WORKING PAPER · NO. 2021-102

Errors in Reporting and Imputation of Government Benefits and Their Implications

Pablo A. Celhay, Bruce D. Meyer, and Nikolas Mittag

AUGUST 2021

Becker Friedman Institute
FOR ECONOMICS AT UCHICAGO

5757 S. University Ave.
Chicago, IL 60637
Main: 773.702.5599
bfi.uchicago.edu

ERRORS IN REPORTING AND IMPUTATION OF GOVERNMENT BENEFITS AND THEIR IMPLICATIONS

Pablo A. Celhay
Bruce D. Meyer
Nikolas Mittag

August 2021

This paper, which has been subject to a limited Census Bureau review, is released to inform interested parties of research and to encourage discussion. Any opinions and conclusions expressed herein are those of the authors and do not represent the views of the U.S. Census Bureau, the New York Office of Temporary and Disability Assistance (OTDA), AEI, or the NBER. All results were approved for release by the Disclosure Review Board of the U.S. Census Bureau, with approvals dated August 3, 2015 and August 18, 2016. We thank Matthew Stadnicki for superb research assistance, Michael Davern, Jonathan Rothbaum, Frank Limehouse, Derek Wu and the Census Bureau for their help and audiences at the Joint Statistical Meetings, APPAM and the University of Manchester for their comments. We thank the Russell Sage Foundation, the Alfred P. Sloan Foundation, the Charles Koch Foundation and the Menard Family Foundation for their support. Mittag is also thankful for financial support from the Czech Academy of Sciences (through institutional support RVO 67985998), the Czech Science Foundation (grant number 20-27317S) and Charles University (UNCE/HUM/035).

Errors in Reporting and Imputation of Government Benefits and Their Implications
Pablo A. Celhay, Bruce D. Meyer, and Nikolas Mittag
August 2021
JEL No. C81,D31,I32,I38

ABSTRACT

We document the extent, nature, and consequences of survey errors in cash welfare and SNAP receipt in three major U.S. household surveys. We find high rates of misreporting, particularly failure to report receipt. The surveys inaccurately capture patterns of multiple program participation, even though there is little evidence of program confusion. Error rates are higher among imputed observations, which account for a large share of false positive errors. Many household characteristics have significant effects on both false positives and false negative errors. Error rates sharply differ by race, ethnicity, income and other household characteristics. The errors greatly affect models of program receipt and estimated effects of income and race are noticeably biased. We examine error due to item non-response and imputation, as well as whether imputation improves estimates. Item non-respondents have higher receipt rates than the population conditional on covariates. The assumptions for consistent estimates in multivariate models fail both when excluding item non-respondents and when using the imputed values. In binary choice models of program receipt, linked data estimates favor excluding item non-respondents rather than using imputed values. Biases are well predicted by the error patterns we document, helping researchers make informed decisions on whether to use imputed values.

Pablo A. Celhay
School of Government
and Economics Institute
Pontificia Universidad Católica de Chile
Avda. Vicuña Mackenna 4860
Macul, Santiago
Chile
pablocelhay@gmail.com

Bruce D. Meyer
Harris School of Public Policy
University of Chicago
1307 E 60th Street
Chicago, IL 60637
and NBER
bdmeyer@uchicago.edu

Nikolas Mittag
CERGE-EI
Politických vězňů 7
Praha
Czech Republic
nikolas.mittag@cerge-ei.cz

1. Introduction

Both policy makers and academics heavily rely on household surveys. Survey statistics such as the rates of unemployment and poverty are the basis of important policy decisions. Survey data are also frequently used in more complex policy analyses such as documenting distributional impacts of taxes and transfers or assessing the likely consequences of new legislation (e.g. CBO 2013,2015). Models of transfer receipt help to increase take-up and better target transfers (e.g. U.S. GAO, 2004). The surveys we examine here are also frequently used in academic studies of transfer receipt (e.g. Blank and Ruggles, 1996; Ganong and Liebman, 2018). However, key household surveys suffer from an alarming and growing extent of survey error and few studies attempt to correct for this error (Meyer, Mittag and Goerge, forthcoming). Among others, survey error affects key variables, such as income, education, employment and health insurance coverage.[1] For the case of government transfers, some recent studies link administrative records of key transfer programs to survey data to examine survey error.[2] They document substantial survey error arising from item non-response and measurement error. The errors are large and systematically related to other variables in the surveys, so they severely bias studies of poverty and program receipt as well as analyses of the safety net and its effectiveness (e.g. Meyer and Wu, 2018; Meyer and Mittag, 2019a).

Still, past studies leave many important questions unanswered, mainly because the relatively small linked samples used to date tend to yield imprecise results and often do not allow for more detailed analyses. The studies consistently document high error rates and systematic underreporting but vary in their findings on the nature and extent of misreporting even when analyzing the same program in the

[1] See Abowd and Stinson (2013), Bound and Krueger (1991), Bollinger (1998), Hokayem, Bollinger and Ziliak (2015), Bollinger et al. (2019) and Dahl, DeLeire and Schwabish (2011) for income, Black, Sanders and Taylor (2003) for education, Poterba and Summers (1986) and Chua and Fuller (1987) for employment and Davern et al. (2008) for health insurance coverage. Bound, Brown and Mathiowetz (2001) provide an overview of earlier studies.

[2] Studies of errors using linked data include Marquis and Moore (1990), Taeuber et al. (2004), Kirlin and Wiseman (2014), Meyer, Mittag and Goerge (forthcoming) for the Supplemental Nutrition Assistance Program, Lynch et al. (2007) for Public Assistance and Nicholas and Wiseman (2010), Gathright and Crabb (2014), Bee and Mitchell (2017) for social security and pensions.

same survey. The high and systematic error rates affect standard analyses, such as program take-up and poverty, but neither theory nor empirical evidence provides much guidance on the consequences of survey error for applied researchers. Most studies also focus on overall measures of error and do not examine different sources of error such as misreporting and item non-response separately. Consequently, little is known about the effects of item non-response and imputation error despite their importance.

We study survey error in unprecedented detail by comparing survey data to an accurate measure of program receipt at the household level. To obtain such an (approximate) measure of truth, we link administrative records from two important transfer programs, Supplemental Nutrition Assistance (SNAP) and Public Assistance (PA), to three large household surveys, the American Community Survey (ACS), the Current Population Survey Annual Social and Economic Supplement (CPS) and the Survey of Income and Program Participation (SIPP). Our administrative records are exceptionally accurate and cover the entire population of program recipients in a large state, New York State (NY) over six years. The linked data provide us with the sample sizes and detail necessary to make progress on several open questions.

In particular, we first confirm high error rates for both programs in all surveys. Validating receipt of two programs shows that the surveys also misrepresent patterns of participation in multiple transfer programs. This error pattern casts doubt on the suitability of survey data to analyze crucial questions, such as whether the multitude of transfer programs jointly form an effective safety net and how people navigate this complex welfare system. We then analyze the nature of survey errors by examining which household characteristics predict over- and underreporting of transfer receipt. Contrary to prior studies (Bollinger and David 1997, Meyer, Mittag and Goerge, forthcoming), our large samples yield precise estimates of the determinants of both over- and underreporting. We show that household characteristics such as income, employment, reported receipt of other transfers as well as demographics, such as gender, race and ethnicity are strong and reliable predictors of survey errors. These variables are crucial to important analyses, such as program targeting, poverty and the distribution of income. Their relation to survey errors suggests bias that may skew substantive conclusions and renders even instrumental variable methods inconsistent. In lieu of general results on bias from non-classical measurement error, we examine the bias for binary choice models of program receipt. We indeed find sizeable bias in key predictors of program receipt, but despite large and systematic error, survey estimates preserve several substantive conclusions. As the theory in Hausman, Abrevaya and Scott-Morton (1998) and Meyer and Mittag (2017) suggest, this stability is mainly due to a reliable tendency for estimates to be attenuated.

Finally, we examine item non-response and the merits of imputation as a remedy. Both whether item non-response is independent of the true outcome conditional on covariates and whether imputations accurately reproduce (the distribution of) actual outcomes is unknown, but crucial to the consistency of estimates. More pragmatically, researchers frequently need to decide whether to restrict their sample to respondents or to include imputed observations without much evidence on the relative merits of each strategy. This lack of evidence is likely due to the fact that studying these issues requires an accurate measure of the outcome for both respondents and (item) non-respondents, which is rarely available. Our linked data provide us with such an accurate measure and the large sample necessary to study item non-respondents. We first examine the extent to which item non-respondents differ from the overall population and how imputed program receipt differs from actual receipt. In both cases, we soundly reject the assumptions usually required for consistency. Thus, data users face a choice between two strategies neither of which yields estimates close to the truth even in large samples. For models of program receipt, we provide evidence that using only respondents likely leads to less bias and thus seems preferable. More generally, our results suggest that analyses of the nature of item non-response and imputation error, such as the ones we conduct allow researchers to make more informed decisions whether to use observations with imputed values or not.

The next section describes our data sources and linkage. Section 3 analyzes the extent of misclassification in program receipt and participation in multiple programs. Section 4 examines which covariates predict survey errors. Section 5 studies misclassification bias in models of program receipt. Section 6 analyzes item non-response and imputation. Section 7 summarizes our conclusions.

2. Data Sources and Linkage

Studying survey error at the household level requires an accurate measure of the variable of interest for each household. Linking administrative records to survey data can provide this measure. We use the same data as Celhay, Meyer and Mittag (2017), Meyer and Mittag (2019a, forthcoming) and Mittag (2019), who provide further detail on the data sources, the linkage process and the accuracy of the final linked data.

a. Survey data

We study measurement error in program receipt in three major U.S. household surveys: the ACS, the CPS and the SIPP. The ACS is the largest household survey in the U.S., with more than 290,000 households selected each month to participate. We use the ACS for calendar years 2008 through 2012. The CPS is one of the most important economic surveys in the U.S. with 60,000 households participating in the survey each month of the year. It is the official source of labor force statistics. We use the Annual Social and Economic Supplement (ASEC) to the CPS for calendar years 2007 to 2012. The ASEC has a sample of about 98,000 households, and is the official source of income and poverty statistics in the U.S. Finally, the SIPP is the highest quality source of information on low income households and the receipt of government transfers. We use wave 10 through 12 of the 2004 SIPP panel (covering calendar year 2007) and wave 1 to 14 of the 2008 SIPP panel (covering August 2008 to December 2012). Both panels sampled approximately 50,000 households intended to be surveyed for a period of 4 years. For all three surveys, our sample is households in NY state, because our administrative records provide us with an accurate measure of receipt for them.

The three surveys are large-scale, general interest surveys, which makes them similar in survey design. Yet, there are also pronounced differences in survey design features known to be related to both non-response and measurement error. See Celhay, Meyer and Mittag (2017) for a discussion. The ACS questionnaire is administered by mail/internet, telephone, or in-person interview. The CPS conducts interviews in person and by phone. While the ACS and CPS only interview one household member, the SIPP strives to conduct in-person interviews with every member of the household over age 15 every four months. The documentation of the surveys (U.S. Census Bureau, 2006, 2008, 2014) provides further detail.

In terms of information on government transfers, all three surveys ask about receipt of SNAP and PA. For both programs, the questions in the ACS refer to the 12 months prior to the interview date. The CPS asks about the previous calendar year. The SIPP asks for monthly information in the four months before the interview. A large extent of misreporting in the SIPP is known to stem from seam bias, i.e. respondents correctly report receipt of the program, but in the wrong months within each four-month wave (Moore, 2008). To focus on whether households report or not rather than the timing of reports, we aggregate the monthly information on program receipt to one observation on receipt anytime during the four-month wave, like Ribar (2005) and Acs, Phillips, and Nelson (2005). We then analyze each wave as a separate cross section.

Following Meyer, Mittag and Goerge (forthcoming), we analyze program receipt at the household level. We consider a household to receive a program according to the survey if any member reports program receipt at any time during the reference period. For the ACS and CPS, we thereby exactly match the survey question. For the SIPP, we aggregate the individual responses to one observation per household to obtain a variable for receipt by any household member.

Item non-response rates differ substantially between surveys and questions. For SNAP, our sample of imputed observations accounts for 1.1 percent of the population in the ACS, 13 percent in the CPS and 7.2 percent in the SIPP. The rates for PA are similar in the CPS (13 percent) and SIPP (7.1 percent), but higher in the ACS (6.1 percent). All three surveys impute missing values using a hot deck procedure. In short, the hot deck sorts observations into cells based on categorical variables reported in the survey. If a value is missing for an observation, the value of a respondent from the same cell is assigned to this observation instead. The details of the implementation vary across surveys and variables. For example, for SNAP, the ACS constructs cells based on few demographic characteristics (family type, presence of children, poverty status, and the race of the reference person). However, the ACS hot deck incorporates detailed geographic information by only using values from the same state and assigning the value of the most recent respondent in the corresponding cell at the smallest geographic level available. In contrast, the CPS hot deck for SNAP classifies households into a much larger number of cells (648), but at the national level. Different from its processing of SNAP, the CPS imputes PA jointly with other missing income components from a single donor. The SIPP hot deck uses a comparable number of cells (864) to impute SNAP at the national level, but also incorporates some geographic information and restricts imputed values to come from the same wave. For more details see U.S. Census Bureau (2006, 2008 and 2014) for the CPS, SIPP, and ACS, respectively, and Meyer, Mittag and Goerge (forthcoming) for a summary.

b. Administrative Data and Data Linkage

Our administrative data are records of payments from the NY Office of Temporary and Disability Assistance (OTDA) for all SNAP and PA recipients in the state. They include monthly payment amounts and dates, as well as basic demographic information and addresses from 2007 through 2012. The availability of monthly information allows us to exactly match the reference periods of each survey. The accuracy of the individual identifiers and amounts paid is crucial to the validity of our estimates and they appear to be of high quality. As part of eligibility determination, the individual information in these records is checked against social security records by OTDA. The data are from actual payments and audited. For SNAP, estimates of total amounts paid are also published by OTDA and the Burau of Economic Analysis (BEA). The overall total from our administrative SNAP records matches aggregate reports by OTDA almost exactly and differs from the BEA numbers by less than a percent in all years.[3] That these numbers are virtually identical provides additional evidence of the accuracy of our administrative microdata.

[3] Published aggregates comparable to our administrative PA records are not available.

We link the administrative data to the three surveys at the household level using person identifiers created by the Person Identification Validation System (PVS) of the U.S. Census Bureau. Wagner and Layne (2014) discuss the PVS in detail. In short, the PVS uses the person data (such as address, name, gender, and date of birth) from the administrative records and survey data to search for a matching record in a reference file that contains all transactions recorded against a social security number (SSN). If a matching record is found, the SSN of the record from the reference file is transformed into a protected identification key (PIK)[4] and attached to the corresponding records in our data. For the administrative data, a PIK is obtained for more than 99 percent of the records from each program. The administrative data include records for each recipient person, so we can link the information from a program case to the correct survey household if any true recipient in the household is assigned a PIK.[5] Therefore, we consider a household to have a PIK if a PIK was obtained for someone in the household. The household-level PIK rates of our survey sample are 93 percent in the ACS, 91 percent in the CPS, and 95 percent in the SIPP.

We cannot validate receipt information for survey households without a PIK, so all analyses are based on the survey sample with a PIK (the "linked data"). Despite the low rate of missed links, PIKs are not missing completely at random in the survey data. To restore representativeness of the linked data for the NY survey population, we use inverse probability weighting (Wooldridge 2007) as in Meyer and Mittag (2019a, forthcoming). To do so, we estimate probit models to predict the probability that a household has a PIK and multiply the survey weights by the inverse of this predicted probability. This correction assumes that conditional on the covariates in the probit model, whether a household has a PIK or not does not predict receipt or reporting. Meyer and Mittag (forthcoming) discuss this assumption. As the high rate of PIK-linking suggests, our results do not appreciably change when using the unadjusted weights.

[4] PIKs are perturbed SSNs used to protect the anonymity of individuals in the data.
[5] We cannot link households in which all members with a PIK are true non-recipients, but there are true recipients among those without a PIK. Usually only few PIKs are missing per household, as 89 percent of individuals have a PIK in the ACS and 86 percent in the CPS and SIPP, and only few non-recipients cohabit with recipients. See Meyer, Mittag and Goerge (forthcoming) for arguments why these exceptions should be uncommon.

3. The Extent of Survey Errors

a. Misclassification of Program Receipt

We first use our accurate measure of program receipt to examine the extent of misclassification. Prior research has found high rates of false negatives, i.e. recipient households who fail to report receipt. False positives, i.e. non-recipient households reporting program receipt are usually much less frequent. For example, Meyer, Mittag and Goerge (forthcoming) find false negative rates of 23 to 48 percent and false positive rates below 1.5 percent for SNAP in Illinois and Maryland in earlier years of the surveys we examine here. These numbers lead to low net reporting rates (the ratio of the weighted number of survey recorded recipient households to administrative recipient households in the linked data). Table 1 reports population estimates of the false negative, false positive and the net reporting rates of SNAP and PA for NY in the ACS, the CPS and the SIPP. The uppermost panel presents error rates for the entire sample, while the middle panel restricts the sample to respondents and the lowermost panel restricts the sample to item non-respondents. Appendix Table A3 provides crosstabulations of administrative and reported receipt including error rates conditional on reported receipt status and unconditional rates.

All false negative rates in columns 1 and 2 of Table 1 are high but the magnitudes vary between surveys. For SNAP, the CPS misses 42 percent of true recipients. The ACS and SIPP more closely capture receipt, but 26 and 19 percent of true recipients, respectively, still fail to report SNAP receipt. For PA, the surveys are ranked in the same order of accuracy, but the rates are substantially higher. At the high end, the CPS misses almost two out of three households receiving PA. The ACS misses 57 percent of recipients. Even the most accurate survey, the SIPP, misses almost every other recipient household. Nonetheless, the lower two panels of Table 1 show that reports are still more accurate than imputed values in all cases. The error rates among reports in the middle panel are similar to the error rates in the overall sample, because reports make up most of the overall sample. Error rates among imputed observations are high. False negative rates reach 86 percent for PA in the CPS. Even in the best case, SNAP in the SIPP, imputation only classifies a third of all recipient households correctly. The false negative rate is higher among imputed observations than among respondents except for PA in the ACS. As the remaining columns and the results below show, this lower false negative rate comes at the expense of a false positive rate more than 2.5 times as high as in the other surveys, leading the ACS imputations to overstate PA participation.

Table 1: Error and Reporting Rates by Survey and Imputation Status (around here)

The false positive rates in columns 3 and 4 of Table 1 are much lower, but are percentages of the much larger population of non-recipients so they still indicate a substantial number of misclassified observations. Contrary to the case of false negatives, neither surveys nor programs can be ranked in terms of accuracy. The ACS has the lowest false positive rate for SNAP among the three surveys, but a higher rate for PA than SNAP and higher than that for PA in the other two surveys. In contrast, the false positive rates are much lower for PA than for SNAP in the CPS and SIPP. The high false positive rate for SNAP in the CPS is likely explained by the fact that the U.S. Census Bureau imputes SNAP receipt at a high rate to reduce the problem of underreporting in the CPS. Correspondingly, almost half of the false positives in the CPS are imputed. The lowermost panel of Table 1 shows that false positive rates are generally high among imputed observations. Even excluding SNAP in the CPS, imputed observations account for 10 to 25 percent of all false positives in all cases, although they are just a small fraction of the samples. Without imputed observations, the false positive rates for SNAP become remarkably similar across surveys at around 1.1 percent. However, we should note that some households are likely to be erroneously classified as false positives due to unlinkable administrative records, leading to spurious false positives.

The combination of high false negative rates and low false positive rates leads all three surveys to understate the number of participating households. Receipt rates according to the administrative data are 17.4, 17.8 and 17.7 percent for SNAP, but the recorded rates are 13.9, 12 and 15.5 percent in the ACS, CPS and SIPP, respectively. Similarly, 4, 4.4 and 3.6 percent of the population receive PA according to the administrative data, but only and 3.3, 2.2 and 2.4 percent have recorded receipt in the ACS, CPS and SIPP, respectively. Thus, the surveys substantially understate program participation, ranging from understating SNAP recipients by 12 percent in the SIPP to understating PA receipt by 50 percent in the CPS. However, the net underreporting rates are lower than the high false negative rates may suggest, because of the offsetting effect of false positives. Net underreporting is more severe for PA than for SNAP, because false negative rates are higher and false positive rates lower for PA. As an exception, the ACS captures a higher fraction of PA than of SNAP recipient households, but this exception is due to the high false positive rate among imputed observations that causes net overimputation.[6] Imputations are based on survey reports, so it is not surprising to see net underimputation in all surveys except for PA in the ACS.[7] Despite the high SNAP receipt imputation rate in the CPS, the imputed values still fall short of receipt among item non-respondents by 40 percent.

Overall, these results confirm in another state the findings for reporting of SNAP in Illinois and Maryland in Meyer, Mittag and Goerge (forthcoming). For SNAP, we confirm alarmingly high rates of false negatives that are highest in the CPS, followed by the ACS and the SIPP and much lower rates of false positives, many of which are due to imputation. We find the same pattern of errors to be even more pronounced for a second transfer program, PA. Thereby, our results show that survey error is indeed pervasive and likely to affect how well surveys capture receipt of transfers in general. However, there are also a few differences. SNAP false negative rates in NY are 15-22 percent lower than those in IL and MD, but false positive rates seem to be slightly higher, with the exception of the SIPP. Contrary to Meyer, Mittag and Goerge (forthcoming), the false positive rates we find in the SIPP are similar to the other surveys for SNAP (and low for PA). While the main patterns we find are closely aligned with prior results,

[6] To see why the ACS has such a high net reporting rate, note that PA is a very small program, so a high false positive rate has a large effect on net reporting,

[7] The substantial false positive rates among imputed observations have a much smaller effect on net receipt among imputed observations than the false positives in the general population, because the share of recipients among item non-respondents is much higher than the share in the overall population in all surveys.

the differences raise the question of how stable survey error is across time and geography, an issue partially addressed in Meyer and Mittag (2019b).

b. Reporting of Participation in Multiple Programs

It is important to understand how the multitude of U.S. transfer programs interact and whether they jointly form an effective safety net. To obtain even basic facts such as how many individuals depend on this safety net, we need surveys to correctly capture patterns of participation in multiple programs. Our linked data contain accurate measures of receipt for both SNAP and PA, so we can examine whether the survey data correctly reproduce the joint distribution of program receipt. Table 2 summarizes joint receipt rates for SNAP and PA according to the administrative and the survey variables in the linked data. Patterns of participation are similar across surveys when using the administrative receipt variables, but the survey data poorly reflect these patterns of participation. The first two columns show that all surveys understate how many households depend on government transfers. The share of households receiving either program is 20, 46 and 14 percent higher than the ACS, CPS and SIPP suggest. As the lower panel of Table 2 shows, the understatement of the number of households reached by SNAP or PA is even more severe among imputed observations. These differences are sizeable, but smaller than suggested by the rates of underreporting for each individual program. The surveys yield more accurate estimates of receipt rates when looking at receipt of either program, than when looking at each program separately.

This pattern arises from survey reports understating dependence in program participation, i.e. from understating joint program participation more severely than participation in one program only. Columns 3 to 6 of Table 2 report the fraction of households receiving only SNAP or only PA, but not the other program. All three surveys still understate the fraction of households receiving SNAP, but not PA. However, the difference is smaller than the difference for all SNAP recipients in Table 1. The surveys overstate the fraction of households receiving PA only.[8] Both biases are even more pronounced in the imputed sample. Instead, columns 7 and 8 show that the surveys understate the fraction of households receiving both programs by slightly more than one-third (ACS and SIPP) and more than one-half (CPS). The surveys also underimpute joint program receipt. Imputations in the ACS and SIPP capture the probability of receiving both programs better than the survey reports but fall further short of it in the CPS.

Table 2: Joint Receipt Rates of SNAP and PA According to Survey Reports and Administrative Records (around here)

These differences are partly driven by the high rates of underreporting but reinforced by a downward bias in the survey estimates of the probabilities of receiving the second program given receipt of the first. This problem is pronounced for the probability of receiving SNAP conditional on receiving PA. Households receiving PA are categorically eligible for SNAP receipt in NY, so as one would expect, most households that receive PA also receive SNAP (96 percent in the ACS and CPS, and 94 percent in the SIPP). However, the probability of reporting PA conditional on reporting SNAP is 77, 84 and 89 percent in the ACS, CPS and SIPP respectively. Thus, survey errors can explain the puzzling fact that many eligible households do not seem to receive all programs for which they are likely to be eligible.[9]

The linked data allow us to examine how survey errors at the household level lead to these aggregate differences in the joint distribution of program receipt. Table 3 summarizes error rates conditional on the number of programs the household receives according to our administrative measure of participation. For each group, we omit the column corresponding to those who correctly reported receipt. The upper panel presents error rates for the entire sample. The lower panel restricts the sample to item non-respondents. The rates of erroneously reporting one program only in columns 1 and 7 show

---

[8] Separately looking at overreporting of each program explains the overstatement of receipt of PA only, which seems surprising given the high false negative rate among PA recipients. However, the overstatement is due to false positives among households receiving neither program. The fraction of actual PA recipients who report PA only is small in all samples.

[9] The high probability of receiving SNAP when receiving PA suggests that imputing or probing for SNAP receipt for those reporting PA may be worth examining as a strategy to improve survey accuracy.

that the high reported rates of receiving only one program we document above is driven by both recipients of no program and recipients of two programs often reporting one program. The overreporting rates in column 5 are higher than those in columns 1 and 2, but the population share of non-recipients is much larger than the population share of recipients of one program. Thus, most false positives are due to non-recipients recorded as recipients of one program. However, we should emphasize again that some households are likely to be erroneously classified as false positives due to unlinkable administrative records, leading to spurious false positives.

Column 4 reports the fraction of households that receive one program according to the administrative variable and report receipt of the other program in the survey. Both the rate among those receiving one program (0.1 to 0.8 percent) and the share in the population (0.01 to 0.1 percent) are small. Consequently, program confusion is rare and plays a minor role for aggregate error rates. However, SNAP and PA differ in very salient ways. --Program confusion may play a larger role for programs that operate in more similar ways. The rate of imputing the wrong program is two to four times higher, which is not surprising given that the recipient populations are similar.

Table 3: Reporting of Participation in Multiple Programs (around here)

Finally, columns 6 and 7 show that the fraction of households who correctly report both programs is low at 35 to 52 percent. Error rates are high for both programs, so getting both programs right is relatively uncommon. In fact, those receiving both programs report more accurately than the overall population: with the exception of PA in the SIPP, the false negative rates among those receiving both programs are lower than the false negative rates in the entire population. Most people who report two programs actually receive two programs, with a rate close to 80 percent in the CPS and SIPP and 63 percent in the ACS.

