# National Experimental Wellbeing Statistics
# Version 1

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## Abstract

This is the U.S. Census Bureau's first release of the National Experimental Wellbeing Statistics (NEWS) project. The NEWS project aims to produce the best possible estimates of income and poverty given all available survey and administrative data. We link survey, decennial census, administrative, and third-party data to address measurement error in income and poverty statistics. We estimate improved (pre-tax money) income and poverty statistics for 2018 by addressing several possible sources of bias documented in prior research. We address biases from 1) unit nonresponse through improved weights, 2) missing income information in both survey and administrative data through improved imputation, and 3) misreporting by combining or replacing survey responses with administrative information. Reducing survey error substantially affects key measures of well-being: We estimate median household income is 6.3 percent higher than in survey estimates, and poverty is 1.1 percentage points lower. These changes are driven by subpopulations for which survey error is particularly relevant. For householders aged 65 and over, median household income is 27.3 percent higher and poverty is 3.3 percentage points lower than in survey estimates. We do not find a significant impact on median household income for householders under 65 or on child poverty. Finally, we discuss plans for future releases: addressing other potential sources of bias, releasing additional years of statistics, extending the income concepts measured, and including smaller geographies such as state and county.

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# 1 Introduction

Accurately tracking household income and poverty is essential to understanding the nation's overall economic wellbeing. Recent studies show that measurement error stemming from non-response, item non-response and misreporting biases key official statistics such as mean or median income and the official poverty rate. The direction of bias differs between these sources of measurement error. Unit and item nonresponse have been found to bias income up and poverty down (Rothbaum et al., 2021; Rothbaum and Bee, 2022; Bollinger et al., 2019; Hokayem, Raghunathan and Rothbaum, 2022), while misreporting can bias income down and poverty up (Bee and Mitchell, 2017; Meyer et al., 2021*b*; Larrimore, Mortenson and Splinter, 2020). Since these error components are typically studied in isolation, their overall impact on the accuracy of survey estimates remains unclear.[1]

This paper summarizes the first effort to integrate this research and address each of these sources of bias simultaneously in order to produce more accurate estimates of household income and poverty. Through the National Experimental Wellbeing Statistics (NEWS) Project, we leverage extensive administrative data sources matched at the individual level to the CPS ASEC. To address nonrandom household nonresponse, we improve on the work in Rothbaum et al. (2021) and Rothbaum and Bee (2022) by conditioning on more data to better adjust for selection into survey response and linkage to administrative data. For nonrandom item nonresponse, we build on Hokayem, Raghunathan and Rothbaum (2022) using improved imputations that condition on administrative data. And for income misreporting, we combine survey income items with administrative data. We also integrate work in a companion paper, Bee et al. (2023), that develops a model to combine survey and administrative earnings data given measurement error in both sources, replacing ad hoc assumptions that have been used in prior work.

[1] Meyer and Mittag (2021) is an example of a study that evaluated these issues together, decomposing the measurement error in estimates of means-tested program benefits. They also found different sources of error have different signs and magnitudes, which vary by program and survey analyzed. They found that correcting for one bias and not the others can actually make the overall bias worse in some cases.

To demonstrate the importance of more accurate data, we estimate pre-tax money income and poverty statistics for 2018, mirroring the Census Bureau's annual income and poverty report (Semega et al., 2019). Under our approach, median household income is 6.3 percent higher than the survey-only estimate. The official poverty rate is 1.1 percentage points lower, with 9.4 percent fewer people in poverty.[2] However, there is considerable heterogeneity in these estimates. Median household income is 27.3 percent higher for householders aged 65 and older, 5.0 percent higher for those aged 55-64, and not statistically different or lower for all other householder ages. Likewise, poverty is 3.3 percentage points lower for householders aged 65 and over (34.2 percent fewer people in poverty), compared to 0.7 percentage points lower for those aged 18-64 (6.7 percent fewer people in poverty), and not statistically different for children 17 and under.

Our combined nonresponse bias corrections (weighting and improved income imputation) generally adjust the point estimates of income down and poverty up.[3] Including administrative wage and salary earnings to address underreporting, particularly when survey-reported earnings are zero, adjusts income up and poverty down. Addressing retirement income (defined benefit pensions and defined contribution withdrawals) underreporting has the biggest impact on household income across much of the distribution, as found in Bee and Mitchell (2017). For householders under 55 whose income comes predominantly from wage and salary earnings (one of the best reported income sources in surveys), we find limited differences in income and poverty estimates. However, for those 55 and over and particularly for those 65 and over, who have more income in underreported sources (retirement, interest, dividends, etc.), the increase in income due to the underreporting adjustment is greater than the decline in income from the nonresponse bias correction.

We use multiple survey and administrative data sources because each source has its own strengths and shortcomings, making it difficult to produce accurate estimates of income and

[2] All comparisons are statistically significant at the 5 percent level unless otherwise noted.

[3] The differences are not generally statistically significant in this paper, however.

poverty when relying only on a single source. We have already discussed potential biases in survey estimates, but administrative-only estimates are also imperfect. For example, Larrimore, Mortenson and Splinter (2020) created households using addresses from tax filings and information returns to estimate poverty over time addressing income underreporting in surveys. However, their work highlights a challenge of administrative data-only estimates of income and poverty since they cannot observe income information for individuals and households that do not receive any information return or file taxes. Therefore, they must impute the presence of 4 to 6 million poor individuals per year in their poverty estimates. In contrast, random-sample surveys do not have coverage gaps of the same magnitude that we see in administrative data. In the 2019 Current Population Survey Annual Social and Economic Supplement (CPS ASEC), 7 percent of occupied housing units cannot be linked to any administrative or third-party data. Thanks to information from survey responses, we can generate weights, imputations, and income measures to better approximate our target universe of individuals and households, even in the absence of administrative data for some.

Survey responses can include informal earnings that are not well captured in administrative data. For example, 5 percent of adults report wage and salary earnings in the CPS ASEC but do not receive a W-2 (Bee, Mitchell and Rothbaum, 2019). The absence of these earnings would bias our estimates of income. By bringing data sources together, we can get more accurate estimates of income and poverty. Furthermore, the additional survey information (demographics, education, etc.) allow us to estimate income and poverty for subgroups as well as the relationship between income and poverty and other characteristics not generally available in administrative data.

We also use administrative data from many different state and federal agencies, which helps address missing information in individual sources, an issue even for comprehensive data providers like the Internal Revenue Service (IRS). For example, in 2018 there are millions of jobs present in state unemployment insurance records that we do not find in the universe of W-2s (discussed in Section 3.3). Also, with IRS data alone, we would not observe nontaxable income that can be particularly important to low-income households, such as Supplemental Security Income and Temporary Assistance for Needy Families.

There are several other projects that attempt to address shortcomings in survey data to estimate improved income and poverty statistics. The Congressional Budget Office (Habib, 2018) adjusts underreported transfer income by imputing income to nonrecipients using conditional relationships estimated in the survey. Unfortunately, underreporting is often not well captured by the observable survey information (as shown in Mittag 2019 and Fox et al. 2022). The Bureau of Economic Analysis (Fixler, Gindelsky and Johnson, 2019, 2020; Gindelsky, 2022) also adjusts survey data to estimate the distribution of personal income. Their estimates include a number of adjustments for survey underreporting, using parameters estimated in auxiliary data sets (share of income missing by source by comparing the National Income and Product Account (NIPA) and survey estimates, IRS published statistics on high income tax units, etc.). One important adjustment BEA makes is to scale income up on the intensive margin by source to address survey underreporting relative to NIPA aggregates. However, survey underreporting is often an extensive margin phenomenon, as shown for retirement income by Bee and Mitchell (2017) and means-tested program benefits by Shantz and Fox (2018) and Meyer and Mittag (2019). BEA's scaling adjustment risks imputing missing income to individuals who have already reported that income relatively accurately, rather than to individuals that have misreported positive income as zero. The Urban Institute, under contract with the Department of Health and Human Services, developed the Transfer Income Model (TRIM), in part to address survey underreporting (Zedlewski and Giannarelli, 2015). TRIM uses a rule-based approach, in conjunction with unlinked auxiliary data[4] to impute underreported income and benefits to individuals and households. However,

[4] Such as Supplemental Nutrition Assistance Program Quality Control data from the U.S. Department of Agriculture.

Shantz and Fox (2018) and Mittag (2019) show that the underreported program benefits may not be missing from households that appear to qualify for them either through the rules-based imputations or from matching to external data sources. The challenges to each of these approaches highlights the importance of having linked data available to estimate income and poverty statistics. If we impute income or benefits to the wrong individuals and households, we risk introducing biases of unclear direction and magnitude.

There has also been work under a separate project at the Census Bureau, the Comprehensive Income Database (CID, refer to Medalia et al. 2019), including Meyer and Wu (2018), Meyer et al. (2021*b*), Meyer et al. (2021*a*), and Corinth, Meyer and Wu (2022). They focus on addressing misreporting in income and means-tested program benefits. We additionally address nonresponse bias, missing administrative data, and model measurement error in survey and administrative earnings in lieu of ad hoc assumptions.[5]

This release is the first under the NEWS project. We focus on national estimates of income and poverty in a single year as measured in the Census Bureau's regular releases (money income and the official poverty measure), which exclude in-kind transfers (such as the Supplemental Nutrition Assistance Program, SNAP) and taxes and credits. We lay out in detail the methods we use to address biases in income statistics, which will form the baseline of fu-

[5] There has been considerable other work on measurement error in income data, as well as comparing survey income to administrative data. As far back as the 1970's, Kilss and Scheuren (1978) used CPS data linked to data from the Internal Revenue Service (IRS) and Social Security Administration (SSA) to evaluate survey income data. More recently examples include Abowd and Stinson (2013), Bee (2013), Benedetto, Stinson and Abowd (2013), Harris (2014), Bee, Gathright and Meyer (2015), Giefer et al. (2015), Hokayem, Bollinger and Ziliak (2015), Bhaskar et al. (2016), Chenevert, Klee and Wilkin (2016), Noon, Fernandez and Porter (2016), Bee and Mitchell (2017), Fox, Heggeness and Stevens (2017), O'Hara, Bee and Mitchell (2017), Abowd, McKinney and Zhao (2018), Benedetto, Stanley and Totty (2018), Bhaskar, Shattuck and Noon (2018), Bollinger et al. (2019), Brummet et al. (2018), Eggleston and Reeder (2018), Meyer and Wu (2018), Murray-Close and Heggeness (2018), Rothbaum (2018), Shantz and Fox (2018), Bee, Mitchell and Rothbaum (2019), Imboden, Voorheis and Weber (2019), Jones and Ziliak (2019), , Eggleston and Westra (2020), Larrimore, Mortenson and Splinter (2020), Abraham et al. (2021), Eggleston (2021), Larrimore, Mortenson and Splinter (2021), Meyer and Mittag (2021), Rothbaum et al. (2021), Carr, Moffitt and Wiemers (2022), Fox et al. (2022), Hokayem, Raghunathan and Rothbaum (2022), Larrimore, Mortenson and Splinter (2022), McKinney and Abowd (2022), Moffitt et al. (2022), Moffitt and Zhang (2022), Rothbaum and Bee (2022), and others. For a more complete discussion of nonsampling error in income and poverty statistics, refer to Bee and Rothbaum (2019), which also discusses the challenges in addressing these issues and discussed the research agenda that led to this project.

ture releases. In the future, we will extend the work to include more years of data, additional income and resource concepts, estimates at finer levels of geography, and additional corrections to address measurement error in other income sources — particularly self-employment earnings which have been shown to be substantially underreported in both surveys (Hurst, Li and Pugsley, 2014) and administrative data (Internal Revenue Service, Research, Analysis & Statistics., 2016). As we improve the methods in future releases, we expect to revise our estimates.

# 2 Data Sources

We would like to use any available data that can help inform estimates of income, resources, or wellbeing, broadly defined. This includes survey and decennial census data collected by the Census Bureau, administrative data, and third-party data. The data could be useful to directly measure resources, to model estimates of resources, to validate measures, to address nonresponse, etc. In this section, we discuss each source of data, also shown in Table 2. Figures 1 and 2 show how we put these data sources together to create the files we use to generate the income and poverty estimates, which are discussed in Section 4.

