which included the NCVS and CPS), Groves and Couper (1998) report a general decline in cooperation as socio-economic status increases. Consequently, our finding that higher income households tend to be late interviews also lends some support to the continuum of resistance theory.

However, other findings appear contrary to the theory. For example, Groves and Couper did not find race to be a significant predictor of cooperation rates once other household and environmental factors are controlled. Neither did they find renters less cooperative in the surveys studied. We measured age at the person level in the logit models and found that late interviews tend to reflect younger persons compared to early interviews. However, other studies of survey refusers (Cohen, Machlin and Branscome 2000; Fitgerald and Fuller 1982) suggest that older persons are more prone to nonresponse. This tends to weaken the suggestion that our youthful NCVS and CPS late responders are also characteristic of nonparticipants. Finally, we did not find that urbanicity was a predictor of late interviews once other household variables are held constant. Since numerous studies have noted higher nonresponse in urban areas compared to suburbs and rural areas, this finding tends to refute the continuum of resistance theory.

Our second research question asks: are survey estimates biased without late interviews? To answer this we converted late cases to unit nonresponse cases, re-ran the crime and unemployment rates, and then compared the new rates to rates produced from all responding

units. Due to several factors, however, our analysis has somewhat limited generalizability. Because our analysis is at a national level, uses cross-sectional data, and happens to reflect statistics that have been relatively stable over recent months, our discovery of significant bias may be understated. Additionally, we did not explore the data quality of late interviews -- it is possible such interviews exhibit a high degree of item nonresponse and response error that compromise their usefulness and contribution toward full sample estimates. But, despite these limitations, our simulation helps illustrate the probable effects of two closely related concepts, 1) reducing resources devoted to obtaining late/difficult interviews, and 2) a decrease in unit response rates of approximately 5 percent (from 94 percent to 89 percent for the NCVS and from 93 percent to 88 percent for the CPS). Our reproduction of estimates without late cases also helps us understand whether the standard nonresponse adjustments, weights, and imputation procedures adequately adjust for the absence of late cases.

For both surveys, we found that the absolute difference between crime and labor force rates calculated with and without late cases is small. Nonetheless, many of the differences are significant with the crime rates mostly biased in a positive direction when late cases are removed. Similarly, unemployment rates also tend to be higher without late interviews. This suggests that despite their small percentage of total interviews (5-6 percent), late interviews are sufficiently different from earlier interviews so as to influence critical estimates. Our results suggest (subject to all limitations) that these data are less precise without the "last 5 percent" and that dropping them would have negative consequences on our goal to provide definitive national statistics. Consequently, we recommend that the extra time and resources required to obtain late interviews

is worth the effort. Late cases may or may not be reflective of nonrespondents, but their input is still critical to producing unbiased rates and minimizing nonsampling error.

We conclude by mentioning additional areas of research related to late/difficult interviews. First, we encourage the exploration of additional call-record variables that were unavailable in our analysis. In addition to date of interview, it would be helpful to use number of contacts, and the distinction between noncontacts, initial nonresponse, and refusal conversion when building a definition of difficult interviews. In the current study, data limitations restricted our definition to include date of interview and in some cases number of contacts.

We also encourage research into the data quality of late interviews. We hope to get a sense of this by further examining proxy rates, allocation rates, and occurrence of "don't knows" and "refused" for late cases. In the current analysis we have not explored trade-offs between the last few percentage points of response and the potential for increased error as a result of poor data quality. We also suggest that our study should be replicated over several months and at lower geographic levels. Finally, we recommend that researchers conduct studies similar to the current one for different surveys, especially surveys with longer field periods and different modes, interview frequency, and subject matter.

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Table 1.
Probability of Being a Late Interview: Person and Household Models for NCVS

	Person Model		Household Model	
Independent Variable	**Estimate**	**Adjusted Odds Ratio**	**Estimate**	**Adjusted Odds Ratio**
Respondent Characteristics				
Proxy Interview	0.85***	2.35		
Mode (telephone)	-0.81***	0.45		
Number of Crimes Reported	-0.11*	0.90		
Age	-0.38***	0.68		
Education (<high school; h.s.; >h.s.)	0.15***	1.16		
Relation to ref. person (Ref. Persons omitted)				
Spouse	-0.15***	0.86		
Child	-0.48***	0.62		
Parent	-0.03	0.97		
Sibling	-0.12	0.89		
Other Relative	-0.44***	0.65		
Non Relative	0.11	1.11		
Sex (male)	-0.05	0.95		
Race (whites omitted)				
Black	0.23***	1.26		
American Ind/Alaska Nav.	-0.12	0.88		
Asian/Pacific Islander	0.34***	1.40		
Other race	0.25*	1.28		
Hispanic Origin	-0.08	0.93		
Household Characteristics				
Tenure (owners omitted)				
Renter			0.35***	1.41
No payment for rent			-0.11	0.89
Household size (1-6+)			0.08***	1.09
Income (low income omitted)				
Medium income			0.13***	1.13
High Income			0.22*	1.25
Income missing			0.68***	1.98
Number of Crimes Reported (total household)			-0.14**	0.87
Previous Inter. Outcome (interviews omitted)				
Noninterview			1.00***	2.72
Replacement Household			1.12***	3.06
Other outcome			1.35***	3.84
Person-level nonresponse (yes)			0.79***	2.21

*p<.10, **p<.01, ***p<.001 level

TABLE 2.

Victimization Rates for Selected Crime Statistics with and without Late Cases NCVS Quarters 1 and 2, 1999
(per 1,000 people)

Type of Crime	All Cases	Excluding Late Cases
Personal	36.8	37.2
Crimes of Violence	35.8	36.2*
Rape/Sexual Assault	1.5	1.5
Assault	30.4	30.7
Personal Theft	1.0	0.9
Property Crime	209.1	212.0***
Burglary	34.5	34.9
Motor Vehicle	10.3	10.2
Theft	164.4	166.9***

Difference significant at the *.10 level; ** at .01; *** at .001.

TABLE 3.

Victimization Rates of Violent Crimes by Sex, Age, Race, Ethnicity, Income, and Residence
NCVS Quarters 1 and 2, 1999
(per 1,000 people)

	All Cases	Excluding Late Cases
Sex		
Male	39.4	38.7
Female	34.3	33.9
Age		
12-15	90.3	90.3
16-19	86.8	88.6***
20-24	24.5	24.8
25-34	40.7	42.2***
35-49	24.2	24.3
50-64	15.4	15.8
65+	3.9	3.7
Race		
White	34.8	35.1
Black	44.3	45.2
Other	30.7	32.1***
Hispanic Origin		
Hispanic	36.2	34.5*
Non-Hispanic	35.4	36.0***
Income		
< $7,500	57.3	56.6
$ 7,500-14,999	50.6	50.3
$15,000-24,999	42.9	43.9*
$25,000-34,999	41.0	41.8
$35,000-49,999	28.9	28.8
$50,000-74,999	35.9	37.1***
$75,000+	25.2	25.0
Residence		
Urban	43.8	43.5
Suburban	34.7	35.5***
Rural	28.9	29.7***

Difference significant at the *.10 level; ** at .01; *** at .001.

Table 4.
Probability of Being a Late Interview: Person and Household Models for CPS

	Person Model		Household Model	
Independent Variable	**Estimate**	**Adjusted Odds Ratio**	**Estimate**	**Adjusted Odds Ratio**
Respondent Characteristics				
Age	-0.15***	0.86		
Education (<high school; h.s.; >h.s.)	-0.01	0.99		
Relation to ref. person (Ref. Persons omitted)				
Spouse	-0.23***	0.85		
Child	-0.21***	0.87		
Parent	-0.11	0.96		
Sibling	0.17	1.27		
Other Relative	0.28*	1.41		
Non Relative	0.17**	1.27		
Sex (male)	-0.01	0.99		
Race (whites omitted)				
Black	0.20**	1.64		
American Ind/Alaska Nav.	-0.00	1.35		
Asian/Pacific Islander	0.10	1.49		
Hispanic Origin	0.24	1.27		
Labor Force Status (employed/at work omitted)				
Employed - currently absent	0.28**	1.06		
Unemployed	0.10	0.90		
Retired	-0.49***	0.50		
Disabled	-0.23***	0.61		
Not working - other reason	0.23***	1.02		
Household Characteristics				
Mode (telephone)			-0.86***	0.42
Tenure (owners omitted)				
Renter			0.28***	1.32
No payment for rent			-0.33	0.72
Household size (1-6+)			0.07***	1.08
Income (low income omitted)				
Medium income			0.21***	1.23
High Income			0.28***	1.33
Income missing			0.66***	1.94
Poverty Area (yes)			-0.18	0.98
Region (Midwest omitted)				
South			0.18*	1.19
West			0.37***	1.45
Northeast			0.25**	1.20
Urbanicity (Urban omitted)				
Suburban			-0.09	0.92
Rural			-0.09	0.91
Previous outcome (interviews omitted)				
Noninterview			1.04***	2.84
Ineligible			0.49***	1.64

*p<.10, **p<.01, ***p<.001 level

TABLE 5.

Labor Force Participation Rates by Sex, Age, Black and Hispanic
Current Population Survey, May 1999

	Both Sexes		Men		Women	
	All cases	Excluding late cases	All cases	Excluding late cases	All cases	Excluding late cases
Total 16+	67.03%	67.00%*	74.76%	74.77%	59.89%	59.79%*
16-24	65.00%	65.05%	68.07%	68.40%*	61.91%	61.68%*
25-54	84.12%	84.06%*	91.73%	91.68%	76.81%	76.74%
55+	31.89%	31.80%	39.69%	39.61%	25.60%	25.51%
Black 16+	65.30%	65.16%	68.90%	68.53%*	62.37%	62.41%
16-24	56.08%	55.78%	55.23%	54.61%*	56.83%	56.83%
25-54	81.07%	80.94%	85.00%	84.54%***	77.82%	77.96%
55+	28.29%	28.39%	32.69%	33.22%	25.21%	25.04%
Hispanic 16+	67.24%	67.13%	79.19%	79.22%	55.54%	55.29%
16-24	60.98%	60.59%	70.50%	70.82%	50.83%	49.69%**
25-54	78.47%	78.51%	91.00%	91.07%	65.96%	65.94%
55+	32.97%	32.93%	42.59%	42.17%	25.25%	25.53%

Difference significant at the *.10, **.01, ***.001 level.

TABLE 6.

Unemployment Rates by Sex, Age, Black and Hispanic
Current Population Survey, May 1999

	Both Sexes		Men		Women	
	All cases	Excluding late cases	All cases	Excluding late cases	All cases	Excluding late cases
Total 16+	4.05%	4.10%**	4.01%	4.08%**	4.09%	4.12%
16-24	9.92%	10.07%*	10.25%	10.42%*	9.56%	9.67%*
25-54	2.99%	3.02%	2.90%	2.94%*	3.08%	3.10%
55+	2.66%	2.70%	2.65%	2.67%	2.67%	2.74%**
Black 16+	7.69%	7.93%***	7.79%	7.99%*	7.61%	7.87%***
16-24	17.55%	18.15%**	19.42%	20.03%*	15.93%	16.59%**
25-54	5.83%	5.97%*	5.66%	5.78%	5.98%	6.15%*
55+	3.80%	4.00%*	3.25%	3.34%	4.30%	4.74%
Hispanic 16+	6.23%	6.28%	5.75%	5.86%	6.90%	6.88%
16-24	10.94%	10.90%	11.07%	11.14%	10.76%	10.53%
25-54	4.94%	5.01%	4.24%	4.33%	5.90%	5.94%
55+	5.36%	5.53%**	4.81%	5.02%	6.09%	6.21%

Difference significant at the *.10, **.01, ***.001 level.

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 2

1

Defining hard-to-survey populations

ROGER TOURANGEAU

1.1 Introduction

This book is about populations that are hard to survey in different ways. It focuses on populations of people rather than establishments or institutions. In an era of falling response rates for surveys (Brick & Williams, 2013; Curtin, Presser, & Singer, 2005; de Leeuw & de Heer, 2002), it may seem that *all* household populations are hard to survey, but some populations present special challenges of various sorts that make them harder to survey than the general population. Some of these hard-to-survey populations are rare; others are hidden; some are difficult to find or contact; still others are unlikely to cooperate with survey requests. This chapter tries to distinguish the major challenges that make populations hard to survey and reviews attempts to quantify how hard to survey different populations are.

One way to classify the various sources of difficulty is by what survey operation they affect. In this chapter, we distinguish populations that are *hard to sample*, those whose members who are *hard to identify*, those that are *hard to find or contact*, those whose members are *hard to persuade* to take part, and those whose members are willing to take part but nonetheless *hard to interview*. These distinctions reflect the main steps in many surveys. First, a sample is selected. Often, the next operation is identifying members of the target population, for example, through screening interviews. Then, the sample members must be found and contacted. Once contact is made, sample members have to be persuaded to do the survey. And, finally, the willing respondents have to have whatever abilities are needed to provide the requested data or special steps have to be taken to accommodate them. As we shall see, with any given population, problems can arise with each of these operations, making the population hard to survey. And, as will become clear, some hard-to-survey populations present combinations of several kinds of trouble.

1.2 Hard-to-sample populations

In the ideal case, there is a complete and up-to-date list of the target population and the sample can be drawn from this list. Unfortunately, this ideal is rarely realized in practice; for most populations of interest in surveys, there is no list frame and sampling begins with some general purpose sampling frame, such as an area, address, or random digit dial

(RDD) frame. Problems arise when the target population represents a small fraction of the frame population. Kalton (2009; see also Chapter 19 of this volume) distinguishes major subgroups or domains (constituting more than 10 percent of the total population), from minor subgroups (1 to 10 percent) and from mini-subgroups (less than 1 percent of the total population). To pick out the members of the target population from the other members of the general population, surveys often begin by administering a short battery of screening questions. In the absence of a special frame or frames, then, one reason that a population can be hard to sample is that its members are rare, representing a small fraction of the larger frame population, often the general population. (Another source of difficulty, to which we return later, is that it may be hard to identify the members of the rare population in a short screener.)

Discussions of the issues involved in sampling rare populations (e.g., Chapter 19; Kalton & Anderson, 1986; Sudman, Sirken, & Cowan, 1988) often point to two other population characteristics, apart from overall prevalence within the general population, that affect the level of difficulty in finding members of the population in a screening survey. The first is the level of variation across areas or sampling strata in the prevalence of the rare subgroup. It is sometimes possible to increase sampling efficiency by oversampling strata where the prevalence of the rare subgroup is relatively high and undersampling areas where the prevalence is relatively low. It is easier to find members of the rare population when a substantial proportion of them is concentrated in a small number of areas or strata that can be identified prior to sampling. For example, a recent experiment in the National Household Education Survey attempted to boost the number of Hispanics in the sample by targeting census tracts in which at least 13 percent of the population was Hispanic (Brick, Montaquila, Han, & Williams, 2012).

The other variable affecting the difficulty of locating members of a rare population is the cost of a screening interview relative to the cost of the main interview. If screening interviews are relatively cheap (for example, only a few questions are needed to identify members of the target population), then having to carry out a lot of them will not affect the final data collection costs so much as when screening is relatively expensive. Consider a situation in which members of the rare population constitute 5 percent of the total population. If we ignore the effects of nonresponse to the main interview, this implies that twenty screeners will have to be done for each main interview. However, if the screening interviews cost only one twentieth of the main interview, then the total costs per case are only doubled by the screening costs (that is, twenty screeners plus one main interview cost twice as much as a main interview alone). But if the screening interviews are expensive – say, half the cost of the main interview – then the need to complete twenty screenings per main interview will drive up the total cost per case by a factor of 11. Screening costs can be high if medical tests or a long series of questions are needed to identify members of the target population or if it is difficult to get people to complete the screener. Some surveys use a two-phase screening process, where the first-phase screener casts a broad net and the second phase screener applies more stringent criteria. Clearly, sampling efficiency matters more when the screening process is expensive.

Kalton (2009) provides a measure (R) of the gains in sampling efficiency that can be achieved with a disproportionate allocation of the initial sample across strata that vary in the prevalence of the rare population:

$$R = \frac{\left[\Sigma W_h \sqrt{P(c-1) + P/P_h}\right]^2}{P(c-1) + 1}, \quad (1.1)$$

in which W_h is the proportion of the rare group in stratum h, P is the overall prevalence of the rare group, P_h is its prevalence within stratum h, and c is the ratio of data collection costs for a member of the rare population to the costs for the nonmembers (that is, for cases who screen out).

One way to measure the difficulty of sampling members of the rare population is by the added cost per case due to the need to conduct screening interviews. With a proportionate allocation of the screening sample, the added cost (Δ_c) per case, expressed as a proportion of the total cost per case, depends on the prevalence (P) of the rare group and the cost ratio parameter (c) described earlier in Equation 1.1:

$$\Delta_c = 1 + c/P.$$

Under an optimal allocation across strata, the added cost would be $R \times \Delta_C$, where R is the efficiency gain factor defined in Equation 1.1. For example, if the efficiency factor was .8 and screening increased the data collection cost per case by a factor of 1.5, the net effect would be an increase of 20 percent (that is, $.8 \times 1.5 = 1.2$). Δ_C and $R \times \Delta_C$ provide measures of the sampling difficulty associated with a rare population. In summary, then, a population is harder to sample as its overall prevalence becomes lower, as its prevalence varies less across the sampling strata, and as the screening costs increase relative to the cost of a main interview. In the best case, most of the rare target population falls within a few strata or a single high prevalence stratum and the screeners are relatively inexpensive.

A related situation involves selecting the sample from two frames – a general purpose frame with low prevalence but high coverage of the rare population, and a special frame with higher prevalence but less complete coverage of the rare population. The latter might be a list of known members of the rare population. The dual frame sample yields the highest gains compared to the general purpose frame alone when the special frame has a much higher prevalence than the general purpose frame and when it includes a large fraction of the rare population (e.g., Lohr & Rao, 2000).

Another type of population that presents particular difficulties for sample designers are mobile or "elusive" populations. These are populations, such as the homeless and similar groups (e.g., migrant workers), that are not easily linked to any one place. Here, the best sampling strategy often involves sampling places where the members of the elusive population are likely to be found rather than sampling the members of the population directly. Kalton (2009; see also Chapter 19 in this volume) describes this approach as "location sampling." Examples include sampling homeless shelters and soup kitchens as a strategy for capturing the homeless (e.g., Ardilly & Le Blanc, 2001) or sampling oases or

waterholes to capture nomadic herdsman. Sampling is likely to continue for some period of time and, precisely because such populations are mobile, members may have multiple chances for selection. Moreover, the frame of locations is likely to be incomplete; thus, elusive populations may well be undercovered even when a location sample is selected. If the main goal of the survey is to estimate the size of the population, capture-recapture methods can be used. These methods, initially developed for estimating the size of nonhuman populations, are now used in estimating the coverage of censuses of human populations (see Mulry, Chapter 3 in this volume). Two samples are taken; in the best case, the samples are completely independent. (With the census, one of the samples is the post-enumeration survey sample; the other is a sample from the census enumerations.) The estimate of the size of the population reflects the proportion of cases found in both samples. A potential problem with this method is "correlation bias" – that is, the violation of the assumption that the capture and recapture probabilities are independent. When members of the rare population systematically vary in their elusiveness (or when they vary in their elusiveness within sampling strata), this variation will produce correlation bias. Imperfections in the sampling frame can also lead to correlation bias. For example, if the frame for a survey of the homeless omits certain sites, then the homeless linked only to those sites are likely be missed in both the initial and recapture survey.

Mobility presents challenges not only for sampling a population, but also for locating the members of the group. We have more to say about these problems in Section 1.4 below.

1.3 Hard-to-identify populations

A screening survey is predicated on the assumption that the respondents are both willing and able to answer the screening questions accurately. Screening data are often provided by household informants, who provide information about themselves and about the other members of the household. In some cases, a neighbor may be used as a last resort when screening is based on age, race, or some other visible characteristic. And, in network samples, screener respondents may be asked not only about their own households but also about the members of linked households (e.g., the households of their siblings; see Sudman *et al.*, 1988, and Chapters 23 and 24 in this volume, for discussions). Regardless of the exact method of screening, the accuracy of the screening data will depend on the screening respondents knowing the relevant characteristics of each person they are asked about and their willingness to report that information. Unfortunately, these conditions may not always be met, creating a second type of hard-to-survey population.

1.3.1 *Stigma, sensitivity, and motivated misreporting*

Consider the difficulties in identifying the members of some cultural or religious minority, such as immigrants (see Massey, Chapter 13 in this volume), men who have sex with men, or Muslims (Keeter, Smith, Kennedy, Turakhia, Schulman, & Brick, 2008). Members of a highly stigmatized population, such as illicit drug users, may keep this characteristic secret

even from other household members. And household informants may be reluctant to identify persons with the relevant characteristics to outsiders.

Even when the characteristic of interest is not sensitive (for example, when the population of interest is a specific age group), screening interviews often miss members (Horrigan, Moore, Pedlow, & Wolter, 1999; Judkins, DiGaetano, Chu, & Shapiro, 1995). Although almost all surveys are prone to *some* undercoverage (see, for example, Shapiro, Diffendal, & Cantor, 1993, on the coverage of the Current Population Survey, or CPS), the undercoverage in screening surveys seems to be worst for the very groups targeted by the survey. One of the best documented instances of such underreporting involves the National Longitudinal Survey of Youth, 1997 Cohort (NLSY-97). The eligible population for this survey was young people, aged 12–23. Horrigan and his colleagues (Horrigan *et al.*, 1999) compared the numbers of persons found in the NLSY-97 screening effort with the expected numbers based on CPS figures for the different age groups. The NSLY screening data show roughly the same numbers as expected for the age groups above 23 and slight undercoverage for those below 12 (roughly 90 percent coverage relative to the CPS). For the age range targeted in the screening effort (12–23 years old), however, the coverage dropped to about 70 percent. Similar problems have been found with several other national surveys (see Judkins *et al.*, 1999); in each case, undercoverage was considerably worse for the survey's target population than for other groups. To avoid the biases produced by this sort of underreporting, surveys sometimes retain some of the households that screen out for further data collection. Of course, this increases data collection costs.

Tourangeau, Kreuter, and Eckman (2012) argue that the underreporting of eligible household members in screeners is an example of *motivated misreporting*, in which respondents, interviewers, or both, shade the answers to minimize the work they have to do (see also Kreuter, McCulloch, Presser, & Tourangeau, 2011). When eligible households screen out, they do not have to complete the main interview, reducing the burden for both the potential respondent and on the interviewer. My co-authors and I carried out an experiment in which we varied how much the screening questions in a telephone survey disguised the target population (Tourangeau *et al.*, 2012). Some households got questions that asked directly about the eligible population ("Is anyone who lives there between the ages of 35 and 55?"); a second group of households got questions about younger and older age groups ("Is everyone who lives there younger than 35? Is everyone who lives there older than 55?"); a final group got a series of questions for each member of the household, including their sex, race, and age. The last method is known as the full roster approach. The full roster clearly beat both the direct questions and the complement questions for finding members of the target population. With the full roster version of the screening questions, 45 percent of the households screened in versus 32 percent with the direct questions and 35 percent with the complement questions. We knew from the frame data that some of the sample households included an eligible household member; the full roster led to the least underreporting within these households.

The downside was that the full roster also produced the lowest overall response rates (24 percent versus 32 percent for the direct question group and 29 percent for the complement question group); these response rates reflect nonresponse to both the screener and the main interview. Both interviewers and nonrespondents seem to contribute to the shortfall in eligible household members. There was a highly significant negative correlation (-.58) across interviewers between their screener response rates and their screener eligibility rates. The interviewers with the highest response rates to the screener also found the lowest proportions of eligible households.