Errors are more frequent among imputed observations than among respondents in the CPS and SIPP throughout. Surprisingly, the ACS imputations for households that receive both programs are more accurate than the ACS reports of respondents who receive both programs. While the error rates in imputations are high, one may have expected worse for participation in multiple programs given that the imputation procedures use little information on other programs. It is important to keep in mind that our sample of imputed observations includes households where the entire income record was imputed in the CPS and households where some, but not all household members refused to answer the question on program receipt in the SIPP. Such observations preserve the reported patterns of joint participation. However, the fact that we do not find the imputations to be systematically worse in the ACS without this feature than in the other surveys suggests that these observations are not driving the patterns we find.

4. The Determinants of Survey Errors

We next examine how misreporting of SNAP and PA receipt differs between households. The previous section shows that misreporting rates differ by true receipt status. Therefore, we examine the determinants of misreporting separately for recipients and non-recipients according to the administrative variable. For both groups, we estimate probit models for each program that relate whether receipt status of the household is misclassified to household composition, demographics, economic characteristics, language, reported receipt of other transfer programs and a time trend. We use the entire linked survey sample including imputed observations in order to provide a description of the accuracy of the entire data. Table 4 reports probit marginal effects of the determinants of failure to report SNAP or PA among recipient households. Table 5 reports probit marginal effects of the determinants of reporting receipt among non-recipient households.

Table 4: The Determinants of Mis-reporting, False Negatives, Probit Average Derivatives, Full Linked Sample (around here)

Table 5: The Determinants of Mis-reporting, False Positives, Probit Average Derivatives, Full Linked Sample (around here)

Overall, the results are well aligned with the findings of Meyer, Mittag and Goerge (forthcoming), who estimate similar models[10] for SNAP using linked data from IL and MD. We reject the hypothesis that the coefficients are jointly zero with p-values below 0.001 in all models. This rejection confirms that survey errors are systematically related to household characteristics, even conditional on true receipt status. Such a relation invalidates the assumptions of most common error models and corrections for measurement error, so that the bias depends on the covariates in the model and is difficult to correct (Meyer, Mittag and Goerge, forthcoming). The patterns of misreporting are qualitatively similar for the two programs we examine. The marginal effects vary across surveys, but tend to be of similar magnitudes and usually have the same sign for a given program in all surveys. Income, employment, and reported receipt of other government programs as well as race, ethnicity and gender of the householder are key predictors of misreporting. These key predictors are nearly the same as the variables with significant marginal effects on misreporting SNAP in Meyer, Mittag and Goerge (forthcoming). We find only few significant effects to differ in their sign compared to this prior study, maybe most notably the differences in the time trend. However, our much larger sample size yields more precise estimates for specific predictors, which allow us to clarify several points on which the earlier findings were ambiguous.

Our results on household composition are more precisely estimated than those in Meyer, Mittag and Goerge (forthcoming), but vary across surveys and programs. Single households tend to have fewer false negatives, but for PA, single households with children have more false negatives. False negatives tend to decrease with the number of children and increase with the number of adults. The results point to false positive rates increasing with the presence of children and the number of adults in the household. Contrary to Meyer, Mittag and Goerge (forthcoming), we do not find reporting to be better in rural areas.

---

[10] They exclude households with income above twice the poverty line. Mittag (2019) and Meyer and Mittag (2019a) document fairly high error rates among households with income higher than twice the poverty line, so we include these households in the analyses in this section. Parameter estimates of models that exclude these observations are provided in Appendix Tables A4 and A5.

The effects of demographic characteristics of the householder are consistently significant and large for minorities. In line with Meyer, Mittag and Goerge (forthcoming), false negative rates are 4 to 8 percentage points higher among minorities. The effects are pronounced and consistent for households with a black or Hispanic householder, but noisier for other minorities. Our more precise results show that false positive rates are also higher among minorities, with the exception of Hispanics who overreport PA less. Consequently, in most cases minorities report program participation worse and not just less. These sizeable differences in reporting are concerning for the numerous analyses that compare receipt rates across ethnic groups. Contrary to Meyer, Mittag and Goerge (forthcoming), who find large, but imprecise marginal effects for indicator variables for male and disabled householders, we find that false negative rates for households with a male householder are 3 to 8 percentage points higher. At the same time, they have lower false positive rates. Thus, households with a male householder tend to report less program receipt. All effects for a disabled householder go in the opposite direction, i.e. they report more program participation. The reduction in false negatives for the disabled is consistently significant and among the largest effect sizes we find. The effect on false positives is important for SNAP, but noisy for PA.

We do not find evidence of a systematic age gradient for either program but the results suggest that reporting differs for the elderly (60 and older). The effects of age on false negatives are ambiguous, as the elderly have fewer false negatives for SNAP in the ACS and SIPP, but more false negatives for SNAP in the CPS and PA in the SIPP. Yet, the elderly clearly commit fewer false positives. These findings suggest that the low estimated program participation rates of the elderly (Haider, Jacknowitz and Schoeni, 2003, Wu, 2010) are not due to more underreporting. The lower false positive rates make understatement of receipt more severe among the elderly, but any reduction in receipt due to reporting errors stems from better, rather than worse reporting. For education, we find some evidence of an increase in false negatives as education rises for SNAP. False positive rates seem to decrease with education.

Our results on household language and English ability of the householder are noisy and differ between surveys and programs. For example, a householder with poor English skills is associated with more reporting in the ACS, but lower reporting in the SIPP. While one may expect language effects to differ across survey modes, we also find effects in opposite directions within the same survey (e.g. for SNAP and PA false positives in the ACS). As in Meyer, Mittag and Goerge (forthcoming), we find little evidence of systematic differences in reporting by households with a non-citizen householder. Such households have higher false negative rates for SNAP in the ACS. False positive rates are higher in the SIPP, but lower for PA in the CPS.

The economic circumstances of households are among the most reliable predictors of misreporting. Our results confirm that false negatives increase and false positives decrease as reported income relative to the poverty line rises. Thus, households with higher incomes report less program participation. The effects are large and precisely estimated. Whether anyone in the household is employed is significant with only one exception. With differences in false negatives of 3 to 22 percentage points, the differences are large enough to skew substantive conclusions. The differences in false positive rates are sizeable (0.4 to 1.4 percentage points) relative to the low false positive rates. Interestingly, the directions of the associations disagree across surveys, but agree across programs within each survey: Households in which at least one member works have higher false negative rates and lower false positive rates in the ACS and CPS for both programs, but both associations are in the opposite direction in the SIPP. That is, they report less program participation in the ACS and CPS, but more in the SIPP.

Reported receipt of other transfer programs (housing assistance and reported PA/SNAP receipt) is among the strongest predictors of misreporting. The marginal effects are significant in all but one case and consistently show that those who report one program are more likely to report other programs as well. This correlation is positive for both recipients and non-recipients, i.e. reporting receipt of another program is associated with lower false negatives and higher false positives. With lower false negatives of

6 to 20 percentage points for reported housing assistance receipt and 20 to 35 percentage points for reported PA/SNAP, the associations are very strong. The association with false positives are weaker, but large relative to the false positive rates. They are larger for PA/SNAP (1 to 5 percentage points) than for housing assistance (0.3 to 2.2 percentage points). These results are closely aligned with Meyer, Mittag and Goerge (forthcoming).

Whether the problem of misreporting has increased over time is an important question. Meyer, Mittag and Goerge (forthcoming) find that false negative rates are increasing over time and provide suggestive evidence that false positives are becoming more frequent. Our models include linear time trends estimated over the more recent and longer time period our data cover. The estimates confirm that false positive rates have increased over time, but at a slow pace. The relationship is only substantively important for SNAP in the CPS, where the false positive rate has been increasing by almost 0.3 percentage points per year. However, we do not find evidence of increasing false negative rates, even though the time trend is precisely estimated. False negative rates decrease by one percentage point per year for SNAP in the ACS and CPS. For PA, the time trend in the false negative rate is positive in the ACS at 1.5 percentage points per year, but negative and imprecise in the CPS. It is positive, but negligible in magnitude in the SIPP.

5. The Consequences of Survey Errors

Survey users are ultimately mainly concerned with the accuracy of their estimates. The high error rates and their relation to common covariates violate the conditions for consistency of most estimators, but analytic results on the size and direction of bias from such non-classical measurement error are rare and at best case specific. The linked data provide us with an accurate measure of the dependent variable in addition to the reports. Thus, we can analyze the bias from misreporting for any specific estimate by comparing estimates using the reported and the accurate administrative variable. In lieu of more general results, we examine the consequences of survey errors for probit models of program receipt. These models are frequently estimated to analyze program targeting (e.g. Currie 2006, Haider, Jacknowitz and Schoeni 2003). The theoretical results on the bias in Meyer and Mittag (2017) and the prior empirical evidence in Meyer, Mittag and Goerge (forthcoming) allow us to put our findings in context.

Table 6 and Table 7 report results for models of SNAP and PA receipt. In both cases, we restrict the sample to households with income below twice the poverty line and include imputed observations to focus on a commonly used sample for which receipt is likely. For each survey and program, the tables contain three columns: The marginal effects according to the survey reports, the same estimates when using administrative receipt as the dependent variable instead and the p-value of a test of equality. We can reject the hypothesis that all estimates are jointly the same at conventional significance levels in all cases. Consequently, our results show that survey error indeed leads to bias.

However, the results also indicate that qualitative conclusions are similar despite the large and systematic error. Only 42 out of 152 estimated marginal effects are significantly different. So even with the much larger sample, we do not find a larger fraction of significant differences than Meyer, Mittag and Goerge (forthcoming). Rather, the results provide further evidence that coefficient signs are likely to be robust even with high rates of misreporting. In line with Meyer and Mittag (2017), only 15 out of our 152 pairs of point estimates switch sign when we replace the survey reports with the administrative values. The survey estimate is insignificant in 13 of these cases and there is only one case in which both estimates are significant. Thus, the results suggest that it is very rare for the survey data to imply an incorrect direction of the marginal effects.

The results in Meyer and Mittag (2017) also imply a tendency for attenuation, which our results confirm. Overall, 99 out of 137 marginal effects (excluding the 15 marginal effects that change sign) are attenuated when using the survey reports. Only 3 of the marginal effects that are biased away from zero change significantly (at the 10 percent level) when using the administrative variable instead of the survey reports. Thus, most point estimates are indeed attenuated and we can only reject the hypothesis that they are biased toward zero for 3 out of 152 marginal effects. Consequently, our results provide evidence that estimate signs are robust and confirm a strong tendency to attenuation.

Table 6: SNAP Receipt in Survey Data and Combined Data, Probit Average Derivatives, Households with Income less than Twice the Poverty Line (around here)

Table 7: PA Receipt in Survey Data and Combined Data, Probit Average Derivatives, Households with Income less than Twice the Poverty Line (around here)

The survey data reproduce the main qualitative results surprisingly well, but there are some important substantive differences. The most pronounced differences are for households with a black or Hispanic householder, for whom receipt rates of both SNAP and PA are significantly biased downward in all cases. With receipt rates 4 to 8 percentage points higher than the survey indicates, the differences are among the largest ones we find and clearly large enough to skew important conclusions. The survey data also severely understate how quickly PA receipt declines with income, which can help to explain the large differences in PA receipt Meyer and Mittag (2019a) find as income rises. However, despite the importance of income and employment as predictors of misreporting, we do not find any other important differences in how receipt varies with income and employment. As in Meyer, Mittag and Goerge (forthcoming), estimated SNAP receipt rates decline more slowly with income when correcting for misreporting, but the difference is small and not statistically significant.

We also confirm that the surveys understate SNAP and PA receipt by those with children and households with more adults. Contrary to Meyer, Mittag and Goerge (forthcoming), we do not find that the surveys understate receipt by the elderly. For SNAP, we do not find any systematic effect on the age profile. The survey data actually overstate the probability of receipt for the elderly in the ACS and SIPP, although the difference is not significant in the SIPP. This effect is more pronounced for PA, where the surveys overstate receipt by those 70 and older by 3 to 5 percentage points. For both programs, the survey data suggest a slightly flatter decline with education than the administrative data, but the differences are small and few are statistically significant. Despite their large marginal effects on reporting rates, the marginal effects of reported receipt of other programs on PA and SNAP receipt are surprisingly accurate in the survey. So while the surveys severely misrepresent the patterns of multiple program participation in the unconditional analyses above, this problem appears less severe in our multivariate analyses.

From a methodological perspective, the results show that a better understanding of misreporting can help assess and explain the bias in survey estimates in practice. Key predictors of misreporting indeed translate in many cases into large biases in models of receipt. Nevertheless, the effects of other strong predictors of misreporting remain accurate in the survey data. In addition, few marginal effects are significantly different despite the high rates of misreporting. The patterns of misreporting we document can also help to explain this robustness. If misreporting reinforces the true receipt gradient, i.e. when a variable predicts higher true receipt rates and also predicts more false positives and fewer false negatives, two errors partially cancel: The correlation of misreporting with covariates biases estimates away from zero, which reduces the attenuation due to misreporting. Conversely, if a variable predicts lower true receipt rates and also predicts fewer false positives and more false negatives, the errors partially cancel so that misreporting reinforces the (attenuated) true receipt gradient. This relatively benign form of misreporting reliably holds for the variables where we find surprisingly small bias. For example, misreporting consistently reinforces the effect of gender and receiving other programs. Very much in line with our findings, for income, it holds for SNAP, but not for PA. This alignment of misreporting and receipt also contributes to the low fraction of significant differences that we find. While misreporting reinforces receipt for roughly half of the marginal effects, less than one quarter of the significant differences are among these marginal effects. Consequently, one may expect to find more significant differences in applications where the effects of the covariates on survey error and outcomes are less aligned.

Our estimates also show that the theoretical results in Meyer and Mittag (2017) correctly predict key features of the bias in a typical application. Their results not only predict the robustness of signs and the tendency to attenuation we document above, but can also help to understand the exceptions we find. They predict that sign changes become more likely as the probability of misclassification rises. Indeed, the error rates for PA are higher and 11 out of 15 sign changes occur in the models of PA receipt. Meyer and Mittag (2017) also show that attenuation may not hold if misreporting is strongly and systematically related to the covariates, which explains the three marginal effects that are not attenuated: the pattern of receipt rates is reinforced by misreporting we discuss above in all three cases. Consequently, the theoretical results in Meyer and Mittag (2017) are useful to interpret estimates and the conditions under which significant survey estimates likely indicate larger and significant true effects. However, the fact that the patterns predicted by the bias formulas is much clearer with the larger samples here than in Meyer, Mittag and Goerge (forthcoming) also shows that one should be careful when using results on the asymptotic bias to interpret finite sample estimates of parameters.

6. Item Non-response and Imputation

Item non-response, i.e. respondents refusing to answer specific questions, is a pronounced problem for government transfers (Meyer, Mok and Sullivan, 2015) and other sources of income (Bollinger and Hirsch 2006). Applied researchers usually deal with the consequences of item non-response either by excluding observations with missing values or by using imputed values. Both strategies assume that the part of the true response that is not predicted by observed covariates does not predict the outcome of interest. For the problem of item non-response, the response is missing by definition, so with survey data alone these assumptions can only be partially assessed by examining proxy variables. We know even less about the bias when these assumptions fail, which makes it difficult for researchers to decide between the two options. Fortunately, data linkage can provide us with an accurate measure of the variable in question for both respondents and non-respondents. In this section, we first use this accurate measure in our linked data to test the assumptions underlying each strategy. Then we examine and compare the bias from each strategy for a specific case, models of program receipt.

Specifically, we first examine whether item non-response is independent of the response conditional on covariates in the model the researcher estimates. If this assumption (usually referred to as MAR) fails, excluding item non-respondents will lead to bias. Compared to the prior literature that examines the nature and selectivity of non-response (Groves and Cooper 1998, Groves 2001), the linked data allows us to compare respondents and non-respondents in terms of the accurate program receipt variable from the administrative data. We then examine the nature of imputation error. Most imputation procedures require item non-response to be independent of the response conditional on the variables used to predict imputations. The advantage of using linked data to examine this assumption over previous studies (see e.g. Little and Rubin 2002; Andridge and Little 2010) is that the data contains both the imputed receipt status and the accurate receipt status from the administrative data. Thereby, data linkage enables us to compare imputed values to accurate values, just as we compared reported values to the linked administrative variable above.

Applied researchers are often particularly concerned with bias in their estimates, especially in large samples. Our results on the nature of item non-response and imputation error show that neither including imputations nor excluding non-respondents is likely to yield consistent estimates. Therefore, the key question is whether it is better to exclude non-respondents or to include them using the imputations provided in the data. Unfortunately, there is little general advice and considerable disagreement on this issue (see e.g. Angrist and Krueger, 2001). Analytic results on the bias from either strategy do not exist, so we use our linked data to directly examine the bias in a specific case, models of program receipt.

a. Differences Between Respondents and Item Non-Respondents

Using only the sample of respondents yields consistent estimates if non-response is (conditionally) independent of the value of the response. See Heitjan and Rubin (1991) and Heitjan (1994) for discussions. Similar conditions are required for the consistency of most corrections based on respondents only. Chenevert, Klee and Wilkin (2016) and Bollinger et al. (2019) compare administrative values from linked tax records of respondents and non-respondents to SIPP and CPS responses to earned income questions. Both reject the assumption that item non-response is conditionally random. Our linked data put us in a unique position to provide direct evidence on differences, both unconditional and conditional on covariates, between non-respondents and the overall population in terms of SNAP and PA receipt and its association with household characteristics.

Unconditional receipt rates according to the administrative variable are much higher among item non-respondents than in the overall population for both programs in all surveys. In the ACS, they are 16 percentage points higher for SNAP and 4.1 percentage points higher for PA, making receipt among item non-respondents almost twice as likely. The differences are smaller in the CPS (4 percentage points for SNAP and 0.5 percentage points for PA) and the SIPP (5 and 1.5 percentage points). Thus, item non-response is not completely random, so that respondents are not representative of the population. Consequently, excluding item non-respondents will bias estimated receipt rates. However, excluding non-respondents may still yield unbiased estimates of model parameters if non-response is conditionally random. To provide evidence on the presence and likely nature of this bias, we next examine whether these differences between non-respondents and the overall population are captured by covariates commonly used to study program receipt. That is, we test the MAR assumption and study the extent to which the outcomes of respondents and non-respondents differ conditional on covariates.

We conduct two sets of tests of the MAR assumption. We conduct both tests in the framework of probit models where administrative receipt is the dependent variable. First, we test whether the probability of program receipt among non-respondents still differs from the overall population after conditioning on the covariates from section 5 by testing whether the intercept differs by response status. in. Marginal effects of probit models that include an indicator for item non-response are in Appendix Table A6. The test rejects MAR in three of the six cases we examine and shows that unobserved factors causing program receipt are more prevalent among item non-respondents. The differences are smaller than the unconditional differences, but large enough to skew substantive conclusions. Even conditional on the covariates, non-respondents are still more likely to receive both programs in the ACS (by 6 percentage points for SNAP and 1.5 for PA) and SNAP in the SIPP (by 4 percentage points). At less than one percent, the difference is small and insignificant in the CPS for both programs and for PA in the SIPP.

The tests above examine whether non-respondents differ from the overall population in their (conditional) level of receipt. It is likely that not only the rates of receipt, but also the associations of receipt with covariates differ between respondents and non-respondents. If so, sample-selection bias in multivariate models depends on how the effects of the predictors of receipt differ for item non-respondents. If they do not differ, some common multivariate analyses still yield correct conclusions regarding the determinants of program receipt, because only the intercept is biased. Therefore, our second test of MAR examines whether the conditional distribution of program receipt differs between non-respondents and the entire population by testing whether the slope coefficients of the probit models differ by response status: We estimate the models of (administrative) program receipt from section 5 separately for the entire sample and for item non-respondents only and test coefficient equality. We reject the hypothesis that all coefficients are the same for all surveys and both programs. The p-values are below 0.001 for all models except for PA in the SIPP, which has a p-value of 0.017. Consequently, the relation between receipt and some covariates differs between non-respondents and the population, so that excluding non-respondents will affect estimates of the effect of these characteristics.[11]

Table 8: Examining How Non-Respondents Differ From the Population, Coefficients (around here)

[11] The bias and its size depend on the model of interest and are usually intractable. It is plausible that bias arises in general, because the joint distribution of program receipt and the covariates for the entire population, $f(y,X)$ is a mixture of this joint distribution among respondents, $f(y,X|R=1)$ and non-respondents, $f(y,X|R=0)$, where R indicates whether the household provided a response. If they differ, using only respondents includes only one of the components, $f(y,X|R=1)$, which differs from the distribution of interest, $f(y,X)$.

To obtain a better understanding of how the determinants of program receipt differ between non-respondents and the overall population and hence which coefficient estimates are likely to be biased, we also test equality of individual coefficients. Table 8 reports coefficient estimates and the p-values of tests for coefficient equality.[12] We reject the hypothesis that non-respondents are the same as the overall population in their relation between program receipt and income. With the exception of PA in the SIPP, program receipt decreases more slowly with income among item non-respondents. Non-respondents to the question on SNAP who report PA are far less likely to receive SNAP than PA reporters in the population overall and the results suggest that the same pattern holds for reported receipt of housing assistance. Male non-respondents are less likely than male respondents to receive either program in the CPS and there is some evidence of higher receipt rates among non-respondents than respondents when the householder is non-white.

b. How well does imputation reproduce the (distribution of) missing values?

All three surveys include imputed values from the hot deck procedures described in section 2. Even though many researchers use these imputed values, little is known about their accuracy and, more generally, the conditions under which imputation works well. As Andridge and Little (2010) point out, the hot deck requires non-response to be random within each hot deck cell. The previous subsection provides evidence that this assumption does not hold in the three surveys we examine. Thus, it is no surprise that our results from section 3 confirm the finding of Meyer, Mittag and Goerge (forthcoming) that imputation biases estimated receipt rates. With the exception of PA in the ACS, imputation understates true rates of receipt, so that including imputed observations will bias estimates of receipt rates downward.

Many survey users are concerned with estimates of multivariate models rather than population statistics, which raises the question whether the imputed values accurately reproduce the joint

[12] Appendix Table A7 reports the corresponding marginal effects. We examine coefficients here, because differences in marginal effects could also arise from differences in the distributions of covariates between respondents and non-respondents, even when non-response is conditionally random.

distribution of receipt and other covariates among non-respondents. A necessary condition for consistency in multivariate models is that the imputation procedure correctly conditions on the other covariates in the model of interest. If the model that predicts the imputed values omits variables that are included in the model of interest, then the parameter estimates of the model of interest are affected by match bias (Hirsch and Schumacher, 2004). If the imputation model includes all variables in the model of interest, but misspecifies the functional form of the relation between the imputed variable and its predictors (for example by only including a linear term in the imputation model when the true function is quadratic), imperfect match bias (Bollinger and Hirsch 2006) may still affect the parameter estimates.

Like most other applications of hot deck methods, the imputation of transfer receipt only uses a small subset of the covariates of typical models as predictors. However, hot decks usually discretize all predictors and use all possible interactions. These flexible fully interacted models may allow the imputations to capture multivariate relationships well despite using only few predictors. In addition, the hot deck procedures are more complex than the assumed procedure for which Bollinger and Hirsch (2006) derive their results on bias: the ACS and SIPP use geographic information in the imputations in addition to other variables and the CPS uses a sequential hot deck, so that the cell variables can differ between observations. All three surveys simultaneously impute some related variables, such as cash income in the CPS, by assigning a block of responses from the same matched respondent. At the extreme, the CPS sometimes imputes the responses for all ASEC questions from the same matched record for households that provided answers to the other CPS questionnaires, but not the ASEC. Such block or whole imputes preserve correlations of the imputed values better than the theory for the standard hot deck suggests. Thus, it is not clear how applicable existing theory is to the bias in current methods as implemented.

Nevertheless, by showing examples where the effect of including imputed values on parameter estimates conforms to their theoretical expectations, Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006) provide strong evidence that using imputed values in multivariate models is likely problematic. However, adding observations with imputed values also changes the sample to represent the entire population rather than just respondents. Thereby, the changes in the estimates combine match bias and selection bias. Our linked data provide us with an accurate measure of the response for the entire population, which allows us to isolate the bias due to imputation error. It also makes it possible to study how accurately imputed values reproduce the distribution of true values including the correlation with other covariates and thereby provide direct evidence on the nature of imputation error.

To do so, we compare models of program receipt with imputed and administrative receipt as the dependent variable as above but restrict the sample to item non-respondents. Table 9 reports the coefficient estimates as well as joint and individual tests of equality.[13] The tests of model equality reject that the imputations correctly reproduce the distribution of program receipt in all cases. Thus, including item non-respondents with imputed values in the sample yields a sample from a distribution different from the distribution of the variables of interest in the overall population, which causes bias in most models.

Table 9: Examining Imputation Error, Coefficients (around here)

The differences between the coefficients when using the imputed dependent variable and the accurate dependent variable characterize the differences in the imputed and true distribution and are thereby informative about the likely bias. We reject the equality of individual coefficients in one-third of all cases, which is impressive given the small samples of non-respondents. There are large differences in many coefficients, but they vary between surveys and programs as one would expect given the differences in item non-response rates and the imputation procedures. In line with the results on match bias and the fact that the hot decks do not include disability status, receipt by households with a disabled householder is systematically understated. As our findings on imputed participation in multiple programs from section

[13] Appendix Table A8 reports the corresponding marginal effects.

3 suggest, there are large differences in the estimated coefficients on reported receipt of other programs. However, the direction of the differences varies between surveys: imputations understate the importance of reporting receipt of other programs in the ACS and overstate it in the CPS and SIPP.

In addition to this evidence of match bias, the estimated coefficients on several variables used in the hot decks are badly biased nonetheless. Most notably, the imputations understate the difference between households with a white householder and a black or Hispanic householder, even when this information is used in the hot deck. Similarly, imputations tend to understate receipt rates of single households with children and how much receipt rises with additional individuals, even though the hot decks include information on household composition. The results thereby also confirm that even for variables included in both the imputation and the outcome models, misspecifying the functional form of the imputation model can lead to imperfect match bias (Bollinger and Hirsch 2006).

The large differences in the estimated determinants of receipt based on imputations and true receipt status are concerning, but one may have expected worse based on Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006). These authors show that, when conditioning on all predictors of the imputations, the coefficients on variables that are not used in the imputation procedure will be biased toward zero in the entire sample and zero among non-respondents. As pointed out above, their results do not directly apply to our case, but compared to this standard, our results are encouraging. Several variables that are not included in the hot deck procedures, such as disability, gender and receipt of other programs still significantly predict imputed receipt in several cases. We only find a tendency for attenuation and loss of significance in the CPS, but not in the ACS and SIPP. Yet it is not clear why the imputations capture multivariate relationships better than expected. A potential cause is that we include observations, such as entirely imputed records, that are likely to preserve the relevant correlations in the CPS and SIPP. However, this finding also applies to SNAP in the ACS, which is never imputed jointly with other variables. Therefore, other factors such as the use of geographic information in the ACS and SIPP are likely to play a role as well.

c. Do Imputed Observations Improve Estimates?