## 2.1 Survey Data

Surveys collect information on many characteristics of individuals and households that are not available or well-measured in administrative data for all or subsets of the population. These include race, Hispanic origin, tenure (homeownership vs. renting), educational attainment, household composition, etc. Surveys also include information on income, although we have considerable evidence on mis- and underreporting of income on surveys.

Surveys operations also provide information that can be crucial for these estimates. First, major surveys conducted by the Census Bureau are stratified random samples of addresses, in which the occupancy status of housing units (vacant/occupied) is assessed as part of the survey. This provides us with a sample of households in our target universe, occupied housing units, and their sampling probability. In administrative data, it can often be unclear or impossible to assess the set of units with no available data (i.e., households and individuals that received no W-2 or other information return and did not file taxes). The unobserved units in administrative data may be more likely to be at one end of the income distribution than the other — making them particularly important for measures of inequality or hardship, such as poverty.

First, we use the **Current Population Survey's (CPS) Annual Social and Economic Supplement (ASEC)**. The CPS ASEC is an annual survey conducted from February to April each year as a supplement to the monthly CPS. Respondents are asked social and demographic questions, as well as questions about their income and resources in the prior calendar year. CPS ASEC data are available at the Census Bureau from 1967 to the present. In 2019, approximately 95,000 addresses were sampled for the CPS ASEC.[6] It is the source of the official poverty measure produced by the Census Bureau as well as widely cited measures of the household income distribution (Semega et al., 2019). In Version 1, we estimate income and poverty statistics on the 2019 CPS ASEC sample for income in year 2018.

We also use the **American Community Survey (ACS)**, which is available from 2005 to the present. The ACS is an ongoing survey with 3.5 million addresses in sample each year, of which over 2 million respond. Respondents are asked similar (although generally less detailed) questions than the CPS ASEC, particularly for income. Additionally, ACS respondents are asked about income in the prior 12 months, rather than prior the calendar year as in the CPS ASEC.[7]

Both the CPS and ACS use field representatives to assess the occupancy status of housing

[6] Refer to the CPS ASEC technical documentation at `https://www2.census.gov/programs-surveys/cps/techdocs/cpsmar19.pdf`.

[7] ACS technical documentation is available at `https://www.census.gov/programs-surveys/acs/technical-documentation.html` and `https://www.census.gov/content/dam/Census/library/publications/2020/acs/acs_general_handbook_2020.pdf`.

units, the CPS as part of the Housing Vacancy Survey and ACS for estimates of vacancy rates.[8]

## 2.2 Other Census Bureau Data

The Census Bureau has other data available on the nation's people and households that we use. First, we use data from the **short-form decennial census**. This includes information on each individual's race, Hispanic origin, and age.

We also use information from the **Master Address File (MAF)**. The MAF contains continuously updated information of all known living quarters in the United States. The MAF is used to select housing units for inclusion in household surveys, including the CPS and ACS, as well as for decennial census operations. The MAF also includes housing unit characteristics, such as whether addresses are in single-family or multi-family units.

We also use the **Master Address File Auxiliary Reference File (MAF-ARF)** which links addresses in the MAF to individuals who reside there in each year. The MAF-ARF is constructed from administrative data, including from the IRS, Department of Housing and Urban Development (HUD), and the U.S. Postal Service, among other data sources.

Each of these other Census Bureau data sources provide information that can help us address nonresponse bias and better estimate income and poverty statistics on representative samples of individuals, families, and households.

## 2.3 Federal Government Administrative Data

The federal government data we use are provided primarily by the IRS and Social Security Administration (SSA). The Census Bureau also has an agreement with the Department of Health and Human Services (HHS) for data on the Temporary Assistance for Needy Families

[8] Refer to `https://www.census.gov/topics/housing/guidance/vacancy-fact-sheet.html` for a discussion of housing vacancy estimates in the Housing Vacancy Survey (from the CPS), ACS, and American Housing Survey.

(TANF) program from some states. That data will be discussed in Section 2.4, as TANF data are also shared with the Census Bureau by individual partner state agencies.

### 2.3.1 IRS Data

From the IRS, we have the following data:

1. the **Information Return Master File (IRMF)** from 2005 to the present,
2. the universe of **Form 1099-R** returns on "Distributions From Pensions, Annuities, Retirement or Profit-Sharing Plans, IRAs, Insurance Contracts, etc." from 1995 to the present,
3. the universe of **Form W-2** returns on "Wage and Tax Statement" for all W-2 covered jobs from 2005 to the present, and
4. the universe of **Form 1040** tax filings every five years from 1969 to 1994, 1995, and then each year from 1998 to the present.

The IRMF includes an indicator for each individual that received one of several information returns in a given year as well as their address, including for Forms 1098, 1099-DIV, 1099-G, 1099-INT, 1099-MISC, 1099-R, 1099-S, SSA-1099, and W-2. The IRMF allows us to link individuals to their addresses and is used in constructing the MAF-ARF. The IRMF does not include any information on income amounts.

The 1099-R extracts provided by the IRS include information on amounts of defined-benefit pension payments (including for survivor and disability pensions) and withdrawals from defined-contribution retirement plans.

The W-2 extracts provided by the IRS include select W-2 boxes, including wages and salary net of pre-tax deductions for health insurance premiums and deferred compensation, as well as the total amount of deferred compensation. This means that employee and employer pre-tax contributions to health insurance premiums are not available in the W-2 data.

The 1040 extracts provided by the IRS include information on tax-unit wage and salary income, gross rental income, gross Social Security income, taxable and tax-exempt interest income, dividends, Adjusted Gross Income, and a constructed measure of Total Money Income (TMI). TMI is the sum of taxable wage and salary income, interest (taxable and tax-exempt), dividends, gross Social Security income, unemployment compensation, alimony received, business income or losses (including for partnerships and S-corps), farm income or losses, and net rent, royalty, and estate and trust income.[9] The 1040 also includes information on marital status through filing status and filer information and identifies up to four dependents.

We use IRS data to address nonresponse bias and measurement error.

### 2.3.2 Social Security Administration (SSA) Data

From the SSA, we use the following data:

1. the **Numerical Identification System (Numident)** file,
2. extracts from the **Detailed Earnings Records (DER)**.
3. several files from the **Payment History Update System (PHUS)**, and
4. several files from the **Supplemental Security Records (SSR)**.

The Numident contains information on any individual to ever receive a Social Security Number (SSN), including their date of birth, date of death, information on their citizenship status, and their location of birth.

The DER contains job-level W-2 information that generally corresponds to the data provided by IRS, but with the potential for additional cleaning and error correction from SSA as part of their administration of the Social Security system. The DER also includes Social Security

[9] Prior to tax year 2018, TMI also includes total pensions and annuities. However, this was removed from TMI due to a change to income reporting on the Form 1040 and the regulations regarding data sharing between IRS and the Census Bureau.

covered self-employment earnings reported on the Form 1040 SE (if at least $400). Like many SSA data sets, including some PHUS and SSR files, the DER is only available for linked respondents from specific surveys and years.[10]

The PHUS contains monthly Old Age, Survivors, and Disability Insurance (OASDI) program payment information from 1984 to the present. There are several PHUS files available to the Census Bureau. One set of PHUS files includes OASDI recipients in 2020 and 2021, with one record per address. There are also PHUS files for linked respondents from specific surveys and years.

The SSR contains monthly Supplemental Security Income (SSI) payments, for both federal SSI payments and state payments administered by the SSA, from 1984 to the present. One set of PHUS files includes SSI recipients in 2020 and 2021, with one record per address. There are also SSR files for linked respondents from specific surveys and years.

We use the survey-linked SSA data (DER, PHUS, and SSR) to address item nonresponse bias and measurement error. The Numident and address-level SSA data (PHUS and SSR) are useful for weighting to address nonresponse bias.

## 2.4 State Government Data

We use several data sets shared with the Census Bureau from state government agencies:

1. the **Longitudinal Employer-Household Dynamics (LEHD)** files,
2. data on **Supplemental Nutrition Assistance Program (SNAP)** participation, and
3. data on cash assistance under the **Temporary Assistance for Needy Families**

[10] Specifically, the DER includes respondents with an assigned Protected Identification Key (discussed in Section 3) who can be linked to the Numident from the CPS ASEC in 1973, 1979, 1981-1991, 1994, and 1996-present, the Survey of Income and Program Participation (SIPP) in 1984, 1990-83, 1996, 2001, 2004, 2008, 2014, and 2018-present, and the ACS in 2019.

**(TANF)** program.

### 2.4.1 LEHD

Under the LEHD program, states provide data on wage and salary earnings reported by firms for the administration of the unemployment insurance (UI) program. Firms report gross earnings to UI offices, so the LEHD should include non-taxable earnings that are not reported on a Form W-2 for the same job such as pre-tax employee contributions for health insurance premiums. However, coverage in the LEHD is not complete, as many government employees (such as federal civilian employees postal workers, and Department of Defense employees) are not covered by state UI benefits. Furthermore, some private-sector employees, including those employed by religious organizations, are not covered by UI, and are therefore not present in the LEHD data. Finally, data sharing agreements between a state and the Census Bureau are not always available, resulting in LEHD earnings missing for all jobs in specific states and years.[11]

LEHD data are useful for addressing nonresponse bias and misreporting.

### 2.4.2 SNAP

The Census Bureau has agreements with many states to provide data on SNAP participation, although the available states vary by year.[12] The SNAP data includes benefits received for each case as well as the individual members recorded in that SNAP case.

SNAP data are useful for addressing misreporting of other income items. They will also be used to address misreporting of in-kind benefits in future releases.

[11] More information on the LEHD program and data is available at `http://lehd.ces.census.gov/data/lehd-snapshot-doc/latest/`, accessed 12/16/2022. While the LEHD program does receive data from the Office of Personnel Management (OPM) for many federal employees, those data are not part of the more recent years of data in the LEHD Interleave file used in this project.

[12] For example, SNAP data are available for 17 states in 2018, 20 states in 2014, 16 states in 2010, and 6 states in 2006. In 2018, the states with available SNAP data are Arizona, Connecticut, Florida, Hawaii, Idaho, Indiana, Kentucky, Maryland, Mississippi, Montana, Nevada, New Jersey, New York, North Dakota, Tennessee, Utah, and Wyoming.

### 2.4.3 TANF

In addition to the state agency data, the Census Bureau has data on TANF cash assistance receipt from HHS. As with SNAP, the available states vary by year.[13] TANF data are also available by case (benefit amounts) with individuals in each TANF case recorded as well.

TANF data are useful for addressing misreporting.

## 2.5 Third-Party Data

We use information on home values from Black Knight, which can be useful in correcting for selection into nonresponse on surveys.[14]

These data are useful for weighting to address nonresponse bias.

## 2.6 Firm Data

We also use data on firm characteristics from the Longitudinal Business Database (LBD), which is described in Chow et al. (2021). The LBD contains establishment-level information on firm employment and payroll.

Firm data are useful in addressing nonresponse bias (as they help predict survey responses) and can be used to address misreporting when there is measurement error in both survey and administrative data as firm information might help us diagnose error in both data sources.

[13] TANF data are available for 36 states in 2018, 37 states in 2014, 36 states in 2010, and one state in 2006.

[14] Chapin et al. (2018) evaluated the use of similar data from CoreLogic in ACS production and discuss some strengths and limitations of this kind of data.

# 3 Data Linkage

To make use of all of this data, we link them to create two main files: 1) the Address File and 2) the Person File, with linkages made at the following levels:

- Individual - using Protected Identification Keys (PIKs),
- Address - using Master Address File identifiers (MAFIDs),
- Job - using PIKs and Employer Identification Numbers (EINs) and by the job matching procedure described below,
- Firm - using the LBD firm identifiers (LBDFID) and Employer Identification Numbers (EINs), and
- Geography - by state, county, and census tract

## 3.1 Person Linkage[15]

The Census Bureau developed the Person Identification Validation System (PVS) to probabilistically match individuals records in survey and other data to their SSN or Individual Taxpayer Identification Number (ITIN) using personally identifying information (PII), such as name, date of birth, and residential address (Wagner and Layne, 2014). Linked records are assigned a Protected Identification Key (PIK) and the PII and SSN or ITIN are removed. The PIK serves as the anonymized linkage key to match individuals across data sets.