So, there is clear evidence that members of even nonstigmatized groups can be hard to identify in screening interviews. It seems quite likely that the undercoverage of members of stigmatized groups will be even worse. At least one line of evidence provides support for this conjecture. Tourangeau, Kearney, Shapiro, and Ernst (1996) carried out an experiment that varied the procedures used to roster the members of sample households. We found that an anonymous rostering procedure led to better coverage of young Black males, a group often underrepresented in surveys and censuses. This study was done mainly in poor neighborhoods, where coverage is often low. The respondents in our screening sample may have deliberately omitted some household members (especially Black male members) because they were worried about losing welfare benefits or incurring some other penalty if they included them. Such concerns may lead to concealment on the part of respondents; my colleagues and I argued that the anonymous rostering procedure helped allay such concerns and reduced omissions from the rosters. These results suggest that omissions may occur more often the more respondents that are worried about the potential costs of reporting a member of the target population.

1.3.2 Metrics for the hard to identify

There are several ways to quantify the level of difficulty in identifying members of a given population. My discussion of the prior work in this area has already mentioned some of these potential metrics.

The most commonly used measure of the difficulty of identifying members of a specific population is its *coverage rate*. The coverage rate is the estimate of the size of the population from the survey to the estimated size based on some benchmark survey or the census:

$$CR = \frac{\hat{N}_i}{N_{Bi}}, \tag{1.2}$$

in which $\hat{N}_i$ is the estimated size of population group i from the survey (typically, the sum of the weights for the respondents in that group after any nonresponse adjustments) and N_{Bi} is the benchmark for that group (such as the estimate of the subgroup's size from the American Community Survey).

The coverage rate reflects the joint effects of all sources of error (including frame problems, screener nonresponse, and so on), not just misreports in the screening interviews;

in addition, it captures the net impact of all these forms of error. That is, overreports and underreports can cancel out so that a coverage rate near 1.0 may mask a high level of offsetting errors (see Mulry, Chapter 3 in this volume, who describes additional measures used to assess coverage in a census). The screening classifications can sometimes be compared to more accurate measures of the relevant characteristics. This allows the proportion of those who should have screened in but were incorrectly classified as ineligible to be computed (this is the *false negative rate*); similarly, it also allows the proportion of those who screened in but should have been classified as ineligible (the *false positive rate*) to be computed. False negatives are generally more problematic than false positives, since the latter can be removed once they are identified in the main interview.

1.3.3 Other methods for hard-to-identify populations

Snowball sampling, and its more recent outgrowth respondent-driven sampling (RDS; see Chapters 23 and 24 for discussions), are methods intended to reduce the problems of identifying members of rare or stigmatized populations. As Goodman recently pointed out (Goodman, 2011), snowball sampling was originally introduced by Coleman (1958–59) as a method for selecting a sample of the members of a social network, such as groups of friends at a school. Coleman started with a random sample of network members and used this initial sample to identify other members of the network. As Goodman noted, his method yielded a probability sample. Over time, however, snowball sampling has come to mean recruiting a convenience sample of members of some population, typically members of a "hidden" population (such as illicit drug users or illegal immigrants); these initial "seeds" then recruit additional members of the population, who then recruit additional members, and so on. In a series of papers, Heckathorn (1997, 2007, 2011) has explored the statistical properties of RDS and introduced several estimators that can be used with such samples. Under certain assumptions, Heckathorn argues, the estimators are unbiased. For our purposes here, three of the assumptions underlying RDS are crucial (these quotations are all taken from Heckathorn, 2011, p. 363):

(1) "Respondents know one another as members of the target population, as is typical of groups such as drug users or musicians";
(2) "The network of the target population forms a single component"; and
(3) "Respondents can accurately report their personal network size, i.e., the number of those they know who fit the requirements of the study such as drug injectors or jazz musicians."

If these assumptions are met, the members of the hidden population are not hidden to each other, but only to members outside the population. Of course, even if members of the hidden population know each other, this does not mean they are willing to reveal each other to the researchers. (Consider using RDS to recruit a sample of illegal immigrants.) It remains to be seen how often these and the other assumptions on which RDS rests are met in practice and how robust the method and associated estimators are when its assumptions are violated (see Chapter 24).

1.4 Hard-to-reach populations

So, some populations are rare or elusive and, as a result, hard to sample. With other populations, the challenge is picking out the members of the target group from some larger population (such as the general population), particularly when the members of the target group do not want to be identified. But there is still another source of difficulty that can make a population hard to survey – the members may be hard to locate or hard to contact. For example, Kelleher and Quirke (Chapter 10) describe a survey of Irish Travellers, a group that is hard to survey for several reasons, not the least of which is their mobility.

1.4.1 The hard to locate

There are at least four types of mobile populations that may be hard to locate:

- Members of traditionally nomadic cultures (such as the Bedouins of Southwest Asia and the Tuareg of North Africa);
- Itinerant minorities (such as the Romani in Europe or the Travellers in Ireland);
- Persons who are temporarily mobile or displaced (recent immigrants, homeless persons, refugees); and
- Persons at a mobile stage in their life cycle (college students).

Some of these populations are quite large. Passel (2006) estimates that there are 11.1 million "unauthorized migrants" in the United States (although these are probably mostly in households and thus not especially mobile) and estimates of the size of the Romani population in the US range up to a million. Mobility can make the members of some populations hard to locate. As we noted earlier, one strategy for capturing the members of mobile populations is to sample places where they are likely to be found. For example, in the United States, the 2010 Census sent enumerators to migrant worker camps, soup kitchens, and homeless shelters in an effort to count these mobile populations.

Mobility can also be a problem for longitudinal, or panel, surveys. There are a few papers on movers in such surveys (e.g., Couper & Ofstedal, 2009; Lepkowski & Couper, 2002). Couper and Ofstedal examined sample members who moved between rounds of the Panel Study of Income Dynamics (PSID) and the Health and Retirement Survey (HRS). They note that some 13.7 percent of the US population moved in 2004; the corresponding rates in Western Europe were somewhat lower. Both of the surveys that Couper and Ofstedal looked at were quite successful at finding sample members who had moved. The PSID located 96.7 percent of the 1,441 cases that needed to be tracked for the 2003 round and the HRS located 98.7 percent of its 1,294 movers for the 2004 round of that survey. Still, although these tracking efforts were very successful, they also required considerable resources. On average, it took 10.2 tracking calls to find the movers in the PSID and 7.4 tracking calls to find the movers in the HRS. Still, as these results suggest, the vast majority of movers are eventually found.

The correlates of being found, according to Couper and Ofstedal (see also Lepkowski & Couper, 2002) are, not surprisingly, related to the person's level of attachment to a specific place. People who are married, employed, older, and engaged in community activities are more likely to stay put and are easier to find if they do move. Despite a tendency to change their surnames, women seem to be easier to track than men are. In general, populations that are only loosely attached to a specific home or place are difficult to find. Thus, the homeless are notoriously difficult to count and to interview and they are missed by virtually all general population surveys (although see Chapter 9 in this volume). A less extreme case involves persons with weak attachments to several households. They are at risk of being omitted from household rosters and thus missed by surveys; Martin (1999) estimated that some 4 million persons in the United States might have such tenuous connections to a household. And people displaced by storms, other natural disasters, and wars can require extraordinary efforts to find and interview (see Chapters 6, 7, and 8 in this volume).

1.4.2 Barriers to access

Even when sample members can be found, it may still be difficult to contact them. One long-term trend that has probably contributed to the decline in response rates throughout the developed world over the last two decades is the widespread adoption of lifestyles and devices that shield people from unwanted solicitations. More and more Americans live in gated communities, locked apartment buildings, or other residential settings in which they are protected by gatekeepers, and the trends are similar in Western Europe. By the mid-1990s, nearly 40 percent of new residential developments in the US were gated (Blakely & Snyder, 1997). Even before cell telephones became popular, Americans used caller-ID and answering machines to screen out their telephone calls; now, as the population shifts to cell telephones, almost everyone is able to filter his or her calls.

It is not clear whether this shift to cell telephones has made it harder or easier to reach potential respondents. According to Blumberg & Luke (2012), about 25 percent of the adult population in the US was cell-only by mid-2010. Hispanics, young adults (18–34 years old), people living with roommates, poor people, and renters were more likely to be cell-only than the rest of the population. The figure for Hispanics was nearly 35 percent; for 25–29 year olds, it was more than 51 percent; and for adults living with unrelated adults, it was 69 percent. Although cell phones do encourage the screening of incoming calls, they are mobile devices and many cell users have their telephones with them all the time. In general, though, it seems that many of the same groups that are hard to survey for other reasons (such as young adults) are also getting harder to contact; these groups seem to be overrepresented in the cell-only population. At the other end of the spectrum, Groves and Couper (1998) suggest that two groups are relatively easy to contact – the elderly and parents with young children. Members of both of these groups are more likely to be at home than members of other subgroups of the general population. On the other hand, access to elderly in assisted-living settings may be limited by gatekeepers.

1.4.3 Metrics for contactability

Many survey researchers routinely distinguish between various forms of nonresponse, including the failure to locate the sample person, failure to make contact, and failure to persuade the sample person to take part (which we discuss further in the next section). Lepkowski and Couper (2002) present a model in which the overall response propensity for a given sample member is the product of his or her likelihood of being located, being contacted, and agreeing to take part. Thus, a natural metric of a population's difficulty on each of these scores is the complement of their average propensities – that is, the observed or modeled proportion of the population that could not be found, that could not be contacted given that they were found, or that could not be persuaded to take part given that they were contacted. Another statistic commonly used to measure the level of difficulty in contacting sample members is the average number of contact attempts or calls until contact was made. Hard-to-contact populations are those where relatively high proportions are never contacted and those whose members require high numbers of contact attempts to reach.

1.5 Hard-to-persuade populations

Once the sample person is reached, there is still the problem of getting him or her to agree to do the survey. As response rates have fallen in the US and elsewhere, survey researchers have increased their efforts to find sample members and to make contact with them; as a result, the rise in nonresponse rates mostly reflects rising levels of noncooperation (Groves & Couper, 1998, pp. 160–63; Steeh, Kirgis, Cannon, & DeWitt, 2001). In addition, the distinction between noncontact and noncooperation may be breaking down. Screening one's telephone calls or choosing to live in a gated community may be a means of preemptively fending off unwanted requests, including unwanted survey requests.

Two variables are often singled out as potential sources of general resistance to surveys – the sense of busyness that seems to pervade contemporary life and falling levels of civic engagement (Brick & Williams, 2013; Groves & Couper, 1998, ch. 5). Abraham, Maitland, and Bianchi, (2006) pitted these two explanations against each other in a study of nonresponse in the American Time Use Survey (ATUS). Several features of the ATUS make it particularly useful for studying nonresponse. First, the sample consists of respondents to the CPS and so detailed information is available for the ATUS nonrespondents as well as for the respondents. Second, for a survey conducted by a federal agency, the ATUS has a relatively low response rate (in the high 50s). Finally, if the busyness hypothesis were true so that busier people are less likely to respond to the ATUS than less busy people, then this would introduce noticeable biases into ATUS estimates, which concern how people spend their time. Abraham and her colleagues found more support for the civic engagement hypothesis than for the busyness hypothesis; in particular, they found that ATUS sample members with lower levels of community engagement were less likely to be *contacted* for their ATUS

interviews than those with stronger community ties. This was somewhat surprising since community ties are usually seen as linked with willingness to participate.

Several other studies find that various forms of community involvement are related to survey participation. Groves, Singer, and Corning (2000) showed that respondents who reported high levels of civic involvement in one survey were much more willing to complete an unrelated mail survey later on than those who had reported low levels of involvement, at least when neither group was offered a monetary incentive. The difference in response rates to the mail survey was substantial – nearly 30 percentage points (50 percent for the high involvement group versus 21 percent for the low). Tourangeau, Groves, and Redline (2010) found that voters were more likely to complete a survey than nonvoters, and this difference was apparent even when the survey topic was not political. Finally, Abraham, Helms, and Presser (2009) found that nonresponse in the ATUS was affected by sample members' volunteering behaviors. They showed that sample members who reported in the CPS that they had done volunteer work in the past year were much more likely to become ATUS respondents than sample members who did not report any volunteer work. ATUS respondents were almost twice as likely as ATUS nonrespondents to have reported volunteering in the CPS. Activities such as community involvement, voting, and volunteering may reflect a generalized willingness to help others, clearly a characteristic related to willingness to take part in surveys.

These findings contain hints about populations that are likely to exhibit high levels of resistance to surveys in general. Persons who are socially isolated or who are low on altruism may be hard to recruit for surveys or may require special incentives to get them to take part (see Groves *et al.*, 2000). Many surveys are conducted by government agencies or academic researchers so that groups with hostile views toward the government or toward social science in general may be less likely to cooperate in a range of surveys.

This is not to say that the specifics of the survey do not matter. According to Groves *et al.* (2000), people decide whether to participate in a survey based on their evaluations of whatever features of the survey happen to be salient at the time they make their decisions. The topic of the survey, its sponsor, its length, or the incentives it offers may all affect who cooperates with a specific survey request and who refuses, although the effect of topic interest on cooperation rates has turned out to be surprisingly hard to demonstrate (Groves, Presser, & Dipko, 2004; Groves *et al.*, 2006). Still, the leverage-salience theory of Groves *et al.* (2000) indicates that groups that are easy to persuade for one survey may be hard to persuade for another. Indeed, the decision whether to take part may have a large chance component, reflecting whatever features of the survey momentarily draw the sample person's attention.

The natural metric for assessing how hard the members of a given population are to persuade to take part is its refusal rate – that is, the proportion of those who were contacted but who declined to take part. Additional indicators of reluctance are the proportion of population members who required refusal conversion, special incentives, or other extraordinary measures to obtain their cooperation. Ideally, one could compare

populations on their average rates of refusal, need for conversion efforts, and so on across a range of surveys.

1.6 Hard-to-interview populations

There are at least three additional reasons why some populations may be difficult to survey:

- They may be vulnerable populations (such as prisoners or young children), requiring explicit consent from a caretaker, parent, or guardian to interview;
- They may have cognitive or physical impairments that makes them difficult or impossible to interview at least under the standard survey protocols; or
- They may not speak (or read) the language in which the survey questionnaire is written.

In all three cases, it may be difficult to collect the survey data of interest – difficult but not necessarily impossible. Children often take part in surveys (for example, in surveys of students), although parental consent is generally required and data may be gathered both from the sample children directly and from other informants (such as teachers or parents). For example, the Early Childhood Longitudinal Survey – Kindergarten Cohort (ECLS-K) collected cognitive assessment data from kindergartners as well as getting additional information about the children from their parents, teachers, and school principals (West, Denton, & Germino Hausken, 2000). Chapter 15 in this volume presents a more general discussion of surveys of children and young people.

Surveys are generally designed for respondents who are in reasonably good health, who have intellectual abilities in the normal range (or above), and who are not suffering from serious sensory impairments. Thus, people who are very ill, who have extreme intellectual handicaps, or who are deaf (in the case of surveys administered aurally) or blind (in the case of surveys administered visually) are left out of many surveys. This may not be a major problem for surveys of the general population, since the conditions like these that prevent participation are likely to be rare. In the CPS, for example, all of these sources of difficulty (including language barriers) account for less than 8 percent of the nonresponding households (vs. 53 percent for refusal; see US Census Bureau, 2006) and less than 1 percent of all eligible households. Although the CPS is a household survey and can collect information from anyone in the household who is at least 15, our impression is that physical, cognitive, and linguistic obstacles are generally relatively minor contributors to nonresponse in most surveys of the *general* population.

The picture changes, though, for surveys aimed at population subgroups where these problems are common. In surveys of immigrants, for example, linguistic issues are likely to loom larger. For some states within the US, non-English speakers constitute substantial minorities. Reflecting the makeup of California's population, the California Health Interview Survey conducts interviews in five languages – English, Spanish, Chinese, Korean, and Vietnamese (Edwards, Fraser, & King, 2011). Translating the survey questionnaire is only one of the accommodations that a survey may offer to reduce the impact of physical, cognitive, or linguistic barriers to participation. The ECLS-K study designed and

fielded cognitive assessments that did not require the children to read (West *et al.*, 2000). Chapter 16 describes a national survey of people with intellectual disabilities. And some telephone surveys use text telephone (TTY) to accommodate the deaf.

Another tactic for getting around the problems presented by the sample person's limitations is to collect the data from someone else. Parents are often used to provide information about sample children, especially young children, rather than having children provide the data themselves. Similarly, caretakers, spouses, or other proxies may be asked to provide information about frail or severely disabled sample persons. Most researchers regard self-report data as superior to proxy data (for example, Moore, 1988), at least when the sample persons are old enough and healthy enough to respond for themselves. Self-reports are more likely to be based on first-hand experiences than proxy reports and the answers of self-reporters are more likely to be based on recall rather than on estimation or guessing strategies (Tourangeau, Rips, & Rasinski, 2000, pp. 65–67). However, when the topic is the sample person's impairments, self-respondents may minimize their problems relative to proxy reporters (Lee, Mathiowetz, & Tourangeau, 2007). The question naturally arises as to how to determine whether a proxy is needed in a particular instance. One survey, conducted on behalf of the Social Security Administration, used a three-item screener to identify cases with cognitive impairments so severe that a proxy was tapped to provide the data (Skidmore, Barrett, Wright, & Gardner, 2012).

We are not aware of any existing metrics for assessing the level of difficulty of conducting interviews with the members of a given population, but at least two obvious measures suggest themselves. One is the proportion of persons who are unable to provide the survey data (or at least to provide the data without some special accommodation). That is, members of one population or subgroup are harder to interview than members of another population to the extent that a higher percentage of them cannot provide the data at all or can only provide data under special data collection procedures. If proxies are allowed, then a simple measure of difficulty at the interviewing stage is the percentage of cases for which proxies were needed. A second possible metric is the added cost per case associated with hard-to-interview sample members of the population or subgroup, a measure similar to Δ_C described earlier in Section 1.2.

1.7 General metrics for difficulty

So far, this chapter has looked at individual components of survey difficulty, ranging from problems in sampling to problems in collecting the data. This section looks at attempts to create overall measures of difficulty. Both the US Census Bureau and the UK Office of National Statistics have created hard-to-count indices to classify areas for their population censuses. We focus on the US efforts here; Chapter 4 describes the parallel effort in the UK, where a hard-to-count (HtC) index was used to stratify areas for inclusion in the sample for the post-census coverage survey (see also Brown, Diamond, Chambers, Buckner, & Teague, 1999).

Bruce and Robinson (2003) describe the hard-to-count measure created in the US. It encompasses twelve tract-level variables known to be associated with mail return rates in the 2000 Census. The twelve area-level percentages used to calculate the scores were:

(1) Percent of dwelling units that were vacant;
(2) Percent that were not single-family units;
(3) Percent of occupied units that were occupied by renters;
(4) Percent of occupied units with more than 1.5 persons per room;
(5) Percent of households that were not husband/wife families;
(6) Percent of occupied units with no telephone service;
(7) Percent of persons below the poverty line;
(8) Percent of households getting public assistance;
(9) Percent of persons over 16 who were unemployed;
(10) Percent of households where none of the adults (over 14) spoke English well;
(11) Percent of households that moved in the past year; and
(12) Percent of adults without a high school education.

Each census tract received a score ranging from 0 to 11 on each of these indicators, depending on which of twelve categories the tract fell into for each variable, with overall scores ranging from 0 to 132. This hard-to-count index correlated .77 with the tract mail return rate in the 2000 Census. The twelve variables reflect a mix of the sources of difficulty that are thought to contribute to nonreturn of census forms, including complex living arrangements, lack of trust in the government, low socioeconomic status (SES), mobility, and nontraditional addresses (Robinson, Johanson, & Bruce, 2007). The British HtC index, similarly, is based on variables that reflect the area's SES, the percentage of the population who are young or minority group members, and the number of persons per dwelling (see Abbott and Compton, Chapter 4 in this volume).

Bates and Mulry (2011) reanalyzed the data from Census 2000, using a cluster analysis procedure to group the population of census tracts into eight clusters. One variable that differentiated four of the clusters from the remaining four was the percentage of occupied units occupied by homeowners as opposed to renters. The four clusters that included a high proportion of homeowners differed in SES (one cluster was economically advantaged, one was average, and one was economically disadvantaged) and in ethnic makeup (one cluster had especially high levels of non-English speakers, among other characteristics). The clusters with high proportions of renters paralleled those with high percentages of homeowners, with one exception. There was no cluster corresponding to the economically advantaged homeowners; instead, there was a cluster of tracts with high proportions of unmarried renters living in multiunit structures. The cluster with the lowest census return rates in both 2000 and 2010 were the tracts dominated by economically disadvantaged renters, with a 58 percent return rate in Census 2000 and a similar return rate in 2010. In terms of the distinctions presented here, census nonreturn probably results mainly from unwillingness to take part and linguistic barriers to participation. Apart from their practical value, these results on hard-to-count areas in the US Census are useful in highlighting many

of the groups that are traditionally thought to be hard to survey – renters (especially those in large buildings), persons who are low in education or economic resources, movers, and unmarried people.

1.8 Conclusions

Although researchers try to tailor their surveys to the population of interest, surveys as a method are often better suited to some populations than to others. The populations that are hard to survey (or at least relatively hard) are those that are hard to sample, hard to identify, hard to find or contact, hard to get to cooperate, hard to interview, or that offer some combination of these difficulties. Populations are hard to sample when there is no good list of the population members and when the members of the population represent a small fraction of the units on the available general population frames. They are hard to identify when membership in the target group is based on characteristics that are hidden or sensitive or when household informants mistrust the researchers. Populations are hard to find when their members are mobile or when they erect barriers to access. They are hard to persuade when they have low levels of engagement in the community and are unwilling to help the sponsors and researchers out. They are hard to interview when the researchers must first get consent from third parties to carry out data collection, when the sample persons do not have the requisite cognitive and linguistic skills, or when they are not healthy enough. There are sizable literatures on many of the dimensions that contribute to being hard to survey. For example, Groves and Couper (1998) examine a number of variables that affect how hard it is to contact a given household for a survey and how likely they are to cooperate if they are contacted.

Many hard-to-survey groups present more than a single form of difficulty. Imagine trying to conduct a survey of survivalists living in isolated areas around the United States. Certain ethnic minorities, such as the Romani, are rare, hard to identify, hard to locate and contact, and likely to resist taking part in surveys once they are found. In the US, unmarried young men living in apartments with roommates are generally hard to survey and these problems are even worse for young African-American males.

The implications of a group's being hard to survey depend in part on the purposes of the survey. If the survey is attempting to characterize some larger group, then members of hard-to-survey subgroups of this population are likely to be underrepresented in the survey, thus biasing overall estimates. The size of any bias will depend on the size of hard-to-survey subgroup, the level of underrepresentation, and the degree that members of the subgroup differ from members of more easily surveyed groups on the variables of interests. If a survey takes special pains to increase the representation of hard-to-survey subgroups of the target population, these steps may drive up the data collection costs. But the consequences of a group's being hard to survey are likely to be even worse when the goal is to characterize the hard-to-survey group. As we shall see in later chapters, it is in this situation that has driven researchers into devising innovative strategies for sampling, identifying, locating, contacting, and interviewing populations that offer unusual obstacles to conventional survey methods.