Applied researchers usually face the choice between excluding non-respondents or using the imputed values. Their main concern likely is the accuracy of the final estimates. However, our results above show that neither strategy is likely to yield consistent estimates. Item non-response is not conditionally random, so excluding item non-respondents causes bias from sample selection, but imputation error causes bias from measurement error. This impasse raises the question which of the two strategies yields more accurate estimates. To conceptualize the trade-off in minimizing bias, consider the following decomposition of a generic parameter estimate when imputed observations are included:

$$\hat{\beta}^{IMP} = \hat{\beta} + \hat{b}^{Non-Response} + \underbrace{\hat{b}^{Imputation\ Error} - \hat{b}^{Non-Response}}_{\text{Effect of Including Imputations}}$$

where $E[\hat{b}^{Non-Response}]$ is the bias from excluding non-respondents. Including imputations removes this bias, but introduces new bias from measurement error in the imputed values, $E[\hat{b}^{Imputation\ Error}]$, to the equation. This bias from imputation error is one of the sources of the overall bias found in section 5, which we isolate and examine further here. The decomposition above underlines that one should include imputed observations when the bias from item non-response is larger than the bias from imputation error.[14]

Which of the two bias terms is larger and hence whether including imputed observations increases or decreases bias is case specific. It depends on how non-respondents differ from the population conditional on the covariates and on differences between the joint distribution of the imputed values and the true outcome for item non-respondents. Consequently, our results from above may allow researchers to form a reasonable opinion regarding which term is likely to be larger. However, the magnitude of the

[14] A complication this overlooks (that is discussed below), is that errors in opposite directions might cancel.

biases also depends on the model of interest. Neither the literature on missing data (e.g. Little and Rubin 2002) nor the literature on measurement error (e.g. Bound, Brown and Mathiowetz, 2001, and Carroll et al. 2006) provides results on the size of the bias under the conditions we examine.

In light of this lack of general results, we examine the bias from each source and whether excluding non-respondents or including imputed values improves estimates for the models of program receipt from section 5. We thereby provide guidance for a common application and show that the differences we document are indeed informative about the size of the bias. We can evaluate the merits and perils of each strategy by comparing estimates that exclude item non-respondents and estimates that replace the accurate measure from the administrative data by imputed values for non-respondents to the standard set by estimates using administrative receipt for the entire sample. To do so, we need a measure of distance between vectors of parameters. We adopt a χ^2 measure to put estimates of different size and precision on a common scale before aggregating the differences. We follow Meyer and Mittag (2017) and use the variance matrix of the accurate estimates as the weighting matrix. Compared to using the standard χ^2-test statistic, which uses the variance of the difference between the estimates, our measure does not suggest an improvement when the inaccurate estimates are less precisely estimated (without changing the point estimates). In practice, researchers are likely to be concerned with specific coefficients rather than the entire coefficient vector, which makes the comparison easier.

Table 10: Comparing Bias From Item Non-response and Imputation (around here)

Table 10 reports the distance between probit coefficient estimates from models using the entire sample and administrative receipt as the dependent variable and estimates using respondents only (first row), as well as using imputations instead of administrative receipt for non-respondents (second row). All models use the same covariates as the models in section 5. Coefficient estimates from all three models are provided in Appendix Table A9. The last row of Table 10 reports the difference between the two measures, which can be interpreted as the change in the distance to the consistent coefficient estimates when adding the imputed values to the sample of respondents. Including imputed values minimally improves estimates for SNAP in the SIPP. In all other cases, the increase in the bias when including imputations is large at 50 to 500 percent of the non-response bias. Thus, these results clearly favor using only the sample of respondents.

In practice, researchers are often more concerned with specific parameters than with an overall assessment of a model. We begin by comparing the effects of variables for which we have found large differences in coefficient estimates between non-respondents and the overall population or between the imputed and administrative variable when predicting receipt. These results show that our analyses above of the assumptions required for each approach are informative about the accuracy of the estimated parameters of interest. For example, male non-respondents are substantially less likely to receive either program in the CPS. Accordingly, we find a strong upward bias in the estimates of program receipt for males when excluding non-respondents and less bias when including imputed observations. For the imputations, we found large differences between models using imputed and the accurate receipt for households with a black or Hispanic householder and find large biases in the receipt rates of such households when using the imputed observations. This bias is particularly pronounced in the CPS.

The effects of income are instructive regarding both sources of error. We find receipt to decline more slowly with income among non-respondents, with the exception of PA in the SIPP, where we find receipt to decline more quickly with income among non-respondents. As expected, the estimates that exclude non-respondents indeed overstate the decline with income, again with the anticipated exception of PA in the SIPP, where the sign of the bias in the model of receipt is reversed. However, the effect of income also illustrates that it is important to consider both sides of the trade-off, as the bias from using imputations is worse than the bias from excluding non-respondents in two out of six cases, PA in the ACS and SNAP in the SIPP, despite the differences in the income gradient between respondents and the overall population. These two cases are explained well by our results above: For PA in the ACS, non-respondents only differ minimally from the overall population in terms of the effect of income, so the bias from excluding them is smaller than the bias from using the imputed values. For SNAP in the SIPP, non-respondents differ from the overall population, but the difference in the income gradient between imputed and accurate receipt is enormous. As a consequence, using imputed observations leads to an even larger bias in the income gradient than excluding non-respondents, even though non-respondents differ substantially from the overall population.

The fact that the biases in the coefficients we discuss above are well aligned with our results from the previous two subsections demonstrates that understanding the nature of item non-response and imputation error can provide guidance on the decision whether to use imputed values or not. The accurate estimates from the linked data allow for other interesting comparisons and decompositions. The distances to the accurate model for additional approaches are in Appendix Table A10. For example, researchers who exclude non-respondents have more sophisticated strategies at their disposal than just dropping them from the sample. The effects of using inverse probability weighting (Wooldridge 2007) to adjust for missing data on coefficient estimates are minimal and increase the bias, but marginal effects seem to improve slightly.

Our approaches above take the accurate estimates as the reference point and examines whether excluding non-respondents or using them with imputed values biases the estimates further from this reference point. Researchers usually do not have an accurate measure for the sample of respondents and use survey reports instead. Our results above show that adding non-respondents with imputed values to the sample of respondents moves estimates further away from the consistent estimates, which does not imply that doing so degrades estimates that are also biased by other sources of survey error. Thus, another important question is whether adding imputed observations to a model using survey reports of respondents as the dependent variable moves estimates closer to the accurate estimates. Rows 4 and 5 of Appendix Table A10 report our distance metric for these two models. In line with Meyer and Goerge

(2011), this comparison favors including imputed observations. Some of the improvements are sizeable, but they tend to be small compared to the large bias from measurement error among respondents. This large bias from a different source of error affects the estimates both with and without non-respondents. Therefore, a major determinant of whether using imputed values improves estimates is the direction of sample selection and imputation bias. If they go in opposite directions, the strategy that mitigates rather than reinforces the additional bias from measurement error always yields lower overall bias. Thus, this comparison picks the strategy with a convenient sign of the bias, rather than being informative about which bias term is smaller. Consequently, this comparison may be of more interest for this specific case, but is less informative about the question of including or excluding imputed observations more generally.

7. Conclusions

We link administrative and survey data to provide a detailed description of survey error in transfer receipt for three of the most important U.S. household surveys. Our results add to prior evidence of high false negative rates and low false positive rates that lead to severe net understatement of transfer receipt. We show that the survey reports poorly reflect patterns of multiple program participation. The substantial underreporting of each program leads the survey data to understate both the fraction of the population that relies on government transfers and the fraction that receives both programs. In addition to the pronounced underreporting of each program, the surveys also poorly capture the dependence in the probabilities of program receipt. On the positive side, our results suggest that program confusion is a minor source of false positives and survey error seems to lead to less bias when examining participation in any of multiple programs. These error patterns are similar, but more pronounced among imputations: False negative rates are even higher (with the exception of PA in the ACS). In line with Meyer, Mittag and Goerge (forthcoming), false positive rates are substantially higher making imputed values an important source of overreporting. Imputation improves net reporting rates for PA, but makes them worse for SNAP.

With a few exceptions, imputations fall even further short of reports in their ability to capture participation in multiple programs.

Both false negatives and false positives are systematically related to many key covariates, which leads to bias that is difficult to assess and address. Large differences in survey error by race, ethnicity, and income likely bias many important analyses. Households with a disabled householder report more, while those with a male householder report less program receipt. However, we do not find that the elderly have a lower rate of program receipt reporting as prior research suggested. If misreporting affects the age gradient, then it does so through less false reporting of receipt among the elderly. We also do not find meaningful decreases in reporting accuracy over the short time period we study.

Nonetheless, models of program receipt using survey reports reproduce many qualitative conclusions of models of program receipt. In line with Meyer and Mittag (2017), our results show a strong tendency for effect signs to be robust, but marginal effects to be attenuated with measurement error. As suggested by the large reporting differences, our results suggest pronounced survey biases in receipt by race and income. Therefore, better understanding the determinants of misreporting can help to assess and explain the bias in survey estimates in practice.

Finally, we examine survey error due to item non-response and imputation. We show that the conditions for both unconditional analyses and those conditional on covariates to yield consistent estimates neither hold when excluding item non-respondents nor when replacing the missing values by imputed ones. Item non-respondents are more likely to be program recipients than the overall population both unconditionally and conditional on key covariates, so that estimated receipt rates are likely to be biased when excluding item non-respondents. The effects of important determinants of program receipt, such as income and receipt of other programs, also differs between non-respondents and the overall population. Therefore, the effects of these variables will be biased in analyses that do not include item non-respondents. However, we also find that imputed values reproduce neither the actual levels nor the associations of program receipt with covariates either. Consequently, neither excluding non-respondents nor including them with imputed values is likely to yield consistent estimates. This finding raises the question which strategy causes less bias. Our results on the bias for models of program receipt suggest that it is better to use only respondents than to include imputations. More generally, these analyses provide guidance for a common application and show that a better understanding of the nature of item non-response and imputations could allow researchers to make more informed decisions on whether to use imputed values or not.

Our results have several implications for both survey producers trying to improve survey quality and survey users trying to make better use of error ridden data. For survey producers, the results are informative about who misreports and which patterns of misreporting greatly affect data quality. The results directly point to the potential to improve surveys by developing better imputation methods. Using administrative data or geographic information to predict missing values may be promising paths to improve imputations. Survey producers should be encouraged to provide results similar to ours to survey users whenever possible to help survey users gauge the strengths and weaknesses of the data.

This study shows that such results can help survey users assess the accuracy of their estimates and the likely biases, as we find bias to be well predicted by the patterns of survey errors. For example, based on our results for transfer receipt, survey users should be skeptical of survey estimates of receipt rates, because low reporting rates plague them. This problem becomes worse when studying patterns of receipt of multiple programs, but better when examining participation in any program. Our results on the bias in models of program receipt show that precisely estimated survey coefficients still provide reliable evidence of an effect even when the dependent variable suffers from high and systematic misclassification. In fact, the true effect likely is larger than the survey estimate. Our results on the determinants of survey errors can help to predict exceptions from this general pattern of attenuation. More generally, the results on the determinants and consequences of misclassification underline that the results on asymptotic bias in Meyer and Mittag (2017) are useful when examining whether estimates are likely to suffer from large bias and whether substantive conclusions are robust to survey error. The analysis of item non-response and imputation shows that linked data can provide survey users with the needed information to make more informed decisions to exclude item non-respondents or not. For the case of program receipt, the results favor excluding them at least in multivariate analyses.

In conclusion, this study adds detail to the mounting evidence of large and systematic survey error. Failing to take survey error into account makes the survey data likely to mislead both policy makers and academics in need of accurate information regarding who benefits from programs, who chooses not to participate, and which characteristics deter participation. However, our results also imply two positive messages. First, the findings show that we can still obtain reliable substantive insights from the error-ridden survey data if we account for survey error. Second, data linkage enables us to better understand how to use contaminated data and provides the needed information to account for survey error.

References

Abowd, J.M., and Stinson, M.H. 2013. "Estimating Measurement Error in Annual Job Earnings: A Comparison of Survey and Administrative Data." *Review of Economics and Statistics*, 95(5), 1451–1467.

Acs, G., Phillips, K.R. and Nelson, S. 2005. "The Road Not Taken?: Changes in Welfare Entry during the 1990s." *Social Science Quarterly*, 86(S1), 1060-1079.

Andridge, R.R. and Little, R.J.A. 2010. "A Review of Hot Deck Imputation for Survey Non-response." *International Statistical Review*, 78(1), 40‐64.

Angrist, J. D., and Krueger, A.B. 2001. "Empirical Strategies in Labor Economics", in O. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics*, Vol 3A. Elsevier: Amsterdem.

Bee, C. A., and Mitchell, J. 2017. The Hidden Resources of Women Working Longer: Evidence from Linked Survey-Administrative Data. In C. Goldin and L.F. Katz (Eds.), *Women Working Longer: Increased Employment at Older Ages*. Chicago: University of Chicago Press.

Black, D.A., Sanders, S., and Taylor, L. 2003. "Measurement of Higher Education in the Census and Current Population Survey." *Journal of the American Statistical Association*, 98(463), 545–554.

Blank, R.M. and Ruggles, P. 1996. "When Do Women Use AFDC & Food Stamps? The Dynamics of Eligibility vs. Participation," *Journal of Human Resources* 31, 57-89.

Bollinger, C.R. 1998. "Measurement error in the Current Population Survey: a nonparametric look." *Journal of Labor Economics*, 16(3), 576–94.

Bollinger, C.R. and David, M.H. 1997. "Modeling Discrete Choice with Response Error: Food Stamp Participation." *Journal of the American Statistical Association*, 92 (439), 827-835.

Bollinger, C.R. and David, M.H. 2001. "Estimation with Response Error and Nonresponse: Food-Stamp Participation in the SIPP", *Journal of Business and Economic Statistics*, 19:2, 129-141.

Bollinger, C.R., and Hirsch, B.T. 2006. "Match bias from earnings imputation in the Current Population Survey: The case of imperfect matching." *Journal of Labor Economics* 24 (3), 483-519.

Bollinger, C.R., Hirsch, B.T., Hokayem, C.M., & Ziliak, J.P. 2019. "Trouble in the Tails? What We Know about Earnings Nonresponse Thirty Years after Lillard, Smith, and Welch." *Journal of Political Economy.* 127(5), 2143-2185.

Bound, J., and Krueger, A.B. 1991. "The Extent of Measurement Error in Longitudinal Earnings Data: Do Two Wrongs Make a Right?" *Journal of Labor Economics*, 9(1), 1-24.

Bound, J., Brown, C. and Mathiowetz, N. 2001. "Measurement error in survey data." In *Handbook of Econometrics*. Vol. 5, eds. J.J. Heckman and E. Leamer, Cp. 59, 3705–3843. Amsterdam: Elsevier.

Carroll, R.J., Ruppert, D., Stefanski, L.A., Crainiceanu, C.M. 2006. "*Measurement Error in Nonlinear Models: A Modern Perspective*." 2nd ed. Chapman & Hall: Boca Raton.

Celhay, Pablo, Bruce D. Meyer and Nikolas Mittag. 2017. "What Leads to Measurement Error? Evidence from Reports of Program Participation in Three Surveys." Unpublished Manuscript.

Chenevert, R.L., Klee, M.A. and Wilkin, K.R. 2016. "Do Imputed Earnings Earn Their Keep? Evaluating SIPP Earnings and Nonresponse with Administrative Records." U.S. Census Bureau Working Paper.

Chua, T. C., and Fuller, W. A. 1987. "A model for multinomial response error applied to labor flows." *Journal of the American Statistical Association*, 82(397), 46-51.

Congressional Budget Office (CBO). 2013. "The Distribution of Federal Spending and Taxes, 2006." Congressional Budget Office Report 44698, Washington, D.C.

Congressional Budget Office. 2016. "The Distribution of Household Income and Federal Taxes, 2013." Congressional Budget Office Report 51361, Washington, D.C.

Currie, J. 2006. "The Take-up of Social Benefits," in A.J. Auterbach, D. Card, and J.M. Quigley (Eds.) *Public Policy and the Income Distribution*, Russell Sage Foundation: New York.

Dahl, M., DeLeire, T., and Schwabish, J.A. 2011. "Estimates of Year-to-Year Volatility in Earnings and in Household Incomes from Administrative, Survey, and Matched Data." *Journal of Human Resources*, 46(4), 750–774.

Davern, M., Call, K.T., Ziegenfuss, J., Davidson, G., Beebe, T.J., and Blewett, L. 2008. "Validating Health Insurance Coverage Survey Estimates: A Comparison of Self-Reported Coverage and Administrative Data Records." *Public Opinion Quarterly*, 72(2), 241-259.

Ganong, P. and Liebman, J.B. 2018. "The Decline, Rebound, and Further Rise in SNAP Enrollment: Disentangling Business Cycle Fluctuations and Policy Changes," *American Economic Journal: Economic Policy*, 10(4), 153-176.

Gathright, G.M.R., and Crabb, T.A. 2014. "Reporting of SSA Program Participation in SIPP." Working Paper, U.S. Census Bureau.

Groves, R.M. 2001. “*Survey nonresponse*.” New York: Wiley.

Groves, R.M., and Couper, M.P. 1998. “*Nonresponse in household interview surveys*.” New York: Wiley.

Haider, S., Jacknowitz, A. and Schoeni, R. 2003. “Food Stamps and the Elderly: Why is Participation so Low?” *Journal of Human Resources*, 38:S, 1180-1220.

Hausman, J.A., Abrevaya, J., Scott-Morton, F.M., 1998. “Misclassification of the dependent variable in a discrete-response setting.” *Journal of Econometrics*, 87(2): 239-269.

Heitjan, D.F. 1994. “Ignorability in General Incomplete-Data Models.” *Biometrika*, 81, 701-708.

Heitjan, D. F., and Rubin, D. B. 1991, “Ignorability and Coarse Data.” *Annals of Statistics*, 19, 2244-2253.

Hirsch, B.T., and Schumacher, E. 2004. “Match Bias in Wage Gap Estimates Due to Earnings Imputation.” *Journal of Labor Economics*, 22(3): 689–722.

Hokayem, C., Bollinger, C.R. and Ziliak, J.P. 2015. The Role of CPS Nonresponse in the Measurement of Poverty. *Journal of the American Statistical Association*, 110(511), 935-945.

Kirlin, J.A., and Wiseman, M. 2014. “Getting it Right, or at Least Better: Improving Identification of Food Stamp Participants in the National Health and Nutrition Examination Survey.” Working Paper.

Little, R.J.A. and Rubin, D.B. 2002. “*Statistical analysis with missing data*.” 2nd ed., New York: John Wiley.

Lynch, V., Resnick, D.M., Stavely, J. and Taeuber, C.M. 2007. “Differences in Estimates of Public Assistance Recipiency Between Surveys and Administrative Records.” U.S. Census Bureau Working paper.

Marquis, K. H., and Moore, J.C. 1990. “Measurement Errors in SIPP Program Reports.” In *Proceedings of the 1990 Annual Research Conference*. 721–745. Washington, D.C.: U.S. Bureau of the Census.

Meyer, B.D. and Goerge, R. 2011. “Errors in Survey Reporting and Imputation and Their Effects on Estimates of Food Stamp Program Participation”. U.S. Census Bureau CES Working Paper 11-14.

Meyer, B.D., Mittag, N. and Goerge, R. forthcoming. “Errors in Survey Reporting and Imputation and Their Effects on Estimates of Food Stamp Program Participation.” *Journal of Human Resources*.

Meyer, B.D. and Mittag, N. 2017. “Misclassification in Binary Choice Models.” *Journal of Econometrics.* 200(2), 295-311.

Meyer, B.D., and Mittag, N. 2019a. “Using Linked Survey and Administrative Data to Better Measure Income: Implications for Poverty, Program Effectiveness and Holes in the Safety Net.” *American Economic Journal: Applied Economics* 11(2): 176-204.

Meyer, B.D. and Mittag, N. 2019b. “Misreporting of Government Transfers: How Important are Survey Design and Geography?” *Southern Economic Journal*. 86(1), pp. 230-253.

Meyer, B.D. and Mittag, N. forthcoming. “An Empirical Total Survey Error Decomposition Using Data Combination.” *Journal of Econometrics*.

Meyer, B.D. and Wu, D., 2018. The poverty reduction of social security and means-tested transfers. *ILR Review*, *71*(5): 1106-1153.

Meyer, B.D., Mok, W.K.C. and Sullivan, J.X. 2015. “Household Surveys in Crisis.” *Journal of Economic Perspectives*, 29(4): 199–226.

Mittag, N. 2019. “Correcting for Misreporting of Government Benefits.” *American Economic Journal: Economic Policy* 11(2): 142-16.

Moore, J.C. 2008. “Seam Bias in the 2004 SIPP Panel: Much Improved, but Much Bias Still Remains.” U.S. Census Bureau Statistical Research Division Survey Methodology Research Report Series 2008-3.

Nicholas, J. and Wiseman, M. 2010 “Elderly Poverty and Supplemental Security Income, 2002-2005.” *Social Security Bulletin* 70(2), 1-29.

Poterba, J.M., and Summers, L.H. 1986. “Reporting Errors and Labor Market Dynamics.” *Econometrica*, 54(6), 1319.

Ribar, D.C. 2005. “Transitions from Welfare and the Employment Prospects of Low-Skill Workers.” *Southern Economic Journal* 71(3), 514–33.

Taeuber, C., Resnick, D.M., Love, S.P., Stavely, J. Wilde, P. and Larson, R. 2004. “Differences in Estimates of Food Stamp Program Participation Between Surveys and Administrative Records” Working Paper, U.S. Census Bureau.

U.S. Census Bureau. 2006. “Design and Methodology: Current Population Survey.” Technical Paper 66, U.S. Census Bureau.

U.S. Census Bureau. 2008. “Survey of Income and Program Participation: User’s Guide.” U.S. Census Bureau.

U.S. Census Bureau. 2014. “American Community Survey: Design and Methodology.” U.S. Census Bureau.

U.S. General Accounting Office (GAO). 2004. “Food Stamp Program: Steps Have Been Taken to Increase Participation of Working Families, but Better Tracking of Efforts is Needed.” GAO-04-346. Washington, DC: GAO.

Wagner, D., and Layne, M. 2014. "The Person Identification Validation System (PVS): Applying the Center for Administrative Records Research and Applications' (CARRA) Record Linkage Software." U.S. Census Bureau.

Wooldridge, J.M. 2007. "Inverse Probability Weighted Estimation for General Missing Data Problems." *Journal of Econometrics*, 141(2), 1281–1301.

Wu, Yanyuan 2010. "Essays on the Economic Well-Being of the Elderly and Public Policy." Ph.D. Dissertation, University of Chicago.

Table 1 - Error Rate by True Receipt Status and Net Reporting Rate

	False Negative Rate		False Positive Rate		Net Reporting Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
	SNAP	*PA*	*SNAP*	*PA*	*SNAP*	*PA*
			Full Sample			
ACS	26%	57%	1.2%	1.6%	80%	82%
CPS	42%	63%	2.0%	0.6%	67%	50%
SIPP	19%	46%	1.5%	0.5%	88%	68%
			Respondents			
ACS	25%	59%	1.1%	1.3%	80%	75%
CPS	37%	59%	1.2%	0.3%	69%	49%
SIPP	18%	46%	1.3%	0.4%	88%	66%
			Imputed Observations			
ACS	67%	44%	13.7%	6.2%	60%	126%
CPS	68%	86%	7.6%	2.4%	60%	61%
SIPP	33%	51%	4.9%	1.9%	83%	84%

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The false negative rate is the estimated fraction of true recipient households with receipt not recorded in the survey. The false positive rate is the estimated fraction of true non-recipient households recorded as recipients in the survey. The net reporting rate is the ratio of the (weighted) number of recipient households according to the survey and the administrative variable in the linked data. In the SIPP, we collapse receipt to the wave level. All analyses use household weights adjusted for incomplete linkage. Appendix Table A3 provides full cross-tabulations of administrative and reported receipt.

Table 2: Reported and Administrative Rates of Joint Receipt of SNAP and PA

	No program		**SNAP Only**		**PA Only**		**Both Programs**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Source	*Report*	*Admin.*	*Report*	*Admin.*	*Report*	*Admin.*	*Report*	*Admin.*
				Full Sample				
ACS	85.4%	82.5%	11.4%	13.5%	0.7%	0.1%	2.5%	3.9%
CPS	87.7%	82.0%	10.1%	13.6%	0.3%	0.2%	1.9%	4.2%
SIPP	84.2%	82.0%	13.3%	14.4%	0.3%	0.2%	2.2%	3.4%
				Imputed Observations				
ACS	75.6%	73.0%	14.4%	19.0%	2.9%	0.2%	7.1%	7.9%
CPS	86.5%	78.5%	10.7%	16.4%	1.1%	0.2%	1.7%	4.9%
SIPP	80.3%	76.8%	15.5%	18.2%	0.3%	0.1%	3.9%	4.9%

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. All numbers are in percent of the number of households in NY. All analyses use household weights adjusted for incomplete linkage.

Table 3 - Reporting of Participation in Multiple Programs by True Number of Programs Received

True Receipt	No Program		One Program			Two Programs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reported Receipt	*One Program*	*Two Programs*	*No Program*	*The Wrong Program*	*Two Programs*	*No Program*	*One Program*
				Full Sample			
ACS	1.7%	0.1%	26.9%	0.8%	6.1%	17.9%	41.0%
CPS	2.1%	0.1%	45.5%	0.4%	2.2%	28.9%	35.9%
SIPP	1.4%	0.1%	20.7%	0.1%	2.2%	13.2%	35.1%
				Imputed Observations			
ACS	5.7%	0.5%	31.1%	3.2%	14.0%	15.7%	33.7%
CPS	7.6%	0.4%	69.4%	1.4%	4.9%	56.1%	31.0%
SIPP	4.8%	0.5%	36.6%	0.1%	6.0%	18.3%	32.5%

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. All numbers are in percent of the population receiving the number of programs stated in the first row according to the administrative data. The omitted group in each case are those correctly reporting receipt of the programs they receive according to the administrative data. All analyses use household weights adjusted for incomplete linkage.