As a result, if PVS is unable to assign a PIK to a given survey respondent, no administrative data are available for that respondent. Bollinger et al. (2019) found a linkage rate in their CPS ASEC sample (2006-2011) of 86 percent, which matches our estimate for the 2019 CPS ASEC. Because observable characteristics, such as race, ethnicity, citizenship status, etc., are correlated with PIK assignment (Bond et al., 2014), we must account for this selection into linkage in our estimates, which we discuss in Section 5.1.

[15] The discussion in this section follows Bee and Rothbaum (2019) closely.

## 3.2 Address Linkage

Brummet (2014) describes the development and performance of the system used to link household records, via residential address fields, to the Master Address File (MAF), called the "MAF Match." Information such as house number (and suffix, such as apartment number), street name (and prefix/suffix, such as rural routes or state highway identifiers), city, state, ZIP code, etc. is used to link addresses in each data set to the MAF, to assign them MAFIDs.

As with PIKs, this means that if the MAF Match process is unable to assign a MAFID to an address, the information associated with that address in that data source cannot be linked to other address-level data. For recent years of surveys such as the ACS, CPS ASEC, and SIPP, every housing unit has a MAFID because the sample was drawn directly from the MAF.

## 3.3 Job Linkage

The W-2, DER, and LEHD files all have information on individual jobs. However, unlike the LEHD, the W-2s and DER do not capture gross earnings. The Census Bureau receives W-2 extracts from the IRS that include Box 1 "Wages, tips, and other compensation," Box 3 "Social Security wages," and the sum of deferred compensation in Box 12 codes D-H.[16] We only observe taxable earnings and deferred compensation, but not other non-taxable earnings. We therefore do not have information on pre-tax employee payments for health insurance and other forms of pre-tax compensation not available in the extract provided by the IRS, such as contributions to Health Savings Accounts. In most of this section, we will primarily discuss W-2s and not the DER, as the two are identical for most workers for whom

[16] These codes include elective deferrals to plans under Box 12 codes D: 401(k), E: 403(b), F: 408(k)(6), G: 457(b), and H: 501(c)(18)(D). These boxes cover 96.3 percent of all elective retirement contributions on W-2s, calculated from IRS Statistics of Income Tax States for Individual Information Return Form W-2 Statistics, Table 7.A at `https://www.irs.gov/statistics/soi-tax-stats-individual-information-return-form-w2-statistics`, accessed 11/17/2021.

the DER is available.

Not all jobs are covered by unemployment insurance, and thus some jobs are out of universe for the LEHD. This includes all federal government employees and some private sector employees.[17]

Census money income includes gross wage and salary earnings, which is what is asked for in the earnings questions on the CPS ASEC and ACS. To match that, we would like gross earnings for each individual job, which we could use to estimate person-level gross earnings. However, we have gross earnings for a subset of jobs (from the LEHD) and taxable earnings + deferred compensation from the universe of jobs (from W-2s). Because the LEHD includes a subset of jobs we should observe in W-2s, it is possible for an individual to have one job in the LEHD and two in the W-2s. Therefore, we cannot just sum the earnings from both sources and take the maximum, because the likely higher valued one in this case (W-2 earnings from 2 jobs) may understate this individual's true gross earnings.

Therefore, we would like to combine the LEHD and W-2 records at the job level. For an individual with one LEHD job and two W-2 jobs, we would then observe gross earnings for one job and taxable earnings + deferred compensation for the other. For the second job, we could impute gross earnings conditional on the other information observed about them (discussed in Section 5.2) and then sum the job-level gross earnings to estimate their administrative gross earnings.

However, linking LEHD and W-2 jobs is not trivial. In the simplest case, a firm files a W-2 and reports the job to the UI office with the same EIN. We can link these "direct

[17] For example, Maryland's Department of Labor lists the following jobs as exempt: barbers and beauticians, taxicab drivers, owner-operated tractor drivers in certain E and F classifications, maritime employment, election workers, church employees, clergy, certain governmental employees, railroad employment, newspaper delivery, insurance sales, real estate sales, messenger service, direct sellers, foreign employment, other state unemployment insurance programs, work-relief and work-training, family members, hospital patients, student nurses or interns, yacht salespersons who work for a licensed trader on solely a commission basis, services of aliens who are students, scholars, trainees, teachers, etc., who enter the U.S. solely to pursue a full course of study at certain vocational and other non-academic institutions, recreational sports officials, home workers, and casual labor. Refer to `https://www.dllr.state.md.us/employment/empfaq.shtml` accessed 11/1/2022.

matches" by PIK and EIN. However, some firms do not file their W-2s and UI reports under the same EIN, and some firms use multiple EINs in one source but a single EIN in the other (i.e., a separate EIN for each state's employment in the LEHD but one EIN in the W-2s). Other firms use other identifiers, such as state EINs, when they report jobs to UI offices. Therefore, we cannot directly link many jobs between the LEHD and W-2 files using PIK/EIN combinations. Since nearly all jobs in both files include a PIK, we can create a set of possible matches that match on PIK but not EIN. We can then identify the W-2 EINs that correspond to a different EIN or state EIN in the LEHD by looking across all workers with unmatched jobs. We create a W-2 EIN to LEHD EIN crosswalk of these "indirect match" jobs.

An example of how we find direct and indirect matches is shown in Figure 3. In the example, we have three workers (PIK = 1, 2, 3) and their W-2 and LEHD jobs. For EIN = 400 and 500, the jobs match at the PIK-EIN level. However, EINs 100 and 600 in the W-2s and 200 in the LEHD do not match. We can see that each worker with EIN = 100 in the W-2s also has a job with EIN = 200 in the LEHD and each of those jobs has similar earnings on the two files. We use this information to infer that W-2 EIN 100 is the same firm as LEHD EIN 200. We would then be left with the W-2 job at EIN = 600 that does not match to any job in the LEHD, perhaps representing a job that is not covered by unemployment insurance.

To create a crosswalk of all indirect matches between W-2 and LEHD EINs, we develop an iterative algorithm using three pieces of information:

1. How close are the earnings reported on the W-2 and LEHD for the possible job match,
2. What share of jobs in the W-2 EIN match to the same LEHD EIN and what share of jobs from the LEHD EIN match to the same W-2 EIN, and
3. How many likely matches from a W-2 EIN to an LEHD EIN are there?

By varying cutoffs for these three criteria, we iteratively match jobs that do not have the same EINs on the two files. We discuss the linkage process in more detail in Appendix A.

In Table 3, we show summary statistics from the linkage process. In the W-2s, there are 257 million unique jobs in 2018, with 238 million in the LEHD. Of those, 216 million are direct matches by PIK-EIN combination. This leaves 41 million unmatched W-2 jobs and 22 million unmatched LEHD jobs. However, we find an additional 15 million indirect matches through our matching algorithm, covering 70 percent of the unmatched LEHD jobs and 37 percent of the unmatched W-2 jobs. We then have 82 percent of jobs matched directly by PIK-EIN, 6 percent matched indirectly, 10 percent unmatched from W-2s, and 3 percent unmatched from the LEHD. We use this linked job information to better estimate gross earnings at the job and person level for use in our income estimates.

Because we believe LEHD earnings should exceed W-2 taxable earnings + deferred compensation in large part due to employee pre-tax payments for health insurance premiums, we compare them in our CPS ASEC sample for individuals who reported whether they have private health insurance coverage.[18] As shown in Table 4, individuals with private coverage are less likely to have LEHD earnings that are approximately the same as their W-2 earnings + deferred compensation (LEHD $\geq$ W-2 by 0-1 percent), and covered individuals are 3 to 5 times more likely to have LEHD values that exceed the W-2 amounts by 1-3 percent, 3-5 percent, 5-10 percent, and 10+ percent. This likely reflects the missing gross earnings for employee pre-tax contributions to health insurance premiums on W-2s.

However, Table 4 also shows that there is a substantial number of jobs whose W-2 taxable earnings + deferred compensation exceeds LEHD gross earnings. At present, we treat these jobs as having measurement issues in the LEHD and default to the taxable earnings + deferred compensation from the W-2 and impute gross earnings for those jobs as discussed in Section 5.2. We plan to investigate this issue further in future NEWS releases.

[18] Note that the CPS ASEC variable we use indicates receipt of private coverage, but not necessarily that the individual's job (rather than a spouse, partner, or other family member) was the source of the coverage.

### 3.4 Firm Linkage

Our firm identifier in the employment data is the EIN. However, as we noted when crosswalking the job-level data between the W-2 and LEHD, an EIN does not necessarily correspond to a firm. Some firms have multiple EINs, for example in each state of operation, which can make matching individual workers to their firm (rather than subunits of the firm) difficult.

This is a challenge for all users of EIN-based administrative data (Joint Committee on Taxation, 2022; Chow et al., 2021). Chow et al. (2021) redesigned the Longitudinal Business Database (LBD) in part to help bridge this gap and to make linkages between various worker- and firm-level datasets easier. We use this redesigned LBD to map EINs to LBD firm identifiers (LBDFID). In the LBD, each establishment is associated with one or more EINs and also to a LBDFID. We create a crosswalk of all EIN to LBDFID combinations by year. If a firm restructures during a given year, it is possible for the same EIN to map to different LBDFIDs in the same year. When that happens, we assign the EIN to the associated LBDFID in the subsequent year. From that, we create a year-by-year EIN-LBDFID crosswalk for all firms in our data. We can then merge the job-level data by EIN to an LBDFID to match each worker to a firm. At the firm level (by LBDFID), we can then use LBD data or create our own summary statistics on firm employment and payroll from the linked job-level data. At present, we use this firm information for modeling, imputation, and weighting.

## 4 File Construction

### 4.1 Address File

The first file we create from the data in Sections 2.1-2.6 is the Address File. We link the sample of occupied (non-vacant) housing units in the survey to the aforementioned sources of administrative, survey, census, and third-party data, as shown in Figure 1. By starting with addresses, we have information from all occupied units, including respondents *and* nonrespondents. In the address file, we do not use any information from survey responses other than whether the unit responded. This file is used to construct the weights that address selection into our sample, discussed in Section 5.1.

First, we link the MAFIDs of occupied housing units to the MAF and Black Knight data to get information on the housing units, such as home value and type (single vs. multi-unit). We then link the same MAFIDs to several files that have both MAFIDs and PIKs, including the IRMF, MAF-ARF, and 1040 tax returns, giving us information on the information returns (W-2, 1099-G, etc.) sent to that address, their income (from tax returns), and PIKs for individuals who are associated with that address. We create a roster of PIKs for the linked individuals in each occupied unit. We then link this roster to various files, including the universe PHUS and SSR files, the Numident, W-2s, LEHD, and the IRMF and 1040 tax returns.[19] We then link the LEHD and W-2 jobs together using the job crosswalk discussed in Section 3.3. We also link those jobs to the characteristics of the employer firm in the LBD using the EIN-firm ID crosswalk discussed in Section 3.4.

Finally, we create geographic summary files at different levels of aggregation (state, county, and tract) that summarize the characteristics of residents of those locations from different files. These include 1) a summary of demographic characteristics from the 2010 decennial census, 2) demographic and socioeconomic characteristics from 5-year ACS files, 3) earnings and information return receipt from the IRMF and W-2 files, 4) citizenship information from the MAF-ARF linked to the Numident, and 5) income and marital status information from 1040 tax returns.