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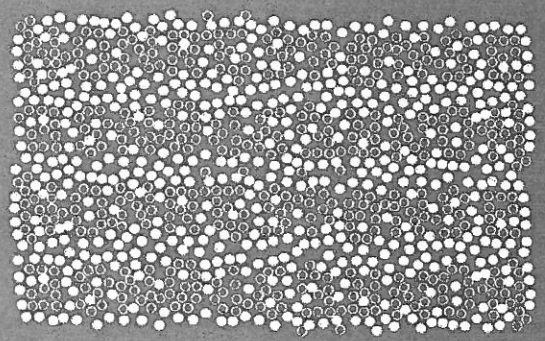
Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 3

Nonresponse in Household Interview Surveys

Robert M. Groves
Mick P. Couper



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CHAPTER FIVE

Influences of Household Characteristics on Survey Cooperation

5.1 INTRODUCTION

This chapter shifts attention to another stage of survey participation. It examines only those sample households the interviewer successfully contacts, and seeks to understand why people do or do not cooperate with the face-to-face interviewer request. The majority of the rest of the book concentrates on this cooperation step. We will see that it is the most complex theoretically, involving the interaction of socio-economic and demographic, social and cognitive psychological, and interactional influences. It is also the component of survey participation that is increasingly problematic in many societies.

As Figure 5.1 reveals, our theoretical perspective asserts that effects on a sample person's behavior arise from multiple levels of aggregation of psychological and sociological phenomena. This chapter, however, focuses on only one of the blocks of hypothesized influences on cooperation—that associated with relatively fixed attributes of the sample household.

We begin with household-level attributes because the survey-methodological literature contains more analysis and commentary on that level of measurement than on any other type. Many times these characteristics of sample units are recorded on the sampling frame or are observable by interviewers. This permits easy comparison of respondents and nonrespondents on these measures. For that reason, the household level will be familiar terrain for many readers.

By starting at the householder block of influences we can also set the stage for later chapters. There we will address how much of the influence of the household level remains, controlling *for* effects of other variables at different levels of aggregation (e.g., the social environment, the interaction between householder and interviewer).

Although the survey literature is replete with socio-demographic correlates of nonresponse, the literature suffers from varying definitions of nonresponse, from reliance on bivariate results, and from an overemphasis on case studies. These all

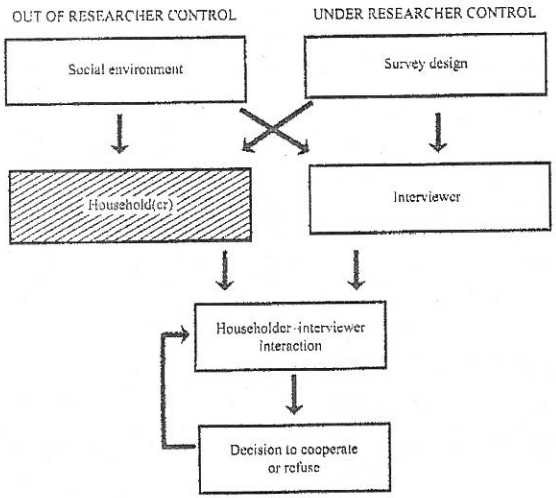


Figure 5.1. A conceptual framework for survey cooperation.

contribute to a lack of conceptual integration of these findings. This chapter uses the theoretical framework outlined in Figure 5.1 as a tool for organizing the findings, both from our own data and from past research on household and householder correlates of cooperation.

We do not hypothesize that many of the socio-demographic variables are direct *causal* influences on cooperation. Rather they are indirect measures of what are essentially social psychological constructs. Furthermore, the mapping of measures into relevant concepts is imperfect. For example, older persons might exhibit *higher* rates of cooperation because of greater perceived civic duty to respond. At the same time, they might exhibit *lower* rates because of increased fear of crime, relative to younger persons. Age thus maps onto two constructs with different hypothesized effects on cooperation. The decision to participate is based on the combined influence of interacting factors (not all considered at the time of the request), some facilitating cooperation, others constraining or mitigating against it.

We view these variables as setting the context within which survey requests are interpreted. Variables like age, race, and socioeconomic status are useful to us in understanding survey cooperation, we believe, to the extent that they are related to certain shared life experiences. The life experiences of import are those shaping the interpretation of a request from a government agency, an educational institution, or a commercial firm to provide private information about persons in a sample household. The shared experiences of socio-demographic groups may produce various predispositions to those requests and reflect features of their current lifestyles that affect how they react to such a request.

We expect the effects of these attributes to be specified by characteristics of the request (e.g., survey topic, agency of the request). For example, we might expect that those who are largely dissatisfied with the service of a firm will be reluctant to respond to a customer satisfaction survey from that firm (see Trice and Layman, 1984), but would have no such reluctance for some other survey. These attributes should not be totally causal of the outcome of the request. Our theory says their effects can be modified by the behavior of the interviewer and by which parts of the survey request are made most salient to the householder.

This chapter introduces the reader to a new set of data to test the theoretical notions discussed. For orientation purposes, a review of Chapter 3, Section 3.4, describing the decennial census match project, is recommended.

To remind the reader, we pooled data across six surveys in analyzing the decennial census match project. Further, at various points in this chapter we examine data from the one-sixth sample (or census long form). Even with the pooled data, this severely restricts the statistical power to detect differences. In addition, we again note that the data are household-level rather than person-level data. Finally, as discussed in greater detail in Chapter 3, the six surveys we examine all have relatively high cooperation rates. This means that we focus on relatively small percentage-point differences.

The chapter is organized about a set of theoretical constructs that help describe the influences on householders' decisions. They range from constructs endemic to rational-choice models of survey participation decisions to those that are much more social psychological in nature. We start by using the notion of opportunity costs that householders must weigh in agreeing to spend their time responding to a survey interview (Section 5.2). We then move to perspectives involving social exchange concepts, which focus on perceived obligations owed to the interviewer, the survey organization, the sponsor of the survey, or the beneficiaries of the survey (Section 5.3). Then we examine a related construct, that those most connected to social institutions in the society would tend to cooperate with surveys, especially those viewed as information collections in support of those institutions (Section 5.4). To complete the theoretical discussion, the chapter proceeds to examine effects of authority (Section 5.6) and other influences (Section 5.7). The chapter ends with multivariate models combining indicators of several concepts in order to measure their marginal impact on cooperation.

5.2 OPPORTUNITY COST HYPOTHESES

A fully "rational" view of the decision making of a prospective survey respondent would have him or her weigh all the costs of participation against the benefits of

participation, with the outcome of this calculus being a decision one way or the other. The costs of the participation would, in this perspective, include the time required to complete the interview, the lost opportunity to perform other activities, the cognitive burden incurred in comprehending and answering the survey questions, and the potential embarrassment of self-revelations that the questions require. The benefits of participation might include the avoidance of alternative, more onerous tasks, the satisfaction of contributing to a socially useful enterprise, the enjoyment of thinking about novel topics, the pleasure of interacting with the interviewer, the gratification that one's opinion was sought by those in authority, and the satisfaction of fulfilling a perceived civic duty, among others.

This perspective is common to a rational choice theory of decision making and to a "central route" protocol for assessing the validity of the interviewer's arguments for participation (Petty and Cacioppo, 1986). It is *not* compatible with the notion that much decision making about survey participation is likely to be based on temporary features of the home situation, peripheral aspects of the interviewer request, and minor components of the survey that become disproportionately salient in specific survey interactions. This latter viewpoint leads to hypotheses that much of the variation in likelihood of participation is explained by what particular features of the request situation become most salient to the householder.

While we do not subscribe fully to this theory of a deliberate and considered view of decision making, it is important to inquire whether it receives any empirical support. This section concentrates on one component of this perspective—the effects of the amount of discretionary time for the sample household on survey cooperation.

All other things being equal, the burden of providing the interview is larger for those who have little discretionary time. Time limitations of the household should affect both contact and cooperation. Those with less discretionary time are less likely to be found at home and, when they are found, less likely to feel free to participate in a survey. Not only are there societal level changes (such as increased labor-force participation of females) over the last few decades that may be contributing to such phenomena (see Chapters 4 and 6), but there is also individual variation in discretionary time available that may be revealed in survey data. In fact, many of the survey households we approach tell us this—witness the relatively large proportion of time constraint reasons provided by householders in initial interactions with interviewers (see Chapter 8).

Large households have increased likelihood of finding someone at home (thereby reducing the noncontact portion of nonresponse). For surveys using a household informant (as opposed to the selection of a random respondent within the household), or permitting proxy reporting, larger households should also present a larger substitution pool, increasing the likelihood that at least one person will have time for the interview. This hypothesis stems from the observation that the tasks required to maintain a household (cooking, cleaning, bill paying, and so on) do not increase proportionally to the size of a household. Larger households share the duties among household members, freeing each for other pursuits.

Is there empirical support for this aspect of a rational choice approach to survey

cooperation? A number of studies (e.g., Kemsley, 1975; Paul and Lawes, 1982; Rauta, 1985; Redpath and Elliot, 1988) report higher response rates among larger households, as we hypothesize. However, many of these report bivariate results (i.e., they do not control for age, presence of children, and other factors), and do not distinguish between noncontact and refusal components of nonresponse. In examining the effects of household size, a distinction should also be made between the number of adults (substitutability) and presence of children (social isolation, discussed in Section 5.4). However, Barnes and Birch (1975), Comstock and Helsing (1973), and Smith (1983) all report a positive relationship between cooperation rates (given contact) and household size, both for surveys with a randomly selected respondent (e.g., Smith, 1983) and for those with a household respondent (e.g., Barnes and Birch, 1975).

One exception to this trend is reported by Foster and Bushnell (1994), who find decreasing cooperation by household size for the British Family Expenditure Survey. However, this survey requires participation by *all* adult members of the household to be considered a responding unit. Thus, this contrary result may be explained by the increased burden associated with larger households, rather than the substitutability of household members.

For three of the surveys in the match study (the Current Population Survey (CPS), the National Health Interview Survey (NHIS), and the National Crime Survey (NCS)), a household respondent was sought for the initial household information, after which person-level data may be sought from individuals within the household. In such cases, households with larger numbers of adults would have greater substitution pools for the initial informant, reducing the potential burden on individual household members. However, Table 5.1 with the pooled data shows no monotonic increase in cooperation as the number of adults gets larger. However, we do find, consistent with the past literature, a larger contrast between single-person households and other household types. Single-person households have lower cooperation rates.

Thus far, we see little support for the hypothesis of increasing cooperation as the number of adults increases. Perhaps the indicator of the total number of adults is too weak an indicator of the discretionary time hypothesis. If we use, instead, measures of the time commitments of the adults, we may get closer to the concept of opportunity costs for the survey interview.

One measure we have on households from the census long form is the number of

Table 5.1. Cooperation rate by number of adults in household

Number of adults	Cooperation rate	(Standard error)
One	94.3%	(0.42)
Two	95.5%	(0.25)
Three	94.5%	(0.68)
Four or more	96.0%	(0.76)

$\chi^2 = 7.65$, $df = 3$, $p = 0.05$

Table 5.2. Cooperation rate by presence of nonworking adults in household

Presence of nonworking adults	Cooperation rate	(Standard error)
One or more nonworking adults	95.0%	(0.73)
No nonworking adults	95.0%	(0.64)
$\chi^2 = 0.005$, $df = 1$, $p = 0.94$		

adults who do not have a job outside of the home. Households where no one works would, presumably, have higher time availability for the survey interviews. Based on census match data from the United Kingdom, Redpath and Elliot (1988) and Kemsley (1975, 1976) report increasing response rates with more employed adults in the household (not controlling for household size). However, the matched sample data once again show little support for this (see Table 5.2), with both households with and without a working adult achieving about a 95% cooperation rate.

Finally, we have available other indicators of the amount of discretionary time for the household—the number of minutes of commute time and number of hours at work. These variables are available only on the sample cases that received the long form of the 1990 census. These are even more direct indicators of the amount of time at home and thus the amount of discretionary time to give to surveys. We created three different measures of the amount of time away from home, presented in Table 5.3. For each of these measures, we expect that the mean time away from home (measured in hours per week) would be higher for noncontacted than contacted households, and higher for households producing refusals than those leading to interviews. While we find significant effects in the expected direction for contacts, we find no differences for cooperation. Kennickell (1997) found similar effects of average commute time on contact and cooperation in the Survey of Consumer Finances.

Table 5.3. Means of three discretionary time indicators by contact versus noncontact and interview versus refusal (given contact) (standard error in parentheses)

			Cooperation, given contact	
Hours away per week	Contact	Noncontact	Interview	Refusal
Mean hours away from home for householders who work	42.6 (0.56)	45.2 (1.27)	42.7 (0.58)	42.2 (2.17)
One-tailed t-test	$p = 0.03$		$p = 0.42$	
Least hours away among those who work	38.5 (0.63)	42.2 (1.64)	38.5 (0.66)	39.2 (2.22)
One-tailed t-test	$p = 0.02$		$p = 0.38$	
Least hours away for those who work outside the home	39.7 (0.63)	43.0 (1.57)	39.6 (0.65)	42.1 (1.97)
One-tailed t-test	$p = 0.03$		$p = 0.12$	

After viewing empirical results on several indicators of discretionary time, we find little support for the hypothesis that reduced time at home leads to reluctance to cooperate with surveys. Further, when we combined these indicators in a multivariate model predicting cooperation, no further insights were gained. What better indicators could we suggest for tests of the hypothesis? We have examined no measures of the household's obligations of time away from employment tasks. Some persons have work obligations, even at their home site; some have commitments to friends and relatives that might raise the opportunity costs of a survey interview; and others, primarily the self-employed, may lose income producing opportunities by participating in interviews. Lindström (1983), Rauta (1985) and Redpath (1986) all report lower response rates among the self-employed. We have thus omitted in our tests of the discretionary time hypothesis a set of measures of what alternative activities the household might pursue, in the absence of providing a survey interview.

We suspect that the impact of this measurement weakness is an attenuation of the effects of the measures. When we compare households who are home often to those who are absent more often (but who were contacted by an interviewer), we have combined those with many other attractive time-use options at home with those having few other attractive time-use options at home.

Going beyond the data, we speculate that empirical support would be stronger if we introduced measures of interviewer behavior as well as householder behavior. Limited householder time availability should increase refusal rates, if interviewers do not effectively communicate their willingness to conduct the interview at any time the respondent might be available. Interviewers are trained to offer such flexibility to sample persons, in order to reduce nonresponse. One interpretation of these results is that most interviewers successfully do this. Indeed, examining tape recorded telephone interactions, it appears that interviewers are more adept at reacting to householder pleas of time pressures than other sources of reluctance (see Chapter 8). (Note this implies that for surveys with limited callbacks, those with reduced discretionary time are disproportionately nonrespondent.)

5.3 EXCHANGE HYPOTHESES

In the next two sections we are going to entertain two extensions of the rational decision-making approach, each adding sociological or social psychological content to the perspective. The first is heavily dependent on social exchange theories; the second, on social isolation theories (treated in Section 5.4). Social exchange considers the perceived value of equity of long-term associations between persons, or between a person and societal institutions (Blau, 1964).

Social exchange hypotheses have been popular in the discussion of survey participation in the literature (see Dillman, 1978; Goyder, 1987). Social exchange may operate at any number of levels, ranging from exchange in dyadic interactions (e.g., reciprocation in social interaction; see Chapter 2) to exchange relationships between an individual and the larger society (e.g., civic duty in return for personal

welfare). Dillman (1978) focused on social exchange within a relatively closed system (survey organization and householder), with relatively small gestures on the part of the survey organization (personalized letters, token incentives, reminder letters) hypothesized to evoke a reciprocating response from the householder. On the other hand, Goyder's (1987) discussion of exchange evokes a wide array of obligations and expectations over an extended period of time between an individual and various institutions of society.

Central to all conceptualizations of social exchange is the notion that, unlike economic exchange, all social "commodities" (ranging from measurable entities, such as time and information, to less tangible socio-emotional goods, such as approval) are part of an intuitive bookkeeping system in which debts (obligations) and credits (demands, expectations) are documented. Thus, virtually any relationship can be described in exchange terms.

Whether the social exchange perspective applies to face-to-face survey requests may depend on whether the householders connect the request to some "relationship" they have or will have with another person or institution. For single-contact surveys, conducted by organizations with no prior connection to the household, the exchange perspective may have little value. For surveys conducted by organizations with an ongoing relationship with the households, however, the exchange notions may be useful.

When given a request for a government survey, householders may consider the cumulative effect of multiple government contacts. This might include the full weight of past relationships with the agency or organization making the survey request (or the broader class of institutions it represents, e.g., government, academia, commercial interests). Those receiving fewer services from the government may feel less need to reciprocate. Since government services are differentially provided across economic groups, indicators of socioeconomic status should broadly reflect exchange influences on survey participation.

Unfortunately, social exchange theory can lead to two different hypotheses about differences in cooperation by different socioeconomic status (SES) groups. First, one can argue that lower SES groups may have the greatest indebtedness to the government for the public assistance they (or others in their community) may receive. Surveys funded by the government might seem like another encounter with an institution to whom they are indebted. In contrast, those at the high end of the SES scale may have the least need for government services, and least sense that they owe the government any sort of repayment. In fact, they may resent government intrusion into their lives, and feel that the balance of exchange lies in their favor. This suggests a monotonically negative relationship between socioeconomic status and cooperation propensity.

Alternatively, those in the lower SES groups may believe that in relationships with those more fortunate they are routinely unjustly disadvantaged. Survey interviewers, to the extent they are viewed as agents of those more fortunate, may evoke memories of that exchange history, and the householder may tend to refuse the survey request. Those in the highest SES groups might perceive similar long-run feelings of inequity, that large-scale social institutions repeatedly target them for contributions of time and money, despite having over the years contributed little to their achievement of the SES status enjoyed. Both groups feel relatively deprived in the relationships and tend to refuse survey data collectors. This suggests a curvilinear relationship between SES and cooperation.

The two hypotheses are distinguished by what relationship is cognitively most accessible and judged relevant by the low SES householder faced with a survey request. Is this a request from a person better off than themselves, again wanting something from them, but offering relatively little in return? Is this a request from the government?

Of the two alternative hypotheses, the more popular in the existing literature may be the latter, positing curvilinear effects. However, the only evidence in the literature supporting a curvilinear hypothesis for income finds effects in the opposite direction to that expected based on social exchange theories. Smith (1983) found that refusers were more likely than respondents to be in the middle-income category, and less likely to be in the low or high categories. However, these data are based on interviewer estimation of gross income categories of nonrespondents, on which there is 31% missing data among refusers. There is support for higher cooperation among low SES groups. DeMaio (1980), in a government survey, found significant differences in refusal rates by income, with middle-income households being most likely to refuse, while low-income households were least likely to refuse. Weaver, Holmes, and Glenn (1975) also found highest refusal rates among the middle-income group in a telephone survey of city employees in San Antonio, and the lowest rates of refusal among the lowest income group. However, there are also contradictory findings. Benus and Ackerman (1971) found the lowest nonresponse among the highest income group. A number of studies report refusal rates decreasing with other indicators of socio-economic status, such as social class (Lindström, 1983; Redpath and Elliot, 1988; Redpath, 1986) and property value (Goyder, 1987; Goyder, Lock, and McNair, 1992). Analyzing data from the 1972 Swedish Household Income Survey, Lindström (1983, p. 44) found that "there is a tendency toward higher income and less social assistance benefits among the respondents than among the non-respondents." Foster and Bushnell (1994) find significant bivariate effects of socioeconomic status (class) on cooperation for three of the five surveys they matched to the 1991 U.K. census, but these effects largely disappear in multivariate analyses. Ekholm and Laaksonen (1990) also report no effect of income on response to the Finnish Household Budget Survey in multivariate analyses.

The most appealing indicator of SES would probably be a combination of education, occupation, and personal and family income. All of these variables are measured only for the one-sixth sample sent the long form of the decennial census. Further, family income and occupation suffer severe item missing data problems. For example, income on the decennial form is missing for over 30% of the households. Hence, we are forced to deal with imperfect measures of SES, available on the short form of the census.

Economic Indicators. One gross measure of SES status available on all cases is the nature of housing costs faced by the household. For renters, this is an estimate of the

Table 5.4. Coefficients from a logistic model predicting cooperation versus refusal rate by housing tenure and costs

Predictors	Coefficient	(Standard error)
Constant	3.38**	(0.13)
Owner occupied (1 = yes)	-0.24	(0.56)
Monthly rent for renters[a]	-0.070**	(0.025)
House value for owners[b]	-0.017**	(0.0049)

Notes: Dependent variable coded 1 = interview, 0 = refusal. Coefficients for dummy variables for individual surveys omitted from table.
[a]Measured in units of $100.
[b]Measured in units of $10,000.
**$p < 0.01$.

mean monthly rent, while for home owners it is a self-estimate of the house value. We thus use three variables in a logistic regression to measure the combined effects of housing costs on cooperation. Table 5.4 shows lower cooperation rates among those renting or owning more expensive housing units. (No changes in conclusions occur if the model includes dummy variables for the different surveys.) Using various transformations of these variables, we fail to find support for the curvilinear effect of SES with these data. We thus find a general decline in cooperation with increasing SES, in support of the first hypothesis forwarded above. One argument for the higher cooperation found among lower SES households (as measured by housing costs) may be the government sponsorship of the surveys in our sample, and the greater perceived reliance of this group on government largesse, leading to greater felt social debts.

Education. Another traditional SES indicator is education, available only on the subsample of cases completing the long form of the census. To the extent that surveys are viewed as research/information gathering, those with higher educational achievement might have benefited from such efforts, might appreciate the utility of such efforts, and thus tend to cooperate. (Note that this is a hypothesis in a different direction from that made above for higher economic groups.) This hypothesis should apply both to government-sponsored surveys and those conducted by academic organizations, and even to some surveys conducted for commercial purposes.

The past literature tends to show that lower education groups disproportionately fail to participate in surveys (Benson, Booman, and Clark, 1951; Dohrenwend and Dohrenwend, 1968; Foster and Bushnell, 1994; Kemsley, 1975, 1976; O'Neil, 1979; Wilcox, 1977). Using a measure of education of reference person (from the census long form), our data tend to show somewhat higher cooperation among lower-education groups, particularly those with less than high school education (see Table 5.5) (statistically significant only at $p = 0.10$). This is consistent with the finding on SES as measured by housing costs. Given that the surveys we include in the match are conducted or sponsored by the federal government, this again may suggest support for the notion of indebtedness to government among those of lower SES.

Table 5.5. Cooperation rate by education of reference person

Education	Cooperation rate	(Standard error)
Less than high school	97.0%	(0.78)
High school	94.2%	(1.03)
Some college	95.1%	(0.87)
Completed college	93.9%	(1.21)
$\chi^2 = 6.2$, $df = 3$, $p = 0.10$		

Government Transfer Payments. A more direct measure of exchange "debts" owed to the government may be whether the household currently receives any public assistance from a governmental agency. From an exchange perspective, we would expect that those households currently receiving benefits would be more willing than others to comply with a survey request from a government agency. We are again restricted to a question from the census long form for this analysis, and find no support for this hypothesis in the data (see Table 5.6).

We end this section with a test of the combined effects of those socioeconomic measures that have some marginal bivariate effect on cooperation. For the exchange hypotheses, this is housing costs and education. Given that the latter is measured only on the long form, whereas the former comes from the short form, we do this in three stages. First we model the effect of the housing cost variables only, using full data (decennial short form data). This is the first model in Table 5.7. We see that housing costs have a significant negative relationship with cooperation, as shown earlier. This relationship generally holds when restricted to long-form cases only (second model in Table 5.7). The third model in Table 5.7 shows the combined effect of housing costs and education on cooperation. The effect of housing costs does not change much with the inclusion of the education measure, while the latter set of indicators does not reach statistical significance ($p = 0.10$).