Table 4: The Determinants of False Negatives, Probit Marginal Effects, Full Linked Sample of True Recipients

	ACS		CPS		SIPP	
	(1)	(2)	(3)	(4)	(5)	(6)
	SNAP	*PA*	*SNAP*	*PA*	*SNAP*	*PA*
Single adult, no children	-0.0592***	-0.0148	-0.0035	0.2271***	-0.0454*	0.1950**
	(0.0081)	(0.0180)	(0.0388)	(0.0711)	(0.0271)	(0.0937)
Single adult, with children	-0.0306***	-0.0427***	0.0227	0.0577	-0.0392*	-0.0008
	(0.0069)	(0.0136)	(0.0293)	(0.0573)	(0.0233)	(0.0594)
Multiple adults, no children	-0.0303***	-0.0474***	-0.0485	0.1370**	0.0604***	0.0397
	(0.0065)	(0.0145)	(0.0325)	(0.0582)	(0.0220)	(0.0653)
Number of members under 18	-0.0211***	-0.0202***	-0.0222*	0.0405***	-0.0058	-0.0919***
	(0.0023)	(0.0041)	(0.0116)	(0.0150)	(0.0078)	(0.0198)
Number of members 18 or older	0.0031	-0.0149***	0.0307***	0.0726**	-0.0086	0.0480*
	(0.0022)	(0.0053)	(0.0112)	(0.0298)	(0.0077)	(0.0259)
Rural	0.0040	-0.0026	-0.0146	-0.0248	0.0776***	0.0096
	(0.0065)	(0.0183)	(0.0326)	(0.0689)	(0.0208)	(0.0686)
Hispanic	0.0410***	0.0467***	0.0554***	0.0744*	0.0382**	0.0508
	(0.0059)	(0.0145)	(0.0208)	(0.0450)	(0.0171)	(0.0512)
Black non-hispanic	0.0678***	0.0877***	0.0958***	0.0683	0.0841***	0.0800*
	(0.0049)	(0.0114)	(0.0210)	(0.0451)	(0.0135)	(0.0430)
Other non-hispanic	0.0343***	0.0132	0.0953**	0.0014	-0.0031	0.1229
	(0.0085)	(0.0230)	(0.0376)	(0.1271)	(0.0198)	(0.0749)
Male	0.0485***	0.0384***	0.0312*	0.0767**	0.0791***	-0.0436
	(0.0038)	(0.0100)	(0.0172)	(0.0376)	(0.0127)	(0.0509)
Disabled	-0.0947***	-0.0594***	-0.0531		-0.1439***	-0.1215**
	(0.0045)	(0.0097)	(0.0691)		(0.0187)	(0.0472)
Age 16-29	-0.0295***	0.0361***	0.0340	-0.0530	-0.0113	0.0189
	(0.0066)	(0.0131)	(0.0279)	(0.0446)	(0.0239)	(0.0539)
Age 30-39	-0.0005	0.0111	0.0227	-0.0366	-0.0297	0.2616***
	(0.0058)	(0.0123)	(0.0252)	(0.0422)	(0.0196)	(0.0470)
Age 50-59	-0.0158***	0.0053	-0.0046	-0.0116	-0.0225	-0.0291
	(0.0057)	(0.0124)	(0.0267)	(0.0468)	(0.0179)	(0.0515)
Age 60-69	-0.0164**	0.0713***	0.0294	0.0717	-0.0806***	-0.0444
	(0.0065)	(0.0162)	(0.0304)	(0.0627)	(0.0218)	(0.0691)
Age 70 or more	0.0009	0.0784***	0.0562*	0.2028	-0.0697***	-0.0191
	(0.0068)	(0.0224)	(0.0315)	(0.1450)	(0.0211)	(0.0804)
Less than high school	-0.0370***	-0.0137	-0.0842***	-0.0502	-0.0288*	0.0033
	(0.0051)	(0.0109)	(0.0222)	(0.0407)	(0.0171)	(0.0476)
High school graduate	0.0074	-0.0094	-0.0091	-0.0308	-0.0082	0.0270
	(0.0047)	(0.0108)	(0.0210)	(0.0408)	(0.0136)	(0.0418)
Complete graduate and beyond	0.0106*	0.0040	0.0225	-0.0525	0.0346*	-0.3240***
	(0.0061)	(0.0162)	(0.0278)	(0.0554)	(0.0198)	(0.0634)
Household language is English only	0.0136**	-0.0402***				
	(0.0054)	(0.0126)				
Speaks English poorly	-0.0781***	0.0128			-0.0000	0.1285*
	(0.0062)	(0.0158)			(0.0192)	(0.0711)
Non-citizen	0.0257***	0.0080			0.0310	-0.0295
	(0.0060)	(0.0136)			(0.0215)	(0.0579)
Household income/poverty line	0.0449***	0.0150***	0.0798***	0.0273*	0.0529***	-0.0196
	(0.0014)	(0.0038)	(0.0085)	(0.0157)	(0.0050)	(0.0160)
Household income/poverty line >10	-0.2396***	-0.1358*	-0.4967***		-0.2854***	0.1404
	(0.0231)	(0.0720)	(0.1598)		(0.1102)	(0.2035)
Anyone in household employed	0.0697***	0.1733***	0.0627***	0.0281	-0.0284*	-0.2194***
	(0.0049)	(0.0096)	(0.0206)	(0.0349)	(0.0168)	(0.0454)
Reported housing assistance receipt			-0.1957***	-0.0593*	-0.1130***	-0.0604
			(0.0175)	(0.0314)	(0.0134)	(0.0406)
Reported public assistance receipt	-0.2189***		-0.3252***		-0.2074***	
	(0.0065)		(0.0331)		(0.0270)	
Reported SNAP receipt		-0.3115***		-0.3479***		-0.3320***
		(0.0105)		(0.0338)		(0.0469)
Linear time trend	-0.0085***	0.0147***	-0.0096**	-0.0027	0.0000***	0.0001**
	(0.0013)	(0.0029)	(0.0046)	(0.0091)	(0.0000)	(0.0000)
Number of observations	81,772	16,962	3,539	908	4,771	931
chi2 statistic of joint significance	9,337	3,451	1,269	385	1,173	658
p-value of joint significance	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The samples include imputed observations, but are restricted to recipients according to the linked data. The dependent variable is an indicator for failure to report receipt in the survey. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The Determinants of False Positives, Probit Marginal Effects, Full Linked Sample of Non-Recipients

	ACS		CPS		SIPP	
	(1)	(2)	(3)	(4)	(5)	(6)
	SNAP	*PA*	*SNAP*	*PA*	*SNAP*	*PA*
Single adult, no children	-0.0029***	-0.0024**	-0.0084	-0.0041	-0.0014	-0.0040**
	(0.0010)	(0.0010)	(0.0053)	(0.0026)	(0.0039)	(0.0020)
Single adult, with children	0.0022**	0.0029***	0.0057	-0.0033	0.0079*	0.0029
	(0.0011)	(0.0010)	(0.0050)	(0.0022)	(0.0043)	(0.0019)
Multiple adults, no children	-0.0026***	-0.0001	-0.0085*	-0.0011	0.0041	-0.0052***
	(0.0008)	(0.0008)	(0.0046)	(0.0019)	(0.0035)	(0.0017)
Number of members under 18	0.0005	-0.0000	-0.0014	0.0016***	-0.0010	-0.0014*
	(0.0003)	(0.0003)	(0.0018)	(0.0006)	(0.0015)	(0.0008)
Number of members 18 or older	0.0025***	0.0043***	0.0020	0.0016**	0.0046***	0.0001
	(0.0003)	(0.0002)	(0.0017)	(0.0008)	(0.0012)	(0.0005)
Rural	-0.0024***	-0.0017***	0.0012	-0.0033	0.0007	0.0021
	(0.0006)	(0.0006)	(0.0045)	(0.0023)	(0.0027)	(0.0019)
Hispanic	0.0085***	-0.0028***	0.0139***	-0.0017	0.0044	-0.0027*
	(0.0007)	(0.0008)	(0.0031)	(0.0015)	(0.0033)	(0.0014)
Black non-hispanic	0.0098***	0.0005	0.0116***	0.0015	0.0137***	0.0059***
	(0.0006)	(0.0006)	(0.0031)	(0.0014)	(0.0022)	(0.0010)
Other non-hispanic	0.0060***	0.0046***	0.0060	-0.0070**	0.0093***	-0.0056**
	(0.0008)	(0.0008)	(0.0043)	(0.0031)	(0.0029)	(0.0023)
Male	-0.0019***	0.0006	-0.0034	-0.0020*	-0.0069***	0.0004
	(0.0004)	(0.0004)	(0.0024)	(0.0012)	(0.0017)	(0.0009)
Disabled	0.0069***	0.0006	0.0274***		0.0076*	-0.0010
	(0.0006)	(0.0006)	(0.0088)		(0.0042)	(0.0014)
Age 16-29	0.0071***	-0.0000	0.0011	0.0062***	0.0041	-0.0038
	(0.0008)	(0.0008)	(0.0043)	(0.0017)	(0.0037)	(0.0024)
Age 30-39	0.0023***	0.0009	0.0029	-0.0006	0.0130***	-0.0037**
	(0.0007)	(0.0007)	(0.0038)	(0.0017)	(0.0034)	(0.0016)
Age 50-59	0.0005	-0.0012*	0.0045	0.0002	0.0000	-0.0003
	(0.0007)	(0.0007)	(0.0042)	(0.0018)	(0.0028)	(0.0012)
Age 60-69	-0.0006	-0.0054***	-0.0032	-0.0058**	-0.0041	-0.0038**
	(0.0008)	(0.0008)	(0.0045)	(0.0026)	(0.0035)	(0.0015)
Age 70 or more	-0.0040***	-0.0072***	-0.0087*	-0.0074***	-0.0021	-0.0046***
	(0.0009)	(0.0008)	(0.0049)	(0.0028)	(0.0033)	(0.0018)
Less than high school	0.0068***	0.0035***	0.0088**	0.0023	0.0056*	0.0021*
	(0.0007)	(0.0007)	(0.0036)	(0.0017)	(0.0030)	(0.0012)
High school graduate	0.0031***	0.0012**	0.0011	0.0025*	0.0050**	0.0004
	(0.0006)	(0.0006)	(0.0031)	(0.0014)	(0.0023)	(0.0011)
Complete graduate and beyond	-0.0017***	-0.0004	-0.0075*	-0.0045**	-0.0043*	-0.0026*
	(0.0006)	(0.0006)	(0.0039)	(0.0021)	(0.0022)	(0.0015)
Household language is English only	-0.0006	0.0004				
	(0.0006)	(0.0006)				
Speaks English poorly	0.0051***	-0.0008			0.0058	-0.0033**
	(0.0008)	(0.0008)			(0.0036)	(0.0015)
Non-citizen	0.0003	-0.0032***			0.0063**	0.0034**
	(0.0007)	(0.0008)			(0.0031)	(0.0015)
Household income/poverty line	-0.0030***	-0.0010***	-0.0121***	-0.0010***	-0.0035***	0.0002
	(0.0002)	(0.0001)	(0.0015)	(0.0004)	(0.0006)	(0.0003)
Household income/poverty line >10	0.0112***	-0.0007	0.0517***		0.0095	-0.0021
	(0.0017)	(0.0011)	(0.0164)		(0.0076)	(0.0039)
Anyone in household employed	-0.0038***	-0.0094***	-0.0094***	-0.0035**	0.0137***	0.0075***
	(0.0007)	(0.0006)	(0.0031)	(0.0015)	(0.0027)	(0.0017)
Reported housing assistance receipt			0.0089**	0.0029**	0.0222***	0.0029***
			(0.0036)	(0.0014)	(0.0027)	(0.0010)
Reported public assistance receipt	0.0341***		0.0461***		0.0479***	
	(0.0010)		(0.0075)		(0.0047)	
Reported SNAP receipt		0.0334***		0.0103***		0.0123***
		(0.0006)		(0.0016)		(0.0015)
Linear time trend	0.0008***	0.0003**	0.0029***	0.0003	0.0000***	0.0000***
	(0.0001)	(0.0001)	(0.0007)	(0.0003)	(0.0000)	(0.0000)
Number of observations	461,756	526566	14,525	17,156	20,226	24,066
chi2 statistic of joint significance	2,546	5,025	257	121	344	162
p-value of joint significance	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The samples include imputed observations, but are restricted to non-recipients according to the linked data. The dependent variable is an indicator for reporting in the survey. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. Standard errors in parentheses,*** p<0.01, ** p<0.05. * p<0.1

Table 6: The Determinants of Reported and Administrative SNAP Receipt, Probit Marginal Effects, Linked Households with Income less than Twice the Poverty Line

	ACS			CPS			SIPP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Survey Report	Admin. Receipt	P-value (1)=(2)	Survey Report	Admin. Receipt	P-value (4)=(5)	Survey Report	Admin. Receipt	P-value (7)=(8)
Single adult, no children	-0.0711***	-0.0854***	0.137	-0.0293	-0.0101	0.648	-0.0411	-0.0409	0.997
	(0.0067)	(0.0069)		(0.0294)	(0.0302)		(0.0258)	(0.0271)	
Single adult, with children	0.0313***	0.0480***	0.062	0.0290	0.1173***	0.013	0.1063***	0.1333***	0.400
	(0.0062)	(0.0064)		(0.0242)	(0.0259)		(0.0225)	(0.0229)	
Multiple adults, no children	-0.0567***	-0.0592***	0.762	-0.0121	-0.0252	0.719	-0.0418*	-0.0227	0.546
	(0.0059)	(0.0060)		(0.0256)	(0.0257)		(0.0222)	(0.0226)	
Number of members under 18	0.0297***	0.0335***	0.162	0.0312***	0.0424***	0.383	0.0266***	0.0382***	0.282
	(0.0019)	(0.0019)		(0.0091)	(0.0090)		(0.0075)	(0.0078)	
Number of members 18 or older	0.0081***	0.0202***	0.001	0.0028	0.0393***	0.017	0.0194**	0.0198**	0.976
	(0.0025)	(0.0025)		(0.0107)	(0.0110)		(0.0097)	(0.0100)	
Rural	-0.0228***	-0.0276***	0.407	0.0333	0.0569***	0.434	-0.0101	0.0027	0.513
	(0.0041)	(0.0042)		(0.0211)	(0.0216)		(0.0138)	(0.0139)	
Hispanic	0.0987***	0.1405***	<0.001	0.0743***	0.1445***	0.001	0.0697***	0.1096***	0.078
	(0.0044)	(0.0044)		(0.0148)	(0.0151)		(0.0156)	(0.0163)	
Black non-hispanic	0.1098***	0.1708***	<0.001	0.0291*	0.1075***	<0.001	0.0759***	0.1232***	0.005
	(0.0038)	(0.0038)		(0.0157)	(0.0160)		(0.0119)	(0.0122)	
Other non-hispanic	-0.0397***	-0.0438***	0.637	-0.0363	-0.0003	0.298	0.0704***	0.0773***	0.770
	(0.0060)	(0.0061)		(0.0244)	(0.0244)		(0.0166)	(0.0163)	
Male	-0.0550***	-0.0593***	0.282	-0.0531***	-0.0640***	0.540	-0.0722***	-0.0293***	0.004
	(0.0029)	(0.0029)		(0.0125)	(0.0128)		(0.0105)	(0.0107)	
Disabled	0.1599***	0.1544***	0.222	0.1263**	0.0937	0.704	0.2535***	0.2549***	0.945
	(0.0031)	(0.0032)		(0.0580)	(0.0633)		(0.0145)	(0.0147)	
Age 16-29	0.0531***	0.0475***	0.447	-0.0219	0.0229	0.138	0.0319*	0.0356*	0.888
	(0.0051)	(0.0053)		(0.0210)	(0.0217)		(0.0187)	(0.0189)	
Age 30-39	0.0271***	0.0365***	0.178	-0.0034	0.0002	0.899	0.0943***	0.0817***	0.611
	(0.0048)	(0.0050)		(0.0191)	(0.0201)		(0.0170)	(0.0178)	
Age 50-59	0.0113**	0.0047	0.331	0.0279	0.0528**	0.395	0.0174	0.0140	0.880
	(0.0047)	(0.0048)		(0.0202)	(0.0212)		(0.0157)	(0.0157)	
Age 60-69	-0.0045	-0.0109**	0.389	-0.0463**	-0.0310	0.637	0.0421**	0.0246	0.516
	(0.0053)	(0.0053)		(0.0224)	(0.0234)		(0.0191)	(0.0191)	
Age 70 or more	-0.1041***	-0.1262***	0.003	-0.1053***	-0.0987***	0.837	0.0506***	0.0354*	0.565
	(0.0052)	(0.0053)		(0.0224)	(0.0230)		(0.0186)	(0.0187)	
Less than high school	0.0735***	0.0745***	0.853	0.0829***	0.0882***	0.824	0.0574***	0.0762***	0.380
	(0.0039)	(0.0039)		(0.0164)	(0.0172)		(0.0150)	(0.0152)	
High school graduate	0.0143***	0.0238***	0.061	-0.0075	0.0060	0.541	0.0157	0.0270**	0.503
	(0.0035)	(0.0036)		(0.0155)	(0.0160)		(0.0119)	(0.0119)	
Complete graduate and beyond	-0.0790***	-0.0862***	0.275	-0.0780***	-0.0654***	0.664	-0.0885***	-0.0908***	0.914
	(0.0046)	(0.0047)		(0.0202)	(0.0206)		(0.0145)	(0.0158)	
Household language is English only	-0.0157***	-0.0187***	0.602						
	(0.0039)	(0.0041)							
Speaks English poorly	0.1558***	0.1478***	0.242				0.1300***	0.1503***	0.341
	(0.0048)	(0.0050)					(0.0177)	(0.0185)	
Non-citizen	-0.1096***	-0.1302***	0.002				-0.0809***	-0.0982***	0.565
	(0.0047)	(0.0048)					(0.0190)	(0.0188)	
Household income/poverty line	-0.1346***	-0.1394***	0.186	-0.0912***	-0.0957***	0.774	-0.0938***	-0.0973***	0.774
	(0.0026)	(0.0026)		(0.0107)	(0.0115)		(0.0085)	(0.0087)	
Anyone in household employed	-0.1018***	-0.1066***	0.370	-0.1058***	-0.1304***	0.267	0.0677***	0.0689***	0.951
	(0.0037)	(0.0039)		(0.0152)	(0.0161)		(0.0130)	(0.0132)	
Reported housing assistance receipt				0.2191***	0.2349***	0.402	0.2367***	0.2223***	0.517
				(0.0127)	(0.0139)		(0.0105)	(0.0109)	
Reported public assistance receipt	0.3616***	0.3481***	0.130	0.3792***	0.3277***	0.253	0.3794***	0.3181***	0.127
	(0.0060)	(0.0066)		(0.0291)	(0.0344)		(0.0286)	(0.0282)	
Linear time trend	0.0247***	0.0268***	0.109	0.0192***	0.0177***	0.738	-0.0000***	-0.0000***	0.428
	(0.0009)	(0.0009)		(0.0033)	(0.0034)		(0.0000)	(0.0000)	
Number of observations	149,318	149,318		5,711	5,711		8,819	8,819	
Joint test of equality chi2 statistic			533			68			39
Joint test of equality p-value			<0.001			<0.001			0.0443

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. For each survey, the first column contains estimates from a probit model using reported receipt as the dependent variable. The second column estimates the same model using the administrative receipt measure as the dependent variable. The thrid column contains p-values of a chi-square test whether the estimates are equal. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses conducted using household weights adjusted for PIK probability. Standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1

Table 7: The Determinants of Reported and Administrative Public Assistance Receipt, Probit Marginal Effects, Linked Households with Income less than Twice the Poverty Line

	ACS			CPS			SIPP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Survey Report	Admin. Receipt	P-value (1)=(2)	Survey Report	Admin. Receipt	P-value (4)=(5)	Survey Report	Admin. Receipt	P-value (7)=(8)
Single adult, no children	-0.0236***	-0.0545***	<0.001	-0.0284*	-0.0179	0.643	-0.0266**	-0.0359***	0.610
	(0.0037)	(0.0041)		(0.0147)	(0.0172)		(0.0118)	(0.0138)	
Single adult, with children	0.0225***	0.0332***	0.020	0.0082	0.0601***	0.004	0.0447***	0.0549***	0.480
	(0.0031)	(0.0033)		(0.0118)	(0.0135)		(0.0094)	(0.0110)	
Multiple adults, no children	-0.0054*	-0.0250***	<0.001	-0.0024	-0.0158	0.478	0.0039	0.0258**	0.164
	(0.0033)	(0.0035)		(0.0119)	(0.0147)		(0.0102)	(0.0120)	
Number of members under 18	0.0063***	0.0064***	0.936	0.0047	0.0065*	0.717	0.0127***	0.0134***	0.853
	(0.0009)	(0.0010)		(0.0032)	(0.0038)		(0.0027)	(0.0032)	
Number of members 18 or older	0.0110***	0.0174***	0.001	-0.0022	0.0164***	0.034	0.0070*	0.0118**	0.431
	(0.0013)	(0.0014)		(0.0063)	(0.0061)		(0.0037)	(0.0049)	
Rural	-0.0104***	-0.0281***	<0.001	0.0120	0.0304*	0.358	0.0187*	0.0232*	0.780
	(0.0028)	(0.0036)		(0.0119)	(0.0161)		(0.0138)	(0.0139)	
Hispanic	0.0114***	0.0579***	<0.001	0.0122	0.0761***	<0.001	-0.0011	0.0286***	0.006
	(0.0027)	(0.0030)		(0.0077)	(0.0100)		(0.0070)	(0.0082)	
Black non-hispanic	0.0177***	0.0753***	<0.001	0.0204**	0.0835***	<0.001	0.0258***	0.0647***	<0.001
	(0.0022)	(0.0025)		(0.0081)	(0.0102)		(0.0057)	(0.0072)	
Other non-hispanic	0.0147***	0.0076*	0.216	-0.0290	-0.0270	0.943	-0.0160*	0.0274**	0.002
	(0.0035)	(0.0044)		(0.0179)	(0.0216)		(0.0091)	(0.0109)	
Male	-0.0051***	-0.0058***	0.790	-0.0049	0.0105	0.168	0.0014	-0.0147**	0.073
	(0.0018)	(0.0021)		(0.0071)	(0.0086)		(0.0058)	(0.0069)	
Disabled	0.0099***	0.0136***	0.222	-0.0535*	0.0057	0.209	0.0175**	0.0317***	0.209
	(0.0020)	(0.0023)		(0.0323)	(0.0344)		(0.0074)	(0.0085)	
Age 16-29	-0.0030	0.0122***	<0.001	0.0232***	0.0136	0.516	-0.0075	0.0028	0.416
	(0.0028)	(0.0029)		(0.0090)	(0.0118)		(0.0082)	(0.0096)	
Age 30-39	-0.0044*	-0.0052*	0.819	-0.0025	-0.0219**	0.134	-0.0408***	-0.0104	0.009
	(0.0026)	(0.0028)		(0.0080)	(0.0101)		(0.0078)	(0.0087)	
Age 50-59	-0.0100***	-0.0043	0.147	-0.0156	-0.0251**	0.531	-0.0142*	-0.0234**	0.444
	(0.0027)	(0.0029)		(0.0096)	(0.0118)		(0.0077)	(0.0093)	
Age 60-69	-0.0517***	-0.0546***	0.565	-0.0657***	-0.0849***	0.346	-0.0519***	-0.0860***	0.034
	(0.0033)	(0.0037)		(0.0132)	(0.0155)		(0.0103)	(0.0124)	
Age 70 or more	-0.0708***	-0.1187***	<0.001	-0.1105***	-0.1654***	0.030	-0.0582***	-0.0922***	0.051
	(0.0032)	(0.0043)		(0.0172)	(0.0185)		(0.0115)	(0.0131)	
Less than high school	0.0151***	0.0175***	0.471	0.0174**	0.0090	0.526	0.0200***	0.0285***	0.447
	(0.0023)	(0.0025)		(0.0082)	(0.0105)		(0.0073)	(0.0086)	
High school graduate	0.0045**	0.0023	0.502	0.0190**	0.0077	0.395	0.0009	-0.0018	0.791
	(0.0022)	(0.0024)		(0.0082)	(0.0105)		(0.0067)	(0.0075)	
Complete graduate and beyond	-0.0085***	-0.0218***	0.005	-0.0022	-0.0045	0.902	0.0131	-0.0097	0.099
	(0.0031)	(0.0036)		(0.0115)	(0.0143)		(0.0091)	(0.0105)	
Household language is English only	0.0107***	0.0160***	0.145						
	(0.0025)	(0.0027)							
Speaks English poorly	-0.0120***	-0.0237***	0.005				-0.0289***	-0.0392***	0.435
	(0.0028)	(0.0031)					(0.0084)	(0.0102)	
Non-citizen	-0.0035	-0.0007	0.476				0.0132	0.0006	0.193
	(0.0027)	(0.0028)					(0.0084)	(0.0102)	
Household income/poverty line	-0.0147***	-0.0487***	<0.001	-0.0221***	-0.0521***	0.003	-0.0127**	-0.0322***	0.012
	(0.0017)	(0.0020)		(0.0067)	(0.0078)		(0.0053)	(0.0057)	
Anyone in household employed	-0.0607***	-0.0466***	<0.001	-0.0256***	-0.0447***	0.112	0.0549***	0.0498***	0.674
	(0.0022)	(0.0024)		(0.0074)	(0.0095)		(0.0086)	(0.0086)	
Reported housing assistance receipt				0.0219***	0.0228***	0.925	0.0179***	0.0110	0.341
				(0.0061)	(0.0079)		(0.0054)	(0.0069)	
Reported SNAP receipt	0.1234***	0.1208***	0.380	0.0958***	0.1041***	0.444	0.0996***	0.0984***	0.915
	(0.0020)	(0.0020)		(0.0075)	(0.0080)		(0.0077)	(0.0073)	
Linear time trend	-0.0029***	-0.0039***	0.261	0.0002	-0.0033	0.197	-0.0000	-0.0000**	0.436
	(0.0006)	(0.0006)		(0.0017)	(0.0022)		(0.0000)	(0.0000)	
Number of observations	149,318	149,318		5,711	5,711		8,819	8,819	
Joint test of equality chi2 statistic			1,558			120			96
Joint test of equality p-value			<0.001			<0.001			<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. For each survey, the first column contains estimates from a probit model using reported receipt as the dependent variable. The second column estimates the same model using the administrative receipt measure as the dependent variable. The thrid column contains p-values of a chi-square test whether the estimates are equal. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Differences Between the Full Sample and Item Non-Respondents in the Determinants of Program Receipt, Probit Coefficients