This gives us information on the income, earnings, industry, race, Hispanic origin, marital status, presence of children, home value, housing unit type, etc., as well as information

[19] For the IRMF and tax return link, we do this in case an individual associated with the address received an information return at a different address or was on a 1040 tax return filed from a different address.

about the neighborhoods in which each household lives. However, data coverage is not perfect. As shown in Table 5, we can link 93 percent of occupied CPS ASEC addresses to at least one data set (excluding the MAF, from which the addresses were sampled). That leaves 7 percent of addresses that we cannot link to any data other than the MAF. For these, we have no additional address-level information, and we cannot link the address to possible residents, which means that we cannot observe any address-level demographic or socioeconomic characteristics for these households (apart from the survey responses). For them, we only have information about their communities from the geographic summary files and about their housing unit from the MAF. Furthermore, we do not directly observe some characteristics that may be related to wellbeing and survey response, such as educational attainment, health insurance status, disability status (except if receiving SSI or OASDI), etc.[20]

## 4.2 Person File

The second file we create from the data in Sections 2.1-2.6 is the Person File. We create this file by linking survey respondents to administrative data, as shown in Figure 2. In combination with the weights created using the Address File, the Person File is used to create our income and poverty estimates.

The Person File contains survey responses, including demographics, socioeconomic characteristics, income, etc. as well as administrative information on income on the following files: 1040s, W-2s, DER, LEHD, 1099-Rs, PHUS, SSR, and TANF. Table 6 shows the data sources with information by income type (wage and salary earnings, Social Security, etc.) for tax filers and nonfilers. For tax filers, most income types are available in the administrative data, either as separate variables or as part of 1040 Total Money Income. For nonfilers, we

[20] Rothbaum and Bee (2022) evaluate how well weighting can control for differences between respondents and nonrespondents by one of the dimensions unobserved in our linked data, educational attainment, by linking the subset of housing units to prior ACS responses. They find that most, but not all, of the selection into response by educational attainment is addressed by weights created using similar linked data.

observe wages and salary earnings (W-2s, DER, and LEHD), OASDI benefits (PHUS), SSI (SSR), retirement income (10999-R), and TANF income (state data), as well as flags for the potential presence (but not amount) of interest income (1099-INT), dividends (1099-DIV), and unemployment compensation (1099-G). Several types of income are only available on the survey, regardless of tax filing status, including worker's compensation, veteran's benefits, educational assistance, and inter-household financial assistance. Table 7 shows the share of the sample that can be assigned a PIK and the share of individuals with a PIK that can linked to each of the administrative data sources.

# 5 Missing Data

A major challenge to estimating income is that we do not observe all the information that we would like for all individuals. There are several potential sources of missing data, such as:

1. Survey unit nonresponse - not all individuals respond to the survey;
2. Survey item nonresponse - individuals who do respond may choose not to respond to specific questions (a particular problem for income questions);
3. Selection into administrative data - not all households or firms may be present in the administrative data as part of how the program is structured. For example, a married couple did not need to file a tax return if their income was less than $25,100 in 2021;
4. Administrative data "nonresponse" - some records may be absent from the administrative data that should have been present. For example, although firms are required to file a W-2 for nearly all workers, some may choose not to for tax avoidance purposes and pay the worker "under the table";
5. Incomplete linkage - we may not be able to link all individuals across datasets; and
6. Incomplete data coverage - we may not have access to the data for specific individuals.

For example, SNAP and TANF data is not available for all states.

We use weighting to address survey unit nonresponse (1) and incomplete linkage (5). We use imputation and administrative record replacement to address survey item nonresponse (2), selection into administrative data (for missing LEHD gross earnings, 3), and incomplete data coverage (6). We use survey reports for unobserved income items to address selection into administrative data (other than gross earnings, 3) and administrative data "nonresponse" (4).

## 5.1 Weighting[21]

Weighting is one method for addressing missing data, where variables are completely unobserved for a subset of the sample. Let $R$ be an indicator for whether the information is available for an individual or unit (i.e., response to a survey). Given a set of $k$ variables $X = \{x_1, x_2, \ldots, x_k\}$ for $n$ units (individuals, households, firms). These covariates are observed for some units, but not others, $X = \{X_O, X_M\}$, where $O$ indicates observed ($R = 1$) and $M$ indicates missingness ($R = 0$).

There are several possible relationships between missing data and the individual and household characteristics we are interested in estimating. The simplest possible pattern of missingness (for the analyst) is if the data are missing completely at random (MCAR). In this case, nonresponse is completely random and not related to $X_O$ or $X_U$, or $R \perp (X_O, X_M)$. For example, if a unit flips a coin when deciding whether to respond to the survey, nonresponse would be MCAR. If the data are MCAR, then the solution is easy – we do not need any adjustment to the data to get an unbiased estimated. We can just drop missing observations. Only precision is affected by MCAR data, as the sample is smaller than if all individuals were observed.

Another possibility is that the data are missing at random (MAR), conditional on the ob-

[21] The discussion in this section follows Rothbaum and Bee (2022) closely.

servable information. Given a distribution $f(\cdot)$, data are MAR if $f(R|X) = f(R|X_O)$, which means that missingness is conditionally independent of the unobserved information ($X_U$). This is the underlying assumption of most nonresponse bias adjustments, such as survey weights.

However, another possibility is that the data are not missing at random (NMAR), where $f(R|X) \neq f(R|X_O)$. This is much more challenging to address. Suppose the probability of information availability varies with income, which is in $X$. Then $f(R|X) \neq f(R|X_O)$, and we cannot easily recover the true underlying income distribution from the observed data in $X_O$ without strong, generally difficult to verify assumptions about $f(R|X)$.

However, MAR is an independence assumption conditional on $X$. Suppose there is another set of variables $A$ that are observed for the full sample, independent of response. In that case it is possible that the data are NMAR with respect to $X$, but MAR with respect to $A$, or more formally $f(R|X) \neq f(R|X_O)$ but $f(R|X, A) = f(R|X_O, A)$. Rothbaum and Bee (2022) found that from 2020 to 2022, nonresponse in the CPS ASEC was NMAR with respect to $X$ and that income statistics were biased by 2-3 percent as a result. They used additional information from administrative data linked at the address level to the addresses of respondent *and* nonrespondent households to adjust the weights for nonresponse.[22]

There are several aspects of our data that lend themselves to weighting to address missing information — where a subset of variables is completely missing for some units. For survey nonresponse, none of the survey information is observable for the nonresponding units. For incomplete linkage, none of the administrative data is available for the unlinkable individuals. If survey nonresponse or linkage are MAR, we can address the bias through weighting.

To include additional characteristics in the weighting model, we use entropy balancing (Hainmueller, 2012). Entropy balancing is an application of exponential empirical calibration. Empirical calibration has a long history of use in survey weighting (Deming and Stephan,

[22] Rothbaum et al. (2021) did the same to address nonresponse bias in the 2020 ACS.

1940; Deville and Särndal, 1992) – the existing weighting models (using raking) in the ACS and CPS ASEC are applications of empirical calibration.[23]

We use the unobservable information (in the survey) from the linked administrative and decennial census data, which are available for all linkable households regardless of whether they responded as well as the geographic summary information. Entropy balancing estimates weights that match a specified set of moment constraints (i.e., to adjust the weights according to $f(R|X_O, A)$) while keeping the final weights as close as possible to the initial weights. Refer to Appendix Section B for a formal description of entropy balancing.

Entropy balancing has several appealing features for this application. The first is flexibility. Inverse probability weighting (or any simple regression-based reweighting technique) is only amenable to matching characteristics of the distribution in the sample, but not external targets. Empirical calibration will adjust the weights to match any properly specified target moment, whether that moment was estimated on the sample or with external data. The second is statistical efficiency, which is achieved by keeping the final weights as close as possible to the initial probabilities of selection.[24] Third, entropy balancing directly adjusts the weights to the moment conditions, like with raking but unlike single-index propensity score weighting approaches (such as inverse probability weights). In propensity score approaches, the adjustment is made to the single index generally estimated from a regression. The resulting balance must be assessed to evaluate the success and quality of the propensity score model. In some cases, a misspecified propensity score model can make balance worse on a given set of dimensions. As entropy balancing directly targets those moments, balance is assured. Fourth, unlike raking, or cell-based empirical calibration methods, entropy balancing allows for the inclusion of continuous variables in the weighting model.

The fifth is computational efficiency – entropy balancing allows matching to a high-dimensional

[23] Raking, also called iterative proportional fitting, adjusts the weights for each group to match the population total for that group. It is solved by iterating across groups to match the different population targets in stages.

[24] Through the minimization in equation B.1.

vector of moment constraints. In terms of our MAR assumption, if $A$ or $X$ is high dimensional, then the computational efficiency makes it feasible to include all of $A$ and $X$ in the weighting model. As in Rothbaum and Bee (2022), we use state-level population controls that include estimates of the share of the population in 20 separate groups in each of the 50 states and the District of Columbia. That yields 1,020 separate target population moments before even considering information from the linked administrative data. The computational efficiency of the entropy balancing optimization algorithm allows us to match to both the linked administrative and population control targets simultaneously. This eliminates the need for an additional population control raking step that can undo the balance from the nonresponse adjustment.[25]

The full reweighting procedure is described in Table 8 and discussed in detail in Appendix B. Stage 1 adjusts for nonresponse at the housing unit level by reweighting respondent households to match the characteristics of occupied households estimated from the linked administrative, decennial, and third-party data. Stage 2 creates individual weights that maintain the adjustment from Stage 1, but additionally adjust the person weights to match the external population controls. As in Rothbaum and Bee (2022), the Stage-2 weights adjust the sample for selection into survey response.

However, because we are using administrative data to address survey misreporting, inclusion in our sample is also conditional on linkage to a PIK, as that is the key to linking each individual to *every* source of administrative data. Our final sample includes only those

[25] Several studies have implemented first-stage nonresponse adjustments followed by second-stage raking to population controls that do not condition on the first-stage adjustment. Slud and Bailey (2010) found that for some metrics of weight quality, the benefits of the first-stage adjustment disappeared after the application of the second-stage raking to population controls. Eggleston and Westra (2020) found that for some measures used in the first-stage adjustment, the bias is not improved or can be greater using the final weights after raking to population controls, although most statistics show reduced bias after the second-stage raking. Rothbaum et al. (2021) found something similar in follow-up work on the ACS when applied to the 5-year release. Without including very detailed population controls in the 2020 1-year ACS weights (down to tract-level population), when the 2016-2020 files were combined and raked to the 5-year population controls, the 2020 nonresponse adjustment had little impact on the 5-year estimates. Only when the 2020 file was simultaneously reweighted to detailed population controls and the linked administrative targets, limiting the need for additional raking adjustments, did the nonresponse bias adjustment persist on the final 5-year file.

households where all those old enough to receive survey income questions (15+) are assigned a PIK. To address this selection, we add a third stage to the entropy balancing weighting procedure used in Rothbaum and Bee (2022), as shown in Table 8, Stage 3. The Stage-3 weights maintain the adjustments of the Stage-2 weights, but also control for selection into linkage, to the extent possible given the observable survey and linked administrative data.

For valid inference, we repeat the above two-stage reweighting procedure 160 additional times using the baseline successive difference replicate factors created during the sampling process, which are available for all households regardless of response status. These replicate factors account for the sampling design of the monthly Basic CPS and CPS ASEC. Also, the first-stage target moments from the March Basic CPS sample are estimates and thus subject to sampling error. By repeating the procedure with the base weights and replicate factors, the target moments for each replicate will vary and variation in the final weights across the replicates will reflect the uncertainty in our linked data estimates. All standard errors reported using EBW are calculated with these 160 replicate-factor EBW.

As noted in Rothbaum et al. (2021), in addition to changing point estimates, improved weights can also affect standard errors. It is generally understood that increased variability among the survey weights can increase the standard errors, so weighting adjustments aimed at reducing bias are often done at the expense of increasing variance. However, Little and Vartivarian (2005) showed that this may not hold if variables used to adjust for nonresponse are correlated with survey variables of interest, a property they call "super-efficiency." This also has implications for how weighting models should be constructed, as including variables that are not strongly predictive of response, but are correlated with outcomes of interest, can reduce variance of an estimate even if they do not affect its bias.