What can we conclude from these various tests of social exchange concepts? We have comments both on the theory and our indicators of its constructs. First, with regard to the theory, we initially note that it implied two different empirical relationships with SES. This alone portended low falsifiability of the theory in this case. (If low SES households had high or low relative cooperation, the theory provides an explanation.) Blau (1964, p. 93) distinguishes social exchange from economic exchange by stressing that the former entails "diffuse" and "unspecified" obligations (see also Goyder, 1987, p. 170). But therein may lie a key drawback in applying so-

Table 5.6. Cooperation rate by receipt of public assistance

Receipt of public assistance	Cooperation rate	(Standard error)
One or more household members receive assistance	95.6%	(0.90)
No household members receive assistance	94.7%	(0.58)
$\chi^2 = 0.65$, $df = 1$, $p = 0.42$		

Table 5.7. Coefficients for logistic models predicting cooperation versus refusal using social exchange indicators, simple model using short form variables, expanded model using short and long form variables, by sample type (standard error in parentheses)

Predictor	Simple model		Expanded model
	Total sample	Long-form cases	(long-form cases)
Constant	3.38**	3.56**	3.66**
	(0.14)	(0.33)	(0.46)
Owner occupied	-0.24	-0.28	-0.25
	(0.16)	(0.39)	(0.42)
Monthly rent for renters[a]	-0.070**	-0.031	-0.056
	(0.025)	(0.059)	(0.069)
House value for owners[b]	-0.017**	-0.036**	-0.035**
	(0.0050)	(0.011)	(0.013)
Education			
Less than high school			0.29
			(0.36)
High school			-0.43
			(0.30)
Some college			-0.089
			(0.27)
Completed college			--

Notes: Dependent variable coded 1 = interview, 0 = refusal. Coefficients for dummy variables for individual surveys omitted from table.
[a]Measured in units of $100.
[b]Measured in units of $10,000.
**$p < 0.01$.

cial exchange theory to survey participation: it may be *too* diffuse or unspecified to lead to testable hypotheses of specific behavior. Almost every behavior can be interpreted *post hoc* in an exchange context, but to be able to predict behavior would require knowledge of the accumulation of exchanges over an undefined period of time, and using exchange criteria (valuation of acts) that may differ from one individual to another. Social exchange theory thus too infrequently leads to testable hypotheses that can be refuted.

Second, with regard to the indicators, we find support in our data for the notion that those in high SES households cooperate less with surveys than those in the low SES groups. The indicators available are housing costs and education. SES, as a concept, can at most be an indirect indicator of perceived exchange relationships. Direct indicators would include perceptions by the householder of previous or future obligations to the interviewer, the agency collecting the data, or those potentially aided by the survey results.

The appeal of the social component to cost–benefit analyses remains. However, a narrowing of the scope of the candidates exchanges may be more appropriate to survey requests. We believe the more narrow concept of reciprocation is useful, especially for understanding the effects of survey design decisions such as the use of incentives or questionnaire length (see Chapter 10) and in the context of the interaction between interviewer and respondent (see Chapters 8 and 9).

5.4 SOCIAL ISOLATION HYPOTHESES

Theories of social connectedness, isolation, or disengagement are related to those of social exchange. Social exchange influences arise from ongoing relations between two actors, or more broadly between an actor and a social group. The influence arises because of the ubiquitous norm that benefits to such relationships must be roughly equally shared over long periods of time. On the other hand, social isolates are out of touch with the mainstream culture of a society. They tend not to feel the influence of norms of that dominant culture, but behave in accordance with subcultural norms or in explicit rejection of those of the dominant culture.

The approaches are related conceptually. For example, a long history of inequitable social exchange relationships between a subgroup and the larger society may lead to the development of a subculture that explicitly fails to include the norms of the larger culture. If a person feels cheated by larger society because of their membership in a group, he or she might tend to ignore the norms of the larger society. This logic has been applied to findings of lower response rates among racial and ethnic subgroups. Similarly, the absence of ongoing relationships between one group and the larger society will lead to the absence of shared norms. This logic has been applied to findings of lower response rates among the elderly (Glenn, 1969; Mercer and Butler, 1967).

Those with strong feelings of social isolation will not be guided by norms of the dominant culture. Survey researchers sometimes have noted that feelings of "civic duty" prompt survey participation, especially when the agency collecting the data represents an important social institution (e.g., government or academia). Those who are alienated or isolated from the broader society/polity would be less likely to cooperate with survey requests that represent such interests. To the extent that large-scale national surveys are a tool of the central institutions of society, those more at the periphery of society would feel less obligation to participate.

Another rationale behind this set of hypotheses is the view that surveys are inherently "social" events because they contribute to knowledge about the full society. To the extent that sample persons feel cohesion with the defined population, cooperation will be enhanced. This is one explanation for the relatively higher rates of cooperation in organization membership surveys. Cooperation in household surveys may be seen both as an obligation of membership in a society or group, and indeed as an affirmation of one's membership.

There are both structural and social psychological aspects of alienation or social isolation. Some groups (e.g., the "underclass"), by virtue of their position in society, may not be bound to the larger society to the same extent as others. This may be reflected both in "input alienation" (e.g., powerlessness, lack of political efficacy) and "output alienation" (lack of trust in government or in the responsiveness of govern-

ment institutions) (see Southwell, 1985; Weatherford, 1991). Both would lead, we assert, to lower levels of cooperation with surveys representing those agents of government. This view equates survey participation with other acts of political or social participation such as voting (see Couper, Singer, and Kulka, 1997; Mathiowetz, DeMaio, and Martin, 1991). The attachment of alienated groups (often defined in terms of race and/or socioeconomic status in the United States and by class in other countries) to society is believed to be weak, and such groups are posited to be less likely to participate in a variety of social and political activities, including responding to surveys.

In the previous section, we examined housing costs and education as indicators of social-exchange influences. They could also be used to provide insight into the social isolation hypothesis. Social isolation theory suggests the opposite effects of SES than do social exchange theories. The lower SES groups should be likely to be alienated from the central institutions of society, and resentful of their dependence on the government. The higher SES group may perceive themselves to hold an important place in society, and may as a consequence have a greater sense of civic obligation or recognize the value of survey data for the common good. This suggests a positive relationship between SES and cooperation propensity. The reader will recall that a negative relationship was found, and thus the data refute isolation theory applicability to survey cooperation, at least as indicated by SES.

5.4.1 Demographic Indicators of Social Isolation

As with many social psychological theories of survey cooperation, our tests of concepts of social isolation will depend on proxy indicators that are socio-demographic characteristics of sample households.

Race and ethnicity. Social isolation has been a popular hypothesis with regard to the behavior of racial and ethnic subgroups in surveys. However, there is little evidence in the literature that nonWhites cooperate with survey requests at different rates than Whites. O'Neil (1979) found lower rates of resistance among Blacks to a telephone survey in Chicago, while Hawkins (1975) reports similar results for a face-to-face survey in Detroit (see also Brehm, 1993). Although Weaver, Holmes, and Glenn (1975) found more problems of accessibility among Blacks; they obtained refusal rates of 4% for Blacks, 10% for Mexican-Americans and 15% for White Anglos. Both DeMaio (1980) and Smith (1983) fail to find effects of race on cooperation rates. As we see in Table 5.8, we find, if anything, higher rates of cooperation among these minority groups. We will examine the influence of race/ethnicity later, in a multivariate model.

Table 5.8. Cooperation rate by race/ethnicity of reference person

Race/ethnicity	Cooperation rate	(Standard error)
Hispanic	96.9%	(0.59)
Black nonHispanic	95.8%	(0.46)
Other	94.8%	(0.68)

$\chi^2 = 11.72$, $df = 2$, $p < 0.01$

Age. Social isolation has also been used to explain the behavior of elderly persons. The disengagement hypothesis, first articulated by Cummings and Henry (1961; see also Glenn, 1969; Krause, 1993; Mercer and Butler, 1967) has been used to explain lower cooperation rates among the elderly. For example, in the political context, both Abramson (1983) and Jennings and Markus (1988) find declines in political involvement and efficacy among the elderly. A mitigating factor, however, may be that the current cohort of elderly in the United States have a greater sense of civic duty, and are more likely to perceive government as making legitimate demands on its citizenry. Thus, lower levels of political participation among the elderly may be attributable to such factors as encroaching infirmities and the likelihood that older cohorts have less education and lower SES, rather than higher levels of disengagement (see Bennett and Bennett, 1986; Rosenstone and Hansen, 1993).

As we've seen in Chapter 4, elderly persons tend to be at home more frequently than other age groups, because of their lower employment rates and, at advanced ages, reduced mobility. However, the group also is disproportionately classified as "other noninterviews" because of health problems preventing their survey participation. These two nonresponse categories thus counteract one another. It is important, therefore, whenever possible in citing past research findings to distinguish between overall nonresponse rates, contact rates, cooperation rates, and other noninterview rates.

First, let us examine what the literature finds regarding overall response rates by age. We would expect this literature to show mixed age differences, as a function of noncontact rates. That is, in surveys with few callbacks, relatively more of the contacted cases would be elderly, generating higher response rates for elderly than for nonelderly, other things being equal. A mixed picture is exactly what the literature shows.

The most striking demonstration of the age effect is from British data starting with Kemsley, for the 1971 Family Expenditure Survey (FES) (Kemsley, 1975) and 1971 National Food Survey (NFS) (Kemsley, 1976) (see Figure 5.2). Similar declines in overall response rates by age are found by Norris (1987), Rauta (1985), Redpath (1986), and Redpath and Elliot (1988), all on U.K. government surveys.

In a census match study much like ours, Foster and Bushnell (1994) find much higher nonresponse rates for older heads of households on NFS and FES in 1991, but not for the Labor Force Survey, General Household Survey, and National Travel Survey. In fact, the lowest response rate for the LFS is among those 16–24. This may reflect a survey design effect. Both NFS and FES are fairly burdensome surveys involving both interviews and diaries (2-week expenditure diary and 1-week food consumption diary).

Christianson (1991) found response rates to be highest among those 65–79 and lowest among those 15–24 in Swedish TV audience surveys conducted by telephone. However, nonresponse to these surveys appears to be dominated by noncontacts (5/6ths of all nonresponse). Similarly, in summarizing the results from a num-

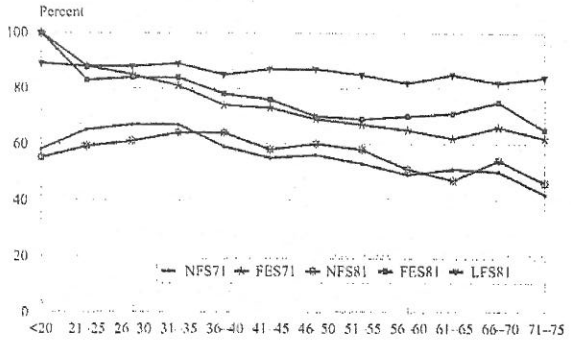


Figure 5.2. Response rates by age of household head for five British surveys

ber of face-to-face surveys in Japan, Sugiyama (1991) found that response rates tend to increase with age. In a study of nonresponse to the Swedish Labor Force Survey, Kristiansson (1980) found no tendency for the elderly to have lower response rates. Paul and Lawes (1982) demonstrate the importance of controlling for household size when examining the impact of age on nonresponse. They find that for each household size group considered separately, individuals 65 or older exhibited the highest response rates, while this age group had the lowest response rates overall. This results from the fact that the majority of persons 65 or older live in households of size 1 or 2, where the response rate was the lowest (Paul and Lawes, 1982, p. 62).

What does the literature suggest if we focus exclusively on refusal rates? Refusal rate studies produce fewer mixed findings on age differences. There are a set of studies showing higher refusals among older persons (Brown and Bishop, 1982; Dohrenwend and Dohrenwend, 1968; Goyder, 1987; Hawkins, 1975; Herzog and Rodgers, 1988; Smith, 1983; and Weaver, Holmes, and Glenn, 1975). Comstock and Helsing (1973) found that refusals increased with age in bivariate analyses; however, when controlling for other variables in multivariate analyses, a reversal of this trend was found, with refusal rates of 16% for those 25–39, 11% for those 40–54 and 10% for those over 55. Benson, Booman, and Clark (1951) also found no effect of age on refusals in a face-to-face survey in Minneapolis.

In short, there appears to be more consistent support for higher rates of refusals among the elderly than for overall lower response rates. The literature also suggests that we might expect different results in multivariate models (especially controlling on household size) than in bivariate models.

Given that we have household-level data from the match, we can examine the effect of age in different ways. One is to look at age of reference person, as was done in the U.K. match studies. The cooperation and contact rates by age of reference person are presented in Figure 5.3. Tests of these effects show a significant relationship ($\chi^2 = 54.37$, $df = 7$, $p < 0.01$) between age and contact, with the likelihood of contact increasing with increasing age, with the most pronounced effect being for those households where the reference person (head of household) is under 25. The relationship with cooperation is not statistically significant, however ($\chi^2 = 12.0$, $df = 7$, $p = 0.10$), nor can any clear trend be discerned in the figure.

Another operationalization of the age variable would distinguish "young" households and "old" households from other types of households. In a survey seeking cooperation from any household members (as do most of those included in the match), the effect of age is likely to manifest itself through the person contacted in the household, rather than the reference person. Table 5.9 shows that "young" households have cooperation rates higher than other households (in which at least one person is between the ages of 30 and 69). However, social isolation hypotheses would have led us to expect lower cooperation rates for the old households versus the other two groups. Thus, with neither of these measures do we find support for the contention that elderly persons (or households) have lower cooperation rates.

A number of hypotheses can be advanced for why we don't find the expected effect of age. One is that older persons in the United States have a higher level of civic duty that mitigates the potential effects of age for government surveys. Another is that the increased legitimacy of government surveys (relative to those conducted by other organizations) may decrease the fear of criminal victimization disproportionately experienced by the elderly (see Chapter 6 and Section 5.5).

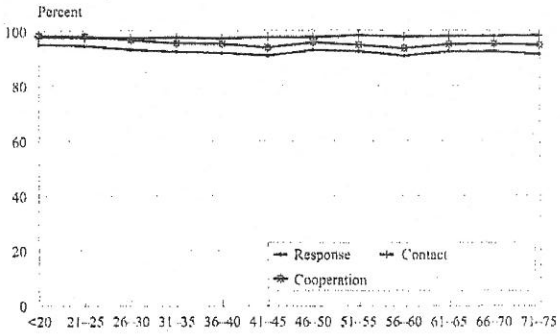


Figure 5.3. Cooperation and contact rates by age of reference person, decennial census match data.

Table 5.9. Cooperation rate by age composition of household

Household age composition	Cooperation rate	(Standard error)
All household members under 30	97.5%	(0.31)
Some household members between 30 and 69	94.6%	(0.25)
All household members 70 or older	95.1%	(0.66)

$\chi^2 = 45.12$, $df = 2$, $p < 0.01$

In an attempt to explore this issue further, we have examined a number of interaction effects. For example, is fear of crime a factor; are older persons in inner-city areas more likely to refuse than their rural counterparts? Are elderly cooperation rates related to the composition of the interviewer labor pool (e.g., older female interviewers may be less threatening to elderly respondents)? Does topic salience play a role; that is, do older persons have relatively higher refusal rates on topics not directly affecting them (e.g., labor force participation) than those of more direct interest (e.g., health and retirement issues)? We tested these and other interactions, but failed to find any effects that provide plausible explanations for our findings on age. The finding also appears robust to other manipulations of the age variables. However, we will return to the age question again when examining multivariate models of household-level effects on cooperation (Section 5.6).

Gender. Another demographic variable commonly examined in nonresponse studies is the gender of the sample householder. Most all studies have found either no gender effect on cooperation or the tendency for males to have lower cooperation rates (Smith, 1983; Lindström, 1983). Some of the findings confound noncontact with refusals, and there are clear tendencies for males to be at home less frequently than females, who more often accept larger responsibilities for child care and other household duties.

There are several possible theoretical explanations for lower cooperation among males. Many of them are related to social isolation and role differentiation of males and females on that dimension. Females are more frequently telephone answerers in mixed sex households (Groves, 1990); they more often take on the role of maintaining social relations with friends and neighbors. Hence, when a request for information comes from outside the home, they are more accustomed to interacting with nonhousehold members from the home setting. As we mentioned above, we do not have measures at the person level in these data, so we cannot estimate the marginal effect of householder gender on cooperation. However, we can and did examine cooperation rates in all-female versus all-male households. Such a test is a purer effect of gender rather than gender-based roles. There was no evidence for different cooperation rates in these two types of households.

We suspect that gender-based roles lead to lower cooperation rates in surveys of randomly selected adult respondents when males are the sampled persons. We thus speculate that respondent rules disproportionately targeting males lead to lower response rates versus rules that permit any adult to respond.

Table 5.10. Coefficients of a logistic model predicting cooperation versus refusal using socio-demographic indicators of social isolation

Predictor	Coefficient	(Standard error)
Constant	2.72**	(0.057)
Race/ethnicity		
Black reference person	0.26	(0.14)
Hispanic reference person	0.52*	(0.23)
Other	—	
Age composition		
All < 30 yrs.	0.89**	(0.15)
Mixed ages	—	
All > 69 yrs.	0.088	(0.161)

Notes: Dependent variable coded 1 = interview, 0 = refusal. Coefficients for dummy variables for individual surveys omitted from table.
*$p < 0.05$.
**$p < 0.01$.

Combined effects of demographic variables. Table 5.10 presents a logistic regression model measuring the joint effects of race, Hispanic status, and household age composition. The table shows that Hispanics and young households (with everyone under 30 years of age) tend to exhibit higher cooperation rates. Elderly households are similar to households with mixed age groups; Black households resemble those of other races (dominantly White). To summarize the results of the demographic indicators of social cohesion, we find generally the opposite of those hypothesized. If minority ethnic or racial groups and the elderly are relatively socially isolated from those institutions connected with the survey request, there is no evidence that this alone affects their rates of survey cooperation. (If dummy variables reflecting the different surveys are included in the model, the coefficient for Hispanic households is reduced sufficiently to lose statistical significance at the traditional 0.05 level.)

5.4.2 Household Status Indicators of Social Isolation

The social isolation hypothesis also has both macro and micro features to it: at a macro level, the connections involve the polity/society as whole; at a micro level, "community" ties might generate civic duty that has impacts on requests affecting the locality. Thus, groups considered isolated at a macro level (e.g., Southeast Asian immigrants, Blacks) may be part of highly cohesive local communities, in which a local survey focusing on community needs may receive a very different reaction than a national survey on topics deemed important by the central institutions of society.

The disengagement hypothesis, on the other hand, refers to an individual's withdrawal from society at both macro and micro levels. Some indirect indicators of social isolation might be derived from various properties of households. These might include whether the household is a single-person household (those living alone

tending to be less socially integrated), whether there are children in the household (those with children having higher social integration through schools and friends networks), whether the household had moved recently (those more transient having fewer community roots), and whether the household lives in a large multiunit structure (greater transiency, less contact with neighbors). Glorioux (1993, p. 171) notes that "having children seems above all to have a strong positive effect on defining activities in terms of duty." He continues, "the more people are alone, the less they define their activities in terms of 'obligation,' 'social alliance,' 'duty' and 'instrumentality.' The family, on the other hand, seems to be the most fundamental social tie in our society." He thus links household structure (single-family home and presence of children) not only to social isolation, but also to social obligation. Atchley (1969) found that refusers to a study of retired women tended to be loners, that is, people with few contacts with friends, and those who prefer to do things alone.

Single-Person Households. Findings from the series of survey census match studies in Great Britain all point to lower overall response rates for single-person households (e.g., Kemsley, 1975, 1976; Norris, 1987). However, Barnes and Birch (1975) found that single-person households have the highest noncontact rates, but their refusal rates are not much different from 2–3 person households. Foster and Bushnell (1994) found higher refusal rates to the National Food Survey for single-person households, but lower refusal rates to the Family Expenditure Survey. The latter survey requires interviews with all adult members of the household, thus substantially increasing the burden of participation for large households. They fail to find significant effects of single-person households on cooperation rates in multivariate analyses of the remaining three surveys they examined. Paul and Lawes (1982) find higher rates of nonresponse among single-person households (but as reported above, note that these households disproportionately consist of older persons). Smith (1983) reports higher refusal rates among single-person households, as do Brown and Bishop (1982), Ekholm and Laaksonen (1990), and Wilcox (1977).

In our data (see Table 5.11), we also find lower cooperation rates for single-person households than for multiperson households.

Presence of Children. Without exception, every study that has examined response or cooperation finds positive effects of the presence of children in the household (e.g., Cartwright, 1959; Kemsley, 1975; Norris, 1987; Rauta, 1985; Redpath, 1986; Lievesley, 1988; Ekholm and Laaksonen, 1991; Lindström and Dean, 1986; Foster and Bushnell, 1994). While Kemsley found that the presence of children has a positive effect on response, the *number* of children has no effect. Our data support these findings, whether for young children under 5 (Table 5.12) or for all children under 18 years of age (not shown).

Table 5.11. Cooperation rate by household size

Household size	Cooperation rate	(Standard error)
Single-person household	93.7%	(0.52)
Other	95.4%	(0.22)
$\chi^2 = 10.28$, $df = 1$, $p < 0.01$		

Table 5.12. Cooperation rate by presence of children in household

Presence of children under 5	Cooperation rate	(Standard error)
Yes	97.6%	(0.29)
No	94.6%	(0.24)
$\chi^2 = 51.44$, $df = 1$, $p < 0.01$		

Household Mobility. Contrary to expectation, the findings on residential mobility suggest that higher refusal rates are found among nonmovers than among movers. Barnes and Birch (1975) found that this difference persists when comparing mobility in the past year, or the past 5 years, or whether examining mobility of the head of household or all members of the household. Redpath (1986) found that those who had moved in last 12 months were more likely to respond to the Family Expenditure Survey. Comstock and Helsing (1973) also found more refusals among nonmovers. However, while Foster and Bushnell (1994) found higher noncontact rates among movers for 4 of the 5 surveys they examined, they found no apparent effect of mobility on cooperation, given contact.

From the decennial census long-form data at our disposal, we have a measure of household mobility. As with the other studies cited above, our data run counter to the social isolation hypothesis. We find significantly higher rates of cooperation among households that have moved in the last 5 years (Table 5.13). However, this does not include appropriate controls for correlates of residential mobility such as age, household size, and socioeconomic status.

Previous residential mobility appears not to be a threat to cooperation in cross-sectional surveys (or for the first wave of panel studies). This could be because more mobile persons tend to have young children that facilitate integration into their new communities, that they are of lower socioeconomic status (see Section 5.3), or some combination of these factors. We need multivariate analysis to unravel these effects.

Type of Housing Structure. Another indicator of social isolation at the community level is the type of structure the household occupies. It could be argued that those who

Table 5.13. Cooperation rate by household mobility in last 5 years

Household mobility	Cooperation rate	(Standard error)
All household members moved	96.4%	(0.60)
Some or none moved	94.4%	(0.64)
$\chi^2 = 5.39$, $df = 1$, $p = 0.02$		

live in single-family homes have more contact with neighbors and are more integrated into their community than those who live in large multiunit structures. A common theme in the literature on urban architecture (see, e.g., Aiello and Baum, 1979; Baum and Valins, 1977) is the impersonality of such large structures relative to single-family dwellings that are the stereotypical neighborhoods of middle America.