	ACS						CPS						SIPP					
	SNAP			PA			SNAP			PA			SNAP			PA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Sample:	Full Sample	Non-respondents	P-value (1)=(2)	Full Sample	Non-respondents	P-value (4)=(5)	Full Sample	Non-respondents	P-value (7)=(8)	Full Sample	Non-respondents	P-value (10)=(11)	Full Sample	Non-respondents	P-value (13)=(14)	Full Sample	Non-respondents	P-value (16)=(17)
Single adult, no children	-0.093***	-0.054	0.723	-0.381***	-0.265***	0.087	0.099	0.115	0.914	-0.132	0.230	0.104	-0.078	-0.817***	<0.001	-0.604***	0.205	0.016
	(0.016)	(0.110)		(0.025)	(0.071)		(0.067)	(0.168)		(0.097)	(0.246)		(0.069)	(0.218)		(0.107)	(0.355)	
Single adult, with children	0.348***	0.378***	0.793	0.303***	0.368***	0.275	0.455***	0.296**	0.228	0.496***	0.414**	0.619	0.593***	0.400**	0.302	0.437***	0.710***	0.205
	(0.015)	(0.117)		(0.020)	(0.062)		(0.061)	(0.147)		(0.078)	(0.179)		(0.061)	(0.196)		(0.081)	(0.221)	
Multiple adults, no children	-0.069***	-0.064	0.956	-0.158***	-0.025	0.023	0.043	-0.108	0.229	-0.071	-0.052	0.915	-0.027	-0.409***	0.007	-0.049	0.257	0.185
	(0.013)	(0.095)		(0.020)	(0.061)		(0.057)	(0.137)		(0.078)	(0.195)		(0.060)	(0.153)		(0.093)	(0.238)	
Number of members under 18	0.109***	0.143***	0.372	0.056***	0.069***	0.466	0.145***	0.148***	0.943	0.070***	0.170***	0.056	0.119***	0.002	0.033	0.082***	0.112	0.642
	(0.005)	(0.038)		(0.006)	(0.019)		(0.022)	(0.051)		(0.024)	(0.058)		(0.022)	(0.059)		(0.026)	(0.070)	
Number of members 18 or older	0.180***	0.119***	0.023	0.122***	0.116***	0.773	0.217***	0.158***	0.209	0.139***	0.167***	0.628	0.198***	0.171***	0.559	0.032	0.090	0.398
	(0.005)	(0.027)		(0.007)	(0.020)		(0.021)	(0.052)		(0.031)	(0.061)		(0.022)	(0.050)		(0.031)	(0.074)	
Rural	-0.114***	-0.232***	0.146	-0.196***	-0.185***	0.873	0.123**	0.462***	0.002	0.201**	0.327	0.474	0.000***	0.000**	0.127	0.000***	0.000	0.597
	(0.010)	(0.082)		(0.020)	(0.068)		(0.050)	(0.127)		(0.085)	(0.200)		(0.000)	(0.000)		(0.000)	(0.000)	
Hispanic	0.493***	0.477***	0.811	0.400***	0.351***	0.308	0.560***	0.725***	0.055	0.471***	0.345**	0.363	0.386***	0.571***	0.149	0.383***	0.311	0.713
	(0.011)	(0.067)		(0.018)	(0.050)		(0.037)	(0.094)		(0.058)	(0.152)		(0.045)	(0.133)		(0.063)	(0.206)	
Black non-hispanic	0.678***	0.670***	0.899	0.604***	0.586***	0.660	0.463***	0.652***	0.024	0.595***	0.682***	0.469	0.494***	0.409***	0.435	0.670***	0.529***	0.387
	(0.009)	(0.059)		(0.015)	(0.043)		(0.038)	(0.092)		(0.058)	(0.132)		(0.037)	(0.113)		(0.058)	(0.170)	
Other non-hispanic	0.022	0.012	0.920	0.099***	0.038	0.434	0.085	0.253*	0.213	-0.175	-0.005	0.485	0.422***	0.078	0.054	0.207**	0.666***	0.044
	(0.015)	(0.095)		(0.027)	(0.081)		(0.059)	(0.147)		(0.120)	(0.280)		(0.048)	(0.185)		(0.086)	(0.249)	
Male	-0.207***	-0.168***	0.384	-0.042***	-0.085**	0.196	-0.122***	-0.342***	0.001	0.013	-0.409***	<0.001	-0.105***	-0.052	0.539	-0.149***	-0.337**	0.215
	(0.007)	(0.045)		(0.012)	(0.035)		(0.030)	(0.075)		(0.048)	(0.122)		(0.029)	(0.090)		(0.053)	(0.157)	
Disabled	0.479***	0.309***	0.002	0.083***	0.120***	0.337	0.346**	0.636**	0.320	0.079	0.923***	0.001	1.027***	1.219***	0.321	0.293***	0.243	0.819
	(0.008)	(0.054)		(0.015)	(0.040)		(0.139)	(0.316)		(0.179)	(0.327)		(0.046)	(0.200)		(0.068)	(0.227)	
Age 16-29	0.223***	0.137	0.404	0.107***	0.218***	0.033	0.183***	0.437***	0.030	0.080	0.218	0.341	0.306***	0.362**	0.709	0.132	0.082	0.804
	(0.013)	(0.103)		(0.018)	(0.054)		(0.051)	(0.131)		(0.066)	(0.162)		(0.060)	(0.161)		(0.087)	(0.201)	
Age 30-39	0.124***	-0.060	0.028	-0.034**	-0.032	0.975	0.156***	0.394***	0.022	-0.165***	0.014	0.173	0.275***	0.267**	0.952	-0.051	-0.009	0.816
	(0.011)	(0.084)		(0.017)	(0.051)		(0.047)	(0.115)		(0.060)	(0.150)		(0.047)	(0.129)		(0.067)	(0.188)	
Age 50-59	0.029***	0.095	0.374	0.010	-0.037	0.301	0.094*	-0.008	0.374	-0.043	-0.135	0.568	0.091**	0.072	0.877	0.097	0.090	0.974
	(0.011)	(0.074)		(0.017)	(0.048)		(0.048)	(0.125)		(0.068)	(0.179)		(0.044)	(0.131)		(0.070)	(0.216)	
Age 60-69	-0.056***	0.041	0.211	-0.304***	-0.455***	0.010	-0.082	0.015	0.449	-0.332***	-0.471**	0.500	0.062	-0.107	0.288	-0.522***	-1.001**	0.260
	(0.013)	(0.078)		(0.022)	(0.062)		(0.056)	(0.142)		(0.087)	(0.223)		(0.054)	(0.164)		(0.098)	(0.433)	
Age 70 or more	-0.425***	-0.231***	0.019	-0.764***	-0.915***	0.021	-0.298***	-0.242	0.680	-1.066***	-1.241***	0.579	0.100**	0.127	0.872	-0.645***	-0.553**	0.711
	(0.013)	(0.083)		(0.026)	(0.070)		(0.059)	(0.149)		(0.128)	(0.331)		(0.051)	(0.177)		(0.097)	(0.259)	
Less than high school	0.291***	0.023	<0.001	0.143***	0.055	0.034	0.305***	0.437***	0.185	0.103	0.174	0.633	0.342***	0.745***	0.005	0.186***	0.174	0.949
	(0.010)	(0.065)		(0.016)	(0.044)		(0.043)	(0.107)		(0.063)	(0.164)		(0.046)	(0.149)		(0.070)	(0.203)	
High school graduate	0.105***	0.027	0.195	0.043***	-0.021	0.119	0.031	0.196**	0.055	0.085*	0.142	0.662	0.175***	0.267***	0.339	-0.053	0.142	0.224
	(0.008)	(0.061)		(0.014)	(0.043)		(0.037)	(0.095)		(0.059)	(0.145)		(0.034)	(0.101)		(0.060)	(0.167)	
Complete graduate and beyond	-0.188***	-0.110	0.258	-0.149***	-0.246***	0.088	-0.237***	-0.253**	0.882	0.006	-0.076	0.601	-0.323***	-0.072	0.029	-0.170**	-0.019	0.485
	(0.010)	(0.069)		(0.018)	(0.059)		(0.044)	(0.113)		(0.071)	(0.174)		(0.040)	(0.122)		(0.074)	(0.230)	
Household language is English only	-0.121***	-0.141**	0.729	0.086***	0.105**	0.655							0.153***	0.002	0.167	0.036	0.105	0.744
	(0.009)	(0.059)		(0.016)	(0.045)								(0.039)	(0.119)		(0.079)	(0.220)	
Speaks English poorly	0.419***	0.374***	0.560	-0.201***	-0.167***	0.553							0.489***	0.293	0.344	-0.417***	-0.234	0.520
	(0.013)	(0.078)		(0.020)	(0.061)								(0.060)	(0.213)		(0.090)	(0.292)	
Non-citizen	-0.348***	-0.158**	0.009	-0.014	0.031	0.387							-0.292***	-0.315*	0.891	0.122	-0.290	0.062
	(0.013)	(0.073)		(0.018)	(0.054)								(0.066)	(0.170)		(0.098)	(0.217)	
Household income/poverty line	-0.250***	-0.108***	<0.001	-0.119***	-0.098***	0.046	-0.222***	-0.143***	0.001	-0.110***	-0.041	0.039	-0.250***	-0.176***	0.004	-0.088***	-0.197***	0.069
	(0.003)	(0.012)		(0.004)	(0.011)		(0.012)	(0.025)		(0.019)	(0.037)		(0.013)	(0.027)		(0.022)	(0.060)	
Anyone in household employed	-0.428***	-0.246***	0.004	-0.368***	-0.335***	0.417	-0.487***	-0.519***	0.722	-0.395***	-0.530***	0.291	0.245***	0.328***	0.437	0.502***	0.374**	0.478
	(0.010)	(0.064)		(0.015)	(0.043)		(0.041)	(0.096)		(0.058)	(0.137)		(0.037)	(0.112)		(0.067)	(0.185)	
Reported housing assistance receipt							0.810***	0.420***	<0.001	0.187***	0.017	0.246						
							(0.043)	(0.113)		(0.053)	(0.154)							
Reported public assistance receipt	1.307***	0.604***	<0.001				1.121***	0.247	<0.001				1.157***	0.860***	0.144			
	(0.017)	(0.100)					(0.099)	(0.187)					(0.085)	(0.215)				
Reported SNAP receipt				1.032***	1.091***	<0.001				0.830***	0.405***	0.794				0.890***	0.920***	0.840
				(0.013)	(0.036)					(0.053)	(0.131)					(0.055)	(0.155)	
Linear time trend	0.084***	0.074***	0.483	-0.022***	-0.025**	0.773	0.058***	0.064***	0.771	-0.005	-0.036	0.255	0.820***	0.659***	0.261	0.138**	-0.155	0.122
	(0.002)	(0.016)		(0.004)	(0.011)		(0.008)	(0.020)		(0.013)	(0.031)		(0.039)	(0.147)		(0.058)	(0.197)	
Constant	-0.798***	-0.693***	0.510	-1.875***	-1.694***	0.076	-1.093***	-1.225***	0.545	-1.922***	-1.842***	<0.001	-1.587***	-1.077***	0.025	-2.447***	-2.537***	0.827
	(0.023)	(0.160)		(0.036)	(0.107)		(0.098)	(0.240)		(0.138)	(0.340)		(0.091)	(0.239)		(0.149)	(0.427)	
Number of observations	543,528	6,096		543,528	35,589		18,064	2,336		18,064	2,346		24,997	1,941		24,997	1,928	
Chi2 distance measure			19,313			2,554			460			354			656			267
Joint test of equality chi2 statistic			753.21			319.28			107.71			88.12			109.97			44.86
Joint test of equality p-value			<0.001			<0.001			<0.001			<0.001			<0.001			0.017

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The dependent variable is receipt according to the administrative data in all columns. For each survey and program, the first column estimates the model using the full linked sample, the second column estimates the same model using the sample of item non-respondents. The third column contains p-values of chi-square tests whether the estimates in the first two columns are equal. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. The distance measure is the squared distance between the two coefficient vectors, weighted by the variance matrix of the coefficients from using the full sample with the administrative dependent variable. SEs in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: The Determinants of Administrative and Imputed Program Receipt Among Item Non-Respondents, Probit Coefficients

	ACS						CPS						SIPP					
	SNAP			PA			SNAP			PA			SNAP			PA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Dependent variable:	Admin. Receipt	Imputed Receipt	P-value (1)=(2)	Admin. Receipt	Imputed Receipt	P-value (4)=(5)	Admin. Receipt	Imputed Receipt	P-value (7)=(8)	Admin. Receipt	Imputed Receipt	P-value (10)=(11)	Admin. Receipt	Imputed Receipt	P-value (13)=(14)	Admin. Receipt	Imputed Receipt	P-value (16)=(17)
Single adult, no children	-0.054	-0.298**	0.102	-0.265***	-0.181***	0.338	0.115	0.178	0.804	0.230	-0.898***	0.004	-0.817***	-0.306	0.027	0.205	0.600*	0.345
	(0.110)	(0.119)		(0.071)	(0.063)		(0.168)	(0.202)		(0.246)	(0.284)		(0.218)	(0.212)		(0.355)	(0.362)	
Single adult, with children	0.378***	0.048	0.049	0.368***	0.199***	0.025	0.296**	0.163	0.547	0.414**	-0.168	0.030	0.400**	0.201	0.274	0.710***	0.537**	0.541
	(0.117)	(0.124)		(0.062)	(0.057)		(0.147)	(0.168)		(0.179)	(0.212)		(0.196)	(0.200)		(0.221)	(0.266)	
Multiple adults, no children	-0.064	-0.258**	0.124	-0.025	-0.012	0.860	-0.108	0.160	0.203	-0.052	-0.376*	0.276	-0.409***	-0.205	0.206	0.257	0.260	0.993
	(0.095)	(0.103)		(0.061)	(0.054)		(0.137)	(0.175)		(0.195)	(0.216)		(0.153)	(0.158)		(0.238)	(0.272)	
Number of members under 18	0.143***	0.016	0.011	0.069***	0.073***	0.866	0.148***	0.058	0.180	0.170***	0.086	0.398	0.002	0.078	0.103	0.112	0.240***	0.120
	(0.038)	(0.039)		(0.019)	(0.018)		(0.051)	(0.062)		(0.058)	(0.070)		(0.059)	(0.060)		(0.070)	(0.088)	
Number of members 18 or older	0.119***	-0.102***	<0.001	0.116***	0.082***	0.185	0.158***	-0.012	0.033	0.167***	0.041	0.243	0.171***	0.082	0.139	0.090	0.013	0.396
	(0.027)	(0.034)		(0.020)	(0.019)		(0.052)	(0.069)		(0.061)	(0.082)		(0.050)	(0.059)		(0.074)	(0.079)	
Rural	-0.232***	-0.142	0.481	-0.185***	-0.121**	0.381	0.462***	0.067	0.049	0.327	-0.296	0.036	0.000**	0.000***	0.660	0.000	0.000	0.412
	(0.082)	(0.094)		(0.068)	(0.047)		(0.127)	(0.153)		(0.200)	(0.232)		(0.000)	(0.000)		(0.000)	(0.000)	
Hispanic	0.477***	0.310***	0.080	0.351***	-0.003	<0.001	0.725***	0.207*	<0.001	0.345**	-0.164	0.023	0.571***	0.533***	0.760	0.311	-0.325	0.006
	(0.067)	(0.075)		(0.050)	(0.046)		(0.094)	(0.114)		(0.152)	(0.168)		(0.133)	(0.143)		(0.206)	(0.207)	
Black non-hispanic	0.670***	0.434***	0.007	0.586***	0.160***	<0.001	0.652***	0.084	<0.001	0.682***	0.115	0.006	0.409***	0.358***	0.702	0.529***	0.256	0.214
	(0.059)	(0.064)		(0.043)	(0.036)		(0.092)	(0.112)		(0.132)	(0.162)		(0.113)	(0.122)		(0.170)	(0.187)	
Other non-hispanic	0.012	0.249**	0.057	0.038	-0.030	0.454	0.253*	0.104	0.488	-0.005	-0.936**	0.047	0.078	0.240	0.485	0.666***	-0.506	0.003
	(0.095)	(0.100)		(0.081)	(0.062)		(0.147)	(0.175)		(0.280)	(0.367)		(0.185)	(0.180)		(0.249)	(0.406)	
Male	-0.168***	-0.217***	0.459	-0.085**	-0.174***	0.028	-0.342***	-0.208**	0.248	-0.409***	-0.212	0.297	-0.052	-0.083	0.747	-0.337**	-0.097	0.236
	(0.045)	(0.050)		(0.035)	(0.029)		(0.075)	(0.094)		(0.122)	(0.142)		(0.090)	(0.096)		(0.157)	(0.172)	
Disabled	0.309***	0.078	0.003	0.120***	0.125***	0.911	0.636**	0.479	0.678	0.923***	-0.115	0.096	1.219***	0.909***	0.108	0.243	-0.208	0.042
	(0.054)	(0.059)		(0.040)	(0.034)		(0.316)	(0.366)		(0.327)	(0.445)		(0.200)	(0.199)		(0.227)	(0.247)	
Age 16-29	0.137	0.054	0.588	0.218***	0.252***	0.596	0.437***	-0.225	<0.001	0.218	0.748***	0.038	0.362**	0.313**	0.775	0.082	0.094	0.962
	(0.103)	(0.114)		(0.054)	(0.052)		(0.131)	(0.149)		(0.162)	(0.190)		(0.161)	(0.156)		(0.201)	(0.259)	
Age 30-39	-0.060	0.132	0.096	-0.032	0.116**	0.014	0.394***	-0.275*	<0.001	0.014	0.108	0.700	0.267**	0.508***	0.067	-0.009	-0.210	0.415
	(0.084)	(0.087)		(0.051)	(0.046)		(0.115)	(0.146)		(0.150)	(0.194)		(0.129)	(0.142)		(0.188)	(0.240)	
Age 50-59	0.095	0.020	0.478	-0.037	-0.100**	0.272	-0.008	0.043	0.791	-0.135	0.243	0.159	0.072	0.324**	0.111	0.090	-0.013	0.688
	(0.074)	(0.082)		(0.048)	(0.044)		(0.125)	(0.154)		(0.179)	(0.205)		(0.131)	(0.144)		(0.216)	(0.219)	
Age 60-69	0.041	-0.143	0.107	-0.455***	-0.372***	0.257	0.015	-0.302*	0.150	-0.471**	-0.037	0.198	-0.107	0.090	0.268	-1.001**	-0.683*	0.314
	(0.078)	(0.089)		(0.062)	(0.050)		(0.142)	(0.172)		(0.223)	(0.268)		(0.164)	(0.185)		(0.433)	(0.358)	
Age 70 or more	-0.231***	-0.251***	0.863	-0.915***	-0.606***	<0.001	-0.242	-0.424**	0.429	-1.241***	-0.394	0.072	0.127	0.107	0.915	-0.553**	-0.018	0.098
	(0.083)	(0.093)		(0.070)	(0.051)		(0.149)	(0.190)		(0.331)	(0.332)		(0.177)	(0.182)		(0.259)	(0.264)	
Less than high school	0.023	0.190***	0.084	0.055	0.102***	0.354	0.437***	0.228*	0.171	0.174	0.292	0.614	0.745***	0.886***	0.347	0.174	0.554***	0.112
	(0.065)	(0.072)		(0.044)	(0.039)		(0.107)	(0.123)		(0.164)	(0.180)		(0.149)	(0.154)		(0.203)	(0.212)	
High school graduate	0.027	0.170**	0.112	-0.021	-0.016	0.910	0.196**	-0.145	0.016	0.142	-0.056	0.359	0.267***	0.316***	0.654	0.142	0.153	0.959
	(0.061)	(0.068)		(0.043)	(0.036)		(0.095)	(0.112)		(0.145)	(0.156)		(0.101)	(0.110)		(0.167)	(0.196)	
Complete graduate and beyond	-0.110	0.036	0.158	-0.246***	-0.146***	0.161	-0.253**	-0.334**	0.673	-0.076	-0.684***	0.040	-0.072	0.108	0.164	-0.019	-0.109	0.752
	(0.069)	(0.080)		(0.059)	(0.047)		(0.113)	(0.163)		(0.174)	(0.243)		(0.122)	(0.120)		(0.230)	(0.300)	
Household language is English only	-0.141**	0.001	0.117	0.105**	0.101**	0.939							0.002	-0.100	0.447	0.105	1.229***	0.005
	(0.059)	(0.070)		(0.045)	(0.040)								(0.119)	(0.127)		(0.220)	(0.380)	
Speaks English poorly	0.374***	0.314***	0.581	-0.167***	0.067	0.001							0.293	0.087	0.396	-0.234	-0.140	0.702
	(0.078)	(0.079)		(0.061)	(0.052)								(0.213)	(0.211)		(0.292)	(0.295)	
Non-citizen	-0.158**	-0.031	0.215	0.031	-0.026	0.387							-0.315*	-0.013	0.085	-0.290	-0.091	0.506
	(0.073)	(0.080)		(0.054)	(0.051)								(0.170)	(0.169)		(0.217)	(0.268)	
Household income/poverty line	-0.108***	-0.149***	0.053	-0.098***	-0.065***	0.010	-0.143***	-0.518***	0.001	-0.041	-0.011	0.553	-0.176***	-0.138***	0.269	-0.197***	-0.149*	0.604
	(0.012)	(0.019)		(0.011)	(0.008)		(0.025)	(0.113)		(0.037)	(0.035)		(0.027)	(0.027)		(0.060)	(0.079)	
Anyone in household employed	-0.246***	-0.164**	0.379	-0.335***	-0.592***	<0.001	-0.519***	-0.189	0.029	-0.530***	-0.282*	0.245	0.328***	0.372***	0.721	0.374**	0.661**	0.291
	(0.064)	(0.071)		(0.043)	(0.036)		(0.096)	(0.129)		(0.137)	(0.159)		(0.112)	(0.113)		(0.185)	(0.260)	
Reported housing assistance receipt							0.420***	0.307**	0.444	0.017	0.355**	0.135						
							(0.113)	(0.120)		(0.154)	(0.172)							
Reported public assistance receipt	0.604***	0.195**	0.002				0.247	1.096***	<0.001				0.860***	1.675***	<0.001			
	(0.100)	(0.099)					(0.187)	(0.182)					(0.215)	(0.269)				
Reported SNAP receipt				1.091***	0.947***	0.001				0.405***	0.942***	0.008				0.920***	1.440***	0.015
				(0.036)	(0.031)					(0.131)	(0.151)					(0.155)	(0.192)	
Linear time trend	0.074***	0.063***	0.660	-0.025**	-0.021**	0.758	0.064***	0.016	0.129	-0.036	0.042	0.084	0.659***	1.210***	<0.001	-0.155	0.194	0.106
	(0.016)	(0.018)		(0.011)	(0.009)		(0.020)	(0.025)		(0.031)	(0.036)		(0.147)	(0.150)		(0.197)	(0.185)	
Constant	-0.693***	-0.419**	<0.001	-1.694***	-1.126***	<0.001	-1.225***	-0.091	<0.001	-1.842***	-2.023***	<0.001	-1.077***	-1.529***	<0.001	-2.537***	-4.463***	<0.001
	(0.160)	(0.175)		(0.107)	(0.093)		(0.240)	(0.281)		(0.340)	(0.366)		(0.239)	(0.274)		(0.427)	(0.647)	
Number of observations			29,610			4,514			4,688			1,209			867			1,355
Chi2 distance measure			359.75			436.72			172.10			62.71			79.86			52.65
Joint test of equality chi2 statistic			<0.001			<0.001			<0.001			<0.001			<0.001			0.002
Joint test of equality p-value	6096	6096		35589	35589		2336	2336		2346	2346		1941	1941		1928	1928	

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The sample is restricted to item non-respondents in all columns. For each survey and program, the first column uses the administrative receipt measure as the dependent variable. The second column estimates the same model using the imputed receipt variable. The third column contains p-values of chi-square tests whether the estimates in the first two columns are equal. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. The distance measure is the squared distance between the two coefficient vectors, weighted by the variance matrix of the coefficients using the administrative dependent variable. SEs in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10 – Bias From Item Non-response and Imputation in Probit Coefficients of the Determinants of Program Receipt

		ACS		CPS		SIPP	
Sample	**Dependent Variable**	*SNAP*	*PA*	*SNAP*	*PA*	*SNAP*	*PA*
Respondents	Administrative Receipt	8.5	27.2	25.7	15.6	12.7	3.2
Full	Admin/Imputed	14.0	163.5	57.0	23.1	11.9	7.1
Effect of Including Imputed Observations		5.5	136.3	31.3	7.5	-0.8	3.9

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The first two rows report distances between the estimated coefficients from the model using the full sample and the administrative dependent variable and using the sample and dependent variable in the first two columns. Coefficient estimates are reported in Appendix Table A6. Distances are the squared distance to the coefficients from the model using the full sample and the administrative receipt variable, weighted by the variance matrix of the coefficients from this model. The effect of including imputed observations in the third row is the difference between the two lines above.