Figure 4 shows the bias in estimates of address-linked characteristics using the various weights. In each panel, we compare the five separate weights to the target moments estimated on the set of all occupied housing units. They are:

1. Respondents — the weights only adjust for the probability the housing unit is selected into the sample

2. Survey — the final survey weights

3. HH EBW — the Stage 1 weights that adjust for response at the household level only

4. EBW — the Stage 2 weights that adjust for response at the household level and to the external population controls

5. EBW + PIKed — the Stage 3 weights that adjust for response at the household level, to external population control, and for selection into linkage.

From Figure 4, we can see that OASDI recipients (linked to the PHUS) are overrepresented with the respondent and survey weights (Panel A), as are housing units with residents that are 65 and over (Panel B). The EBW bias estimates in Panels A and B (those that can be directly targeted in the weighting) are all very close to zero, with few statistically significant differences.[26]

Figure 5 compares statistics estimated on survey responses using the survey weights to those estimated using the Stage 2 (EBW) and Stage 3 (EBW + PIKed) weights. In this case, the survey-weighted and EBW estimates by race, Hispanic origin, and age should match the survey estimates by construction (as they are each weighting to external population controls). However, differences for other statistics for the EBW relative to the survey-weighted estimates reflect potential bias in the survey estimates, which we see, for example, for household income.

[26] Percentiles cannot be directly matched by entropy balancing. Instead, the weighting model weights respondents to match the share of units in different income bins (i.e., the share of households with address-level W-2 earnings $\leq$ \$25,000.

## 5.2 Imputation[27]

Suppose we have two variables $Y_i$ and $Y_j$ with missing values indicated by $R_i = 0$ or $R_j = 0$. Missingness is monotone if $R_j = 0$ in all cases where $R_i = 0$. The pattern of missingness discussed above for weighting is one case of monotone missingness.[28] Missingness is non-monotone if $R_i = 0$ does not imply that $R_j = 0$.

While weighting can address missing data for the monotone missingness discussed in the prior section, it is not optimal as a general missing data correction when missingness is non-monotone. For non-monotone missingness, imputation is a better approach as it fully utilizes the available information (Raghunathan et al., 2001). In this section, we discuss imputations models generally followed by our implementation.

Suppose $O$ is a collection of observable variables with no missing values, with $O = (O_1, O_2, \ldots, O_q)$ and $Y_1, Y_2, \ldots, Y_p$ are variables with missing values, with $Y = (Y_1, Y_2, \ldots, Y_p)$. Further, let $U$ be a set of unobserved characteristics. Let $f(Y|O, U, \theta)$ be the conditional joint density, with $\theta = (\theta_1, \theta_2, \ldots, \theta_p)$ and where $\theta_j$ is a vector of parameters in the conditional distribution for $Y_j$ such as regression coefficients and dispersion parameters. An imputation model imposes some assumptions on $f$ and $\theta$ to assign plausible values to $Y$ where data are missing.

In this case, $Y$ is MAR if missingness can be accounted for by observable characteristics, which can be written as $f(Y|O, \theta) = f(Y|O, U, \theta)$ (Rubin, 1976).[29] Another way to view imputation is through the lens of a researcher or data user. Consider a statistic $Q$, which could be a distributional statistic (such as a mean or median), a regression coefficient, or any other statistic or parameter of interest to the researcher. An imputation model is congenial or proper and results in unbiased estimates of $Q$ if $E(\hat{Q}|O, \theta) = E(\hat{Q}|O, U, \theta) = Q$ and has valid confidence intervals for $\hat{Q}$ (Meng, 1994; Rubin, 1996).

[27] The discussion in this section follows Hokayem, Raghunathan and Rothbaum (2022) and Fox et al. (2022) closely.

[28] In that case, we are assuming that for all variables in $X$, $R_i = R$, where $i = 1, \ldots, k$.

[29] It is NMAR if $f(Y|O, \theta) \neq f(Y|O, U, \theta)$.

This is only true when the imputation model is congenial and proper for the analysis being conducted. There are many examples in the literature where this congeniality condition fails for a given statistic or set of statistics. An example is match bias in the CPS. Bollinger and Hirsch (2006) showed that because the imputation model in the CPS does not include union status, estimates of the relationship between union status and earnings are attenuated in the imputed data. Even in this case, the issue is not that their earnings are misclassified (as very rarely will imputed earnings match the true value for a given individual), but that they are drawn from the wrong distribution – one that does not condition on union status. However, uncongeniality for one statistic does not indicate bias for other related statistics. For example, match bias on union status does not necessarily mean that the CPS imputation model will bias statistics of the unconditional earnings distribution.

It is impossible for congeniality to hold for all possible statistics $Q$, unless the model perfectly predicts the missing values, i.e., there is no misclassification.[30] However, we could assess the quality of an imputation model by comparing a set of the resulting $\hat{Q}$ estimates against known $Q$ values. Fox et al. (2022) took this approach, using a variety of statistics, including regression coefficients and conditional and unconditional distributional statistics to evaluate their imputation model.

Hokayem, Raghunathan and Rothbaum (2022) addressed survey nonresponse in the CPS ASEC in 2009-2013 by including more covariates in the imputation model than the current CPS ASEC hot deck approach and comparing models with and without administrative data on earnings and income in the model. They find further evidence of match bias. However, with sufficient information in the model, they do not find evidence of nonignorable nonresponse (NMAR) when they compare the estimates of imputes that condition on administrative income to those that do not.

[30] In this sense, misclassification can be important. If the imputed value equals true value for all cases, the data are not truly "imputed." However, in practice, imputations are unlikely to have extremely low misclassification rates, and we must evaluate the potential bias of each $\hat{Q}$ with the available information.

This non-monotone missingness is present in several variables in our data. Income items are particularly prone to survey nonresponse - over 40 percent of earnings (and all income) is imputed in the CPS ASEC due to nonresponse in recent years (Hokayem, Raghunathan and Rothbaum, 2022). We also do not observe gross wage and salary earnings (in the LEHD) for all jobs because not all jobs are covered by unemployment insurance and non-covered jobs are not reported to state UI offices. Gross earnings are also missing for jobs that are not available in the LEHD for other reasons, such as firms that erroneously fail to report jobs and states with no data-sharing agreement in a given year. For the missing survey responses and missing gross earnings, we observe a lot of information (variables in $O$) that can help us *predict* the missing values, such as W-2 job-level earnings, survey-reported occupation, hours and weeks worked, educational attainment, private health insurance coverage, etc.

We use Sequential Regression Multivariate Imputation (SRMI) to impute plausible values for the missing data (Raghunathan et al., 2001).[31] SRMI is an iterative resampling technique to estimate $f(Y|O, \theta)$ while imposing fewer strong parametric assumptions on the joint conditional distribution $f$.

We also use imputation rather than reweighting even in the presence of monotone missing data for incomplete data coverage. For example, SNAP, TANF, and, in some years, LEHD data are not available for all states. We could reweight the available individuals in states with available LEHD, SNAP, and TANF data to get a nationally representative estimate of income and poverty, but we would then not be able to estimate poverty at the state level.[32] Therefore, we instead impute the relevant variables for individuals and households in states without available state-level data, following prior work on SNAP in Fox et al. (2022).

In all, we impute four sets of variables for the NEWS estimates:

1. Survey earnings variables,

[31] SRMI has also been called Fully Conditional Specification and Flexible Conditional Models in the literature.

[32] There would be no observations from the states missing LEHD, SNAP, TANF data to use in an estimate.

2. Job-level gross administrative earnings,

3. Household-level means-tested program variables for missing states, and

4. UI compensation, interest, and dividend amounts for individuals who did not file a tax return but did receive the applicable information return.

For survey earnings, we impute extensive margin earnings receipt and intensive margin earnings amounts for all earnings variables. In recent years, over 40 percent of all earnings in the CPS ASEC is imputed for nonresponse (Hokayem, Raghunathan and Rothbaum, 2022). We also impute upstream variables that are highly predictive of earnings, including weeks worked last year and hours worked per week last year.

In the job-level administrative data, we observe gross earnings in the LEHD (when available), but only observe taxable earnings and deferred compensation in the W-2s and DER. However, as noted in Section 2.4, the LEHD is not available for all workers, including uncovered private-sector employees and federal government employees. Likewise, there are years when not all states have data sharing agreements with the Census Bureau, resulting in missing gross earnings for all jobs in specific states for certain years. We impute gross earnings for these jobs (up to the two highest earning jobs per individual)

For state-level means-tested program data, we impute program receipt and, conditional on receipt, the amount received for each program at the household level.

For nonfilers, we observe whether they received several information returns, including Forms 1099-G, 1099-INT, and 1099-DIV in the IRMF. From these we have information on whether they received UI compensation,[33] interest income, and dividends, respectively. Each of these are vastly underreported on surveys (Rothbaum, 2015). Rothbaum (2023) has access to more detailed data available under a separate agreement between the Census Bureau and the IRS, for limited use. In that work, the 1099-G, 1099-INT, and 1099-DIV data are

[33] Form 1099-G's are sent to individuals who received UI payments, as well as other government transfers, including state and local income tax refunds and credits.

available, including income amounts. Rothbaum (2023) released coefficients that can be used to impute these amounts for nonfilers conditional on survey responses and the administrative data more broadly available to the Census Bureau and used in this project. We impute UI compensation, interest income, and dividends using those parameters for nonfilers, as this income would already be reported on tax filings.

We run the imputation model five times, to create five independent implicates to account for the uncertainty in the imputation process through this multiple imputation (Rubin, 1976).[34] To do so for any given statistic, we calculate the total variance by combining the within implicate variation (based on the standard error of an estimate in one implicate) with the between implicate variation (the variance of the estimates for that parameter across the five implicates).

In Table 9, we show the rates of missing data for survey earnings, state program data, and LEHD job-level gross earnings. In the 2019 CPS ASEC, 46 percent of individuals with earnings had their primary job earnings imputed. We do not have state-level administrative TANF data for 47 percent of households. Finally, we impute gross earnings for 18 percent of jobs, either because there is no LEHD information for them (8 percent of highest earning jobs) or because the LEHD and W-2 values disagree substantially (i.e., the LEHD < W-2, about 10 percent of highest earning jobs).

We discuss the imputation models in more detail in Appendix C.

As the imputation models are applications from prior work (Hokayem, Raghunathan and Rothbaum 2022 for earnings, Fox et al. 2022 for means-tested benefits, and Rothbaum 2023 for nonfiler UI, interest, and dividends), we provide limited statistics on the imputation outputs. Table 10 shows summary statistics for survey earnings imputation, comparing the CPS ASEC imputations to the NEWS SRMI imputations conditional on W-2 earnings. The SRMI estimates fewer individuals with zero survey earnings conditional on having zero W-2

[34] Imputation uncertainty is not currently accounted for in most Census Bureau surveys.

earnings. Also, the SRMI estimates higher survey earnings conditional on having higher W-2 earnings (such as in the 5th quintile of W-2 earnings). Table 11 provides some summary statistics for means-tested program imputation.

# 6 Misreporting and Measurement Error[35]

Earnings, which constitutes about 80 percent of household income, has been the most heavily studied component of income in linked data.[36] Alvey and Cobleigh 1975, Duncan and Hill 1985, Bound and Krueger 1991, Bound et al. 1994, Pischke 1995, Bollinger 1998, Bound, Brown and Mathiowetz 2001, Roemer 2002, Kapteyn and Ypma 2007, Gottschalk and Huynh 2010, Meijer, Rohwedder and Wansbeek 2012, Abowd and Stinson 2013, Murray-Close and Heggeness 2018, Bee, Mitchell and Rothbaum 2019, Imboden, Voorheis and Weber 2019, Jenkins and Rios Avila Forthcoming and many others have studied wage and salary earnings. Although survey earnings are relatively well reported when compared to external benchmark aggregates (Rothbaum 2015), work with linked microdata has found systematic differences between administrative records and survey responses.

The aforementioned papers have generally found survey wage and salary earnings are "mean-reverting" relative to administrative reports; i.e., low earners in the administrative data tend to report higher earnings on surveys, and high earners in the administrative data tend to report lower earnings in surveys. There is also extensive margin disagreement between survey and administrative records – about 10 percent of working-age individuals have earnings in only one data source but not the other (Bee, Mitchell and Rothbaum 2019).