Gower (1979) reports higher nonresponse rates (both noncontacts and refusals) for apartment dwellers (in buildings at least 5 stories high and with 30 or more units) relative to other urban dwellers for the Canadian Labor Force Survey. Goyder, Lock, and McNair (1992) similarly find apartment dwelling in Canada to be negatively associated with response. Fitzgerald and Fuller (1982, p. 9) found that apartment dwellers are hard to contact, and also yielded high refusal after contact, but Hawkins (1975, p. 479) found a small positive effect of apartment dwelling on response.

We also find significantly higher rates of nonresponse for residents of large multiunit structures (10 or more units). However, as shown in Table 5.14, this is largely due to differences in contact rates rather than cooperation. We find that residents of multiunit structures have lower contact rates than others. This supports interviewer reports that gaining access to such structures and finding their residents at home are the biggest problems. Once contacted, however, such persons appear no less likely to cooperate with the survey request than other households.

Combined Effects of Variables. The indicators reviewed above are correlated. For example, a common occurrence with the birth of a new child is a residential move, to quarters of a size suitable for the larger household. We expect that the tendency for mobile households to cooperate may reflect the fact that mobile households tend to have young children in them. Theoretically, we believe that the presence of the children induces the social connections leading to cooperativeness.

Table 5.15 presents coefficients from models estimated on the short- and long-form sample cases, examining several of the social isolation correlates. Using the short-form cases, only the presence of young children has positive effects strong enough to be reliably detected with samples of this size. Controlling on that variable, the observed effect of single-person households is diminished to trivial levels. Similarly, there are no stable effects of living in a large multiunit structure.

Table 5.14. Contact and cooperation rates by housing type

	Contact rate	(Standard error)	Cooperation rate	(Standard error)
Single-family homes	98.1%	(0.12)	95.1%	(0.24)
Multiunit (10+) structures	93.7%	(0.71)	94.0%	(0.70)
Other	96.6%	(0.33)	95.1%	(0.54)
	$\chi^2 = 51.96$, $df = 2$, $p < 0.01$		$\chi^2 = 2.77$, $df = 2$, $p = 0.25$	

Table 5.15. Coefficients of logistic model predicting cooperation versus refusal by household status indicators of social isolation, short-form and long-form predictors

	Reduced model short-form cases		Expanded model long-form cases	
Predictor	Coefficient	(Standard error)	Coefficient	(Standard error)
Constant	2.51**	(0.16)	2.83**	(0.15)
Large multiunit structure (1 = yes)	−0.17	(0.15)	−0.32	(0.30)
Single-person household (1=yes)	−0.17	(0.12)	−0.28	(0.23)
Presence of children < 5 (1 = yes)	0.86**	(0.16)	0.80**	(0.27)
Residential change within last 5 years (1 = yes)			0.46*	(0.20)

Notes: Dependent variable coded 1 = interview, 0 = refusal. Coefficients for dummy variables for individual surveys omitted from table.
*$p < 0.05$.
**$p < 0.01$.

Using the long-form cases, we gain access to the residential mobility indicator. The expanded model shows that even the reduced sample size can detect positive effects on cooperation from the presence of children and the experience of residential mobility in the last 5 years.

Table 5.15 shows that the initial finding that households with young children are disproportionately cooperative survives initial multivariate controls. However, the puzzle about the underlying causes of more transient households providing higher cooperation rates remains. We will return to that finding at the end of the chapter with a larger multivariate model, containing controls on socioeconomic status. We are especially interested in observing whether this finding is really masking effects of housing tenure and other socioeconomic indicators. That is, do movers have higher cooperation rates because movers tend to be poorer?

5.5 THE CONCEPT OF AUTHORITY AND SURVEY COOPERATION

A concept related to social isolation is that of "authority," the influence of legitimized power of a person or institution over the behavior of others. Cialdini (1984) and others have noted that when persons or institutions with authority over the lives of the requestees seek assistance, decisions might be made with less attention to the costs and benefits of the task. In a sense, this is a subset of the influences over those who feel connected to a larger society and applies to their behavior with regard to specific instruments of power in the society.

We believe that influences of authority are important in understanding compliance to government survey requests but probably less so to requests from academic

or commercial survey organizations. Many large scale national surveys in the United States are conducted for or by government agencies or using government funds. Such surveys can be viewed as representing the interests of the central power structures in society. To the extent that the authority or power of such institutions over the members of a society are weakened, cooperation with survey requests may be negatively impacted. This could be viewed at a societal level, as part of the "social climate" for surveys (see Chapter 6).

Here we are concerned with individual or household-level indicators of authority. An authority hypothesis may explain why we did not find the expected effect of SES on cooperation (see Section 5.3). It could be argued that the government exercises greater control over those at the lower end of the SES scale, thus producing higher levels of compliance among this group to survey requests from government institutions.

Using the decennial census (long form) data, we examine two indicators of authority—whether a household member was in the military (as a behavioral commitment to government authority), and whether all members of the household were citizens (with those who are not citizens more likely to be sensitive to government authority than others). Households with members in the military do indeed exhibit higher cooperation rates (98% to 95%), but there are no direct effects of citizenship on cooperation. When the two variables are combined, however, reflecting the fact that they are highly correlated, more information about the nature of the effect emerges (see Table 5.16). The lowest cooperation occurs among households with nonmilitary citizens (as the authority hypothesis would suggest), while those with some noncitizens or those with military members have the highest cooperation.

Another variable we have available from the census long form is the language spoken by members of the household. This could be used as a proxy for immigrant status, the expectation being that those with greater dependence on government or under greater threat of sanction may be more likely to cooperate with requests from government institutions or their agents. In Table 5.17 we see that cooperation rates *are* higher in those households where some or all members do not speak English.

This finding is consistent with that reported for race/ethnicity in Table 5.8, in which Hispanics have relatively high cooperation rates. Non-English speaking

Table 5.16. Cooperation rate by citizenship and active military duty of household members

Citizenship/active duty	Cooperation rate	(Standard error)
Some or all noncitizens	97.4%	(1.53)
All citizens, no one on active duty	94.8%	(0.51)
All citizens, someone on active duty	97.9%	(1.65)
$\chi^2 = 4.57$, $df = 2$, $p = 0.10$		

Table 5.17. Cooperation rate by language spoken in household

Language spoken	Cooperation rate	(Standard error)
All household members speak English	94.8%	(0.54)
Some or none speak English	97.0%	(0.92)
$\chi^2 = 4.05$, $df = 1$, $p = 0.04$		

households in the United States are likely to be dominated by Spanish-speakers. An alternative explanation could be the reciprocation inducing effects of extra efforts made to include Spanish-speaking residents in the surveys (e.g., Spanish versions of questionnaires, Spanish-speaking interviewers, etc.). We do not have enough sample cases to separate out the effect for this group relative to other immigrant or non-English speaking groups in the United States.

Noncitizens are more likely to use some other language than English at home. Hence, the two indicators of the authority concept are correlated. A multivariate model might yield a different interpretation than the bivariate tables above. When the indicators of citizenship, military service, and language are combined into a single logistic regression model, the effects of language are diminished. In short, these indicators of the authority hypothesis do not yield much insight into the process of cooperation with the surveys covered in the decennial census match data.

5.6 JOINT EFFECTS OF INDICATORS OF SOCIAL ISOLATION AND AUTHORITY

We could posit interaction effects for the impact of social cohesion on cooperation by survey type. For example, the impact of social integration should be greater for studies impacting the local community (e.g., drugs and crime) than on studies focussing on broader societal issues or of more personal concern (unemployment, cost of living, etc.). Unfortunately, we have insufficient cases in each survey to explore this further, and these remain untested hypotheses.

Finally, we combine the indicators of social isolation and authority in a single multivariate analysis to examine the relative contributions of the different variables. As we did previously, Table 5.18 presents (1) the short-form variables only, modeled on the full sample, (2) the short-form variables modeled on long-form cases only, and (3) short- and long-form variables together. When we combine these indicators into a combined multivariate model, people living in multiunit structures ($p = 0.08$ for the third model), multiperson households, households with children, younger or older households, as well as Blacks, Hispanics, and people experiencing some move in the last 5 years are more likely to be cooperative. We retain these variables for further analysis.

Table 5.18. Coefficients of logistic models predicting cooperation versus refusal by social isolation and authority indicators

Predictor	Simple model: Total sample	Simple model: Long-form cases	Expanded model (long-form cases)
Constant	2.82** (0.076)	2.89** (0.19)	3.37** (0.72)
Social-isolation indicators			
Race/ethnicity			
Black	0.22 (0.13)	-0.0050 (0.34)	0.0084 (0.34)
Hispanic	0.50* (0.20)	1.17** (0.40)	0.94 (0.48)
Other	...	...	...
Household age			
All household members <30	0.86** (0.14)	0.99** (0.28)	0.87** (0.30)
Mixed ages	...	...	...
All household members >69	0.30 (0.16)	0.036 (0.36)	0.12 (0.36)
Large multiunit structure (1 = yes)	-0.22 (0.13)	-0.44 (0.35)	-0.60 (0.34)
Single-person household (1 = yes)	-0.24* (0.11)	-0.30 (0.25)	-0.31 (0.25)
Children < 5 years in household (1 = yes)	0.24* (0.10)	0.17 (0.25)	0.11 (0.25)
Residential change in last 5 years (1=yes)			0.44* (0.21)
Authority indicators			
Citizenship/military duty			
Some or all noncitizens			...
All citizens, no active duty			-0.32 (0.62)
All citizens, some on active duty			0.45 (1.03)
All household members speak English (1 = yes)			-0.29 (0.34)

Notes: Dependent variable coded 1 = interview, 0 = refusal. Coefficients for dummy variables for individual surveys omitted from table.
*$p < 0.05$.
**$p < 0.01$.

5.7 OTHER HOUSEHOLD-LEVEL INFLUENCES ON COOPERATION

Fear of Crime. Suspicions that some persons are reluctant to cooperate with surveys because they fear criminal victimization is a specific form of a script error—the misunderstanding of the survey request as one that might in fact be an attempt to gain entry and physical access to the householder for purposes of theft or assault (see Chapter 8). We would expect that fear of crime might produce reluctance of householders to respond to an unexpected knock on the door, but also to be a stronger influence on behavior when the person attempting contact with the household appears threatening in any way. We note that while fear of physical safety may be a concern in household surveys, the fear of being victimized through telemarketing scams (often targeted at the elderly) may be no less a concern in telephone surveys. The common use of female interviewers in surveys might dampen the effect of fear of crime in face-to-face surveys.

We have very imperfect indicators from the short form to test this hypothesis, namely women living alone, and elderly (those over 69) living alone (see Table 5.19). However, there is little evidence from our data that women living alone (who might have greater fears of victimization) or elderly respondents (who do exhibit more fear of crime; see for example, Miethe and Lee, 1984; Rucker, 1990) have lower cooperation rates.

Topic Saliency. A common metaphor for survey interviews is a "conversation with a purpose" (see Kahn and Cannell, 1957; Schaeffer, 1990). There is much speculation that when the purposes of the conversation are goals shared by the sampled persons, they tend to cooperate. This speculation is based on hypotheses that surveys on salient topics may offer some chance of personal gain to the respondents because their group might be advantaged by the survey information, and also that the chance to exhibit one's knowledge on the topic would be gratifying. When the topic of the interview is used as an important attribute by the interview in persuading the householder, then prior knowledge about the topic and personal relevance to the householder can affect response propensity.

Couper (1997) finds that those who express little interest in politics are more likely to decline participation in electoral behavior surveys. The decennial census data offer few indicators to test hypotheses concerning topic saliency. We would ex-

Table 5.19. Cooperation rate by indicators of fear of crime

Fear of crime indicators	Cooperation rate	(Standard error)
Women living alone	94.4%	(0.44)
Other	95.3%	(0.24)
$\chi^2 = 2.80$, $df = 1$, $p = 0.09$		
Person over 69 living alone	94.2%	(0.97)
Other	95.2%	(0.22)
$\chi^2 = 0.83$, $df = 1$, $p = 0.36$		

pect saliency to be enhanced especially in those cases where the survey purpose is to construct benefit programs affecting sample person (e.g., the CEQ for Social Security beneficiaries, the CPS for Unemployment Insurance beneficiaries).

5.8 MULTIVARIATE MODELS OF COOPERATION INVOLVING HOUSEHOLD-LEVEL PREDICTORS

Many of the hypotheses reviewed above are not independent of one another. For example, those socially isolated in the society are less likely to have the normative guidance of social exchange relationships with survey takers. Hence, deeper understanding of the nature of the process of survey participation might be gained by combined multivariate analysis of the different indicators. In combining these household-level variables we also need to control for survey design and social-environmental variables when possible, as we will do in later chapters.

5.8.1 Combining the Household-Level Predictors

The first step is the examination of a model that combines the indicators of the household-level hypotheses that found some support in the data. Of the social exchange indicators, the housing cost variables and housing tenure variables seemed most powerful. For the social isolation indicators, race, ethnicity, age composition of the household, whether the household is a single-adult household, whether there were children in the household, and whether the household had moved in the last 5 years appeared useful.

As we noted earlier, the concepts of social exchange and social isolation are related to one another. Two socially isolated groups cannot develop ongoing exchange relationships. Further, some of the indicators for the various concepts themselves are correlated. For example, minority racial and ethnic groups tend to live in housing of lower cost than majority groups. For that reason, some of the effects we appear to be measuring in the models examining each hypothesis may be spurious effects of other concepts. A larger multivariate model can be specified to check on that possibility.

The first column of Table 5.20 shows a multivariate logistic regression based on the full sample, predicting the likelihood of an interview relative to a refusal, among sample households contacted for the six surveys. The model controls for base response rate differences among the six different surveys (to control on omitted design differences among the surveys).

The socioeconomic indicators that we use as proxy measures of social exchange influences (tenure and housing costs) retain their impact. The finding is that those incurring lower housing costs tend to cooperate with the survey requests. This is consistent with the notion that requests coming from a government source, with implications for government policy and social services, tend to be viewed as potentially beneficial to those in lower socioeconomic groups.

In the presence of these socioeconomic measures, the race/ethnicity indicators

Table 5.20. Coefficients from logistic models predicting cooperation versus refusal; model using social exchange, social isolation, and authority indicators; model adding environmental indicators (standard errors in parentheses)

Predictor	Household level: Short-form household	Household level: Expanded (long-form) household	Environment + household
Constant	2.78** (0.22)	3.03** (0.52)	2.72** (0.29)
Environment			
Urbanicity			
Central city			-0.27 (0.17)
Balance of CMSA			-0.14 (0.13)
Other			—
Population density[a]			-0.022* (0.011)
Crime rate[b]			0.0053 (0.019)
Percent under 20 years old			0.0096 (0.0047)
Household			
Social exchange			
Owner occupied	-0.10 (0.18)	-0.30 (0.43)	-0.20 (0.18)
Monthly rent	-0.062* (0.029)	-0.031 (0.068)	-0.044 (0.033)
House value[a]	-0.016** (0.0052)	-0.040** (0.013)	-0.011 (0.0060)
Social isolation			
Race/ethnicity:			
Black reference person	0.24 (0.15)	0.092 (0.29)	
Hispanic reference person	0.39 (0.22)	0.92* (0.38)	
Other			
Household age			
All < 30 years	0.70** (0.15)	0.52 (0.35)	0.67** (0.14)
Mixed ages	—	—	—
All > 69 years	0.40* (0.19)	-0.16 (0.45)	0.42* (0.19)
Single person household	-0.37** (0.12)	-0.70* (0.32)	-0.36** (0.13)

(continued)

Table 5.20. Continued

Predictor	Household level: Short-form household	Household level: Expanded (long-form) household	Household level: Environment + household
Household			
Social isolation			
Children <5 in household	0.65**	−0.040	0.63**
	(0.15)	(0.28)	(0.15)
Residential change in last 5 years (1 = yes)		0.16	
		(0.26)	

Notes: Dependent variable coded 1 = interview, 0 = refusal. Coefficients for dummy variables for individual surveys omitted from table.

[a]Measured in thousands of persons per square mile.

[b]Measured in crimes per 1,000 persons.

[c]Measured in $100 units.

[d]Measured in $10,000 units.

*$p < 0.05$.

**$p < 0.01$.

have negligible influence on cooperation. In models addressing only the social isolation hypotheses, these showed higher response rates among Hispanics. We interpret this as evidence of the more pervasive effects of socioeconomic status than any subcultural influences associated with racial and ethnic groups. In other analyses (not presented) we find that the estimated effects of race/ethnicity are also sensitive to what environmental predictors are included in the model. Given these empirical results and the mixed findings about race/ethnicity in the past literature on nonresponse, we excluded these measures from the final model.

The curvilinear effects of age remain in the first model in Table 5.20. Young and old households tend to exhibit higher cooperation rates than do households with persons exclusively in the 30–69 year old range. We'd expect that employment rates might differ across these three types of households. A careful reader will note that we have lost controls on the employment status of householders. We had included such variables under the hypothesis of opportunity costs, with the result that there were no differences by employment status. We suspect that the higher rates among young households reflects an interest in social participation because of greater social engagement in general. The higher rates among the elderly may reflect stronger norms of civic duty among the current cohort of elderly.

There were two other indicators of social connectedness we discussed earlier. Those who live alone tend not to cooperate with the survey request, other things being equal. Conversely, those households with young children tend to participate. These two groups, one assuming all the responsibilities of household maintenance with a single individual, the other sharing it among several persons, are sharp contrasts. While one can easily avoid contact with large numbers of strangers in their

day-to-day life, the other interacts with the community in an ongoing manner. This includes health care and education workers, as well as neighbors or others involved in child care. The adults in these households are well acquainted with requests for their time. With full multivariate controls, younger person households and those with children show higher cooperation.

The second column of Table 5.20 presents model coefficients for a long-form model, on a reduced sample size. This allows us to add to the model an indicator of residential mobility, the variable indicating whether the household moved in the last 5 years. We found earlier, in a model testing effects of social isolation indicators, that movers tended to cooperate at higher rates, in the presence of controls for other social isolation indicators. Absent from those models, however, were indicators of the housing tenure of the household. Those living in rental housing tend to move more frequently than those in owned housing. Hence, we expected that the measured effect might mask the effects of housing tenure. As shown in Table 5.20, when we control on housing costs, the effect of residential mobility diminishes to a negligible level.

5.8.2 Combining Household Predictors with Predictors from Other Levels

The third column of Table 5.20 examines whether the entire set of household-level predictors continues to exhibit hypothesized effects in the presence of controls at the social environmental level (the influences to be discussed in Chapter 6). A comparison of the first column and the third column of the table shows that only the housing cost variables appear to be substantially affected by controls on social environmental variables. When we control on urbanicity, crime rates, and population density, the negative effects of the housing cost indicators decline to just below traditional levels of statistical significance ($p = 0.08$, for the three predictors combined; the coefficient for house value among owners is closest to the chosen level of statistical significance). We interpret this as reflecting the relatively higher cost of housing in urban areas. Thus, the overlapping influences of urbanicity and housing value weakens the marginal impact of the latter coefficient. It appears that the effects of higher living costs we observed in Section 5.3 were in part reflecting the negative influences of urbanicity and urban living conditions on cooperation.

One way to quantify how the addition of environmental predictors affects the estimates of household-level effects is to compare the change in the two likelihood ratio statistics associated with the household-level variables, one without the environmental level controls, and one with the environmental level controls. (We use as the base model one with a constant and the survey dummy variable predictors.) The change due to household predictors is 66.80 without the household controls, and 17.04 with the controls. In short, about (66.80 − 44.00)/66.80 or 34% of the measure of fit of the model associated with the household variables seems to overlap with the environmental predictors.

The last controls we added to the model concern the interviewer (discussed in Chapter 7). These are not shown in Table 5.20. Here we were testing whether the household-level predictors maintain their effects in the presence of characteristics

of interviewers found to influence cooperation. The household-level effects remain substantially the same, and the same conclusions would be made as from the model in the third column of Table 5.20.

5.9 SUMMARY

Earlier chapters in this book have noted that our understanding of survey participation has been heavily shaped by studies examining bivariate relationships between some variable and participation. These have often been case studies, not studies of sets of surveys. They have tended to study demographic indicators, not underlying social psychological concepts. They have emphasized overall response rates without separately studying noncontacts and refusals.

This chapter no doubt has results surprising to some survey professionals. When it is combined with the results of Chapter 4, on the process of contact, a different picture on survey participation is formed for the six surveys in the decennial match study than we assumed prior to examining the data.

Most survey researchers might assume lower cooperation rates among lower socioeconomic groups, among racial/ethnic minority groups, and among the elderly. However, once contacted, poorer groups (as indicated by the proxy measures of housing costs) appear no different from other groups. This result becomes clear only when controls on social environmental influences are applied to the equation. That is, the result that those in more expensive housing do not respond is partially explained by the fact that more of those persons live in urban areas, the sites of low cooperation in general. There is some tendency for owners (versus renters) in expensive housing to not cooperate, but the effect is a weak one, deserving of further attempts to replicate.

A resolution of this chapter's findings regarding age is more complex. We find a curvilinear effect of age (using a proxy indicator of age grouping at the household level). It shows young and old households cooperating at higher rates than the middle-age households. In Chapter 4 we found that the elderly were easier to contact than others. These two findings combine to suggest the largest overrepresentation of the elderly will occur in surveys with low contact rates and no efforts to convert initial refusals (which should convert disproportionately younger persons).

The young and old households may have higher cooperation rates for different reasons. Younger persons may tend to exhibit more curiosity about efforts to seek information from them. They most recently experienced standardized information-seeking associated with school and jobs. The elderly cohort, in contrast, may not have the curiosity but may maintain norms of civic duty regarding requests from the government not shared by younger cohorts. The households in the middle-age category or with mixed ages, on the other hand, as a group do not share as much of the history of standardized measurement nor the civic norms of the elderly. Clearly, we have no direct tests of these notions with these data.

Of further relevance to age effects, our multivariate models control on household size. Most of the past findings of lower response rates among the elderly, for example, did not control on the fact that many elderly live alone. (Controlling on household size, we found older householders more willing to respond.) We have consistently found that those who live alone are a distinctively difficult group to interview in surveys. Chapter 4 shows that, other things being equal, they tend to be difficult to contact. In this chapter, we've learned that once contacted, they tend to refuse, relative to others. This is strong evidence, we believe, of the underlying tendency for those who live alone either to have an outward orientation, nurturing social relations by being away from their home often, or to be relatively socially disengaged, self-oriented in a way that leads to avoidance of acceptance of stranger requests at the door.

The preponderance of the evidence thus far points to support for a two-prong view of household-level correlates of nonresponse. First, the process of contacting some households affects nonresponse error differently than the cooperation step. At the contact step, household size, age of householders, and other household compositional variables can exert their influence on the nature of the nonrespondents. How important the correlates of noncontact remain in the final data set depends on the efforts to reduce noncontacts.

Second, the nature of cooperation is most fully explained by concepts that describe the perceived relationship between the survey requester and the sample household. Notions of social isolation and social exchange appear consistent with the multivariate findings. In this perspective, the request for a survey interview is seen in the context of the prior set of dealings with the sponsor of the survey over time (or with others perceived to be similar to the sponsor). When the government is involved, those lower in socioeconomic status or with stronger senses of civic duty may be more willing to participate because of potential rewards of providing information to the government. We might speculate that when universities are the sponsors, those in higher education groups might be more cooperative.

It is important to note that the data used in this chapter come from surveys with response rates that are higher than those typical of academic or commercial surveys. They result from designs that employ unusual efforts to contact all sample households; thus, their noncontact rates tend to be unusually low. Their refusal rates are also low, but have some parallels in other surveys. In this book, surveys with few noncontacts allow a study of the reaction to the survey request of a more diverse pool of householders. They thus are more valuable for studying the correlates of cooperation.