Table A1: The Determinants of a Household Having a PIK, Probit Coefficients and Marginal Effects

	ACS		CPS		SIPP	
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficients	Marg. Eff.	Coefficients	Marg. Eff.	Coefficients	Marg. Eff.
Single adult, no children	-0.3077***	-0.0390***	-0.4146***	-0.0650***	-0.3882***	-0.0400***
	(0.0185)	(0.0024)	(0.0709)	(0.0112)	(0.0868)	(0.0090)
Single adult, with children	0.0207	0.0026	0.0416	0.0065	0.0909	0.0094
	(0.0205)	(0.0026)	(0.0721)	(0.0113)	(0.0888)	(0.0092)
Multiple adults, no children	-0.1170***	-0.0148***	-0.2023***	-0.0317***	-0.1861**	-0.0192**
	(0.0160)	(0.0020)	(0.0605)	(0.0095)	(0.0744)	(0.0077)
Number of members under 18	0.0399***	0.0051***	0.0473*	0.0074*	0.1018***	0.0105***
	(0.0068)	(0.0009)	(0.0266)	(0.0042)	(0.0330)	(0.0034)
Number of members 18 or older	0.0163***	0.0021***	0.0523**	0.0082**	0.0891***	0.0092***
	(0.0061)	(0.0008)	(0.0246)	(0.0039)	(0.0313)	(0.0032)
Rural	0.0278***	0.0035***	0.0978*	0.0153*	-0.0941*	-0.0097*
	(0.0106)	(0.0013)	(0.0559)	(0.0088)	(0.0513)	(0.0053)
Hispanic	0.0364***	0.0046***	-0.1765***	-0.0276***	-0.2091***	-0.0215***
	(0.0129)	(0.0016)	(0.0376)	(0.0058)	(0.0530)	(0.0056)
Black non-hispanic	-0.1423***	-0.0181***	-0.0929**	-0.0146**	-0.0305	-0.0031
	(0.0108)	(0.0014)	(0.0385)	(0.0060)	(0.0474)	(0.0049)
Other non-hispanic	-0.0778***	-0.0099***	-0.4893***	-0.0767***	-0.2112***	-0.0218***
	(0.0147)	(0.0019)	(0.0452)	(0.0070)	(0.0499)	(0.0052)
Male	-0.0033	-0.0004			0.0682**	0.0070**
	(0.0073)	(0.0009)			(0.0321)	(0.0033)
Disabled	0.1047***	0.0133***	0.0890	0.0140	0.1923***	0.0198***
	(0.0106)	(0.0013)	(0.1577)	(0.0247)	(0.0633)	(0.0065)
Age 16-29	-0.0916***	-0.0116***	-0.0694	-0.0109	-0.2617***	-0.0270***
	(0.0136)	(0.0017)	(0.0489)	(0.0077)	(0.0617)	(0.0064)
Age 30-39	-0.0853***	-0.0108***	-0.1104***	-0.0173***	-0.3849***	-0.0397***
	(0.0118)	(0.0015)	(0.0423)	(0.0066)	(0.0520)	(0.0054)
Age 50-59	0.1265***	0.0161***	0.1678***	0.0263***	0.0306	0.0032
	(0.0116)	(0.0015)	(0.0450)	(0.0071)	(0.0542)	(0.0056)
Age 60-69	0.2048***	0.0260***	0.3751***	0.0588***	0.1200**	0.0124**
	(0.0130)	(0.0017)	(0.0538)	(0.0084)	(0.0593)	(0.0061)
Age 70 or more	0.2618***	0.0332***	0.2849***	0.0446***	0.0663	0.0068
	(0.0141)	(0.0018)	(0.0575)	(0.0090)	(0.0609)	(0.0063)
Less than high school	-0.0503***	-0.0064***	-0.0880*	-0.0138*	-0.0624	-0.0064
	(0.0125)	(0.0016)	(0.0475)	(0.0075)	(0.0570)	(0.0059)
High school graduate	-0.1357***	-0.0172***	-0.1567***	-0.0246***	-0.0068	-0.0007
	(0.0097)	(0.0012)	(0.0377)	(0.0059)	(0.0420)	(0.0043)
Complete graduate and beyond	0.0664***	0.0084***	-0.0045	-0.0007	-0.0117	-0.0012
	(0.0101)	(0.0013)	(0.0409)	(0.0064)	(0.0405)	(0.0042)
Household language is English only	0.1150***	0.0146***				
	(0.0103)	(0.0013)				
Speaks English poorly	-0.2504***	-0.0318***			-0.5738***	-0.0591***
	(0.0144)	(0.0018)			(0.0555)	(0.0058)
Non-citizen	-0.3705***	-0.0470***			-0.4976***	-0.0513***
	(0.0116)	(0.0015)			(0.0520)	(0.0054)
Household income/poverty line	0.0092***	0.0012***	0.0199***	0.0031***	-0.0007	-0.0001
	(0.0009)	(0.0001)	(0.0059)	(0.0009)	(0.0005)	(0.0001)
Anyone in household employed	0.0595***	0.0076***	0.0579	0.0091	0.0535	0.0055
	(0.0105)	(0.0013)	(0.0417)	(0.0065)	(0.0382)	(0.0039)
Linear time trend	0.0136***	0.0017***	0.0134*	0.0021*	0.0163***	0.0017***
	(0.0025)	(0.0003)	(0.0081)	(0.0013)	(0.0036)	(0.0004)
Constant	1.4072***		1.3093***		1.7288***	
	(0.0276)		(0.1003)		(0.1222)	
Number of observations	573,459	573,459	19,852	19,852	26,349	26,349
chi2 statistic of joint significance	255.3	5806.8	22.8	490.8	34.4	651.2
p-value of joint significance	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The dependent variable is an indicator whether someone in the household was assigned a PIK. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Summary Statistics, Full Linked Sample

	ACS		CPS		SIPP	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*
Reported SNAP receipt	0.139	0.346	0.120	0.325	0.155	0.362
Reported PA receipt	0.033	0.178	0.022	0.147	0.024	0.154
Fale Positive Rate SNAP	0.012	0.109	0.020	0.141	0.015	0.122
Fale Negative Rate SNAP	0.257	0.437	0.421	0.494	0.194	0.395
Imputed SNAP receipt	0.011	0.103	0.130	0.336	0.072	0.258
Fale Positive Rate PA	0.016	0.126	0.006	0.078	0.005	0.073
Fale Negative Rate PA	0.568	0.495	0.629	0.483	0.463	0.499
Imputed PA receipt	0.061	0.239	0.130	0.336	0.071	0.258
Single adult, no children	0.296	0.456	0.311	0.463	0.331	0.470
Single adult, with children	0.054	0.227	0.056	0.230	0.065	0.247
Multiple adults, no children	0.386	0.487	0.379	0.485	0.347	0.476
Number of members under 18	0.591	1.037	0.568	1.004	0.583	0.996
Number of members 18 or older	1.926	0.943	1.898	0.921	1.845	0.900
Rural	0.131	0.337	0.087	0.282	0.853	0.354
Hispanic	0.147	0.354	0.140	0.347	0.146	0.354
Black non-hispanic	0.136	0.343	0.147	0.354	0.134	0.340
Other non-hispanic	0.069	0.253	0.073	0.261	0.066	0.249
Male	0.497	0.500			0.451	0.498
Disabled	0.153	0.360	0.009	0.093	0.077	0.267
Age 16-29	0.096	0.295	0.119	0.324	0.097	0.296
Age 30-39	0.169	0.375	0.171	0.377	0.161	0.368
Age 50-59	0.210	0.408	0.199	0.399	0.216	0.411
Age 60-69	0.154	0.361	0.154	0.361	0.143	0.350
Age 70 or more	0.158	0.365	0.156	0.363	0.166	0.372
Less than high school	0.134	0.341	0.131	0.337	0.089	0.285
High school graduate	0.254	0.435	0.288	0.453	0.274	0.446
Complete graduate and beyond	0.350	0.477	0.344	0.475	0.331	0.470
Household language is English only	0.699	0.459				
Speaks English poorly	0.067	0.249			0.055	0.229
Non-citizen	0.095	0.293			0.069	0.253
Anyone in household employed	0.741	0.438	0.714	0.452	0.525	0.499
Household income/poverty line	4.122	2.941	3.972	2.899	3.644	2.730
Reported housing assistance receipt			0.090	0.286	0.110	0.313

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. All demographic characteristics refer to the reference person. All estimates use household weights adjusted for PIK probability.

Table A3: Cross-tabulations of SNAP and Public Assistance Receipt According to Reports and Administrative Records, Full Linked Sample

American Community Survey

SNAP: Admin record		Survey report: No SNAP	SNAP	Total	Public Assistance: Admin record		Survey report: No PA	PA	Total
No SNAP	Pop Est. (1000s)	29333830	358630	29692460	No PA	Pop Est. (1000s)	33931820	556972	34488792
	Overall (%)	81.6%	1.0%	82.6%		Overall (%)	94.4%	1.5%	96.0%
	Row (%)	98.8%	1.2%	100%		Row (%)	98.4%	1.6%	100%
	Colum (%)	94.8%	7.2%	82.6%		Colum (%)	97.6%	47.1%	96.0%
	Sample count	457061	4695	461756		Sample count	518011	8555	526566
SNAP	Pop Est. (1000s)	1607057	4637630	6244686	PA	Pop Est. (1000s)	822890	625464	1448354
	Overall (%)	4.5%	12.9%	17.4%		Overall (%)	2.3%	1.7%	4.0%
	Row (%)	25.7%	74.3%	100%		Row (%)	56.8%	43.2%	100%
	Colum (%)	5.2%	92.8%	17.4%		Colum (%)	2.4%	52.9%	4.0%
	Sample count	18674	63098	81772		Sample count	9365	7597	16962
Total	Pop Est. (1000s)	30940887	4996259	35937146	Total	Pop Est. (1000s)	34754710	1182436	35937146
	Overall (%)	86.1%	13.9%	100%		Overall (%)	96.7%	3.3%	100%
	Row (%)	86.1%	13.9%	100%		Row (%)	96.7%	3.3%	100%
	Colum (%)	100%	100%	100%		Colum (%)	100%	100%	100%
	Sample count	475735	67793	543528		Sample count	527376	16152	543528

Current Population Survey

SNAP: Admin record		Survey report: No SNAP	SNAP	Total	Public Assistance: Admin record		Survey report: No PA	PA	Total
No SNAP	Pop Est. (1000s)	36720	761	37481	No PA	Pop Est. (1000s)	43328	269	43597
	Overall (%)	80.5%	1.7%	82.2%		Overall (%)	95.0%	0.6%	95.6%
	Row (%)	98.0%	2.0%	100%		Row (%)	99.4%	0.6%	100%
	Colum (%)	91.5%	13.9%	82.2%		Colum (%)	97.2%	26.5%	95.6%
	Sample count	14214	311	14525		Sample count	17033	123	17156
SNAP	Pop Est. (1000s)	3422	4703	8125	PA	Pop Est. (1000s)	1264	745	2009
	Overall (%)	7.5%	10.3%	17.8%		Overall (%)	2.8%	1.6%	4.4%
	Row (%)	42.1%	57.9%	100%		Row (%)	62.9%	37.1%	100%
	Colum (%)	8.5%	86.1%	17.8%		Colum (%)	2.8%	73.5%	4.4%
	Sample count	1509	2030	3539		Sample count	575	333	908
Total	Pop Est. (1000s)	40142	5464	45606	Total	Pop Est. (1000s)	44592	1014	45606
	Overall (%)	88.0%	12.0%	100%		Overall (%)	97.8%	2.2%	100%
	Row (%)	88.0%	12.0%	100%		Row (%)	97.8%	2.2%	100%
	Colum (%)	100%	100%	100%		Colum (%)	100%	100%	100%
	Sample count	15723	2341	18064		Sample count	17608	456	18064

Survey of Income and Program Participation

SNAP: Admin record		Survey report: No SNAP	SNAP	Total	Public Assistance: Admin record		Survey report: No PA	PA	Total
No SNAP	Pop Est. (1000s)	101572296	1555772	103128068	No PA	Pop Est. (1000s)	120200782	644334	120845116
	Overall (%)	81.0%	1.2%	82.3%		Overall (%)	95.9%	0.5%	96.4%
	Row (%)	98.5%	1.5%	100%		Row (%)	99.5%	0.5%	100%
	Colum (%)	95.9%	8.0%	82.3%		Colum (%)	98.3%	21.1%	96.4%
	Sample count	19840	386	20226		Sample count	23896	170	24066
SNAP	Pop Est. (1000s)	4309415	17909836	22219251	PA	Pop Est. (1000s)	2085904	2416299	4502203
	Overall (%)	3.4%	14.3%	17.7%		Overall (%)	1.7%	1.9%	3.6%
	Row (%)	19.4%	80.6%	100%		Row (%)	46.3%	53.7%	100%
	Colum (%)	4.1%	92.0%	17.7%		Colum (%)	1.7%	78.9%	3.6%
	Sample count	898	3873	4771		Sample count	422	509	931
Total	Pop Est. (1000s)	105881711	19465608	125347319	Total	Pop Est. (1000s)	122286686	3060633	125347319
	Overall (%)	84.5%	15.5%	100%		Overall (%)	97.6%	2.4%	100%
	Row (%)	84.5%	15.5%	100%		Row (%)	97.6%	2.4%	100%
	Colum (%)	100%	100%	100%		Colum (%)	100%	100%	100%
	Sample count	20738	4259	24997		Sample count	24318	679	24997

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. In the SIPP, we collapse receipt to the wave level. Estimates use household weights adjusted for PIK probability.

Table A4: The Determinants of False Negatives, Probit Marginal Effects, Linked Sample of True Recipients Below 200% of the Poverty Line

	ACS		CPS		SIPP	
	(1)	(2)	(3)	(4)	(5)	(6)
	SNAP	*PA*	*SNAP*	*PA*	*SNAP*	*PA*
Single adult, no children	-0.0209**	-0.0216	-0.0208	0.2381***	-0.0282	0.1090
	(0.0091)	(0.0217)	(0.0486)	(0.0874)	(0.0279)	(0.0989)
Single adult, with children	-0.0130*	-0.0404**	0.0233	0.0609	-0.0080	-0.0283
	(0.0076)	(0.0168)	(0.0358)	(0.0695)	(0.0238)	(0.0637)
Multiple adults, no children	0.0072	-0.0350*	-0.0343	0.0732	0.0553**	-0.0507
	(0.0078)	(0.0184)	(0.0414)	(0.0726)	(0.0236)	(0.0796)
Number of members under 18	-0.0151***	-0.0213***	-0.0305**	0.0401**	-0.0021	-0.1100***
	(0.0024)	(0.0047)	(0.0144)	(0.0173)	(0.0072)	(0.0212)
Number of members 18 or older	0.0118***	-0.0169**	0.0240	0.0812**	-0.0015	0.0425
	(0.0030)	(0.0077)	(0.0157)	(0.0367)	(0.0093)	(0.0296)
Rural	0.0008	-0.0074	-0.0011	-0.0824	0.0687***	-0.0364
	(0.0073)	(0.0224)	(0.0383)	(0.0870)	(0.0225)	(0.0813)
Hispanic	0.0445***	0.0396**	0.0598**	0.0806	0.0319*	0.0785
	(0.0065)	(0.0170)	(0.0242)	(0.0538)	(0.0178)	(0.0574)
Black non-hispanic	0.0635***	0.0735***	0.1076***	0.0615	0.0803***	0.1144**
	(0.0055)	(0.0138)	(0.0247)	(0.0546)	(0.0134)	(0.0475)
Other non-hispanic	0.0606***	0.0211	0.1346***	-0.0080	0.0117	0.1265
	(0.0093)	(0.0284)	(0.0425)	(0.1602)	(0.0200)	(0.0808)
Male	0.0330***	0.0453***	0.0208	0.0581	0.0991***	-0.0565
	(0.0043)	(0.0122)	(0.0209)	(0.0464)	(0.0130)	(0.0579)
Disabled	-0.0799***	-0.0658***	-0.0462		-0.1036***	-0.1085**
	(0.0048)	(0.0112)	(0.0813)		(0.0194)	(0.0494)
Age 16-29	-0.0202***	0.0369**	0.0420	-0.0572	-0.0088	0.0167
	(0.0070)	(0.0149)	(0.0325)	(0.0523)	(0.0240)	(0.0588)
Age 30-39	0.0030	0.0124	0.0103	-0.0457	-0.0250	0.2751***
	(0.0064)	(0.0142)	(0.0299)	(0.0488)	(0.0194)	(0.0502)
Age 50-59	-0.0110*	0.0089	-0.0051	0.0125	-0.0256	0.0079
	(0.0066)	(0.0149)	(0.0320)	(0.0580)	(0.0180)	(0.0578)
Age 60-69	-0.0089	0.1019***	0.0513	0.0756	-0.0796***	-0.0097
	(0.0075)	(0.0205)	(0.0361)	(0.0828)	(0.0223)	(0.0795)
Age 70 or more	-0.0050	0.1096***	0.0628*	0.2257	-0.0516**	-0.0522
	(0.0076)	(0.0292)	(0.0367)	(0.1655)	(0.0220)	(0.0993)
Less than high school	-0.0249***	-0.0139	-0.0766***	-0.0724	0.0017	-0.0074
	(0.0054)	(0.0125)	(0.0260)	(0.0478)	(0.0160)	(0.0551)
High school graduate	0.0114**	-0.0052	0.0076	-0.0412	0.0117	0.0112
	(0.0052)	(0.0128)	(0.0250)	(0.0479)	(0.0141)	(0.0467)
Complete graduate and beyond	0.0192**	0.0067	0.0359	-0.0670	0.0415*	-0.3570***
	(0.0077)	(0.0221)	(0.0351)	(0.0711)	(0.0219)	(0.0849)
Household language is English only	0.0052	-0.0361**				
	(0.0061)	(0.0152)				
Speaks English poorly	-0.0763***	0.0092			0.0013	0.1491**
	(0.0065)	(0.0179)			(0.0177)	(0.0737)
Non-citizen	0.0307***	0.0137			0.0184	-0.0461
	(0.0063)	(0.0156)			(0.0192)	(0.0682)
Anyone in household employed	0.0475***	0.1805***	0.0644***	0.0232	-0.0075	-0.2568***
	(0.0052)	(0.0112)	(0.0241)	(0.0422)	(0.0186)	(0.0495)
Household income/poverty line	0.0605***	0.0440***	0.0580***	0.0806**	0.0132	-0.0257
	(0.0044)	(0.0107)	(0.0203)	(0.0395)	(0.0142)	(0.0370)
Reported housing assistance receipt			-0.1838***	-0.0524	-0.0792***	-0.0653
			(0.0190)	(0.0361)	(0.0126)	(0.0441)
Reported public assistance receipt	-0.1670***		-0.3199***		-0.1765***	
	(0.0068)		(0.0357)		(0.0241)	
Reported SNAP receipt		-0.3165***		-0.4028***		-0.3117***
		(0.0140)		(0.0436)		(0.0537)
Linear time trend	-0.0101***	0.0159***	-0.0118**	-0.0107	0.0000**	0.0001***
	(0.0014)	(0.0034)	(0.0052)	(0.0106)	(0.0000)	(0.0000)
Number of observations	60,231	13,024	2,660	729	3,784	796
chi2 statistic of joint significance	2743	1959	382	194	313	466
p-value of joint significance	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The samples only include recipients according to the linked data with reported household income less than twice the household poverty line. The dependent variable is an indicator for failure to report receipt in the survey. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5: The Determinants of False Positives, Probit Marginal Effects, Linked Sample of Non-Recipients Below 200% of the Poverty Line

	ACS		CPS		SIPP	
	(1)	(2)	(3)	(4)	(5)	(6)
	SNAP	*PA*	*SNAP*	*PA*	*SNAP*	*PA*
Single adult, no children	-0.0212***	-0.0074***	-0.0497**	-0.0079	-0.0090	-0.0038
	(0.0036)	(0.0029)	(0.0225)	(0.0084)	(0.0133)	(0.0060)
Single adult, with children	-0.0041	0.0018	0.0098	-0.0108	0.0109	0.0062
	(0.0036)	(0.0026)	(0.0209)	(0.0071)	(0.0128)	(0.0054)
Multiple adults, no children	-0.0165***	-0.0020	-0.0340*	0.0000	-0.0076	-0.0053
	(0.0032)	(0.0025)	(0.0200)	(0.0067)	(0.0119)	(0.0048)
Number of members under 18	0.0009	0.0010	-0.0051	0.0047***	-0.0032	-0.0024
	(0.0011)	(0.0008)	(0.0071)	(0.0016)	(0.0047)	(0.0020)
Number of members 18 or older	0.0029**	0.0039***	-0.0009	0.0010	0.0113***	0.0031*
	(0.0012)	(0.0009)	(0.0085)	(0.0032)	(0.0043)	(0.0018)
Rural	-0.0064**	-0.0010	-0.0016	-0.0057	0.0080	0.0098
	(0.0025)	(0.0018)	(0.0195)	(0.0073)	(0.0087)	(0.0065)
Hispanic	0.0278***	-0.0043**	0.0460***	-0.0002	-0.0195*	-0.0050
	(0.0026)	(0.0021)	(0.0135)	(0.0044)	(0.0101)	(0.0035)
Black non-hispanic	0.0250***	0.0007	0.0404***	0.0051	0.0218***	0.0119***
	(0.0023)	(0.0017)	(0.0132)	(0.0046)	(0.0063)	(0.0030)
Other non-hispanic	0.0180***	0.0110***	0.0145	-0.0105	0.0085	-0.0116**
	(0.0030)	(0.0023)	(0.0180)	(0.0087)	(0.0091)	(0.0054)
Male	-0.0033**	0.0006	-0.0229**	-0.0063	-0.0175***	0.0057**
	(0.0016)	(0.0013)	(0.0107)	(0.0039)	(0.0054)	(0.0025)
Disabled	0.0206***	-0.0003	0.1010**		0.0156	-0.0034
	(0.0020)	(0.0015)	(0.0459)		(0.0100)	(0.0039)
Age 16-29	0.0209***	-0.0038*	-0.0010	0.0159***	0.0090	-0.0080
	(0.0029)	(0.0022)	(0.0184)	(0.0055)	(0.0115)	(0.0057)
Age 30-39	0.0061**	-0.0010	0.0053	-0.0032	0.0427***	-0.0071*
	(0.0027)	(0.0021)	(0.0167)	(0.0052)	(0.0092)	(0.0040)
Age 50-59	0.0053*	-0.0050**	0.0030	-0.0035	0.0043	-0.0065*
	(0.0028)	(0.0021)	(0.0172)	(0.0058)	(0.0091)	(0.0033)
Age 60-69	0.0003	-0.0169***	-0.0127	-0.0205**	-0.0023	-0.0106**
	(0.0032)	(0.0023)	(0.0190)	(0.0084)	(0.0109)	(0.0042)
Age 70 or more	-0.0084**	-0.0225***	-0.0397**	-0.0297***	0.0045	-0.0204***
	(0.0033)	(0.0023)	(0.0201)	(0.0092)	(0.0097)	(0.0058)
Less than high school	0.0154***	0.0083***	0.0220	0.0068	0.0038	0.0013
	(0.0023)	(0.0017)	(0.0148)	(0.0050)	(0.0098)	(0.0030)
High school graduate	0.0053**	0.0043***	-0.0034	0.0108**	0.0038	0.0001
	(0.0021)	(0.0016)	(0.0134)	(0.0047)	(0.0065)	(0.0031)
Complete graduate and beyond	-0.0093***	-0.0017	-0.0466***	-0.0155*	-0.0039	-0.0067
	(0.0027)	(0.0021)	(0.0173)	(0.0084)	(0.0077)	(0.0045)
Household language is English only	0.0054**	0.0025				
	(0.0022)	(0.0018)				
Speaks English poorly	0.0136***	-0.0013			0.0201**	-0.0024
	(0.0027)	(0.0020)			(0.0095)	(0.0034)
Non-citizen	-0.0033	-0.0044**			0.0128	0.0059
	(0.0023)	(0.0021)			(0.0089)	(0.0036)
Household income/poverty line	-0.0143***	0.0068***	-0.0293***	0.0013	-0.0110**	-0.0029
	(0.0014)	(0.0013)	(0.0089)	(0.0036)	(0.0044)	(0.0026)
Anyone in household employed	-0.0090***	-0.0216***	-0.0357***	-0.0098**	0.0242***	0.0122**
	(0.0023)	(0.0018)	(0.0132)	(0.0048)	(0.0075)	(0.0049)
Reported housing assistance receipt			0.0234*	0.0068*	0.0552***	0.0094***
			(0.0137)	(0.0037)	(0.0074)	(0.0026)
Reported public assistance receipt	0.0835***		0.1582***		0.0909***	
	(0.0034)		(0.0296)		(0.0140)	
Reported SNAP receipt		0.0567***		0.0243***		0.0252***
		(0.0015)		(0.0041)		(0.0043)
Linear time trend	0.0024***	0.0002	0.0109***	0.0002	0.0000***	0.0000***
	(0.0005)	(0.0004)	(0.0030)	(0.0009)	(0.0000)	(0.0000)
Number of observations	89,087	136,294	3,051	4,982	5,035	8,023
chi2 statistic of joint significance	1258	1986	163	90	204	98
p-value of joint significance	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The samples only include non-recipients according to the linked data with reported household income less than twice the household poverty line. The dependent variable is an indicator for reporting in the survey. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. Standard errors in parentheses,*** p<0.01, ** p<0.05. * p<0.1

Table A6: The Determinants of Reported and Administrative SNAP Receipt, Probit Coefficients, Linked Households with Income less than Twice the Poverty Line

	ACS			CPS			SIPP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Survey Report	Admin. Receipt	P-value (1)=(2)	Survey Report	Admin. Receipt	P-value (4)=(5)	Survey Report	Admin. Receipt	P-value (7)=(8)
Single adult, no children	-0.2454***	-0.2874***	0.034	-0.1037	-0.0334	0.495	-0.1578	-0.1544	0.961
	(0.0232)	(0.0232)		(0.1040)	(0.0999)		(0.0991)	(0.1024)	
Single adult, with children	0.1081***	0.1614***	0.004	0.1025	0.3879***	0.002	0.4084***	0.5032***	0.132
	(0.0215)	(0.0216)		(0.0856)	(0.0859)		(0.0871)	(0.0870)	
Multiple adults, no children	-0.1955***	-0.1991***	0.833	-0.0429	-0.0834	0.664	-0.1604*	-0.0856	0.191
	(0.0204)	(0.0201)		(0.0906)	(0.0850)		(0.0851)	(0.0852)	
Number of members under 18	0.1025***	0.1127***	0.075	0.1104***	0.1402***	0.350	0.1020***	0.1443***	0.025
	(0.0067)	(0.0065)		(0.0324)	(0.0300)		(0.0289)	(0.0298)	
Number of members 18 or older	0.0279***	0.0680***	<0.001	0.0099	0.1300***	0.001	0.0745**	0.0748**	0.991
	(0.0085)	(0.0084)		(0.0377)	(0.0366)		(0.0374)	(0.0377)	
Rural	-0.0785***	-0.0927***	0.157	0.1179	0.1884***	0.317	-0.0390	0.0101	0.053
	(0.0140)	(0.0140)		(0.0746)	(0.0715)		(0.0530)	(0.0525)	
Hispanic	0.3404***	0.4726***	<0.001	0.2626***	0.4781***	<0.001	0.2676***	0.4135***	<0.001
	(0.0151)	(0.0151)		(0.0528)	(0.0513)		(0.0600)	(0.0614)	
Black non-hispanic	0.3790***	0.5746***	<0.001	0.1027*	0.3555***	<0.001	0.2917***	0.4650***	<0.001
	(0.0132)	(0.0132)		(0.0557)	(0.0539)		(0.0459)	(0.0461)	
Other non-hispanic	-0.1371***	-0.1473***	0.534	-0.1282	-0.0010	0.119	0.2706***	0.2916***	0.628
	(0.0208)	(0.0207)		(0.0862)	(0.0809)		(0.0637)	(0.0617)	
Male	-0.1896***	-0.1996***	0.205	-0.1876***	-0.2118***	0.570	-0.2772***	-0.1106***	<0.001
	(0.0099)	(0.0099)		(0.0443)	(0.0426)		(0.0407)	(0.0406)	
Disabled	0.5518***	0.5194***	<0.001	0.4464**	0.3098	0.538	0.9736***	0.9618***	0.750
	(0.0112)	(0.0111)		(0.2055)	(0.2097)		(0.0590)	(0.0589)	
Age 16-29	0.1831***	0.1597***	0.120	-0.0775	0.0756	0.043	0.1225*	0.1344*	0.818
	(0.0176)	(0.0177)		(0.0742)	(0.0717)		(0.0718)	(0.0715)	
Age 30-39	0.0935***	0.1228***	0.039	-0.0118	0.0006	0.862	0.3621***	0.3084***	0.327
	(0.0167)	(0.0169)		(0.0674)	(0.0666)		(0.0656)	(0.0669)	
Age 50-59	0.0390**	0.0158	0.083	0.0985	0.1745**	0.289	0.0667	0.0529	0.738
	(0.0164)	(0.0162)		(0.0714)	(0.0702)		(0.0605)	(0.0592)	
Age 60-69	-0.0155	-0.0368**	0.140	-0.1635**	-0.1025	0.436	0.1618**	0.0927	0.129
	(0.0182)	(0.0179)		(0.0792)	(0.0776)		(0.0733)	(0.0723)	
Age 70 or more	-0.3591***	-0.4246***	<0.001	-0.3723***	-0.3265***	0.546	0.1943***	0.1336*	0.196
	(0.0180)	(0.0179)		(0.0796)	(0.0764)		(0.0716)	(0.0706)	
Less than high school	0.2537***	0.2507***	0.783	0.2932***	0.2918***	0.980	0.2207***	0.2875***	0.066
	(0.0134)	(0.0133)		(0.0584)	(0.0571)		(0.0578)	(0.0577)	
High school graduate	0.0493***	0.0799***	0.002	-0.0267	0.0200	0.395	0.0604	0.1018**	0.149
	(0.0122)	(0.0121)		(0.0547)	(0.0528)		(0.0457)	(0.0450)	
Complete graduate and beyond	-0.2726***	-0.2899***	0.149	-0.2756***	-0.2164***	0.385	-0.3398***	-0.3425***	0.950
	(0.0160)	(0.0159)		(0.0716)	(0.0684)		(0.0556)	(0.0602)	
Household language is English only	-0.0542***	-0.0627***	0.432						
	(0.0136)	(0.0137)							
Speaks English poorly	0.5377***	0.4971***	0.003				0.4992***	0.5671***	0.158
	(0.0167)	(0.0170)					(0.0690)	(0.0713)	
Non-citizen	-0.3782***	-0.4381***	<0.001				-0.3106***	-0.3707***	0.265
	(0.0163)	(0.0163)					(0.0736)	(0.0713)	
Household income/poverty line	-0.4644***	-0.4691***	0.529	-0.3223***	-0.3165***	0.888	-0.3603***	-0.3671***	0.790
	(0.0092)	(0.0093)		(0.0384)	(0.0385)		(0.0334)	(0.0339)	
Anyone in household employed	-0.3512***	-0.3586***	0.490	-0.3741***	-0.4313***	0.296	0.2602***	0.2599***	0.993
	(0.0131)	(0.0132)		(0.0540)	(0.0542)		(0.0502)	(0.0504)	
Reported housing assistance receipt				1.3405***	1.0840***	0.957	1.4572***	1.2004***	0.013
				(0.1075)	(0.1168)		(0.1141)	(0.1089)	
Reported public assistance receipt	1.2478***	1.1711***	0.001	0.0680***	0.0584***	0.045	0.9093***	0.8389***	0.006
	(0.0223)	(0.0233)		(0.0117)	(0.0114)		(0.0445)	(0.0445)	
Linear time trend	0.0852***	0.0902***	0.050	0.7745***	0.7771***	0.405	-0.0001***	-0.0001***	0.241
	(0.0032)	(0.0032)		(0.0486)	(0.0497)		(0.0000)	(0.0000)	
Constant	-0.2796***	-0.1618***		-0.4732***	-0.5515***		-0.8775***	-0.9463***	
	(0.0355)	(0.0355)		(0.1508)	(0.1474)		(0.1378)	(0.1425)	
Number of observations	149,318	149,318		5,711	5,711		8,819	8,819	
Joint test of equality chi2 statistic			45			3			6
Joint test of equality p-value			<0.001			<0.001			<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. For each survey, the first column contains estimates from a probit model using reported receipt as the dependent variable. The second column estimates the same model using the administrative receipt measure as the dependent variable. The thrid column contains p-values of a chi-square test whether the estimates are equal. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses conducted using household weights adjusted for PIK probability. Standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1