When discussing measurement error, we cannot assume that it is present in surveys only. Under-the-table earnings are, by definition, not reported to the IRS, which can bias income estimates for particular subgroups of the population (such as by occupation). Some papers

[35] The discussion in this section follows Bee and Rothbaum (2019) closely.

[36] Earnings make up about 80 percent of all personal income in the Bureau of Economic Analysis's National Income and Product Account tables as well as in the CPS ASEC (Rothbaum 2015).

in the survey misreporting literature assumed the administrative records were free of error (Bound and Krueger 1991, Bound et al. 1994, Pischke 1995, for example).[37] However, more recent work considers the possibility that administrative data also contain measurement error, such as unreported earnings. Abowd and Stinson (2013) consider a model in which both survey and administrative reports for a given job may contain error. Under their approach, "true" earnings are a weighted average of the two reports, but they leave the selection of the proper weight to future work. Using Danish administrative data, Bingley and Martinello (2017) cannot rule out that survey income reports have only classical measurement error given the presence of measurement error in administrative records. In the absence of a "truth set" of data, it is an open question how much of this disagreement is due to misreporting on surveys or measurement error in the administrative data.

Compounding the challenge, it is not even always the case that different sources of administrative data agree. Bee, Mitchell and Rothbaum (2019) found a 0.4 percentage point difference in the estimated poverty rate if survey earnings are replaced using administrative earnings data from SSA compared to data from IRS, both of which are based on the same W-2s.

The likelihood of measurement error in administrative data is particularly high for self-employment earnings. The IRS publishes estimates of self-employment under-reporting using data from random audits (Internal Revenue Service, Research, Analysis & Statistics. 2016). As a result of under-reporting to the IRS, the Bureau of Economic Analysis (BEA) adjustment for self-employment income averaged 86 percent of the net profit reported in tax returns from 2010 to 2015 (U.S. Bureau of Economic Analysis 2019). In other words, nearly half of the BEA's National Income and Product Account (NIPA) estimate for self-employment earnings is not reported to the IRS. In contrast to wage and salary earnings, Abraham et al.

[37] In some cases, the authors restrict their analysis to a subset of workers for which the assumption is more likely to be valid. For example, Pischke (1995) compares surveys of employees of a particular firm against firm reports of the same workers' earnings. Bound and Krueger (1991) specifically remove occupations they suspect may have under-the-table earnings.

(2021) found that those who report self-employment earnings in the CPS ASEC are unlikely to report any self-employment earnings (on the 1040-SE) to the IRS. They also find the reverse — those with positive self-employment income in a 1040 SE are likely to report no self-employment income in the CPS ASEC. By looking at the relationship between earnings and consumption, Hurst, Li and Pugsley (2014) estimated that self-employment earnings are under-reported by 25 percent. However, Garin, Jackson and Koustas (2022) found that up to 59 percent of recent increases in administrative self-employment can be explained by changes in reporting that are "independent of changes in the nature of work."

In a companion paper (Bee et al., 2023), we discuss our model of measurement error in survey and administrative wage and salary earnings and how we apply that model to determine whether to use survey or administrative earnings reports for each individual. In the following parts of this section, we describe the observed relationships between survey and administrative record (adrec) earnings and provide a short summary of the model in that paper.

### 6.0.1 Survey and Adrec Earnings Reports in the Data

Our model for combining survey and adrec reports requires independent measurement error across reports. Our different administrative records of earnings are often constructed from similar or even identical sources, and so rather than assume these different adrecs have independent measurement error, we combine them into a single "best" adrec report prior to the model. Here we describe this adrec combination process.

We have several separate reports of administrative earnings, from W-2s, the DER, and the LEHD. In Table 12, we show summary statistics on the share of individuals assigned a PIK with any wage and salary earnings reported from all possible combinations of these sources. We also show the probability of reporting non-zero survey earnings for each combination of administrative wage and salary sources, for survey respondents. As shown in Panel A, 86 percent of individuals with earnings in any source, have earnings in all three of the W-2, DER, and LEHD data, 89 percent of whom report survey wage and salary earnings (conditional on survey earnings response). A further 6 percent of individuals with any administrative earnings have earnings from W-2s and the DER, but no earnings in the LEHD, 70 percent of whom report wage and salary earnings in the survey.

As the DER is a file of W-2 information that has been cleaned by the SSA, we investigate cases in which the W-2 and DER agree and disagree conditional on whether we can link an individual to an entry in the Numident (which is a proxy for whether he or she has a valid SSN) in Panel B. If individuals have W-2 and DER earnings, they are basically always present in the Numident and are very likely to report wage and salary earnings in the survey (87 percent). However, if individuals are in the Numident and have W-2 earnings, but no DER earnings, then they are very likely *not* to report wage and salary earnings in the survey. This suggests that there is measurement error in these cases in the W-2 file that is not in the SSA-provided and cleaned DER data. We therefore default to the DER information in these cases of no job-level administrative earnings. However, if individuals are not in the Numident and have W-2 earnings, but no DER earnings, they are very likely to report wage and salary earnings on the survey (85 percent). In these cases, we believe the DER is missing earnings for those without SSNs that is correctly present in W-2s. For these individuals, we default to the W-2 information of positive job-level earnings. This is an example of how administrative data are not necessarily free of error and different sources of administrative data covering the same concept (wage and salary earnings) from the same tax information do not necessarily agree.

From the three separate administrative job-level wage and salary earnings sources, we construct our job-level estimate of gross earnings. In most cases, this is the annual gross earnings from the LEHD. If that is missing (or we have concerns about LEHD data quality), we impute job-level gross earnings, as discussed in Section 5.2. We aggregate these job-level earnings to estimate total administrative wage and salary earnings for each individual. This gives a measure of total administrative wage and salary earnings $(y_a)$ , which we will then use in the model with our final post-imputation total survey wage and salary earnings $(y_s)$ discussed in Section 5.2.

The survey and administrative earnings can differ on the extensive or intensive margin. With extensive margin disagreement, where earnings are present in one but not both sources, we default to the earnings report that is non-zero. We are assuming that any survey report in the absence of administrative earnings reflects under-the-table income or a reporting or linkage issue in the administrative data. We are also assuming that any administrative earnings without a corresponding survey earnings report reflect under-/misreporting on the survey. These are both assumptions we plan to examine in future work.

The other difference we observe is intensive margin differences in reporting, where the reported values are not equal. Figure 6 shows a scatterplot of survey vs. administrative reports of wage and salary earnings.[38] Several important features of the data are visible in the figure. First, survey and administrative earnings generally agree, reflected in the clustering around the 45° line. However, regressing survey on W-2 wage and salary earnings (in logs) yields a slope of 0.8, which is consistent with mean reversion in survey earnings reports.[39]

However, there is mean reversion in survey relative to administrative earnings, which is reflected in a regression fit line of survey earnings regressed on administrative earnings with a slope of $\hat{\beta} = 0.8$. The slope would be 1 if survey earnings had only classical measurement error (and administrative earnings were equal to true earnings). Finally, there is considerable noise in survey relative to administrative earnings. While the points are generally cluster around the 45° line, there is a lot of dispersion.

[38] The figure is reproduced from O'Hara, Bee and Mitchell (2017) as more recent disclosure rules limit the possibility of releasing such detailed information of individual survey and administrative earnings values.

[39] For example, if we assumed no measurement error in W-2 earnings, then a slope that is less than 1 could indicate mean-reverting error non-classical measurement error in survey responses.

## 6.1 Earnings Choice Model Summary

In our companion paper, Bee et al. (2023) define a model that parametrizes the measurement error in $y_a$ and $y_s$ relative to the unobserved true earnings ($y$) for intensive margin disagreement. We provide a concise summary of the model here. The fundamental challenge of this work is that we believe there can be measurement error in both survey and administrative earnings reports and that we do not have data on "true" earnings for anyone. Therefore, we must impose assumptions on the data that are untestable or can only be tested indirectly. For example, we believe that administrative earnings could be underreported either because some income is missing (such as some portion of tips) or some jobs may be missing. Likewise, we do not assume that administrative earnings are free of classical measurement error, or noise, even if we believe that noise may be of lower variance than the noise in survey earnings reports.

These assumptions provide some structure to our earnings measurement error model. The model setup consists of: (a) survey earnings, which are conditionally unbiased but have potentially downward-biased conditional variances, and (b) adrecs, which can be conditionally biased but have accurate conditional variances. As an example of these assumptions, consider estimating earnings for auto mechanics as a group. Assumption (a) would imply that if you asked auto mechanics what they made on a survey, while some would over-report and some would under-report, you would get an accurate average. On the other hand, on an individual level these mechanics might not remember their exact earnings and so report their earnings from an "average" year, so variation across survey reports would not reflect true variation in earnings for that year. On the other hand, assumption (b) implies that adrecs would fail to generate a correct average for auto mechanic earnings, presumably due to under-the-table payments. But with assumption (b), the adrecs do an accurate job capturing variation across earnings, so that a mechanic who earned twice as much as another on his or her W2 would be expected to have actually earned twice as much.[40]

[40] This would be satisfied if, for example, all auto mechanics reported 50 percent (or any fixed percent)

While these assumptions on survey versus adrec are not directly testable, they were chosen to be both consistent with prior literature on measurement error in earnings and to be consistent with previous measurements of average income. Under our assumptions, the survey would be unbiased for average income measures but may have trouble accurately assessing income in the tails of the distributions. On the other hand, relying only on adrecs may generate significant biases in the estimation of income for populations with income typically not covered in adrecs. Combining these two sources lets us mitigate both these problems simultaneously.

With our assumptions on survey and administrative earnings from above, Bee et al. (2023) define a model in a Mean Squared Error (MSE) framework with a set of parameters on the random noise in $y_s$ and $y_a$ (conditional on $x$) and the relative mean reversion in survey vs. administrative reports. Bee et al. (2023) define a "survey confidence" (SC) measure that is a function of two sets of terms. The first is a measure of the estimated bias in the administrative data by comparing $E(y_s|x)$ to $E(y_a|x)$. The second set of terms compares the relative variance of the random noise in the two reports conditional on $x$. We select the survey report if the squared bias term exceeds the difference in the variance terms, or if in the MSE framework, the estimated administrative bias is exceeded by its relatively lower noise.

However, the model is only identified and able to be estimated if we make an assumption about the degree of mean reversion in survey reports vs. administrative reports. This mean reversion parameter, $\kappa$ (or "kappa" in tables and figures in this paper), cannot be estimated, and must be assumed because true earnings $y$ are never observed. If $\kappa = 1$, there is no mean reversion in the survey relative to the administrative data. We assume greater mean reversion as $\kappa$ decreases from 1. With a given $\kappa$, we can estimate the SC measure for each individual conditional on his or her $x$ characteristics, which would reflect the model's "confidence" by comparing the bias and variance terms in an MSE framework. We use this SC measure in

of their income to the IRS.

our decision rule to select the survey or administrative wage and salary earnings report — if SC $> 0$, we select the survey report.[41]

We select the "best" wage and salary earnings report for individuals based on their observable characteristics $x$, but *not* conditional on their actual survey or administrative reports. This is in contrast with Meyer et al. (2021*b*), which takes the maximum of survey-reported and administrative earnings in at least some cases. In other words, we take survey reports for people whose characteristics suggest that their survey reports are better (according to the SC measure) than their administrative reports. Bee et al. (2023) discuss potential limitations and extensions of this approach to incorporate the actual earnings reports and additional information (such as longitudinal earnings histories) to improve our estimates of earnings given survey and administrative reports.

However, misclassification of wages versus self-employment earnings also complicates the picture for earnings. If individuals report wage and salary earnings on the survey but self-employment earnings on their tax returns, are those two separate sources of income or is it the same income reported in different categories? Only 35 percent of individuals with positive administrative self-employment earnings report any self-employment earnings on the survey and less than 50 percent of the survey self-employed have positive self-employment earnings in the administrative data (Abraham et al., 2021). At this time, we generally defer to the administrative data when there is disagreement about the source of earnings (wage and salary vs. self-employment) or if self-employment is reported in both survey and administrative data. In the future, addressing misclassification of earnings and self-employment earnings misreporting is an important avenue of research and improvement of our income estimates.