Since the analyses concentrate on cooperation of sample persons once they are contacted, the inferential limitations of the work depend on whether the influences on cooperation are different for lower response rate surveys than for these high response rate surveys. We note that there remain large net differences in response rates across surveys, controlling on these demographic factors. These arise from different design features (such as survey topic or length of interview). These design features do not necessarily affect household-level influences because they are part of the survey request for all the demographic and social subgroups. By pooling data over surveys that vary on some of these design features, we measure the effects of household-level attributes that are robust to those design features.

Only if some subgroups of the population react to design features differently than do others will the kinds of models discussed in this chapter be inapplicable to surveys with lower response rates. One example of this might be the tendency for persons uninterested or uninformed about a topic to refuse a survey request (see Couper, 1997) or for those who are busy to disproportionately be nonrespondents to surveys that are conducted over just a few days. For a survey with very low levels of effort to contact sample persons, the refusal cases will consist of those who are easily contacted. In such cases, the magnitude of predictive power for some of the household-level variables may be larger, because their effects have not been attenuated by repeated persuasive efforts of interviewers. For the most part, however, we speculate that the general form of the models will remain the same at lower response rate levels.

In short, we have learned the following key lessons from the empirical analyses in this chapter:

- The tendency for those in high cost housing to refuse survey requests is partially explained by their residence in urban, high density areas. We interpret this as stronger support for hypotheses about the influence of social isolation and disintegration than for social exchange hypotheses concerning cooperation.
- Tendencies for those in military service and those in non-English speaking households to cooperate in surveys appear to be explained by their having household compositions favoring survey participation. We interpret this as partial refutation of the hypothesized role of government authority in survey cooperation.
- The apparent tendency of racial and ethnic minorities to cooperate with surveys when asked is largely explained by their lower socioeconomic status (as measured by housing costs). Once socioeconomic status is analytically controlled, minority cooperation rates are much closer to those of the majority group.
- Multiple indicators of social integration (or isolation) share strong marginal effects on cooperation. Households with young children or young adults tend to cooperate; single-adult households tend not to cooperate.
- In examining the participatory behavior of elderly households, it is important to consider the tendency for the elderly to live alone. After analytic controls on household size, elderly households tend to cooperate.

There are many features that define the psychological and behavioral context of a conversation between the interviewer and a sample person, in which the interviewer requests participation in the survey. These conversations comprise the proximate causes of survey response rates. This chapter has shown that even without measurement of these proximate causes, systematic and measurable variations exist across persons and households in their tendency to comply with survey requests. Adding measurement of these proximate causes, through observation of the conversations

between interviewers and respondents, provides further insight into the process of survey participation (see Chapters 8 and 9). These are necessary for a complete understanding of the phenomenon.

5.10 PRACTICAL IMPLICATIONS FOR SURVEY IMPLEMENTATION

Now that we have studied both the process of contacting households and the process of gaining cooperation, we can for the first time consider design strategies that jointly consider noncontacts and refusals. We learned in Chapter 4 that single-person households are difficult to contact. In this chapter we learned that they also tend to refuse survey requests. We found that elderly households and households with children are easy to contact; here we found they also tend to be cooperative once household size is controlled. (We do know from other work, however, that elderly persons tend to be nonrespondent for health and other physical reasons more often than younger persons.)

Many of the findings of this chapter concern the influence of attributes that are not usually knowable from the sampling frame itself. They are socio-demographic attributes that we believe are imperfect indicators of social psychological states affecting reactions to survey requests.

The most important practical implication of these results is to urge interviewers to learn these attributes of a sample household as early in the survey process as possible. That is, in early face-to-face calls on the household or in the first contact with the household, some effort should be made to observe these demographic characteristics.

For a limited number of these, proxy indicators might be useful to alert the face-to-face interviewer to possible challenges. For example, single-bedroom apartment units are sometimes systematically dispersed in a complex. Knowing the configuration of those in a area segment might give useful, albeit imperfect indicators of single-person households. Similarly, complexes that are devoted to young singles might indicate difficulty with contact and need for flexibility in negotiating the time of the interview once contact is made. The observation of evidence of young children mentioned in Chapter 4 applies here also.

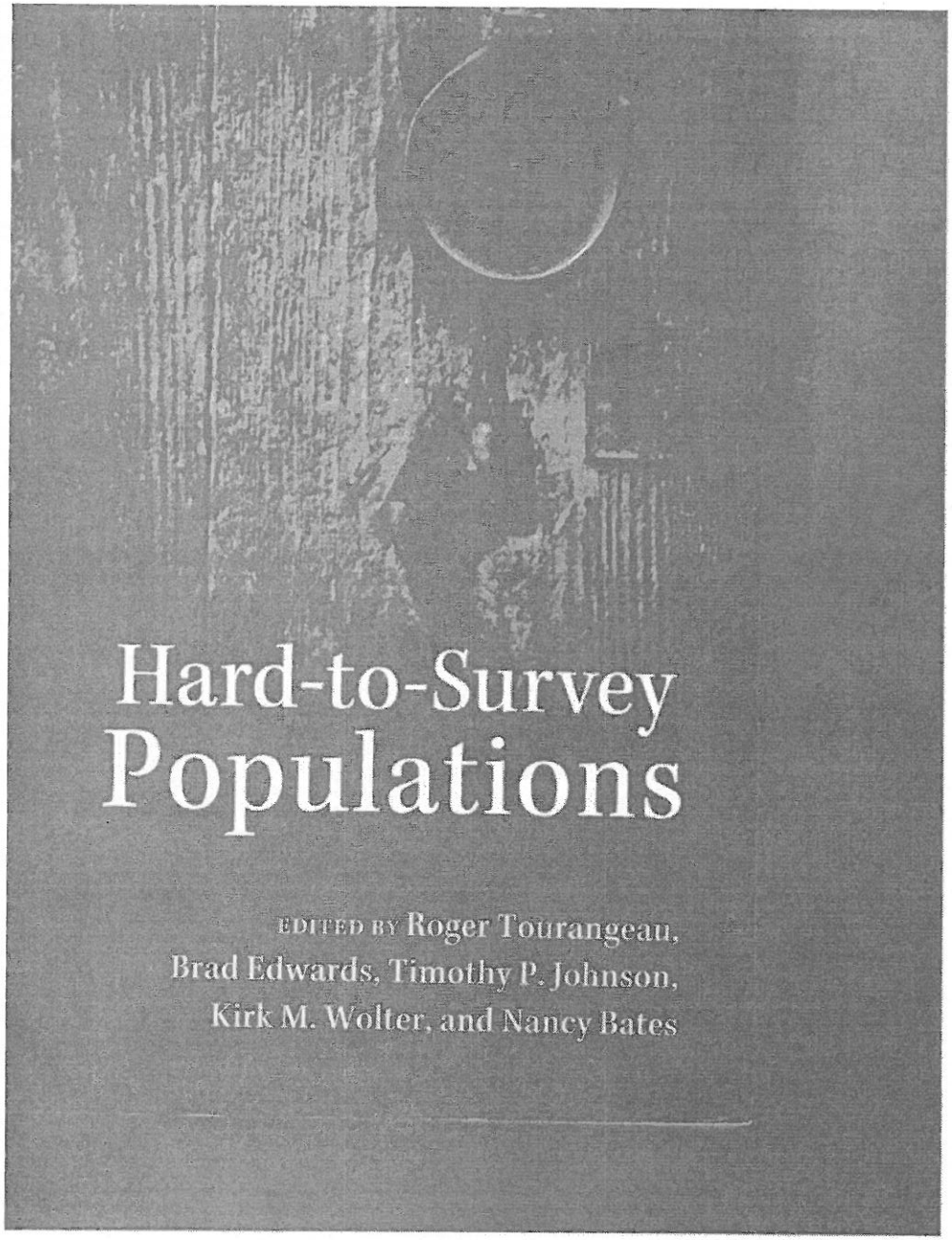
Only if interviewers learned of these attributes can they use knowledge to customize approaches to the households. To younger single-person households, who might have relatively less time at home, flexibility in doing the interview in several shorter segments may be important in gaining cooperation. In urban areas, persistence and explicit argument about the unequal value of the householders' participation might be useful. We have elaborated in Chapter 2 the notion that tailoring of interviewer approaches to real concerns of the householder can make a difference in cooperation rates. This chapter is our first note that much of this tailoring must begin after initial contacts with householders.

There are many features that define the psychological and behavioral context of a conversation between the interviewer and a sample person, in which the interviewer requests participation in the survey. These conversations comprise the proximate

causes of survey response rates. This chapter has shown that even without measurement of these proximate causes, systematic and measurable variations exist across persons and households in their tendency to comply with survey requests. Adding measurement of these proximate causes, through observation of the conversations between interviewers and respondents, provides further insight into the process of survey participation (see Chapter 8). These are necessary for a complete understanding of the phenomenon.

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 4



3

Measuring undercounts for hard-to-survey groups

MARY H. MULRY

3.1 Introduction

Measuring census undercount is an important way of gaining insight about subpopulations that are hard to survey. Although such groups may be a relatively small proportion of the population, they contribute disproportionately to the overall undercount. Several methods are available for measuring census undercounts, so countries are able to choose the one that best suits their situation. The methods for estimating net undercount include post-enumeration surveys, demographic analysis, administrative record matches, and reverse record checks. Countries use these methods to make population estimates that are thought to be more accurate than the census; this estimate is then compared to the census count to give an estimate of the net undercount.

The methods have different data requirements, so not every country has the data needed to apply each one. Even when such data exist, some countries have privacy and confidentiality restrictions on how the data are used. Therefore, not all methods can be applied in all countries. Demographic analysis uses vital records in aggregate calculations. Administrative record matches and reverse record checks require high-quality records systems and laws that permit their use to measure undercounts. A post-enumeration survey is a second enumeration implemented on a sample basis after a census and then matched to the census on a case-by-case basis. One advantage of a post-enumeration survey is that is does not depend on the availability of an administrative or vital records system. Even if such records are available, their quality and the characteristics of individuals that they contain are not issues when a post-enumeration survey is done. Therefore, the method may be applied in developed or developing countries.

This chapter discusses the methods for measuring census undercount and their advantages and disadvantages. Most censuses use an enumeration methodology that involves contacting the population to collect information. However, some countries, almost all of them in Europe, take their census using a register-based methodology where the information about the population is drawn from records maintained in one or more administrative registers (Valente, 2010). The coverage evaluation methodologies discussed in this chapter are typically employed for censuses that use an enumeration methodology, but they can be

used for register-based censuses. Also included are results of applications in some countries to illustrate identifying groups that are hard to enumerate in censuses and surveys.

3.2 Administrative record match

An administrative record match (ARM) is an evaluation procedure in which a sample from the administrative record file is matched case-by-case to the census population or subpopulation of interest. The usual assumption is that the administrative records file is more complete than the census so the percentage in the record sample not matched to the census is a measure of the census coverage error.

For the ARM to be a viable approach, the country has to possess a high-quality records system for the population of interest, which may be a subpopulation. The administrative records used for an ARM are maintained for other purposes, but the records may nonetheless provide sufficient coverage of the population of interest.

In some cases, the country has a centralized administrative record system that contains records for nearly all the residents. In other cases, the country has to merge several administrative record systems to cover the entire population of interest. Then, the combined file is unduplicated and the final product is matched to the census to identify persons missed by the census. The need to merge records systems raises the issue of the laws governing the country's records; these laws must permit matching the administrative records for statistical purposes that include census coverage evaluation.

The ARM method of coverage evaluation has several advantages. One advantage of using administrative lists is that they do not rely on a household survey or a previous census. Therefore, this method does not have the problem that post-enumeration surveys may have of missing many of the same people that the census does. Also, an ARM allows focusing on the hard-to-survey segments of the population by obtaining lists, such as the low-income population receiving Temporary Assistance for Needy Families in the US.

The ARM method also comes with disadvantages. One disadvantage is that there is no guarantee that the administrative list or lists cover the entire population of interest. This method also requires matching to the census, with possible tracing or follow-up of non-matches. An additional complication arises when several lists must be merged. Unduplication of the lists may be difficult because some people use different names and addresses for different purposes. Conversely, common names also create the problem of different people with the same name and birthdates, who may appear to be the same person to an unduplication algorithm. However, when household structure is available, the unduplication algorithms are more effective. If each person has a unique identification number that is present on all the lists and on the census, then finding duplicates on merged lists is much more successful. A crucial difficulty with using administrative records for coverage evaluation is identifying the population for which the records are complete. If the records overcover some subpopulation, this creates problems in generalizing any results to the entire population.

The first documented ARM occurred in the early 1930s in Canada where enumerations in the 1931 Canadian Census for infants under one year of age were matched to the birth registration records in nine provinces (Marks, Seltzer, & Krotki, 1974; Tracey, 1931). The study found that 80 percent of the enumerations matched to birth records, with a range of 80 to 91 percent across the nine provinces for areas not included in the Indian Reserves and a range of 57 to 74 percent in the Indian Reserves. In the US, ARMs also started in the 1930s to evaluate the coverage of birth records (Marks *et al.*, 1974). Most states conducted clerical matches of special-purpose surveys to birth records to show their records were at least 90 percent complete and could be included in the national birth registration system. Few details are available about these studies since only two conducted by the US Census Bureau for Georgia and Maryland are documented (Hedrich, Collinson, & Rhoads, 1939). The first US ARM using census data was the match of 1940 Census enumerations of infants under four months old to birth registration records and represented a substantial advance in matching methodology, although all the matching was clerical at that time (Marks *et al.*, 1974). Over the years, the US Census Bureau has conducted matches between different administrative lists and censuses to evaluate coverage and data quality. We discuss a few examples. ARMs evaluated the coverage of 1960 Census for two groups (Marks & Waksberg, 1966). A sample of Social Security recipients was matched to the 1960 Census and estimated that the number missed was 5.1 to 5.7 percent of those enumerated. A study of the net coverage of college students drew a sample from lists of students obtained from colleges and universities. The selected students received questionnaires asking about all the addresses where they may have been enumerated. Then, the study matched the students to the 1960 Census at all the reported addresses and was able to produce an estimate of net undercoverage of 2.5 to 2.7 percent, taking into account both overcoverage and undercoverage.

Another ARM conducted in conjunction with the 1980 Census assessed the feasibility of using the 1979 Internal Revenue Service (IRS) file as a sampling frame for evaluating census coverage (Childers & Hogan, 1983). A sample from the IRS file was matched to the 1980 Census at their address in the IRS file. When the study could not find a match, the person was traced using mail and personal interviews to find addresses where the person might be enumerated. However, the study was not able to trace 22 percent of the sample and did not make estimates of census undercount.

An ARM conducted in conjunction with the 1996 Community Census Test focused on determining whether there were people in administrative records who were not listed in a census or a post-enumeration survey. The study matched the census enumerations for the 1996 Census Community Test conducted in three locations (seven tracts in Chicago, Illinois; the Fort Hall Reservation in Idaho; and the Pueblo of Acoma in New Mexico) to a file created by merging several federal records files, with the combination of files varying by site (Sweet, 1997). Post-enumeration interviews used computer-assisted personal interviews (CAPI); the computers contained the administrative records but hid them from the interviewers until after they obtained a new roster at a housing unit. When an administrative-records-only person was not on the new roster, the program prompted the interviewer to ask about the person's residency. The percentage of people from administrative records who were residents but not enumerated and not on the rosters in the post-enumeration interview ranged from 2.0 to 2.5 across the three sites.

The US has not had a single administrative records system with high coverage of the entire population. By 2000, computer database processing and storage capacity had developed to the point that the US Census Bureau attempted to create a census-like file by merging and unduplicating five federal sources of administrative records, called the Statistical Administrative Records System (StARS; Leggieri, Pistiner, & Farber, 2002). A comparison between the StARS and Census 2000 found the file covered 95 percent of the population (Judson, 2000). The methodology for creating StARS enabled an ARM to examine the validity of the estimate of 5.8 million duplicate enumerations in the Census 2000 count of 281.4 million; that estimate was based on an algorithm using only census data. Census 2000 was the first to use optical character recognition technology that enabled capturing names in electronic format so the estimate of duplicates was the first of its kind and viewed as surprisingly large. The StARS algorithm chose a "best" address for a person when more than one address appeared in the five files. However, an auxiliary file kept all the addresses for each person. A match between the auxiliary file and a sample of census enumerations provided other addresses where the sample people may have been enumerated. A search of the census file for the sample people at the additional addresses produced an alternative estimate of 6.7 million duplicates, confirming that there were a large number of duplicate enumerations in Census 2000 (Mulry, Bean, Bauder, Mule, & Wagner, 2006).

The 2010 Census presented an opportunity for further research by creating a census-like administrative records file that merged both federal and commercial data sources and then comparing the unduplicated administrative records file to census records. The results of the 2010 Census Match Study showed that 88.6 percent of the 308.7 million 2010 Census enumerations could be matched to an administrative record. The main reason for the low match rate appeared to be not being able to assign unique identification numbers to 9.6 percent of the census person records. However, the census-like administrative records file had 312.2 million records for unique persons, but the study was not able to link 10.7 million to an address on the census file (Rastogia & O'Hara, 2012). Work continues on refining methods, but the focus has turned to identifying ways administrative records can reduce the cost and improve the quality of the 2020 Census, particularly in the design of adaptive operations for the follow-up of nonrespondents to the mailout/mailback questionnaires.

Other countries have different systems, cultures, and laws that have led to administrative records systems with a high level of coverage of their population that makes the systems suitable for the ARM methodology. In some countries, such as Finland and Norway, the coverage of the population and the quality of the data in the system are so high that the country has decided to use a register-based methodology for their census rather than an enumeration-based methodology. These countries typically use their census data to inform policy decisions and not to divide political power, which is different from the U.S. where census numbers are used in the apportionment of the seats in the US House of Representatives among the states.

Finland moved to a register-based census because evaluations of censuses using an enumeration methodology showed the register-based method had quality comparable to the enumeration method. The basis of this decision was not coverage, but the finding that the data on type of activity and labor force status was comparable with the net difference in the

categories considered, ranging between 0.2 and 10.0 percent (Myrskyla, 1991). Finland creates its census by combining its Central Population Register (CPR) with twelve other registers, including the Taxation Register and the Register on Wages, Salaries, and Pensions. The tradition of registers of the population goes back to the 1700s when tax collection and army recruitment used registers of births and deaths recorded in church parishes (Myrskyla, 1991). The central records systems evolved from this practice. Even when enumeration methods were used in 1970 and 1980, the goal was to collect information, not count the population. The addresses on the census mail questionnaires included the name of the resident, which indicates that registers were up to date regarding where the residents live and the purpose of the census was to collect additional information. For further discussion of the evolution of register-based censuses, see Harala und Tammilehto-Luode (1999), Redfern (1986) and Statistics Finland (2004).

Assuring that the records remain accurate enough for a register-based census is also a concern. Recommendations for validation methodology for register-based censuses may be found in a report from the UN Economic Commission for Europe (2006). The Commission also has joined with Eurostat to sponsor expert meetings on register-based censuses that include discussions of potential sources of errors and methods for validating methodology (UN Economic Commission for Europe, 2010, 2012). Interestingly, one of the concerns is that some people are listed in registers in two countries and procedures to unduplicate records between countries are not currently available.

The need to validate registers has become more apparent since the 2011 German Census Count of 80.2 million people was 1.5 million lower than the estimates based on local registers, and 1.1 million of the deficit had foreign citizenship, reducing the previously assumed number of resident foreigners to 6.2 million (German Federal Statistics Office 2013). An example of a validation of register records for a subpopulation is an evaluation of the records for immigrants and foreign-born Norwegians on Norway's CPR. Through a mail survey and other work with CPR records for 218,000 immigrants and foreign-born resident Norwegians, Statistics Norway found that 1.3 percent had emigrated or had expired work permits indicating they probably had emigrated. The conclusion was that there was a considerable delay in updating CPR records when members of this group left the country (Hendriks, 2012).

The post-enumeration survey methodology discussed in Section 3.5 usually is employed for enumeration-based censuses but also may be applied to evaluate a register-based census. Using the demographic analysis methodology discussed in Section 3.3 to evaluate a register-based census is contingent on the availability of other high-quality records independent of the records used in taking the census; this seems unlikely in most countries.

3.3 Demographic analysis

Demographic analysis uses analytical techniques applied to aggregate population data to study populations and estimate their size. As a tool for census evaluation, demographic analysis involves first developing estimates for the population in various categories, such as age, race,

and sex groups, at Census Day based on various types of demographic data. Then, these subgroup estimates are combined to yield an estimate for the population size of the nation as a whole. The data used for demographic analysis estimates include: birth, death, and immigration statistics; sex ratios, life tables, etc.; historical series of census data; and data from sample surveys. Corrections are made to the data for various types of known errors, usually based on secondary sources, such as administrative records, for a specific population. However, the production of the estimates does not involve case-by-case matching of records.

The basic demographic accounting relationship is:

$$Population = Birth - Deaths + Immigrants - Emigrants.$$

To apply the demographic analysis methodology, a country has to have high-quality historical time series for each of these quantities where the consistency of the different series has been confirmed. Another requirement when the records do exist is that there are no barriers to using them in aggregate to form demographic analysis estimates. The existence of records of births and deaths is particularly important because they form the basis for demographic analysis. When the records systems exist, but not all events are entered, then it is sometimes possible to estimate the coverage error rates and to use them in making adjustments. When records of immigrants and emigrants do not exist or are not complete, alternate estimation methods and data, such as survey data, are sometimes used to make adequate estimates for these groups.

In the US, an application of the demographic method of comparing aggregated totals raised the initial concern about the coverage of the census. Prior to the 1940s, the prevailing assumption was that the census had better coverage of the population than the records systems. However, a comparison of the number of males of military age in the 1940 Census to draft registration records dispelled that notion. The study estimated there were 14.9 percent more Black males of 21 to 35 years of age registered for the draft than were counted in the census and 2.8 percent more non-Black males in the same age category. By states, the estimates for non-Blacks ranged from being 4.1 percent too high in Wyoming to 16.0 percent too low for the District of Columbia. All the estimates for Blacks were too low, ranging from a deficit of 5.4 percent in Mississippi to 40.3 percent in the District of Columbia (Price, 1947). The US and other countries saw the need for developing methods to evaluate their census coverage. This led to the development of methodology to evaluate census coverage using demographic analysis, reverse record check (Section 3.4), and post-enumeration surveys (Section 3.5).

The primary advantage of demographic analysis as a tool for evaluating a census is that it uses data sources that are independent of the census being evaluated. However, the overall accuracy of demographic analysis for a country depends on the quality of the demographic data in these data sources and the quality of the corrections for any known errors in the data.

A disadvantage of demographic analysis is that the direct estimates of population size usually are available at the national level only. Another shortcoming is that the estimates are possible only for subgroups identified in the vital records. For example, in the US, population estimates are possible for only two racial groups, Blacks and non-Blacks, because the historical records put people in only those two categories. In recent years, records have also included the category of Hispanic ethnicity, which permitted the 2010 demographic analysis estimates of the US population to include estimates for Hispanics less than 25 years of age.

The first time demographic analysis methods were used to evaluate coverage error was by Coale (1955) for the 1950 US Census. The 1960 US Census was the first to use demographic analysis as an evaluation tool (Siegel & Zelnik, 1966). Many improvements were made in the demographic analysis methodology in 1970 (US Bureau of the Census, 1974). Undocumented immigration surfaced as an issue for the 1980 demographic analysis estimates (Fay, Passel, Robinson, & Cowan, 1988) and persisted in the 1990 and 2000 estimates (Robinson, 2001; Robinson, Ahmed, Das Gupta, & Woodrow, 1993). The methodology for estimating migration for the 2010 estimates changed to one based on data from the American Community Survey (US Census Bureau, 2010).