Table A7: The Determinants of Reported and Administrative Public Assistance Receipt, Probit Coefficients, Linked Households with Income less than Twice the Poverty Line

	ACS			CPS			SIPP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Survey	Admin.	P-value	Survey	Admin.	P-value	Survey	Admin.	P-value
	Report	Receipt	(1)=(2)	Report	Receipt	(4)=(5)	Report	Receipt	(7)=(8)
Single adult, no children	-0.1972***	-0.4158***	<0.001	-0.0270	-0.1166	0.294	-0.3194**	-0.3345***	0.913
	(0.0314)	(0.0314)		(0.1355)	(0.1088)		(0.1398)	(0.1275)	
Single adult, with children	0.1884***	0.2531***	0.027	0.0538	0.0480*	0.013	0.5363***	0.5115***	0.802
	(0.0264)	(0.0253)		(0.0360)	(0.0278)		(0.1149)	(0.1043)	
Multiple adults, no children	-0.0451*	-0.1904***	<0.001	-0.0250	0.1206***	0.548	0.0464	0.2402**	0.099
	(0.0273)	(0.0267)		(0.0715)	(0.0454)		(0.1221)	(0.1122)	
Number of members under 18	0.0528***	0.0489***	0.641	-0.0554	0.0776	0.891	0.1522***	0.1253***	0.355
	(0.0076)	(0.0073)		(0.0811)	(0.0636)		(0.0321)	(0.0299)	
Number of members 18 or older	0.0923***	0.1323***	0.001	0.2649***	0.1004	0.058	0.0835*	0.1101**	0.583
	(0.0109)	(0.0105)		(0.1022)	(0.0867)		(0.0449)	(0.0463)	
Rural	-0.0868***	-0.2139***	<0.001	0.0026	-0.0245	0.514	0.2248*	0.2161*	0.930
	(0.0237)	(0.0277)		(0.0188)	(0.0162)		(0.0530)	(0.0525)	
Hispanic	0.0956***	0.4412***	<0.001	-0.3300	-0.1988	<0.001	-0.0131	0.2667***	<0.001
	(0.0224)	(0.0226)		(0.2035)	(0.1595)		(0.0843)	(0.0761)	
Black non-hispanic	0.1478***	0.5740***	<0.001	-0.2914***	-0.3297***	<0.001	0.3093***	0.6030***	<0.001
	(0.0186)	(0.0192)		(0.0856)	(0.0715)		(0.0697)	(0.0678)	
Other non-hispanic	0.1226***	0.0583*	0.093	-0.2517***	-0.3844***	0.514	-0.1918*	0.2551**	<0.001
	(0.0296)	(0.0337)		(0.0768)	(0.0581)		(0.1082)	(0.1013)	
Male	-0.0426***	-0.0443***	0.922	-0.0282	-0.1612**	0.110	0.0164	-0.1374**	0.024
	(0.0151)	(0.0157)		(0.0913)	(0.0749)		(0.0692)	(0.0644)	
Disabled	0.0829***	0.1038***	0.263	0.2492***	0.1683***	<0.001	0.2104**	0.2957***	0.269
	(0.0167)	(0.0174)		(0.0702)	(0.0589)		(0.0895)	(0.0793)	
Age 16-29	-0.0249	0.0934***	<0.001	-0.1781	-0.1855**	0.132	-0.0900	0.0259	0.190
	(0.0230)	(0.0221)		(0.1095)	(0.0870)		(0.0980)	(0.0896)	
Age 30-39	-0.0364*	-0.0398*	0.889	-0.7487***	-0.6263***	0.151	-0.4895***	-0.0972	<0.001
	(0.0216)	(0.0211)		(0.1491)	(0.1167)		(0.0922)	(0.0814)	
Age 50-59	-0.0838***	-0.0329	0.038	-1.2591***	-1.2203***	0.946	-0.1700*	-0.2180**	0.544
	(0.0225)	(0.0219)		(0.1949)	(0.1362)		(0.0925)	(0.0866)	
Age 60-69	-0.4328***	-0.4159***	0.600	0.1981**	0.0661	0.442	-0.6230***	-0.8018***	0.114
	(0.0273)	(0.0282)		(0.0930)	(0.0774)		(0.1230)	(0.1153)	
Age 70 or more	-0.5928***	-0.9053***	<0.001	0.2169**	0.0569	0.851	-0.6980***	-0.8592***	0.218
	(0.0269)	(0.0326)		(0.0933)	(0.0774)		(0.1381)	(0.1198)	
Less than high school	0.1265***	0.1336***	0.734	-0.0253	-0.0331	0.165	0.2395***	0.2661***	0.739
	(0.0189)	(0.0189)		(0.1312)	(0.1058)		(0.0886)	(0.0805)	
High school graduate	0.0375**	0.0175	0.335	0.1385	0.5611***	0.100	0.0103	-0.0169	0.715
	(0.0182)	(0.0184)		(0.0886)	(0.0774)		(0.0810)	(0.0696)	
Complete graduate and beyond	-0.0712***	-0.1665***	0.002	0.2330**	0.6163***	0.951	0.1574	-0.0908	0.001
	(0.0256)	(0.0272)		(0.0931)	(0.0786)		(0.1091)	(0.0976)	
Household language is English only	0.0897***	0.1223***	0.172						
	(0.0206)	(0.0205)							
Speaks English poorly	-0.1000***	-0.1803***	0.003				-0.3463***	-0.3652***	0.848
	(0.0234)	(0.0233)					(0.0998)	(0.0948)	
Non-citizen	-0.0292	-0.0054	0.347				0.1587	0.0060	0.117
	(0.0227)	(0.0214)					(0.1009)	(0.0950)	
Household income/poverty line	-0.1228***	-0.3716***	<0.001	-0.6100*	0.0420	0.082	-0.1526**	-0.2999***	0.011
	(0.0145)	(0.0151)		(0.3672)	(0.2538)		(0.0639)	(0.0534)	
Anyone in household employed	-0.5083***	-0.3550***	<0.001	-	-	0.648	0.6587***	0.4640***	0.028
	(0.0188)	(0.0186)		0	0		(0.1067)	(0.0811)	
Reported housing assistance receipt				0	0	0.240	1.1948***	0.9173***	0.076
				5,711	5,711		(0.0875)	(0.0675)	
Reported SNAP receipt	1.0323***	0.9212***	<0.001	-1.8903***	-1.4785***	<0.001	0.2144***	0.1029	<0.001
	(0.0167)	(0.0163)		(0.2544)	(0.1915)		(0.0659)	(0.0640)	
Linear time trend	-0.0245***	-0.0297***	0.347	1.0916***	0.7682***	0.175	-0.0001	-0.0001**	0.172
	(0.0048)	(0.0049)		(0.0830)	(0.0601)		(0.0001)	(0.0001)	
Constant	1.0323***	0.9212***		-1.8903***	-1.4785***		0.2144***	0.1029	
	(0.0482)	(0.0482)		(0.1508)	(0.1474)		(0.1378)	(0.1425)	
Number of observations	149,318	149,318		5,711	5,711		8,819	8,819	
Joint test of equality chi2 statistic			67			3			5
Joint test of equality p-value			<0.001			<0.001			<0.001

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. For each survey, the first column contains estimates from a probit model using reported receipt as the dependent variable. The second column estimates the same model using the administrative receipt measure as the dependent variable. The thrid column contains p-values of a chi-square test whether the estimates are equal. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A8: Testing Whether Item Non-Response is Conditionally Random by Adding an Response Indicator to Models of Program Receipt According to the Administrative Measure, Probit Marginal Effects, Full Linked Sample

	ACS		CPS		SIPP	
	SNAP	PA	SNAP	PA	SNAP	PA
	(1)	(2)	(3)	(4)	(5)	(6)
Item Non-Respondent	0.058***	0.015***	0.004	0.005	0.036***	-0.001
	(0.004)	(0.001)	(0.007)	(0.004)	(0.007)	(0.004)
Single adult, no children	-0.015***	-0.022***	0.017	-0.008	-0.012	-0.030***
	(0.003)	(0.001)	(0.011)	(0.006)	(0.010)	(0.005)
Single adult, with children	0.056***	0.017***	0.076***	0.031***	0.087***	0.021***
	(0.002)	(0.001)	(0.010)	(0.005)	(0.009)	(0.004)
Multiple adults, no children	-0.011***	-0.009***	0.007	-0.004	-0.004	-0.002
	(0.002)	(0.001)	(0.009)	(0.005)	(0.009)	(0.005)
Number of members under 18	0.018***	0.003***	0.024***	0.004***	0.018***	0.004***
	(0.001)	(0.000)	(0.004)	(0.001)	(0.003)	(0.001)
Number of members 18 or older	0.029***	0.007***	0.036***	0.009***	0.028***	0.002
	(0.001)	(0.000)	(0.004)	(0.002)	(0.003)	(0.002)
Rural	-0.018***	-0.011***	0.020**	0.012**	0.000***	0.000***
	(0.002)	(0.001)	(0.008)	(0.005)	(0.000)	(0.000)
Hispanic	0.079***	0.022***	0.094***	0.030***	0.058***	0.019***
	(0.002)	(0.001)	(0.006)	(0.003)	(0.007)	(0.003)
Black non-hispanic	0.109***	0.033***	0.077***	0.037***	0.074***	0.033***
	(0.001)	(0.001)	(0.006)	(0.004)	(0.006)	(0.003)
Other non-hispanic	0.003	0.005***	0.014	-0.011	0.064***	0.010**
	(0.002)	(0.002)	(0.010)	(0.008)	(0.007)	(0.004)
Male	-0.033***	-0.002***	-0.020***	0.001	-0.015***	-0.007***
	(0.001)	(0.001)	(0.005)	(0.003)	(0.004)	(0.003)
Disabled	0.077***	0.005***	0.058**	0.004	0.154***	0.014***
	(0.001)	(0.001)	(0.023)	(0.011)	(0.007)	(0.003)
Age 16-29	0.036***	0.006***	0.031***	0.005	0.044***	0.006
	(0.002)	(0.001)	(0.009)	(0.004)	(0.009)	(0.004)
Age 30-39	0.020***	-0.002*	0.026***	-0.010***	0.041***	-0.002
	(0.002)	(0.001)	(0.008)	(0.004)	(0.007)	(0.003)
Age 50-59	0.005***	0.001	0.016*	-0.003	0.014**	0.005
	(0.002)	(0.001)	(0.008)	(0.004)	(0.007)	(0.003)
Age 60-69	-0.009***	-0.017***	-0.014	-0.021***	0.010	-0.026***
	(0.002)	(0.001)	(0.009)	(0.005)	(0.008)	(0.005)
Age 70 or more	-0.069***	-0.043***	-0.050***	-0.066***	0.016**	-0.032***
	(0.002)	(0.001)	(0.010)	(0.008)	(0.008)	(0.005)
Less than high school	0.047***	0.008***	0.051***	0.006	0.052***	0.009***
	(0.002)	(0.001)	(0.007)	(0.004)	(0.007)	(0.003)
High school graduate	0.017***	0.002***	0.005	0.005	0.027***	-0.003
	(0.001)	(0.001)	(0.006)	(0.004)	(0.005)	(0.003)
Complete graduate and beyond	-0.030***	-0.008***	-0.040***	0.001	-0.048***	-0.008**
	(0.002)	(0.001)	(0.007)	(0.004)	(0.006)	(0.004)
Household language is English only	-0.019***	0.005***			0.023***	0.002
	(0.002)	(0.001)			(0.006)	(0.004)
Speaks English poorly	0.068***	-0.011***			0.074***	-0.020***
	(0.002)	(0.001)			(0.009)	(0.004)
Non-citizen	-0.056***	-0.001			-0.044***	0.006
	(0.002)	(0.001)			(0.010)	(0.005)
Household income/poverty line	-0.040***	-0.007***	-0.037***	-0.007***	-0.038***	-0.004***
	(0.000)	(0.000)	(0.002)	(0.001)	(0.002)	(0.001)
Anyone in household employed	-0.069***	-0.020***	-0.081***	-0.025***	0.035***	0.025***
	(0.002)	(0.001)	(0.007)	(0.004)	(0.005)	(0.003)
Reported housing assistance receipt			0.135***	0.012***		
			(0.007)	(0.003)		
Reported public assistance receipt	0.211***		0.187***		0.172***	
	(0.003)		(0.016)		(0.013)	
Reported SNAP receipt		0.058***		0.052***		0.044***
		(0.001)		(0.003)		(0.003)
Linear time trend	0.014***	-0.001***	0.010***	0.000	0.122***	0.007**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.006)	(0.003)
Observations	543,528	543,528	18,064	18,064	24,997	24,997

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The dependent variable is program receipt according to the administrative variable in all columns. Each column contains the same covariates as the models of receipt in Tables 7 and 8 and additionally include an indicator for responding to the survey question on program receipt. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. SEs in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Differences Between the Full Sample and Item Non-Respondents in the Determinants of Program Receipt, Probit Marginal Effects

	ACS				CPS				SIPP			
	SNAP		PA		SNAP		PA		SNAP		PA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample:	Full Sample	Non-Respondents	Full Sample	Non-Respondents	Full Sample	Non-Respondents	Full Sample	Non-Respondents	Full Sample	Non-Respondents	Full Sample	Non-Respondents
Single adult, no children	-0.015***	-0.016	-0.021***	-0.025***	0.017	0.024	-0.008	0.017	-0.012	-0.157***	-0.030***	0.014
	(0.003)	(0.032)	(0.001)	(0.007)	(0.011)	(0.035)	(0.006)	(0.019)	(0.010)	(0.041)	(0.005)	(0.024)
Single adult, with children	0.056***	0.111***	0.017***	0.035***	0.076***	0.062**	0.031***	0.031**	0.089***	0.077**	0.021***	0.048***
	(0.002)	(0.034)	(0.001)	(0.006)	(0.010)	(0.031)	(0.005)	(0.013)	(0.009)	(0.038)	(0.004)	(0.015)
Multiple adults, no children	-0.011***	-0.019	-0.009***	-0.002	0.007	-0.022	-0.004	-0.004	-0.004	-0.079***	-0.002	0.017
	(0.002)	(0.028)	(0.001)	(0.006)	(0.009)	(0.029)	(0.005)	(0.015)	(0.009)	(0.030)	(0.005)	(0.016)
Number of members under 18	0.018***	0.042***	0.003***	0.007***	0.024***	0.031***	0.004***	0.013***	0.018***	0.000	0.004***	0.008
	(0.001)	(0.011)	(0.000)	(0.002)	(0.004)	(0.011)	(0.001)	(0.004)	(0.003)	(0.011)	(0.001)	(0.005)
Number of members 18 or older	0.029***	0.035***	0.007***	0.011***	0.036***	0.033***	0.009***	0.013***	0.030***	0.033***	0.002	0.006
	(0.001)	(0.008)	(0.000)	(0.002)	(0.004)	(0.011)	(0.002)	(0.005)	(0.003)	(0.010)	(0.002)	(0.005)
Rural	-0.018***	-0.068***	-0.011***	-0.017***	0.020**	0.097***	0.013**	0.025	0.000***	0.000**	0.000***	0.000
	(0.002)	(0.024)	(0.001)	(0.006)	(0.008)	(0.026)	(0.005)	(0.015)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic	0.080***	0.140***	0.022***	0.033***	0.094***	0.152***	0.029***	0.026**	0.058***	0.110***	0.019***	0.021
	(0.002)	(0.019)	(0.001)	(0.005)	(0.006)	(0.018)	(0.003)	(0.011)	(0.007)	(0.025)	(0.003)	(0.014)
Black non-hispanic	0.110***	0.197***	0.034***	0.055***	0.077***	0.136***	0.037***	0.051***	0.074***	0.079***	0.033***	0.036***
	(0.001)	(0.017)	(0.001)	(0.004)	(0.006)	(0.018)	(0.004)	(0.010)	(0.006)	(0.022)	(0.003)	(0.012)
Other non-hispanic	0.003	0.004	0.006***	0.004	0.014	0.053*	-0.011	0.000	0.063***	0.015	0.010**	0.045***
	(0.002)	(0.028)	(0.001)	(0.008)	(0.010)	(0.031)	(0.008)	(0.021)	(0.007)	(0.036)	(0.004)	(0.017)
Male	-0.033***	-0.049***	-0.002***	-0.008**	-0.020***	-0.071***	0.001	-0.031***	-0.016***	-0.010	-0.007***	-0.023**
	(0.001)	(0.013)	(0.001)	(0.003)	(0.005)	(0.016)	(0.003)	(0.010)	(0.004)	(0.017)	(0.003)	(0.011)
Disabled	0.077***	0.091***	0.005***	0.011***	0.058**	0.133**	0.005	0.070***	0.154***	0.235***	0.014***	0.016
	(0.001)	(0.016)	(0.001)	(0.004)	(0.023)	(0.066)	(0.011)	(0.025)	(0.007)	(0.037)	(0.003)	(0.015)
Age 16-29	0.036***	0.040	0.006***	0.021***	0.031***	0.091***	0.005	0.016	0.046***	0.070**	0.006	0.006
	(0.002)	(0.030)	(0.001)	(0.005)	(0.009)	(0.027)	(0.004)	(0.012)	(0.009)	(0.031)	(0.004)	(0.014)
Age 30-39	0.020***	-0.018	-0.002**	-0.003	0.026***	0.082***	-0.010***	0.001	0.041***	0.051**	-0.002	-0.001
	(0.002)	(0.025)	(0.001)	(0.005)	(0.008)	(0.024)	(0.004)	(0.011)	(0.007)	(0.025)	(0.003)	(0.013)
Age 50-59	0.005***	0.028	0.001	-0.004	0.016*	-0.002	-0.003	-0.010	0.014**	0.014	0.005	0.006
	(0.002)	(0.022)	(0.001)	(0.005)	(0.008)	(0.026)	(0.004)	(0.014)	(0.007)	(0.025)	(0.003)	(0.015)
Age 60-69	-0.009***	0.012	-0.017***	-0.043***	-0.014	0.003	-0.021***	-0.036**	0.009	-0.021	-0.026***	-0.068**
	(0.002)	(0.023)	(0.001)	(0.006)	(0.009)	(0.030)	(0.005)	(0.017)	(0.008)	(0.032)	(0.005)	(0.030)
Age 70 or more	-0.069***	-0.068***	-0.043***	-0.086***	-0.050***	-0.051	-0.067***	-0.094***	0.015**	0.024	-0.032***	-0.037**
	(0.002)	(0.024)	(0.001)	(0.007)	(0.010)	(0.031)	(0.008)	(0.026)	(0.008)	(0.034)	(0.005)	(0.018)
Less than high school	0.047***	0.007	0.008***	0.005	0.051***	0.091***	0.006	0.013	0.051***	0.143***	0.009***	0.012
	(0.002)	(0.019)	(0.001)	(0.004)	(0.007)	(0.022)	(0.004)	(0.012)	(0.007)	(0.028)	(0.003)	(0.014)
High school graduate	0.017***	0.008	0.002***	-0.002	0.005	0.041**	0.005	0.011	0.026***	0.051***	-0.003	0.010
	(0.001)	(0.018)	(0.001)	(0.004)	(0.006)	(0.020)	(0.004)	(0.011)	(0.005)	(0.019)	(0.003)	(0.011)
Complete graduate and beyond	-0.030***	-0.032	-0.008***	-0.023***	-0.040***	-0.053**	0.000	-0.006	-0.048***	-0.014	-0.008**	-0.001
	(0.002)	(0.020)	(0.001)	(0.006)	(0.007)	(0.024)	(0.004)	(0.013)	(0.006)	(0.024)	(0.004)	(0.016)
Household language is English only	-0.020***	-0.042**	0.005***	0.010**					0.023***	0.000	0.002	0.007
	(0.002)	(0.017)	(0.001)	(0.004)					(0.006)	(0.023)	(0.004)	(0.015)
Speaks English poorly	0.068***	0.110***	-0.011***	-0.016***					0.073***	0.056	-0.020***	-0.016
	(0.002)	(0.023)	(0.001)	(0.006)					(0.009)	(0.041)	(0.004)	(0.020)
Non-citizen	-0.056***	-0.046**	-0.001	0.003					-0.044***	-0.061*	0.006	-0.020
	(0.002)	(0.022)	(0.001)	(0.005)					(0.010)	(0.032)	(0.005)	(0.015)
Household income/poverty line	-0.040***	-0.032***	-0.007***	-0.009***	-0.037***	-0.030***	-0.007***	-0.003	-0.037***	-0.034***	-0.004***	-0.013***
	(0.000)	(0.003)	(0.000)	(0.001)	(0.002)	(0.005)	(0.001)	(0.003)	(0.002)	(0.005)	(0.001)	(0.004)
Anyone in household employed	-0.069***	-0.072***	-0.021***	-0.032***	-0.081***	-0.108***	-0.025***	-0.040***	0.037***	0.063***	0.025***	0.025**
	(0.002)	(0.019)	(0.001)	(0.004)	(0.007)	(0.020)	(0.004)	(0.010)	(0.005)	(0.022)	(0.003)	(0.013)
Reported housing assistance receipt					0.135***	0.088***	0.012***	0.001				
					(0.007)	(0.023)	(0.003)	(0.012)				
Reported public assistance receipt	0.211***	0.178***			0.187***	0.052			0.173***	0.166***		
	(0.003)	(0.029)			(0.016)	(0.039)			(0.013)	(0.041)		
Reported SNAP receipt			0.058***	0.103***			0.052***	0.031***			0.044***	0.062***
			(0.001)	(0.003)			(0.003)	(0.010)			(0.003)	(0.011)
Linear time trend	0.014***	0.022***	-0.001***	-0.002**	0.010***	0.013***	0.000	-0.003	0.123***	0.127***	0.007**	-0.011
	(0.000)	(0.005)	(0.000)	(0.001)	(0.001)	(0.004)	(0.001)	(0.002)	(0.006)	(0.028)	(0.003)	(0.013)
Number of observations	543,528	6,096	543,528	35,589	18,064	2,336	18,064	2,346	24,997	1,941	24,997	1,928
Chi2 distance measure		18,326		11,704		684		383		1,192		617

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The dependent variable is receipt according to the administrative data in all columns. For each survey and program, the first column estimates the model using the full linked sample, the second column estimates the same model using the sample of item non-respondents. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. The distance measure is the squared distance between the two coefficient vectors, weighted by the variance matrix of the coefficients from using the full sample with the administrative dependent variable. SEs in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: The Determinants of Administrative and Imputed Program Receipt Among Item Non-Respondents, Probit Marginal Effects