In Table 13, we summarize the possible combinations of survey and administrative reports

[41] Bee et al. (2023) discuss the implementation details of the estimation and additional features of our decision rule in the case when we determine that $E(y_s|x) < E(y_a|x)$ with some confidence for a given individual.

of wage and salary and self-employment earnings and show which we use in our income estimates. The measurement error model discussed in this section is used for 53 percent of adults[42] and for 74 percent of individuals with any reported earnings in either source. Another 39 percent of adults had no survey or administrative earnings or reported earnings in one source, but not the other. Given that we default to the source with reported earnings under extensive margin disagreement, that leaves above 8 percent of adults or 12 percent of individuals with earnings in either source for whom we ignore survey reported wage and salary earnings and use only administrative data due to potential misclassification or other data issues.

In Table 14, we show the share of individuals whose survey earnings would be used for various $\kappa$ mean-reversion parameter estimates (from the set of people listed as using the measurement error model in Table 13). The share varies from 6 percent ($\kappa = 0.7$) to 31 percent ($\kappa = 1$, no survey-report mean reversion). For the NEWS estimates, we select $\kappa = 0.9$ as it implies a relatively modest level of mean reversion and selects the survey wage and salary earnings report 21 percent of the time. However, we assess robustness to alternative values of $\kappa$ in Section 8.2.

Given our chosen survey mean reversion parameter, in Table 15 we show the share of individuals whose survey earnings were used as part of our measurement error model (as a share of workers from Table 13 for whom the measurement error model was used). Overall, we use survey earnings for 21 percent of workers. As shown in the table, the rate at which survey earnings are used varies by age, race, occupation, and industry. For example, survey earnings are used less often for Black workers and younger (18-24) and older (55+) workers. To give one example by industry and occupation, survey earnings are used 59 percent of the time for workers in the construction industry.

[42] In this context, we define adult as people aged 15 and above who are asked the CPS ASEC earnings questions.

# 7 Estimating Income and Poverty

We are now ready to estimate income and poverty, given the weights estimated on the Address File, the survey and administrative information on the Person File, the earnings, TANF, and nonfiler income imputations, and the earnings measurement error model to select whether to use survey or administrative earnings reports. The full sequence of steps is described in Table 1.

In this section, we discuss the final step — how we combine the data to estimate income for tax filers and nonfilers. We have separate processes for both as there is more income information available for tax filers, but some of it is only available at the tax unit, but not the individual, level.

## 7.1 Tax Filers

For tax filers, we start with Total Money Income (TMI), which is the sum of taxable wage and salary income, interest (taxable and tax-exempt), dividends, alimony received, business income or losses (including from partnerships and S-corps), farm income or losses, net rent, royalty, and estate and trust income, unemployment compensation and gross Social Security benefits (as noted in Section 2.3.1).

For wage and salary earnings, TMI includes taxable wage and salary earnings reported on the 1040. However, this will understate earnings if gross earnings are greater than taxable earnings, for example if individuals have deferred compensation or use pre-tax earnings to pay health insurance premiums. It will also understate earnings if they are under-reported to the IRS. Therefore, we replace the wage and salary earnings component of TMI with our survey or job-level administrative earnings according to the rules shown in Table 13 and discussed in Section 6.0.1. We also replace 1040-reported Social Security income, as we are more confident in the data quality of the SSA data than in the gross 1040 amounts, which may not be well-reported in tax returns (particularly for non-taxable Social Security income).

For retirement income, we cannot distinguish defined contribution (DC) plan withdrawals from defined benefit (DB) pensions in the 1099-R data.[43] In the CPS ASEC, DC withdrawals are only counted as income for people aged 59 and above. We therefore follow that convention and include 1099-R retirement income for all individuals aged 59 and older. For those under 59, we include the 1099-R income if they reported pension or annuity income on the survey. We add this retirement income to TMI.

Finally, we add several income components that are not taxable. From administrative sources, we add SSI and TANF and from the survey, we add educational assistance, financial assistance, worker's compensation, and veteran's benefit payments. For filers, that gives us our adjusted TMI, which we use in the income and poverty estimates.

## 7.2 Nonfilers

For nonfilers, we must add up the available components individually, since we do not have the 1040 TMI to start from. To get the nonfiler equivalent of adjusted TMI, we start with wage and salary and self-employment earnings as indicated in Table 13. From administrative data sources, we add Social Security income (PHUS), retirement income (from the 1099-R following the same rules for as noted above by age), SSI (SSR), and TANF (state data). We add UI compensation, interest, and dividends imputed using the parameters estimated on the complete 1099-G, 1099-DIV, and 1099-INT data (Rothbaum, 2023). From the survey, we add rent and royalty income, educational assistance, financial assistance, worker's compensation, and veteran's benefit payments. For nonfilers, that gives us our adjusted TMI, which we use in the income and poverty estimates.

[43] We plan to apply and extend the work in Bee and Mitchell (2017) to characterize individual withdrawals as defined benefit or defined contribution in future work.

# 8 Estimates

## 8.1 NEWS Estimates

Table 16 and Figure 7 compare the 2018 NEWS estimates for median household income to the survey estimates released in Semega et al. (2019).[44] For all households, the NEWS estimate for median household income was 6.3 percent higher ($67,170 vs. $63,180). Median household income was higher for nearly all subgroups shown. The main exceptions, however, were by age of householder. Pooled together, median household income for households under age 65 were not statistically different (-0.1 percent lower point estimate) whereas households 65 and older had *27.3* percent greater median household income ($55,610 vs. $43,700). For households aged 55-64, the difference was 5.0 percent ($72,430 vs. $68,950). For all age groups below 55, the point estimates were not statistically different from zero or negative.

Figure 8 shows estimates from the $10^{th}$ to $95^{th}$ percentiles of the household income distribution overall and by race and Hispanic origin, age of householder, and educational attainment. Overall, income increased more in proportional terms at the bottom of the distribution than at the top. This is particularly true for age 65 and over households, for which NEWS household income was 31 percent higher at the $25^{th}$ percentile 20 percent higher at the $75^{th}$ percentile and 15 percent higher at the $90^{th}$ percentile.

Comparisons between NEWS and survey estimates for poverty are shown in Table 17 and Figure 9. Overall, poverty was 1.1 percentage points lower than in the survey estimate, a 9.4 percent decline in the number of people in poverty. As with income, poverty was much lower for the 65 and older population — we estimate a 3.3 percentage-point lower poverty rate and 34.1 percent fewer people in poverty. There were no groups for which poverty was statistically higher with the NEWS estimates. However, we did not find a statistically

[44] All estimates are in 2018 dollars. To adjust to 2021 dollars using the R-CPI-U-RS as in official Census Bureau publications, multiply each income estimate by 399.0/369.8 = 1.079.

significant decline in poverty for Black individuals, children, residents of the Midwest, those outside of Metropolitan Statistical Areas, those with a disability, and those with some college education.

Finally, in Table 18, we compare NEWS estimates for inequality statistics to the survey estimates, including for income shares, the Gini index, and various percentile ratios.[45] For shares of income, we find a decrease in the share of income in the $2^{nd}$ to $4^{th}$ quintile and an increase in the share of income in the top quintile and particularly the top 5 percent. We estimate an increase in the Gini coefficient from 0.459 to 0.476. This is likely coming from no top coding and higher extreme income values in the administrative data relative to the survey,[46] despite the larger increase in income at lower percentiles of the income distribution shown in Figure 8, Panel A. However, consistent with that figure, we find declines in the percentile ratio estimates (90/10, 90/50, and 50/10). For example, in the survey responses, household income at the $90^{th}$ percentile is 12.5 times as large as at the $10^{th}$ percentile. With the NEWS estimates, the ratio is 11.5.

## 8.2 Robustness to Alternative Uses of Earnings Data

Figure 13 compares NEWS estimates of household income to estimates with alternative combinations of survey and administrative wage and salary earnings. In Panel A, we show how income varies under different rules for using earnings when the survey and administrative data disagree at the extensive margin, whether any earnings are present. We compare four scenarios to the NEWS estimates (with $y_a$ for administrative earnings and $y_s$ for survey earnings: 1) use $y_a$ unless $y_a = 0$ and $y_s \neq 0$, 2) use $y_a$ (even if $y_a = 0$ and $y_s \neq 0$), 3) use

[45] One important area of future research is how to address potential data issues that affect inequality, including how well our sample captures income at the far right tail of the distribution and how to address administrative data issues (like implausible extreme values) that might bias inequality statistics. We note this when discussing our future plans in Section 10. This will affect statistics such as income shares and the Gini coefficient that condition on the entire income distribution, but have less of an impact on statistics percentile ratios.

[46] Survey income top codes vary by income item, but generally do not exceed \$1.1 million dollars for a given income source.

$y_s$ unless $y_s = 0$ and $y_a \neq 0$, and 4) use $y_s$ (even if $y_s = 0$ and $y_a \neq 0$). Scenarios 1) and 2) give priority to administrative earnings and 3) and 4) give priority to survey earnings. If we use either source of earnings when the other is zero, income declines substantially (2 and 4), particularly at lower income levels. If we use administrative earnings if $\neq 0$ (1), the household income point estimates are generally lower than the NEWS estimates, although most of the differences are not statistically significant. If we use survey earnings if $\neq 0$ (3), the household income point estimates are lower everywhere, but the differences are only statistically significant in the tails of the distribution.

To summarize, how we handle extensive margin disagreement substantially affects our income estimates, as does whether we prioritize survey or administrative earnings. Compared to just using administrative earnings (if $\neq 0$), the measurement error earnings model does not have a substantial impact on household income overall, despite using survey earnings for 21 percent of the individuals the model was used on. In Figure 13 Panel B, we estimate the household income distribution for alternative $\kappa$/survey mean-reversion parameters in the earnings measurement error model. As $\kappa$ varies from 1 to 0.7, the share of individuals whose survey earnings are used changes from 6 to 31 percent. Despite this, while there are statistically significant differences between the NEWS estimates ($\kappa = 0.9$) and estimates with other $\kappa$, there are few economically meaningful differences in the household income estimates. For example, none of the alternative $\kappa$s estimates a statistically significant difference in median household income and the range on the point estimates is from -0.05 percent to 0.03 percent different from NEWS estimate. At the $95^{th}$ percentile, the estimates range from -0.46 percent to 0.89 percent different from the NEWS estimate (with only 0.89 percent different for $\kappa = 0.7$ statistically different from the NEWS estimate).

However, as Panel C of Figure 13 shows, the choice of how to combine survey and administrative earnings *could* matter a lot. In Panel C, we add another possible decision rule, which is to take the maximum of the two reports. This approach might be reasonable if one thinks all misreporting in both survey and administrative data is underreporting, although that does not seem consistent with the noise in survey reports around administrative wage and salary earnings we see in Figure 6.[47] Taking the maximum of reported wage and salary earnings would vastly increase measured household income across the distribution, with an average difference across the percentiles shown of 13.5 percent greater income than the NEWS estimate.

## 8.3 Impact of Different Processing Steps on Income and Poverty Estimates

The NEWS estimates reflect several bias correction steps, including reweighting for nonresponse, reweighting for linkage to administrative data, imputing to address nonrandom nonresponse, replacement of survey responses with administrative income information (including observed and imputed TANF and gross earnings), and the earnings measurement error model to select survey or administrative earnings. We decompose the adjustments to show the impact of each of these steps on the distribution of household income in Figure 10. In Panel A, we show the weighting and survey imputation steps compared to the survey estimates, as these steps use administrative data to adjust for bias in survey-only information (the weights and imputed earnings). In Panel B, we show the impact of using administrative data (as discussed in Section 7) and the earnings measurement error model compared to the adjusted survey estimates from Panel A. In this way, Panel A shows the survey-only adjustments and Panel B shows the effect of the final two steps after accounting for the survey-only adjustments.