Another issue is the lack of measures of uncertainty in demographic analysis estimates due to choices of datasets and choices of assumptions. Most of the construction of the estimates does not use probability models so the uncertainty cannot be quantified with standard statistical methods, although the introduction of survey-based estimates of immigration in 2010 is an exception. The first attempt to quantify the uncertainty was for the 1990 estimates (Robinson *et al.*, 1993). For 2010, the US Census Bureau released a range of estimates based on varying assumptions. The range was 305.6 million to 312.7 million with a middle estimate of 308.4 million that was close to the census count of 308,745,438 (US Census Bureau, 2010). Demographic analysis estimates of population size do offer a way of obtaining historical estimates of census percent net undercount, as defined by

$$Percent\ net\ undercount = \frac{Population\ size - Census\ count}{Population\ size} \times 100.$$

A negative percent net undercount indicates an overcount. Figure 3.1 shows estimates of percent net undercount for the total population, the Black population, and the non-Black population for the decennial censuses in the US from 1940 to 2010 from demographic analysis. Black and non-Black are the only two racial subgroups available in historical records for the entire period. However, the non-Black population became much more heterogeneous over the period from 1940 to 2010 so the racial and ethnic composition of non-Blacks in 1940 is not what was observed for non-Blacks in 2010.

Other countries have used demographic analysis techniques to evaluate censuses, although the US is the only one with a historical series like the one in Figure 3.1. In many instances, countries focus on the internal consistency of the census counts rather than on the level of the estimates. The tools include examining sex ratios for age cohorts and comparisons with historical values. Kerr (1998) compares the demographic analysis methods used in the evaluation of censuses in Canada, US, UK, and Australia. The relatively poor quality of some records for the older population reduces the quality of the estimates of the total

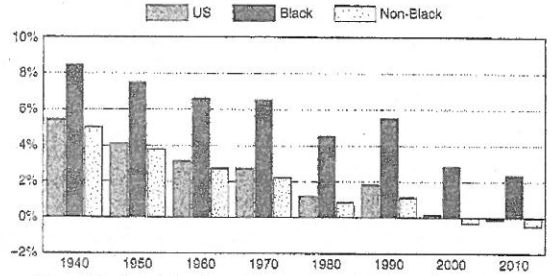


Figure 3.1 Historical estimates of percent net undercount in the US Census 1940 to 2010 based on estimates from demographic analysis.
Note: A negative net undercount denotes a net overcount.
Source: 1940–2000: Long, Robinson, & Gibson, 2003; 2010: US Census Bureau, 2012.

population in all these countries. The US addresses this concern by relying on Medicare data for those 65 years of age and older. However, all four of the countries use demographic analysis techniques to form estimates of the population for years between censuses, called post-censal or intercensal estimates, by using the census as a base and estimating the change in the population (Kerr, 1998). In addition, demographic analysis techniques are used to form estimates of the size of the population in other countries, including developing countries (Brass, 1996).

3.4 Reverse record check

A reverse record check (RRC) is a census evaluation program in which a sample of the population is drawn from records that existed before the census, traced forward to the time of the census, and matched to the census. The frame is usually composed of people enumerated in the previous census, persons missed in the previous census, births, and immigrants. This method differs from the ARM approach since the records from the last census (including the records of people identified as missed in the evaluation of the last census) are not generally classified as administrative records. This type of records sample, if well executed, is likely to have better coverage than a census. The sample for the RRC is composed of separate samples selected from each source. The proportion of the sample that is unmatched provides an estimate of the proportion of the population that was missed in the census. To obtain an estimate of net census coverage error, an RRC has to be supplemented with a separate sample of census enumerations and a validation operation in order to measure erroneous enumerations, which tend to be mainly duplicate enumerations. Then the resulting estimate of population size is

Population size = census count + number missed − number of erroneous enumerations.

A key requirement for implementing a RRC is the availability of current addresses for the people in the records samples; this usually means matching these samples to a high-quality administrative records system. The existence of a unique identifier number assigned to each resident facilitates the matching between the samples and the administrative records. The need to update addresses for the sample from a previous census and the sample of people missed by the previous census is obvious. However, even if records of births, deaths, immigration, and emigration are available for selecting the sample, the addresses at the time of the event that generated the record may be out of date by the time of the census.

A second requirement is that the privacy policies permit interviewers to ask for a person by name at an address found in administrative records. In the US, for example, the privacy policy governing the tax records held by the IRS does not permit revealing a person's name on a tax return or even that the person filed a tax return. Therefore, matching to IRS records and then asking for the person by name at the address found in the IRS records would not be approved by IRS for a statistical program. Research projects may apply for permission to ask for people at their address found in the IRS records, but few are approved (Childers & Hogan, 1983; Sweet, 1997).

A RRC takes advantage of changes over time in the probability that a particular person can be found. For example, except for the very youngest, children are easier to enumerate than young adults, who tend to be highly mobile. Canada, which uses the RRC methodology, provides an illustration. It conducts a census every five years. The sample of people enumerated in the previous census will include some children and in particular, children ages 12 to14. The RRC will be able to determine if they are missed in the next census when they are 17 to 19, ages that have a lower probability of being enumerated. Any who are missed are included in the sample of those missed for the subsequent census. Accumulating a RRC sampling frame from census to census allows the frame to become more complete over time. After several censuses, the quality of the frame of those missed in the previous census becomes very high.

On the other hand, if several implementations are needed to create a high-quality frame of persons missed by the census, this will be a disadvantage for the first few census evaluations. Another potential disadvantage is that even if hard-to-enumerate groups are easier to sample several years before the census, this advantage may be offset to some extent by a type of correlation bias. This type of correlation bias potentially arises if those people who were traced successfully are more likely to be counted in the census than those who could not be traced. Statistics Canada first used the RRC approach to evaluate the coverage of the 1961 Census but there was no frame for people missed by the previous census (Statistics Canada, 2007). The results of the 1961 RRC provided a sample of those missed by the 1961 Census for use in the evaluation of the 1966 Census. The evaluations of the Canadian censuses since 1966 have used the RRC methodology. When censuses are done every five years it is easier to trace sample people to their new addresses than when they are done every ten years.

Canada has accumulated its RRC sampling frame from census to census, thus covering anyone counted in any of the censuses and anyone born or immigrated to Canada between censuses since the start of this effort in the 1970s. Since sample people without current records are traced, a distinction can be made between those who are missed and those who have emigrated. The result is that the Canadian RRC sampling frame has become successively more complete over time. The completeness of the frame enables researchers to estimate coverage of those missed by the census being evaluated. In addition, Canada has a centralized statistical system that facilitates accessing administrative records to find current addresses. The Canadian laws permit asking for a person by name at an address found in administrative records.

The US Census Bureau has experimented with the RRC methodology on several occasions but each time found that locating new addresses for sample members when there are ten years between censuses was more difficult than hoped. The US Census Bureau conducted an RRC to estimate the number of persons omitted by the 1960 Census (US Bureau of the Census, 1964), but the 1960 RRC failed to trace 16.5 percent of the sample, which was too much nonresponse to get reliable estimates of the miss rate. Two research projects in the 1980s indicated that a RRC would not be successful in the US. The estimated nonmatch rate from an attempt to match a sample from the 1977 Current Population Survey to the 1980 Census was 14 percent, which was twice as high as the nonmatch rate from the 1980 Post-enumeration Program (Diffendal, 1986). Also, the US Census Bureau evaluated the tracing component in the Forward Trace Study (Mulry & Dajani, 1989) by selecting the four samples that would be needed for a RRC and tracing them from 1980 to 1985. The estimates of the tracing rates were significantly lower for minorities than for Whites, and overall were too low to merit a recommendation to use a RRC for evaluating the coverage of the 1990 Census. A re-examination of the feasibility of evaluating the 2010 Census coverage with a RRC based on the 2007 American Community Survey (ACS) sample augmented with samples of births and immigrants from the 2008–2009 ACS reached the same conclusion because of the difficulty of tracing sample people to their address on Census Day in 2010 (Mulry & Bell, 2008).

The most recent results available from a Canadian RRC are from the evaluation of the 2006 Census. Although there was a Canadian 2011 Census, the coverage study results are not available as this paper goes to press.

Tables 3.1 and 3.2 display estimated percent net undercount for the 2006 Canadian Census for demographic groups (Statistics Canada, 2007). Table 3.1 shows the percent net undercount by sex and age. Males have a higher percent net undercount than females, and young adults ages 18 to 34 have the higher coverage error rates than the other age groups. Males in this age range have the highest percent net undercount of any age–sex group.

Table 3.2 shows the percent net undercount by sex and marital status for people 15 years of age and older. Again, in these groupings males have a higher percent net undercount than females. The coverage error rates among the never married, separated, and divorced are higher than those among the married and widowed. The widowed probably tend to be older

Table 3.1 *Estimated 2006 Canadian Census percent net undercount, by sex and age*

	Both sexes		Males		Females	
Age and sex	%	SE	%	SE	%	SE
All Canada	**2.67**	**0.17**	**3.89**	**0.26**	**1.48**	**0.23**
0 to 4 years	2.72	0.65	2.89	0.94	2.54	0.92
5 to 14 years	0.86	0.45	0.79	0.63	0.94	0.65
15 to 17 years	−0.76	0.61	−0.49	0.88	−1.05	0.83
18 to 19 years	6.22	1.55	7.78	2.41	4.54	1.93
20 to 24 years	7.63	0.73	9.46	1.11	5.69	0.97
25 to 34 years	8.00	0.56	9.91	0.86	6.08	0.73
35 to 44 years	4.31	0.49	6.66	0.78	1.92	0.61
45 to 54 years	1.50	0.42	2.98	0.69	0.03	0.51
55 to 64 years	−0.29	0.53	0.83	0.76	−1.40	0.73
65 years and over	−1.39	0.39	−1.74	0.56	−1.13	0.55

Source: Statistics Canada (2007). A negative net undercount denotes a net overcount.

Table 3.2 *Estimated 2006 Canadian Census percent net undercount, by marital status and age for persons 15 years of age and over*

	Both sexes		Males		Females	
Marital status and sex for persons 15 years and over	%	SE	%	SE	%	SE
All	**2.94**	**0.19**	**4.43**	**0.30**	**1.49**	**0.25**
Never married	6.70	0.43	8.82	0.62	4.09	0.58
Married or common-law	1.00	0.21	1.37	0.31	0.63	0.29
Separated	9.75	2.26	16.84	4.58	3.63	1.88
Divorced	4.39	1.14	7.86	1.86	1.91	1.43
Widowed	−1.28	0.73	−0.38	1.85	−1.48	0.80

Source: Statistics Canada (2007). A negative net undercount denotes a net overcount.

members of the population and the never married probably tend to be younger members. The males who are never married, separated, or divorced have the highest percent net undercount.

Canada uses the percent net undercount estimates to adjust their census for fund allocation. See Royce (1992) for details about the adjustment methodology.

3.5 Post-enumeration survey

A post-enumeration survey (PES) is a survey conducted after the census for the purpose of measuring census coverage. The survey respondents are matched to the original enumeration on a case-by-case basis. Then, dual system estimation may be used to give an estimate

of the population size. A comparison of the census to the PES estimate of population size yields the net undercount rate. Much of the basic development of the methodology came out of efforts associated with the United Nations to estimate population growth. The methodology and its history are described well in Chandrasekar and Deming (1949) and Marks et al. (1974).

The first attempts to evaluate a census using a PES in the US occurred after the 1950 and 1960 Censuses (US Census Bureau, 1960; Marks & Waksberg, 1966). These efforts attempted to find the truth for each household in sample, but the surveys did not find an undercount as large as the demographic analysis estimates. An attempt to conduct a PES to evaluate the 1970 Census was so flawed that a report was not produced. Dual system estimation (DSE) methodology emerged as the means to produce better estimates from a PES because its underlying assumption that the second enumeration was independent was easier to meet than the previous method's assumption that the PES had no error (that is, that the PES found the truth for every household). So, an evaluation of the 1980 Census used the DSE approach. During the 1980s, computerized matching techniques greatly improved the PES processing and these were applied first in a PES to evaluate the 1990 Census (Hogan, 1993) and in subsequent PESs that evaluated the 2000 and 2010 Censuses (Hogan, 2003; Mule, 2012). Since the 1980 implementation, the PES has produced estimates at the national level that are comparable to those from demographic analysis, differing by at most 0.2 percent (Mulry, 2007). For more details on the evolution of the PES at US Census Bureau, see Mulry (2011).

Two key assumptions underlie the PES method (Mulry & Spencer, 1991). One is that inclusion in the coverage survey is independent of inclusion in the census, which means the operations for the two cannot share information. Office and field staff for the coverage survey cannot work in the areas where they worked on the census. Census staff cannot know which blocks are in the coverage survey sample to prevent them from treating the sample areas differently from the areas that are not in sample. The other assumption is that the probability of being included in the census is not correlated with the probability of being included in the coverage survey. If these probabilities are uniform throughout the population, this assumption is met. When both assumptions are met, the basic relationship holds:

$$\frac{\textit{Number of good census enumerations}}{\textit{Population size}} = \frac{\textit{Number of matched people}}{\textit{Number of survey enumerations}}$$

Then with algebra, the form of the dual system estimator is

$$\textit{Population size} = \textit{Number of good census enumerations} \cdot \frac{\textit{Number of survey enumerations}}{\textit{Number of matched people}}$$

However, it is well known that capture probabilities vary. A method for addressing this problem creates separate estimates for groups thought to differ in their capture probabilities, such as age and sex groups; population estimates are made for each of these groups (post-strata) and then summed to estimate the total population size. However, some variation in capture probabilities may still exist within the post-strata, which introduces a bias in the

estimate of the population size, called a correlation bias. The concern about correlation bias is that it tends to introduce a downward bias in the estimates. Methods to reduce correlation bias include a generalized form of the DSE that uses logistic regression DSE and adjusting the groups that auxiliary data indicate are underestimated (Mulry & Cantwell, 2010). The post-stratified DSE is equivalent to a logistic regression DSE when all the interactions are included in the logistic regression model. The US Census Bureau adjusted the 2010 estimates of coverage error with demographic analysis estimates of the sex ratios of males to females, which are believed to be better than those from the PES because the sex ratios for cohorts from the demographic analysis have been consistent over time (Mule, 2012). The method assumes that correlation bias affects adult males, but not adult females or children 17 years of age and under (Bell, 1993). In 2010, the demographic analysis and PES sex ratios for Blacks 18 to 29 years of age were comparable so no adjustment was made for this group. However, the demographic analysis sex ratios for Blacks 30 to 49 years of age and 50 years of age or over were 0.91 and 0.80, respectively, while the sex ratios from the dual system estimates were 0.81 and 0.74, respectively. The adjustment for correlation bias using the sex ratios in these two age groups added 926,000 Black males, increasing the estimates of Blacks from 40.073 million to 40.999 million (Konicki, 2012).

An advantage of the PES is that it does not require the country to have a records system or historical vital records, which is one reason the UN recommends it for evaluating census coverage in developing countries (United Nations Secretariat, 2010a). The implementation requires a sample of the population that is often an area sample, but the sample must be independent of the census it is evaluating. A legal requirement is that the country's laws permit matching between the independent sample and the census data. A PES is also attractive for developed countries because its design is able to provide estimates for levels of geography below the national level and for race/ethnic groups that may or may not be distinguished in the records systems and vital records. Operational independence of the implementation may be achieved by not releasing the areas in sample to the field staff until after the census.

The PES also has some disadvantages. One disadvantage is that it requires careful implementation to assure as much independence as possible between the census and the post-enumeration survey. Also, the matching between two independent lists, the PES and the census, currently requires a substantial amount of time. The matching requires that the census enumeration files be available in addition to the PES files. Matching people who move between the census and the PES interview is complicated and is one reason so much time is necessary. A technical disadvantage of the PES is that the dual system estimates may be subject to correlation bias, discussed earlier in this section.

We examine estimates of percent net undercount based on PES methodology for the US and Australia. Estimates of net undercount for the 2010 US Census use a variant of the dual system estimation approach that employs logistic regression to produce estimates of net coverage error. New methodology produces estimates of erroneous enumerations and omissions, which are not shown here but may be found in Keller and Fox (2012). Tables 3.3 to 3.5 show estimated percent net undercount for the 2010 US Census for

Table 3.3 *Estimated 2010 US Census percent net undercount, by sex and age*

	Persons		Males		Females	
Age and sex	%	SE	%	SE	%	SE
US total	−0.01	0.14				
0 to 17 years	−0.33	0.22				
0 to 9 years	0.20	0.29				
0 to 4 years	0.72	0.40				
5 to 9 years	−0.33	0.31				
10 to 17 years	−0.97	0.29				
18 to 29 years			1.21	0.45	−0.28	0.36
30 to 49 years			3.57	0.20	−0.42	0.21
50 years and over			−0.32	0.14	−2.35	0.14

Source: Mule (2012), Table 12. A negative net undercount denotes a net overcount.

Table 3.4 *Estimated 2010 US Census percent net undercount, by relationship in the household*

Relationship in household	%	SE
Nuclear family members	−0.32	0.14
Adult children	−2.91	0.38
Other household members	3.53	0.38

Source: Olson (2012), Table 7. A negative net undercount denotes a net overcount.

demographic groups (Mule, 2012). Table 3.3 shows percent net undercount by sex and age. Males have a higher percent net undercount than females, and young adults ages 18 to 49 have a higher percent net undercount than the other age groups. Adult males 30 to 49 have the highest percent net undercount. Table 3.4 shows the percent net undercount by relationship in the household. The percent net undercounts are lowest among the nuclear family members, consisting of the householder and, if present, his or her spouse and their children under 18 years of age. Adult children have a large negative percent net undercount, indicating overcoverage, probably because they tend to be duplicated. Other members of the household have the highest percent net undercount.

Table 3.5 shows the percent net undercount by whether the household lived in a bilingual (English and Spanish) mailing area and by Hispanic ethnicity. The areas that received bilingual questionnaires were known to have high rates of Spanish speakers who did not speak English well. The areas that received a bilingual questionnaire had a higher percent net undercount than the areas that did not. Also, the percent net undercount for the Hispanics in both areas was higher than for non-Hispanics.

Table 3.5 *Estimated 2010 US Census percent net undercount, by whether the area received a Spanish questionnaire and Hispanic ethnicity*

Group	%	SE
US	−0.01	0.14
Bilingual mailing area	0.80	0.40
Hispanic	1.33	0.42
Non-Hispanic	−0.15	0.50
Balance	−0.12	0.16
Hispanic	1.72	0.42
Non-Hispanic	−0.33	0.16

Source: Mule (2012), Table 16. A negative net undercount denotes a net overcount.

Table 3.6 *Estimated 2011 Australia Census coverage error rates, by sex and age*

	Persons		Males		Females	
Age	%	SE	%	SE	%	SE
Total all ages	**1.7**	**0.2**	**2.2**	**0.2**	**1.2**	**0.2**
0–4 years	1.2	0.6	1.2	0.8	1.3	0.7
5–9 years	1.5	0.6	1.9	0.8	1.0	0.8
10–14 years	0.4	0.6	0.6	0.7	0.2	0.8
15–19 years	2.5	0.6	2.4	0.8	2.6	0.9
20–24 years	6.9	0.8	7.8	1.1	6.0	1.0
25–29 years	5.8	0.8	7.5	1.2	4.0	0.9
30–34 years	3.0	0.7	4.1	0.9	1.9	0.9
35–39 years	1.1	0.6	2.1	0.9	0.2	0.8
40–44 years	1.3	0.6	1.2	0.8	1.5	0.8
45–49 years	0.4	0.5	1.5	0.8	−0.8	0.7
50–54 years	0.9	0.6	1.6	0.9	0.3	0.7
55 years and over	−0.1	0.2	−0.2	0.3	−0.1	0.3

Source: Australian Bureau of Statistics (2012). A negative net undercount denotes a net overcount.

Tables 3.6 and 3.7 have estimated percent net undercount for the 2011 Australian Census for selected demographic groups based on a PES. Table 3.6 shows the percent net undercount by sex and age. Males have a higher coverage error rate than females and young adults ages 20 to 34 have a higher percent net undercount than the other age groups. Adult males 20 to 29 have the highest percent net undercount.

Table 3.7 shows the percent net undercount by sex and marital status. Again, in these groupings males have higher percent net undercount than females. The percent net

Table 3.7 *Estimated 2011 Australia Census percent net undercount, by sex and marital status*

	Persons		Males		Females	
Marital status	%	SE	%	SE	%	SE
Total persons	1.7	0.2	2.2	0.2	1.2	0.2
Married	0.2	0.2	0.4	0.3	−0.1	0.2
Widowed, divorced, or separated	−0.8	0.5	−0.8	1.0	−0.8	0.5
Never married	3.7	0.3	4.2	0.4	3.1	0.3

Source: Australian Bureau of Statistics (2012). A negative net undercount denotes a net overcount.

undercount among the never married is higher than among the married and widowed, divorced, and separated. The males who are never married have the highest percent net undercount.

In countries that conduct a post-enumeration survey, the question usually arises as to whether they should adjust the census with the results. Some adjust their official census numbers, while others do not adjust the census numbers but make adjustments for other purposes such as fund allocations. A prime example of incorporating the post-enumeration survey results in the final census numbers occurs in the UK, which adjusts its enumeration-based census. A description of the UK methodology and estimates may be found in Chapter 4 of this volume (see also Abbott, 2009). Such an adjustment also has been done for a register-based census, such as the method used for the 2011 Census in Turkey (Turkish Statistical Institute, 2012). Previously, Turkey had used an enumeration-based census and conducted a post-enumeration survey for evaluation purposes (Ayhan & Ekni, 2003).

The UN continues to encourage countries to evaluate the quality of their censuses. The UN offers manuals and other materials that aid in the implementation of the PES methodology (United Nations Secretariat, 2010a; Whitford & Banda, 2001). In addition, the UN sponsors regional workshops that include lectures on various aspects of PES methodology and presentations by countries about their past experiences with PES methods and their plans for future implementations (United Nations Secretariat, 2009; United Nations Secretariat, 2010b). Some countries use their PES as an evaluation of the census and to improve operations in future censuses, while others use the results of the PES to adjust their census numbers.

3.6 Summary

Estimates of census net undercount may be made for an entire country and for subpopulations. A higher than average census net undercount rate for subpopulation indicates it is hard to count. The people who are hard to count in a census usually are hard to survey as well because the methods for census enumeration and survey interviews are similar.

We have examined four methodologies for measuring census coverage error that focus on measuring coverage on a national basis. They aid in identifying the hard-to-count portions of

the population. All the coverage measurement methods provide information for operational evaluation as well as estimates of coverage at the national level. The ability to make estimates for specific subpopulations and lower geographic areas depends on the method and how it is implemented.

A country's choice of a methodology for measuring census coverage error depends on the availability of the required data and the privacy and confidentiality policies surrounding the use of the data for measuring census undercount. To some extent, the underlying purpose of the census and the purpose of coverage evaluation program also affect the choice of methodology for measuring census coverage and the methodology for taking the census. In most countries, the census collects data for policy decisions, but in the US the Constitution specifies that the census counts be used to allocate seats in Congress among the states.