	ACS				CPS				SIPP			
	SNAP		PA		SNAP		PA		SNAP		PA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable:	Admin. Receipt	Imputed Receipt	Admin. Receipt	Imputed Receipt	Admin. Receipt	Imputed Receipt	Admin. Receipt	Imputed Receipt	Admin. Receipt	Imputed Receipt	Admin. Receipt	Imputed Receipt
Single adult, no children	-0.016	-0.072**	-0.025***	-0.023***	0.024	0.025	0.017	-0.043***	-0.157***	-0.049	0.014	0.031*
	(0.032)	(0.028)	(0.007)	(0.008)	(0.035)	(0.029)	(0.019)	(0.014)	(0.041)	(0.034)	(0.024)	(0.019)
Single adult, with children	0.111***	0.011	0.035***	0.025***	0.062**	0.023	0.031**	-0.008	0.077**	0.032	0.048***	0.028**
	(0.034)	(0.030)	(0.006)	(0.007)	(0.031)	(0.024)	(0.013)	(0.010)	(0.038)	(0.032)	(0.015)	(0.014)
Multiple adults, no children	-0.019	-0.062**	-0.002	-0.002	-0.022	0.023	-0.004	-0.018*	-0.079***	-0.033	0.017	0.014
	(0.028)	(0.025)	(0.006)	(0.007)	(0.029)	(0.025)	(0.015)	(0.010)	(0.030)	(0.025)	(0.016)	(0.014)
Number of members under 18	0.042***	0.004	0.007***	0.009***	0.031***	0.008	0.013***	0.004	0.000	0.012	0.008	0.013***
	(0.011)	(0.009)	(0.002)	(0.002)	(0.011)	(0.009)	(0.004)	(0.003)	(0.011)	(0.010)	(0.005)	(0.004)
Number of members 18 or older	0.035***	-0.024***	0.011***	0.011***	0.033***	-0.002	0.013***	0.002	0.033***	0.013	0.006	0.001
	(0.008)	(0.008)	(0.002)	(0.002)	(0.011)	(0.010)	(0.005)	(0.004)	(0.010)	(0.009)	(0.005)	(0.004)
Rural	-0.068***	-0.034	-0.017***	-0.015**	0.097***	0.009	0.025	-0.014	0.000**	0.000***	0.000	0.000
	(0.024)	(0.023)	(0.006)	(0.006)	(0.026)	(0.022)	(0.015)	(0.011)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic	0.140***	0.074***	0.033***	0.000	0.152***	0.029*	0.026*	-0.008	0.110***	0.085***	0.021	-0.017
	(0.019)	(0.018)	(0.005)	(0.006)	(0.018)	(0.016)	(0.011)	(0.008)	(0.025)	(0.023)	(0.014)	(0.011)
Black non-hispanic	0.197***	0.104***	0.055***	0.021***	0.136***	0.012	0.051***	0.005	0.079***	0.057***	0.036***	0.013
	(0.017)	(0.015)	(0.004)	(0.005)	(0.018)	(0.016)	(0.010)	(0.008)	(0.022)	(0.020)	(0.012)	(0.010)
Other non-hispanic	0.004	0.060**	0.004	-0.004	0.053*	0.015	0.000	-0.045**	0.015	0.038	0.045***	-0.027
	(0.028)	(0.024)	(0.008)	(0.008)	(0.031)	(0.025)	(0.021)	(0.018)	(0.036)	(0.029)	(0.017)	(0.021)
Male	-0.049***	-0.052***	-0.008**	-0.022***	-0.071***	-0.029**	-0.031***	-0.010	-0.010	-0.013	-0.023**	-0.005
	(0.013)	(0.012)	(0.003)	(0.004)	(0.016)	(0.013)	(0.010)	(0.007)	(0.017)	(0.015)	(0.011)	(0.009)
Disabled	0.091***	0.019	0.011***	0.016***	0.133**	0.068	0.070***	-0.005	0.235***	0.145***	0.016	-0.011
	(0.016)	(0.014)	(0.004)	(0.004)	(0.066)	(0.051)	(0.025)	(0.021)	(0.037)	(0.032)	(0.015)	(0.013)
Age 16-29	0.040	0.013	0.021***	0.032***	0.091***	-0.032	0.016	0.036***	0.070**	0.050**	0.006	0.005
	(0.030)	(0.027)	(0.005)	(0.007)	(0.027)	(0.021)	(0.012)	(0.009)	(0.031)	(0.025)	(0.014)	(0.014)
Age 30-39	-0.018	0.032	-0.003	0.015**	0.082***	-0.039*	0.001	0.005	0.051**	0.081***	-0.001	-0.011
	(0.025)	(0.021)	(0.005)	(0.006)	(0.024)	(0.021)	(0.011)	(0.009)	(0.025)	(0.022)	(0.013)	(0.012)
Age 50-59	0.028	0.005	-0.004	-0.013**	-0.002	0.006	-0.010	0.012	0.014	0.052**	0.006	-0.001
	(0.022)	(0.020)	(0.005)	(0.006)	(0.026)	(0.022)	(0.014)	(0.010)	(0.025)	(0.023)	(0.015)	(0.011)
Age 60-69	0.012	-0.034	-0.043***	-0.048***	0.003	-0.043*	-0.036**	-0.002	-0.021	0.014	-0.068**	-0.036*
	(0.023)	(0.021)	(0.006)	(0.006)	(0.030)	(0.025)	(0.017)	(0.013)	(0.032)	(0.029)	(0.030)	(0.019)
Age 70 or more	-0.068***	-0.060***	-0.086***	-0.078***	-0.051	-0.060**	-0.094***	-0.019	0.024	0.017	-0.037**	-0.001
	(0.024)	(0.022)	(0.007)	(0.007)	(0.031)	(0.028)	(0.026)	(0.016)	(0.034)	(0.029)	(0.018)	(0.014)
Less than high school	0.007	0.046***	0.005	0.013***	0.091***	0.032*	0.013	0.014	0.143***	0.141***	0.012	0.029***
	(0.019)	(0.017)	(0.004)	(0.005)	(0.022)	(0.018)	(0.012)	(0.009)	(0.028)	(0.024)	(0.014)	(0.011)
High school graduate	0.008	0.041**	-0.002	-0.002	0.041**	-0.020	0.011	-0.003	0.051***	0.050***	0.010	0.008
	(0.018)	(0.016)	(0.004)	(0.005)	(0.020)	(0.016)	(0.011)	(0.007)	(0.019)	(0.017)	(0.011)	(0.010)
Complete graduate and beyond	-0.032	0.009	-0.023***	-0.019***	-0.053**	-0.047**	-0.006	-0.033***	-0.014	0.017	-0.001	-0.006
	(0.020)	(0.019)	(0.006)	(0.006)	(0.024)	(0.022)	(0.013)	(0.012)	(0.024)	(0.019)	(0.016)	(0.016)
Household language is English only	-0.042**	0.000	0.010**	0.013**					0.000	-0.016	0.007	0.065***
	(0.017)	(0.017)	(0.004)	(0.005)					(0.023)	(0.020)	(0.015)	(0.020)
Speaks English poorly	0.110***	0.075***	-0.016***	0.009					0.056	0.014	-0.016	-0.007
	(0.023)	(0.019)	(0.006)	(0.007)					(0.041)	(0.034)	(0.020)	(0.015)
Non-citizen	-0.046**	-0.007	0.003	-0.003					-0.061*	-0.002	-0.020	-0.005
	(0.022)	(0.019)	(0.005)	(0.007)					(0.032)	(0.027)	(0.015)	(0.014)
Household income/poverty line	-0.032***	-0.036***	-0.009***	-0.008***	-0.030***	-0.073***	-0.003	-0.001	-0.034***	-0.022***	-0.013***	-0.008*
	(0.003)	(0.004)	(0.001)	(0.001)	(0.005)	(0.013)	(0.003)	(0.002)	(0.005)	(0.004)	(0.004)	(0.004)
Anyone in household employed	-0.072***	-0.039**	-0.032***	-0.076***	-0.108***	-0.027	-0.040***	-0.013*	0.063***	0.059***	0.025**	0.035**
	(0.019)	(0.017)	(0.004)	(0.005)	(0.020)	(0.019)	(0.010)	(0.008)	(0.022)	(0.018)	(0.013)	(0.014)
Reported housing assistance receipt					0.088***	0.043**	0.001	0.017**				
					(0.023)	(0.017)	(0.012)	(0.008)				
Reported public assistance receipt	0.178***	0.047**			0.052	0.155***			0.166***	0.267***		
	(0.029)	(0.024)			(0.039)	(0.026)			(0.041)	(0.042)		
Reported SNAP receipt			0.103***	0.121***			0.031***	0.045***			0.062***	0.076***
			(0.003)	(0.004)			(0.010)	(0.008)			(0.011)	(0.012)
Linear time trend	0.022***	0.015***	-0.002**	-0.003**	0.013***	0.002	-0.003	0.002	0.127***	0.193***	-0.011	0.010
	(0.005)	(0.004)	(0.001)	(0.001)	(0.004)	(0.004)	(0.002)	(0.002)	(0.028)	(0.022)	(0.013)	(0.010)
Number of observations	6,096	6,096	35,589	35,589	2,336	2,336	2,346	2,346	1,941	1,941	1,928	1,928
Chi2 distance measure		45,999		13,161		2,375		769		1,085		1,442

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The sample is restricted to item non-respondents in all columns. For each survey and program, the first column uses the administrative receipt measure as the dependent variable. The second column estimates the same model using the imputed receipt variable. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. The distance measure is the squared distance between the two coefficient vectors, weighted by the variance matrix of the coefficients using the administrative dependent variable. SEs in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: The Determinants of Program Receipt in the Full Linked Sample, When Excluding Item Non-Respondents and When Using Imputated Values, Probit Coefficients

	ACS						CPS						SIPP					
	SNAP			PA			SNAP			PA			SNAP			PA		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Dependent Variable	Admin	Admin	Admin/Imp	Admin	Admin	Admin/Imp	Admin	Admin	Admin/Imp	Admin	Admin	Admin/Imp	Admin	Admin	Admin/Imp	Admin	Admin	Admin/Imp
Sample	Full	Respondents	Full	Full	Respondents	Full	Full	Respondents	Full	Full	Respondents	Full	Full	Respondents	Full	Full	Respondents	Full
Single adult, no children	-0.0931***	-0.0985***	-0.1008***	-0.3812***	-0.4058***	-0.3368***	0.0988	0.0699	0.0820	-0.1316	-0.2331**	-0.2742***	-0.0778	-0.0165	-0.0626	-0.6038***	-0.6863***	-0.5998***
	(0.0157)	(0.0159)	(0.0157)	(0.0250)	(0.0268)	(0.0241)	(0.0670)	(0.0736)	(0.0679)	(0.0972)	(0.1077)	(0.1001)	(0.0689)	(0.0751)	(0.0698)	(0.1065)	(0.1121)	(0.1062)
Single adult, with children	0.3477***	0.3445***	0.3413***	0.3031***	0.2934***	0.2928***	0.4551***	0.4766***	0.4147***	0.4957***	0.5023***	0.4221***	0.5926***	0.6052***	0.5562***	0.4365***	0.4080***	0.4121***
	(0.0152)	(0.0154)	(0.0153)	(0.0200)	(0.0212)	(0.0197)	(0.0606)	(0.0669)	(0.0611)	(0.0777)	(0.0873)	(0.0804)	(0.0609)	(0.0649)	(0.0615)	(0.0807)	(0.0862)	(0.0817)
Multiple adults, no children	-0.0693***	-0.0712***	-0.0734***	-0.1580***	-0.1758***	-0.1520***	0.0430	0.0531	0.0565	-0.0710	-0.0853	-0.1169	-0.0274	0.0428	0.0062	-0.0493	-0.0986	-0.0730
	(0.0134)	(0.0136)	(0.0134)	(0.0196)	(0.0208)	(0.0191)	(0.0568)	(0.0626)	(0.0580)	(0.0785)	(0.0866)	(0.0798)	(0.0599)	(0.0650)	(0.0599)	(0.0930)	(0.1000)	(0.0944)
Number of members under 18	0.1092***	0.1078***	0.1066***	0.0561***	0.0543***	0.0581***	0.1451***	0.1439***	0.1281***	0.0695***	0.0431*	0.0465*	0.1191***	0.1380***	0.1238***	0.0817***	0.0736**	0.0831***
	(0.0049)	(0.0049)	(0.0049)	(0.0063)	(0.0067)	(0.0062)	(0.0220)	(0.0243)	(0.0223)	(0.0236)	(0.0261)	(0.0250)	(0.0221)	(0.0240)	(0.0222)	(0.0263)	(0.0289)	(0.0267)
Number of members 18 or older	0.1795***	0.1808***	0.1757***	0.1217***	0.1219***	0.1160***	0.2173***	0.2282***	0.1958***	0.1394***	0.1215***	0.1141***	0.1976***	0.1805***	0.1643***	0.0322	0.0302	0.0181
	(0.0048)	(0.0049)	(0.0048)	(0.0066)	(0.0070)	(0.0065)	(0.0215)	(0.0237)	(0.0218)	(0.0309)	(0.0370)	(0.0335)	(0.0216)	(0.0237)	(0.0220)	(0.0312)	(0.0341)	(0.0314)
Rural	-0.1136***	-0.1126***	-0.1128***	-0.1958***	-0.1933***	-0.1891***	0.1226**	0.0393	0.0416	0.2009**	0.1717*	0.1186	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0097)	(0.0098)	(0.0097)	(0.0203)	(0.0213)	(0.0191)	(0.0501)	(0.0550)	(0.0509)	(0.0848)	(0.0932)	(0.0863)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Hispanic	0.4931***	0.4916***	0.4853***	0.4000***	0.4017***	0.3451***	0.5604***	0.5230***	0.4946***	0.4710***	0.4798***	0.4133***	0.3861***	0.3638***	0.3803***	0.3834***	0.3961***	0.3327***
	(0.0108)	(0.0110)	(0.0109)	(0.0181)	(0.0194)	(0.0176)	(0.0373)	(0.0410)	(0.0380)	(0.0575)	(0.0627)	(0.0577)	(0.0446)	(0.0474)	(0.0449)	(0.0634)	(0.0675)	(0.0635)
Black non-hispanic	0.6778***	0.6760***	0.6675***	0.6042***	0.5976***	0.5399***	0.4634***	0.4249***	0.3690***	0.5949***	0.5728***	0.5008***	0.4942***	0.5001***	0.4906***	0.6700***	0.6919***	0.6362***
	(0.0092)	(0.0093)	(0.0092)	(0.0147)	(0.0157)	(0.0141)	(0.0381)	(0.0424)	(0.0391)	(0.0584)	(0.0654)	(0.0599)	(0.0367)	(0.0391)	(0.0370)	(0.0584)	(0.0617)	(0.0584)
Other non-hispanic	0.0217	0.0198	0.0231	0.0990***	0.1008***	0.0858***	0.0851	0.0510	0.0599	-0.1747	-0.2210*	-0.2804**	0.4215***	0.4583***	0.4515***	0.2073**	0.1506	0.1107
	(0.0153)	(0.0155)	(0.0153)	(0.0267)	(0.0283)	(0.0253)	(0.0589)	(0.0647)	(0.0600)	(0.1199)	(0.1310)	(0.1241)	(0.0477)	(0.0496)	(0.0484)	(0.0860)	(0.0929)	(0.0889)
Disabled	0.4789***	0.4819***	0.4746***	0.0833***	0.0824***	0.0809***	0.3455**	0.3144**	0.3044**	0.0792	-0.2408	-0.2404	1.0267***	1.0099***	0.9984***	0.2934***	0.3041***	0.2632***
	(0.0083)	(0.0084)	(0.0083)	(0.0146)	(0.0156)	(0.0141)	(0.1391)	(0.1544)	(0.1436)	(0.1791)	(0.2130)	(0.1971)	(0.0457)	(0.0472)	(0.0456)	(0.0682)	(0.0717)	(0.0686)
Male	-0.2068***	-0.2083***	-0.2085***	-0.0419***	-0.0388***	-0.0560***	-0.1223***	-0.0860***	-0.0924***	0.0132	0.0905*	0.0618	-0.1052***	-0.1082***	-0.1058***	-0.1494***	-0.1212**	-0.1291**
	(0.0069)	(0.0070)	(0.0069)	(0.0120)	(0.0128)	(0.0116)	(0.0296)	(0.0323)	(0.0300)	(0.0478)	(0.0521)	(0.0485)	(0.0293)	(0.0312)	(0.0295)	(0.0528)	(0.0560)	(0.0530)
Age 16-29	0.2231***	0.2240***	0.2221***	0.1072***	0.1022***	0.1114***	0.1833***	0.1425**	0.0871*	0.0801	0.0532	0.1541**	0.3061***	0.2788***	0.2936***	0.1316	0.1477	0.1296
	(0.0129)	(0.0131)	(0.0129)	(0.0182)	(0.0194)	(0.0179)	(0.0510)	(0.0555)	(0.0516)	(0.0662)	(0.0726)	(0.0671)	(0.0602)	(0.0644)	(0.0592)	(0.0868)	(0.0954)	(0.0881)
Age 30-39	0.1236***	0.1278***	0.1277***	-0.0336**	-0.0294*	-0.0179	0.1556***	0.1085**	0.0701	-0.1651***	-0.2158***	-0.1650***	0.2748***	0.2661***	0.3007***	-0.0507	-0.0453	-0.0622
	(0.0113)	(0.0115)	(0.0113)	(0.0168)	(0.0178)	(0.0164)	(0.0467)	(0.0518)	(0.0479)	(0.0604)	(0.0659)	(0.0616)	(0.0466)	(0.0499)	(0.0470)	(0.0673)	(0.0716)	(0.0682)
Age 50-59	0.0294***	0.0267**	0.0284**	0.0099	0.0157	-0.0005	0.0937*	0.1191**	0.1073**	-0.0433	-0.0292	0.0069	0.0910**	0.0937**	0.1220***	0.0968	0.1091	0.1005
	(0.0110)	(0.0112)	(0.0111)	(0.0168)	(0.0180)	(0.0165)	(0.0482)	(0.0524)	(0.0486)	(0.0683)	(0.0742)	(0.0692)	(0.0441)	(0.0471)	(0.0446)	(0.0702)	(0.0743)	(0.0705)
Age 60-69	-0.0563***	-0.0608***	-0.0618***	-0.3038***	-0.2824***	-0.2913***	-0.0821	-0.0916	-0.1199**	-0.3319***	-0.3111***	-0.2756***	0.0622	0.0778	0.0833	-0.5224***	-0.4983***	-0.5000***
	(0.0125)	(0.0127)	(0.0126)	(0.0216)	(0.0230)	(0.0207)	(0.0562)	(0.0617)	(0.0567)	(0.0871)	(0.0949)	(0.0889)	(0.0537)	(0.0569)	(0.0542)	(0.0984)	(0.1022)	(0.0981)
Age 70 or more	-0.4247***	-0.4332***	-0.4292***	-0.7641***	-0.7426***	-0.6608***	-0.2979***	-0.3076***	-0.3243***	-1.0656***	-1.0536***	-0.9767***	0.0997**	0.0921*	0.0946*	-0.6447***	-0.6452***	-0.5572***
	(0.0134)	(0.0136)	(0.0134)	(0.0255)	(0.0274)	(0.0236)	(0.0590)	(0.0649)	(0.0597)	(0.1283)	(0.1396)	(0.1292)	(0.0508)	(0.0538)	(0.0510)	(0.0969)	(0.1041)	(0.0986)
Less than high school	0.2911***	0.2955***	0.2914***	0.1429***	0.1431***	0.1517***	0.3051***	0.2819***	0.2729***	0.1029	0.0909	0.1080*	0.3422***	0.3165***	0.3607***	0.1863***	0.1873**	0.2068***
	(0.0101)	(0.0102)	(0.0101)	(0.0158)	(0.0169)	(0.0154)	(0.0429)	(0.0471)	(0.0435)	(0.0630)	(0.0687)	(0.0642)	(0.0458)	(0.0482)	(0.0459)	(0.0702)	(0.0752)	(0.0703)
High school graduate	0.1054***	0.1054***	0.1057***	0.0430***	0.0430***	0.0460***	0.0309	-0.0085	-0.0217	0.0849	0.0685	0.0568	0.1745***	0.1717***	0.1880***	-0.0526	-0.0769	-0.0519
	(0.0085)	(0.0086)	(0.0085)	(0.0144)	(0.0153)	(0.0140)	(0.0373)	(0.0407)	(0.0378)	(0.0585)	(0.0642)	(0.0594)	(0.0336)	(0.0357)	(0.0338)	(0.0595)	(0.0641)	(0.0604)
Complete graduate and beyond	-0.1879***	-0.1884***	-0.1862***	-0.1495***	-0.1366***	-0.1467***	-0.2374***	-0.2414***	-0.2384***	0.0057	0.0250	-0.0281	-0.3230***	-0.3545***	-0.3004***	-0.1696**	-0.1806**	-0.1840**
	(0.0099)	(0.0100)	(0.0099)	(0.0179)	(0.0189)	(0.0172)	(0.0437)	(0.0474)	(0.0447)	(0.0709)	(0.0770)	(0.0725)	(0.0405)	(0.0430)	(0.0407)	(0.0740)	(0.0776)	(0.0745)
Household language is English only	-0.1209***	-0.1194***	-0.1179***	0.0857***	0.0842***	0.0786***							0.1526***	0.1824***	0.1489***	0.0364	0.0412	0.0908
	(0.0094)	(0.0095)	(0.0094)	(0.0158)	(0.0168)	(0.0153)							(0.0389)	(0.0399)	(0.0388)	(0.0793)	(0.0851)	(0.0814)
Speaks English poorly	0.4191***	0.4193***	0.4157***	-0.2012***	-0.2013***	-0.1740***							0.4895***	0.5225***	0.4881***	-0.4166***	-0.4503***	-0.4133***
	(0.0132)	(0.0133)	(0.0132)	(0.0203)	(0.0215)	(0.0198)							(0.0595)	(0.0622)	(0.0593)	(0.0899)	(0.0970)	(0.0894)
Non-citizen	-0.3477***	-0.3538***	-0.3446***	-0.0141	-0.0141	-0.0138							-0.2923***	-0.3039***	-0.2651***	0.1220	0.1691	0.1504
	(0.0126)	(0.0127)	(0.0126)	(0.0178)	(0.0188)	(0.0175)							(0.0661)	(0.0724)	(0.0668)	(0.0983)	(0.1067)	(0.0986)
Household income/poverty line	-0.2503***	-0.2547***	-0.2523***	-0.1186***	-0.1227***	-0.1082***	-0.2221***	-0.2359***	-0.2430***	-0.1097***	-0.1280***	-0.1121***	-0.2503***	-0.2636***	-0.2483***	-0.0884***	-0.0774***	-0.0806***
	(0.0028)	(0.0029)	(0.0029)	(0.0044)	(0.0048)	(0.0041)	(0.0120)	(0.0137)	(0.0135)	(0.0186)	(0.0220)	(0.0191)	(0.0128)	(0.0140)	(0.0128)	(0.0220)	(0.0232)	(0.0221)
Anyone in household employed	-0.4276***	-0.4309***	-0.4259***	-0.3684***	-0.3645***	-0.4089***	-0.4873***	-0.4911***	-0.4604***	-0.3954***	-0.3744***	-0.3520***	0.2449***	0.2200***	0.2395***	0.5022***	0.5223***	0.5038***
	(0.0098)	(0.0099)	(0.0098)	(0.0154)	(0.0165)	(0.0149)	(0.0411)	(0.0458)	(0.0415)	(0.0576)	(0.0639)	(0.0590)	(0.0369)	(0.0388)	(0.0367)	(0.0668)	(0.0712)	(0.0689)
Reported housing assistance receipt							0.8097***	0.8645***	0.8126***	0.1869***	0.1943***	0.2371***						
							(0.0428)	(0.0467)	(0.0425)	(0.0530)	(0.0576)	(0.0539)						
Reported public assistance receipt	1.3066***	1.3217***	1.2872***				1.1208***	1.4576***	1.2369***				1.1566***	1.2213***	1.2497***			
	(0.0173)	(0.0176)	(0.0172)				(0.0987)	(0.1326)	(0.0974)				(0.0850)	(0.0929)	(0.0875)			
Reported SNAP receipt				1.0322***	1.0197***	1.0027***				0.8296***	0.9067***	0.9047***				0.8902***	0.9074***	0.9193***
				(0.0131)	(0.0140)	(0.0127)				(0.0528)	(0.0585)	(0.0541)				(0.0554)	(0.0594)	(0.0562)
Linear time trend	0.0845***	0.0846***	0.0839***	-0.0217***	-0.0224***	-0.0190***	0.0582***	0.0553***	0.0457***	-0.0046	-0.0012	0.0037	0.8196***	0.8303***	0.8577***	0.1378**	0.1601***	0.1656***
	(0.0023)	(0.0023)	(0.0023)	(0.0038)	(0.0041)	(0.0037)	(0.0082)	(0.0090)	(0.0083)	(0.0126)	(0.0139)	(0.0128)	(0.0387)	(0.0403)	(0.0388)	(0.0585)	(0.0618)	(0.0580)
Constant	-0.7977***	-0.7896***	-0.7826***	-1.8749***	-1.8789***	-1.8033***	-1.0927***	-1.0394***	-0.9379***	-1.9221***	-1.8780***	-1.9114***	-1.5872***	-1.6127***	-1.5827***	-2.4472***	-2.4752***	-2.5076***
	(0.0235)	(0.0238)	(0.0235)	(0.0361)	(0.0384)	(0.0349)	(0.0976)	(0.1076)	(0.0987)	(0.1380)	(0.1545)	(0.1411)	(0.0907)	(0.0997)	(0.0928)	(0.1486)	(0.1618)	(0.1525)
Number of observations	543,528	537,432	543,528	543,528	507,939	543,528	18,064	15,728	18,064	18,064	15,718	18,064	24,997	23,056	24,997	24,997	23,069	24,997
Chi2 distance measure	0	8.5	14.0	0	27.2	163.5	0	25.7	57.0	0	15.6	23.1	0	12.7	11.9	0	3.2	7.1

Notes: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. There are 3 models per program per survey that differ in the dependent variable and the sample used as noted in the rows at the top of the table. For each survey and program, the first column uses the full sample with the administrative dependent variable. The second column excludes item non-respondents from the sample. The third column uses their imputed values instead of the administrative dependent variable. All demographic characteristics refer to the reference person. The time trend is measured in years for the ACS and CPS, but in waves for the SIPP. The omitted categories are Multiple Adults with Children, Age 40-49, College Graduate and White. All analyses use household weights adjusted for PIK probability. The distance measure is the squared distance between the coefficient vector and the coefficients from the model using the full sample with the administrative dependent variable (in the first column for each survey and program), weighted by the variance matrix of the coefficients of the latter model. SEs in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A12 – Comparing Bias in Probit Coefficients of the Determinants of Program Receipt Using Different Methods to Address Item Non-response

		ACS		CPS		SIPP	
Sample	Dependent Variable	SNAP	PA	SNAP	PA	SNAP	PA
Chi2-Distance Measure of Coefficients							
Respondents	Admin Receipt	8.5	27.2	25.7	15.6	12.7	3.2
Respondents, reweighted	Admin Receipt	8.6	30.9	25.9	15.2	13.3	3.3
Full	Admin/Imputed	14.0	163.5	57.0	23.1	11.9	7.1
Full	Reports/Imputed	6,677.2	4,619.4	1,602.4	960.5	285.2	440.9
Respondents	Reports	6,761.0	6,004.0	1,449.5	1,427.5	376.7	481.0
Respondents, reweighted	Reports	6,765.7	5,950.7	1,455.7	1,431.7	374.7	489.2
Chi2-Distance Measure of Marginal Effects							
Respondents	Admin Receipt	4.8	66.2	23.0	11.5	10.0	3.3
Respondents, reweighted	Admin Receipt	3.2	27.8	21.2	11.3	9.9	2.8
Full	Admin/Imputed	12.5	102.7	46.7	17.7	10.0	6.9
Full	Reports/Imputed	4,365.5	3,452.3	643.6	330.0	172.7	191.5
Respondents	Reports	4,364.6	4,146.8	664.7	344.7	201.0	205.4
Respondents, reweighted	Reports	4,248.5	4,007.2	588.6	345.2	199.8	197.6

Note: Approved for release by the Census Bureau's Disclosure Review Board, approvals dated August 3, 2015 and August 18, 2016. The first and third row of each panel are identical to Table 10. The row with reweighted estimates use inverse probability weights predicted from probit models of item non-response including all covariates of the receipt models. Distances are the squared distance to the coefficients from the model using the full sample and the administrative receipt variable, weighted by the variance matrix of the coefficients from this model. The distances for the coefficients include the intercept of the model, but the ones for the marginal effects do not.