When we examined estimates of percent net census undercount for the US, Canada, and Australia, we found some themes emerge about who is hard to count. These patterns appear even though the methodology of evaluating the census may differ and even though implementations of what is basically the same methodology may differ. The main conclusions are:

- Males are harder to count than females;
- Young adults are harder to count than children and older adults, with young adult males being the most difficult age/sex group;
- Nuclear family members are easier to count than other family members. Adults who have never been married are the most difficult marital status group to count.

We also found some evidence that people who are isolated from the dominant society in some way – whether the isolation is linguistic, cultural, or geographic – are hard to count. That said, the coverage measurement methods may miss very hard-to-count subgroups, such as the homeless, migratory workers, and people who are strongly anti-government. Other methods such as ethnographic techniques may be necessary to identify and contact these subgroups.

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Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 5

For Discussion: NAC 2020 IPC Working Group Check Points

Draft v1.1 (4/5/16)

This document outlines the individual pieces of the 2020 IPC program in which the National Advisory Committee Integrated Partnership and Communication Working Group could potentially provide review and feedback.

	Check Point	Timeline (Fiscal Year)
1	**The 2016 Census Test Integrated Partnership and Communication Plan (IPC)** – WG will receive updates about the test and a close out update after completion of the test	2016 Q3 & Q4
2	Introduce the Integrated Communications Contract Team to the NAC	2017 Q1 (Fall Meeting)
3	**The 2017 Census Test (IPC)** – WG will review the draft IPC plan, receive updates about the test, and a close out update after completion of the test	2017 Q1-Q4
4	**The 2017 Puerto Rico Census Test (IPC)** – WG will review the draft IPC plan, receive updates about the test, and a close out update after completion of the test	2017 Q1-Q4
5	**The 2018 End-to-End Census Test** – WG will review research and creative development. We will brief them on creative concepts that made it through testing, for their comments and buy-in PRIOR to going to production.	2016 - 2018
6	**The 2020 IPC Research Plan** – WG will review the draft and final plan	2016 - 2017
7	**The 2020 Integrated Partnership and Communication Plan** – WG will review draft and revised plan. Each member will also receive a copy of the final 2020 IPC Plan	2016 - 2017
8	**Audience Segmentation Model** – WG will provide feedback on proposed methodology and review the final model	2016 - 2017
9	**Test Messaging** – WG will review specific proposed messages that made it through testing, for their comments and buy-in prior to use. We will clarify specific messages for review at a later time	2016 - 2017
10	**Focus Groups** – WG will be invited to observe at their own expense, receive draft reports to review, and will receive final reports	2016 - 2017
11	**Partnership Plan** – WG will review the draft and final plan	2017 Q1-Q2
12	**Production Events** – WG could be invited to attend production (TV, radio, print) events at their own expense	2018 - 2019
13	**Media Buying Strategy** (TV, Radio, Print, Digital, Out of Home [OOH]) – WG will be briefed on the media strategy and provide feedback. The final media buys will be shared with the WG prior to posting for public view	2018 - 2019
14	**Post-Buy Information** – WG will review the media post-buy report once completed	–2020 - 2021

Note: All dates are estimates and could change

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 6

PCT12 | SEX BY AGE

Universe: Total population
2010 Census Summary File 1

NOTE: For information on confidentiality protection, nonsampling error, and definitions, see http://www.census.gov/prod/cen2010/doc/sf1.pdf.

	United States
Total:	308,745,538 [F38234]
Male:	151,781,326
Under 1 year	2,014,276
1 year	2,030,853
2 years	2,092,198
3 years	2,104,550
4 years	2,077,550
5 years	2,072,094
6 years	2,075,319
7 years	2,057,076
8 years	2,065,453
9 years	2,119,696
10 years	2,135,996
11 years	2,103,264
12 years	2,100,145
13 years	2,104,914
14 years	2,135,543
15 years	2,177,022
16 years	2,216,034
17 years	2,263,153
18 years	2,305,473
19 years	2,341,984
20 years	2,308,319
21 years	2,223,198
22 years	2,177,797
23 years	2,140,799
24 years	2,164,063
25 years	2,161,308
26 years	2,097,088
27 years	2,140,651
28 years	2,118,605
29 years	2,117,939
30 years	2,160,802
31 years	1,988,155
32 years	1,994,476
33 years	1,936,863
34 years	1,916,204
35 years	1,980,916
36 years	1,890,595
37 years	1,953,386
38 years	2,049,720
39 years	2,167,405
40 years	2,191,249
41 years	2,047,818
42 years	2,028,653

	United States
43 years	2,035,990
44 years	2,090,267
45 years	2,237,450
46 years	2,230,982
47 years	2,238,248
48 years	2,237,734
49 years	2,264,671
50 years	2,300,354
51 years	2,190,766
52 years	2,207,246
53 years	2,141,354
54 years	2,093,554
55 years	2,073,473
56 years	1,956,141
57 years	1,905,355
58 years	1,834,808
59 years	1,753,871
60 years	1,745,507
61 years	1,679,077
62 years	1,712,692
63 years	1,672,329
64 years	1,267,895
65 years	1,273,310
66 years	1,248,276
67 years	1,248,906
68 years	1,087,296
69 years	994,759
70 years	945,611
71 years	900,148
72 years	853,726
73 years	787,863
74 years	756,624
75 years	721,008
76 years	647,804
77 years	631,884
78 years	602,458
79 years	579,234
80 years	543,559
81 years	494,870
82 years	462,983
83 years	419,831
84 years	373,131
85 years	336,819
86 years	293,120
87 years	249,803
88 years	217,436
89 years	176,689
90 years	136,948
91 years	103,799
92 years	81,072
93 years	59,037
94 years	43,531
95 years	30,951
96 years	21,424
97 years	14,556
98 years	9,259
99 years	6,073
100 to 104 years	8,295
105 to 109 years	736
110 years and over	131

	United States
Female:	156,964,212
Under 1 year	1,929,877
1 year	1,947,217
2 years	2,004,731
3 years	2,014,490
4 years	1,985,620
5 years	1,984,764
6 years	1,991,062
7 years	1,973,503
8 years	1,981,033
9 years	2,028,657
10 years	2,036,545
11 years	2,011,151
12 years	2,006,098
13 years	2,013,099
14 years	2,030,439
15 years	2,065,798
16 years	2,100,105
17 years	2,132,142
18 years	2,195,382
19 years	2,243,250
20 years	2,210,810
21 years	2,131,096
22 years	2,086,845
23 years	2,057,772
24 years	2,085,300
25 years	2,101,042
26 years	2,055,217
27 years	2,108,218
28 years	2,096,644
29 years	2,105,137
30 years	2,124,866
31 years	1,982,063
32 years	1,992,371
33 years	1,943,287
34 years	1,923,012
35 years	1,975,518
36 years	1,911,492
37 years	1,981,059
38 years	2,072,160
39 years	2,197,391
40 years	2,192,025
41 years	2,067,167
42 years	2,047,451
43 years	2,069,115
44 years	2,121,229
45 years	2,271,418
46 years	2,288,779
47 years	2,297,017
48 years	2,301,062
49 years	2,341,230
50 years	2,359,941
51 years	2,273,865
52 years	2,293,600
53 years	2,239,000
54 years	2,198,445
55 years	2,181,236
56 years	2,081,372
57 years	2,031,031
58 years	1,960,120

	United States
59 years	1,887,398
60 years	1,875,624
61 years	1,813,519
62 years	1,850,490
63 years	1,811,555
64 years	1,389,236
65 years	1,407,451
66 years	1,390,865
67 years	1,400,459
68 years	1,236,376
69 years	1,147,565
70 years	1,097,510
71 years	1,049,175
72 years	1,010,549
73 years	949,097
74 years	927,863
75 years	899,069
76 years	823,266
77 years	823,446
78 years	797,665
79 years	791,961
80 years	764,952
81 years	717,995
82 years	698,438
83 years	654,978
84 years	612,590
85 years	577,904
86 years	521,091
87 years	463,105
88 years	423,183
89 years	361,309
90 years	298,615
91 years	241,188
92 years	200,317
93 years	157,941
94 years	125,918
95 years	98,766
96 years	73,799
97 years	53,582
98 years	36,641
99 years	26,193
100 to 104 years	40,846
105 to 109 years	3,157
110 years and over	199

(r38234) This count has been revised.
Revised count: **308,746,065**
Revision date: **01-31-2014**
For more information, see 2010 Census Count Question Resolution.

Source: U.S. Census Bureau, 2010 Census.

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 7

DP-1 | Profile of General Population and Housing Characteristics: 2010

2010 Census Summary File 1

NOTE: For information on confidentiality protection, nonsampling error, and definitions, see http://www.census.gov/prod/cen2010/doc/sf1.pdf.

Geography: United States

Subject	Number	Percent
SEX AND AGE		
Total population	308,745,538 [F38234]	100.0
Under 5 years	20,201,362	6.5
5 to 9 years	20,348,657	6.6
10 to 14 years	20,677,194	6.7
15 to 19 years	22,040,343	7.1
20 to 24 years	21,585,999	7.0
25 to 29 years	21,101,849	6.8
30 to 34 years	19,962,099	6.5
35 to 39 years	20,179,642	6.5
40 to 44 years	20,890,964	6.8
45 to 49 years	22,708,591	7.4
50 to 54 years	22,298,125	7.2
55 to 59 years	19,664,805	6.4
60 to 64 years	16,817,924	5.4
65 to 69 years	12,435,263	4.0
70 to 74 years	9,278,166	3.0
75 to 79 years	7,317,795	2.4
80 to 84 years	5,743,327	1.9
85 years and over	5,493,433	1.8
Median age (years)	37.2	(X)
16 years and over	243,275,505	78.8
18 years and over	234,564,071	76.0
21 years and over	220,958,853	71.6
62 years and over	49,972,181	16.2
65 years and over	40,267,984	13.0
Male population	151,781,326	49.2
Under 5 years	10,319,427	3.3
5 to 9 years	10,389,638	3.4
10 to 14 years	10,579,862	3.4
15 to 19 years	11,303,666	3.7
20 to 24 years	11,014,176	3.6
25 to 29 years	10,635,591	3.4
30 to 34 years	9,996,500	3.2
35 to 39 years	10,042,022	3.3
40 to 44 years	10,393,977	3.4
45 to 49 years	11,209,085	3.6
50 to 54 years	10,933,274	3.5
55 to 59 years	9,523,648	3.1
60 to 64 years	8,077,500	2.6
65 to 69 years	5,852,547	1.9
70 to 74 years	4,243,972	1.4

Subject	Number	Percent
75 to 79 years	3,182,388	1.0
80 to 84 years	2,294,374	0.7
85 years and over	1,789,679	0.6
Median age (years)	35.8	(X)
16 years and over	118,315,377	38.3
18 years and over	113,836,190	36.9
21 years and over	106,880,414	34.6
62 years and over	22,015,876	7.1
65 years and over	17,362,960	5.6
Female population	156,964,212	50.8
Under 5 years	9,881,935	3.2
5 to 9 years	9,959,019	3.2
10 to 14 years	10,097,332	3.3
15 to 19 years	10,736,677	3.5
20 to 24 years	10,571,823	3.4
25 to 29 years	10,466,258	3.4
30 to 34 years	9,965,599	3.2
35 to 39 years	10,137,620	3.3
40 to 44 years	10,496,987	3.4
45 to 49 years	11,499,506	3.7
50 to 54 years	11,364,851	3.7
55 to 59 years	10,141,157	3.3
60 to 64 years	8,740,424	2.8
65 to 69 years	6,582,716	2.1
70 to 74 years	5,034,194	1.6
75 to 79 years	4,135,407	1.3
80 to 84 years	3,448,953	1.1
85 years and over	3,703,754	1.2
Median age (years)	38.5	(X)
16 years and over	124,960,128	40.5
18 years and over	120,727,881	39.1
21 years and over	114,078,439	36.9
62 years and over	27,956,305	9.1
65 years and over	22,905,024	7.4
RACE		
Total population	308,745,538 (r38234)	100.0
One Race	299,736,465	97.1
White	223,553,265	72.4
Black or African American	38,929,319	12.6
American Indian and Alaska Native	2,932,248	0.9
Asian	14,674,252	4.8
Asian Indian	2,843,391	0.9
Chinese	3,347,229	1.1
Filipino	2,555,923	0.8
Japanese	763,325	0.2
Korean	1,423,784	0.5
Vietnamese	1,548,449	0.5
Other Asian [1]	2,192,151	0.7
Native Hawaiian and Other Pacific Islander	540,013	0.2
Native Hawaiian	156,146	0.1
Guamanian or Chamorro	88,310	0.0
Samoan	109,637	0.0
Other Pacific Islander [2]	185,920	0.1
Some Other Race	19,107,368	6.2

Subject	Number	Percent
Two or More Races	9,009,073	2.9
White; American Indian and Alaska Native [3]	1,432,309	0.5
White; Asian [3]	1,623,234	0.5
White; Black or African American [3]	1,834,212	0.6
White; Some Other Race [3]	1,740,924	0.6
Race alone or in combination with one or more other races: [4]		
White	231,040,398	74.8
Black or African American	42,020,743	13.6
American Indian and Alaska Native	5,220,579	1.7
Asian	17,320,856	5.6
Native Hawaiian and Other Pacific Islander	1,225,195	0.4
Some Other Race	21,748,084	7.0
HISPANIC OR LATINO		
Total population	308,745,538 (r38234)	100.0
Hispanic or Latino (of any race)	50,477,594	16.3
Mexican	31,798,258	10.3
Puerto Rican	4,623,716	1.5
Cuban	1,785,547	0.6
Other Hispanic or Latino [5]	12,270,073	4.0
Not Hispanic or Latino	258,267,944	83.7
HISPANIC OR LATINO AND RACE		
Total population	308,745,538 (r38234)	100.0
Hispanic or Latino	50,477,594	16.3
White alone	26,735,713	8.7
Black or African American alone	1,243,471	0.4
American Indian and Alaska Native alone	685,150	0.2
Asian alone	209,128	0.1
Native Hawaiian and Other Pacific Islander alone	58,437	0.0
Some Other Race alone	18,503,103	6.0
Two or More Races	3,042,592	1.0
Not Hispanic or Latino	258,267,944	83.7
White alone	196,817,552	63.7
Black or African American alone	37,685,848	12.2
American Indian and Alaska Native alone	2,247,098	0.7
Asian alone	14,465,124	4.7
Native Hawaiian and Other Pacific Islander alone	481,576	0.2
Some Other Race alone	604,265	0.2
Two or More Races	5,966,481	1.9
RELATIONSHIP		
Total population	308,745,538 (r38234)	100.0
In households	300,758,215 (r27630)	97.4
Householder	116,716,292	37.8
Spouse [6]	56,510,377	18.3
Child	88,820,256	28.8
Own child under 18 years	64,778,147	21.0
Other relatives	20,411,239	6.6
Under 18 years	7,779,796	2.5
65 years and over	2,941,638	1.0
Nonrelatives	18,300,051	5.9
Under 18 years	1,325,848	0.4
65 years and over	794,726	0.3
Unmarried partner	7,744,711	2.5
In group quarters	7,987,323 (r25736)	2.6
Institutionalized population	3,993,659	1.3

Subject	Number	Percent
Male	2,716,877	0.9
Female	1,276,782	0.4
Noninstitutionalized population	3,993,664	1.3
Male	2,141,333	0.7
Female	1,852,331	0.6
HOUSEHOLDS BY TYPE		
Total households	116,716,292 (r405)	100.0
Family households (families) [7]	77,538,296	66.4
With own children under 18 years	34,743,604	29.8
Husband-wife family	56,510,377	48.4
With own children under 18 years	23,588,268	20.2
Male householder, no wife present	5,777,570	5.0
With own children under 18 years	2,789,424	2.4
Female householder, no husband present	15,250,349	13.1
With own children under 18 years	8,365,912	7.2
Nonfamily households [7]	39,177,996	33.6
Householder living alone	31,204,909	26.7
Male	13,906,294	11.9
65 years and over	3,171,724	2.7
Female	17,298,615	14.8
65 years and over	7,823,965	6.7
Households with individuals under 18 years	38,996,219	33.4
Households with individuals 65 years and over	29,091,122	24.9
Average household size	2.58	(X)
Average family size [7]	3.14	(X)
HOUSING OCCUPANCY		
Total housing units	131,704,730 (r15031)	100.0
Occupied housing units	116,716,292 (r405)	88.6
Vacant housing units	14,988,438 (r10625)	11.4
For rent	4,137,567	3.1
Rented, not occupied	206,825	0.2
For sale only	1,896,796	1.4
Sold, not occupied	421,032	0.3
For seasonal, recreational, or occasional use	4,649,298	3.5
All other vacants	3,676,920	2.8
Homeowner vacancy rate (percent) [8]	2.4	(X)
Rental vacancy rate (percent) [9]	9.2	(X)
HOUSING TENURE		
Occupied housing units	116,716,292 (r405)	100.0
Owner-occupied housing units	75,986,074	65.1
Population in owner-occupied housing units	201,278,493	(X)
Average household size of owner-occupied units	2.65	(X)
Renter-occupied housing units	40,730,218	34.9
Population in renter-occupied housing units	99,479,722	(X)
Average household size of renter-occupied units	2.44	(X)

(r38234) This count has been revised.
Revised count: **308,746,065**
Revision date: **01-31-2014**
For more information, see 2010 Census Count Question Resolution.

(r27630) This count has been revised.
Revised count: **300,758,672**
Revision date: **01-31-2014**

(r27630) For more information, see 2010 Census Count Question Resolution.

(r25736) This count has been revised.
Revised count: **7,987,393**
Revision date: **01-31-2014**
For more information, see 2010 Census Count Question Resolution.

(r405) This count has been revised.
Revised count: **116,716,467**
Revision date: **01-31-2014**
For more information, see 2010 Census Count Question Resolution.

(r15031) This count has been revised.
Revised count: **131,704,954**
Revision date: **01-31-2014**
For more information, see 2010 Census Count Question Resolution.

(r10625) This count has been revised.
Revised count: **14,988,487**
Revision date: **01-31-2014**
For more information, see 2010 Census Count Question Resolution.

X Not applicable.
[1] Other Asian alone, or two or more Asian categories.
[2] Other Pacific Islander alone, or two or more Native Hawaiian and Other Pacific Islander categories.
[3] One of the four most commonly reported multiple-race combinations nationwide in Census 2000.
[4] In combination with one or more of the other races listed. The six numbers may add to more than the total population, and the six percentages may add to more than 100 percent because individuals may report more than one race.
[5] This category is composed of people whose origins are from the Dominican Republic, Spain, and Spanish-speaking Central or South American countries. It also includes general origin responses such as "Latino" or "Hispanic."
[6] "Spouse" represents spouse of the householder. It does not reflect all spouses in a household. Responses of "same-sex spouse" were edited during processing to "unmarried partner."
[7] "Family households" consist of a householder and one or more other people related to the householder by birth, marriage, or adoption. They do not include same-sex married couples even if the marriage was performed in a state issuing marriage certificates for same-sex couples. Same-sex couple households are included in the family households category if there is at least one additional person related to the householder by birth or adoption. Same-sex couple households with no relatives of the householder present are tabulated in nonfamily households. "Nonfamily households" consist of people living alone and households which do not have any members related to the householder.
[8] The homeowner vacancy rate is the proportion of the homeowner inventory that is vacant "for sale." It is computed by dividing the total number of vacant units "for sale only" by the sum of owner-occupied units, vacant units that are "for sale only," and vacant units that have been sold but not yet occupied; and then multiplying by 100.
[9] The rental vacancy rate is the proportion of the rental inventory that is vacant "for rent." It is computed by dividing the total number of vacant units "for rent" by the sum of the renter-occupied units, vacant units that are "for rent," and vacant units that have been rented but not yet occupied; and then multiplying by 100.
Source: U.S. Census Bureau, Census 2010 Summary File 1, Tables P5, P6, P8, P12, P13, P17, P19, P20, P25, P29, P31, P34, P37, P43, PCT5, PCT8, PCT11, PCT12, PCT19, PCT23, PCT24, H3, H4, H5, H11, H12, and H16.

Source: U.S. Census Bureau, 2010 Census.

Unofficial Recommendations and Requests from the National Advisory Committee on Racial, Ethnic and Other Populations

Attachment 8

Administrative Records Inventory

Federal, State or Third Party	Agency Name	Data Provider	Data Type
Federal	Treasury	Internal Revenue Service (IRS)	Form 1040 Returns
			Form 1099 Returns (Information Returns)
Federal	Social Security Administration	Social Security Administration (SSA)	Numident
			Master Beneficiary Record (MBR)
			Supplemental Security Income Record (SSR)
Federal	Housing and Urban Development (HUD)	Housing and Urban Development (HUD)	Public Indian Housing Information Center (PIC)
			Tenant Rental Assistance Certification Center (TRACS)
			Computerized Homes Underwriting Management Syste
Federal	Health and Human Services (HHS)	Center for Medicare and Medicaid Services (CMS)	Medicare Enrollment Database (EDB)
			Medicaid Statistical Information System (MSIS)
			Medicaid Analytic Extract (MAX)
		Indian Health Service (IHS)	Patient Registration
		Administration for Children and Families (ACF)	Temporary Assistance for Needy Families (TANF)
			Child Care Development Fund (CCDF)
Federal	Office of Personnel Management (OPM)	Office of Personnel Management (OPM)	Central Personnel Data File
Federal	Selective Service	SSS	Registration File
Federal	United States Postal Service	USPS	National Change of Address (NCOA)
Federal	Department of Justice	Bureau of Prisons	Releases, Residential Address, Release Address
State	Unemployment Insurance	Unemployment Insurance	All States, DC and PR
State	Supplemental Nutritional Assistance Program (SNAP)	Supplemental Nutritional Assistance Program (SNAP)	CO, IL, HI, MD, NY, OR, VA
	Low Income Energy Assistance Program (LEAP)	Low Income Energy Assistance Program (LEAP)	CO
	Special Supplemental Nutritional Assistance Program for Women, Infants and Children (WIC)	Special Supplemental Nutritional Assistance Program for Women, Infants and Children (WIC)	CO, NV
	Temporary Assistance for Needy Families (TANF)	Temporary Assistance for Needy Families (TANF)	MD, WI, NY
	Child Care Development Fund (CCDF)	Child Care Development Fund (CCDF)	WI

Federal, State or Third Party	Agency Name	Data Provider	Data Type
	Permanent Fund Data	Permanent Fund Data (PFD)	AK
Third Party	Third Party	Corelogic	
		Experian	
		Infogroup	
		Melissa	
		Targus/Neustar	
		VSGI	
		DAR Partners	
		RealtyTrac	
		Commercial Real Estate Information (REIS)	
		MDR Education (Market Data Retrieval - A Division of Dun & Bradstreet)	
		National Exchange Carrier Association - Company Code Assignment (NECA CCA)	
		Health and Retirement Study Markety System Group (HRS-MSG)	

Years Available
2000-2014
2003-2014
Cumulative from 1998
2015
2010-2015
1999-2014
1999-2014
2000-2010
1999-015
2011
2002-2005
1999-2015
2000, 2002-2014
2004-2011
1990-2015
1999-2015
2010-2016
1999-2015
1990-2014
CO 2012-2013, IL 2004-2005, HI 2013-2014, MD 1999-2002, 2009, NY 2007-2012, OR 2009-2014, VA 2009-2013
2009-2014
CO 10/2011-8/2015, NV 2006-2014
MD 2009-11/2015, NY 2007-2012, WI 2014
2008-2009

Years Available
2015
2006-2010, 2014
2010-2011
2010-2011
2010-2011
Address 2010-2011, Federal Consumer 2010-2015, Wireless 2010-2015
2010-2015
2015
2005-2011
2014
2011-2016
2013-2015
2013