The weighting steps lower income across most of the distribution by 1 to 2 percent.[48] Re-

[47] Meyer et al. (2021*b*) take the maximum of survey and administrative earnings (total earnings, not just wage and salary) at least in some cases. However, they argue their estimates of extreme poverty are not affected by this because in most cases both the survey and the administrative earnings measure exceeds their extreme poverty thresholds when they disagree on the intensive margin.

[48] This is slightly different than Rothbaum and Bee (2022), which found no statistically significant differences across the distribution with an average point estimate of -0.23. However, we use more data, particularly

placing the survey earnings imputations (and accounting for uncertainty through multiple imputation) lowers the point estimates at the bottom of the distribution, consistent with the selection into response observed by Bollinger et al. (2019) in the tails and results in confidence intervals that are wider on average.

In Panel B, we show the impact of the final two steps, income replacement and the earnings measurement error model, compared to the estimate after survey earnings imputation from Panel A. In this way, Panel B shows the effect of replacing survey responses with the administrative income data, as discussed in Section 7. We compare the household income distribution with and without the administrative data and find large effects across the distribution, from 17.1 percent at the $10^{th}$ percentile, to 10.3 percent at the $25^{th}$, 6.8 percent at the median, and 3.6 percent at the $75^{th}$. Panel B also shows the impact of the earnings measurement error model and the use of survey earnings, which has a minimal impact on household income.[49] Panel C shows the overall comparison between the NEWS and survey estimates.[50]

Figure 11 shows the same decomposition by survey adjustments (Panel A) and administrative income replacement and measurement error model (Panel B) for the subgroups in Table 16. Figure 12 does the same for poverty. In both, it is generally the case that the survey adjustments move point estimates for median household income down and poverty up, but generally the differences are not statistically significant. The administrative income replacements move income up and poverty down for most subgroups as well.

contemporaneous rather than lagged 1040 income in the NEWS project, which may reflect selection into response that was not captured in that paper using data available during the regular CPS ASEC production schedule.

[49] We discuss how alternative uses of survey earnings could have had a large impact in the next section.

[50] The same information by age of householder (under 65 and 65 and over) is available in the Appendix in Figure A1.

## 8.4 Impact of Different Income Types on Income and Poverty Estimates

Finally, we assess how specific administrative income components affect the household income distribution and poverty. To do so, we start with the NEWS income estimates and replace each administrative income item one by one (not sequentially) with its survey counterpart and compare each statistic after the replacement to the NEWS estimate. The results are shown for income in Figure 14 and poverty in Figure 15.

For income, we start with interest and dividend income in Figure 14 Panel A. We make three replacements: 1) replace administrative interest income with survey interest income, including the survey measure of interest (and other returns) on retirement accounts, 2) replace administrative income with survey interest income, excluding the retirement account interest, and 3) replace administrative dividends with survey dividends. If we include interest on retirement accounts (as is the case in the survey income estimate), we get more income across the distribution than using administrative income (which does not include this interest). Because we already count withdrawals from these same retirement accounts as income, this risks double counting the same income, which is why we exclude it from the NEWS estimate. If we replace interest or dividends excluding this interest from retirement accounts, we see slightly lower income across the distribution.

Figure 14 Panel B shows the impact of replacing Social Security (OASDI), SSI, and TANF income. If we just replace SSI income with survey responses, we see increases in income at the bottom of the distribution, primarily because of misclassification of Social Security and SSI, effectively double counting Social Security for individuals that reported Social Security income as SSI. If we replace Social Security only, we see big declines in income at the bottom and smaller declines higher in the income distribution. If we replace both together, we see slightly smaller declines at the bottom because we are preserving the misclassified income (SSI reported as Social Security on the survey, for example). Replacing TANF with survey

responses results in small declines in income that are only significantly different at a handful of points.

For reference, 14 Panel C shows the effect of replacing NEWS wage and salary earnings with survey responses, as was shown in Figure 13 and discussed in Section 8.2.

Figure 14 Panel D shows the major adjustments from Panels A-C: 1) interest and dividends together, 2) Social Security and SSI together, 3) survey wage and salary earnings (including if survey = 0). It also shows a new adjustment, replacing retirement, survivor, disability, and pension income (retirement income, from Form 1099-R) with the corresponding survey items. Even for overall income, the retirement income replacement has the biggest impact across much of the income distribution.

As shown in Figure 15, overall poverty is higher with survey income for interest and dividends. It is much higher if we use survey retirement income. Likewise, replacing administrative with survey earnings has a big effect on poverty, particularly if we use the survey even if it is equal to zero with positive administrative earnings.

# 9 Transparency and Data Availability

An integral goal of the NEWS project is to be as transparent and open about the data we use, how we clean them, and how we combine them to generate the NEWS income, poverty, and resource estimates. This is crucial for these estimates, as there are many decisions about how to clean, process, and combine survey and administrative data that can have major effects on the results and be relatively opaque and in-the-weeds for even a well-informed outsider. For example, using the maximum of survey and administrative income reports, as shown in Figure 13 Panel C, would drastically bias our income and poverty estimates in a way that is not consistent with the survey reporting noise in Figure 6. Transparency about our methods, code, and estimates is required for an outsider to understand the implications of those kind of detailed data choices.

As such, we commit to making as much of the data as we can and all of the code available in the Federal Research Data Center (FSRDC) system.[51] We also commit to making the code publicly available, with as few edits as possible as required by the rules on the disclosure of code to abide by Titles 13 and 26 and our agreements with data providers.

With each run of the NEWS code, we also plan to log any changes to input extracts so we can track any changes to input data (such as data provided by the IRS or an updated version of a survey file) that may affect our estimates. We also use a software version control system (git) to ensure that the code that generated the results in this paper (or any future paper with updated data, code, and methods) can be replicated.[52]

We also have written documentation for nearly all the files and functions involved in loading and cleaning the data, creating the address and person extracts, implementing the reweighting, imputation, and earnings measurement error model, generating the final person and tax unit income variables, and estimating income and poverty. While no documentation is perfect, we have endeavored to be as detailed as possible in this documentation, detailing what each section of code is doing (including line numbers). This is in addition to the regular commenting provided within the code itself.

# 10 Future Research and Release Plans

This release represents version 1.0 of the NEWS project. There are many aspects of this work that we were not able to include in this release and have left for future work. In this

[51] Subject to the constraints of our data agreements with the various state and federal agencies and third-party data providers.

[52] Up to the limit of what is possible in the software we use. Unfortunately, there are functions we currently use, such as Stata's rmcoll function to remove collinear variables from a regression that do not necessarily remove the same variables even when run with the same random seed. The exact set of variables kept can then affect the results from subsequent steps, such as LASSO regression feature selection. A goal for future releases is to remove our dependence on any function that has this property as we would like to ensure that a rerun of the code with the same data and initial seeds generates exactly the same estimates.

section we discuss our goals for version 2.0 and beyond.

First, we have estimated income and poverty in a single year, 2018, as a proof of concept and first step in this work. We would like to expand this to include more years, both earlier years and years up to the present. This will add challenges. Some administrative data are not available before a specific year. For example, the Census Bureau only has access to the universe of W-2 earnings starting in 2005. Likewise, not all administrative data are available in time for estimates of income in the prior year. For example, we might get data from SSA or state agencies with a lag of a year or more. Creating historical or preliminary estimates in the absence of complete data is an important direction for future research.

Second, we have only estimated statistics at the national level. In the future we want to extend the estimates to smaller geographic units, including states, counties, and possibly census tracts. However, to do so would require changes to how the estimates are generated. First, we would likely move to the ACS as the main source of survey information for subnational estimates. However, the ACS has less detailed income information, which makes this work more challenging. In particular, several income types are grouped in the ACS, so we do not have separate survey reports of interest, dividends, rental income, unemployment compensation, worker's compensation, etc. These items are reported as part of questions that ask about several income items simultaneously. Therefore, it will be difficult to know whether the respondent was also reporting another type of income that is not well-covered by available administrative data. In the long term, we may even move beyond the survey sample (while using survey information in the process) to better estimate statistics for small areas using the available administrative, decennial census, and third-party data.

Third, we have generated estimates only for pre-tax money income, as measured in the Census Bureau's annual income and poverty release (Semega et al., 2019). However, there is considerable interest in how in-kind benefits, taxes, and credits affect measures of material wellbeing. We plan on expanding the notions of resources we measure and as well as the set of wellbeing and deprivation statistics we report. For example, we could measure the distribution of disposable income, disposable income + the cash value of some (or all) in-kind transfers, improved measures of compensation that include employer matches to retirement contributions and employer contributions to health insurance premiums, the Supplemental Poverty Measure (SPM), etc. This will entail estimating taxes and credits and/or addressing household roster disagreement between administrative and survey data (Unrath, 2022), incorporating additional data on housing assistance from the Department of Housing and Urban Development and from states on the Special Supplemental Nutrition Assistance Program for Women, Infants, and Children (WIC), and potentially improved imputation and misreporting corrections for other programs such as the National School Lunch program, etc.

Finally, there are dimensions of misreporting and measurement error that we were not able to address in this version. For example, we have discussed how self-employment earnings are underreported in both survey and administrative data (Hurst, Li and Pugsley, 2014; Internal Revenue Service, Research, Analysis & Statistics., 2016) and how much survey and administrative reports disagree on the extensive margin (Abraham et al., 2021). It is not settled in the literature how to adjust for this underreporting (Auten and Splinter, 2018; Piketty, Saez and Zucman, 2017), much less how one would do so and get unbiased estimates by subgroup. We plan to extend our measurement error model to self-employment earnings for which different assumptions about mis- and underreporting would be necessary. Likewise, it may be the case that survey samples, even those as large as the ACS, do not adequately capture the incomes of the top individuals and households. Imputation, combination, or reweighting may be insufficient to address this issue to estimate unbiased inequality statistics from a survey sample. We plan on also researching methods to better estimate inequality statistics that account for the far-right tail of the income distribution.

We would also like to further investigate how our adjustments affect estimates for subgroups that may be challenging to reach or be unlikely to be present in the administrative data, such as non-citizens. Weighting and imputation, in particular, assume that the data is missing at random conditional on the observable information. However, there may be limited observable information in the address-linked administrative records to identify and adjust for selection into response by citizenship status. Likewise, our weighting adjustment for linkage uses survey response information to reweight individuals and households that can be linked to administrative data to be representative of the full sample. However, it may be that conditional on the observable survey information (and the address-linked administrative data), the data are not missing at random and that our final estimates for this group are biased. Similarly, there are difficult to reach subgroups that are not in sample for the CPS ASEC that we would like to estimate wellbeing statistics for, such as individuals in group quarters and the homeless or unhoused.

# 11 Conclusion

This release under the NEWS project is a first step toward integrating what we know about bias and measurement error in survey and administrative data into a set of “best possible” estimates of income, poverty, and resource statistics. We have attempted to address as many of the sources of bias as possible, including nonresponse bias (unit and item), selection into linkage to administrative data, misreporting of survey and administrative income, and incomplete data. However, much work remains to be done to address additional potential sources of error. As we and other researchers advance our understanding of how to address these measurement challenges, we will revise these estimates.

This work also suggests several additional avenues of possible research at the Census Bureau. For example, estimating income and poverty from linked survey and administrative data could impact the information we depend on surveys to provide. Surveys could focus less on items that are well captured in administrative data (such as Social Security payments) and more on items that improve linkage and those that are less well captured by administrative data (self-employment income, etc.). The Census Bureau could also increase efforts to collect survey responses from hard-to-reach groups who may be less well covered by administrative data.

The focus of this project is on improving our estimates of income and poverty. However, much of the work entails trying to understand the quality of various data sources. This has potential benefits for many data users of both survey and administrative data who are not primarily focused on income and poverty measurement. We hope to extend our work, particularly on earnings, to help characterize the data quality issues that other researchers may confront. For example, for which workers do missing earnings in a given data source (i.e., no W-2 job) reflect true zero earnings as opposed to positive unobserved earnings (i.e., as reported in a survey, the LEHD, or in tax filings), and how might this affect estimates of earnings risk and income dynamics?

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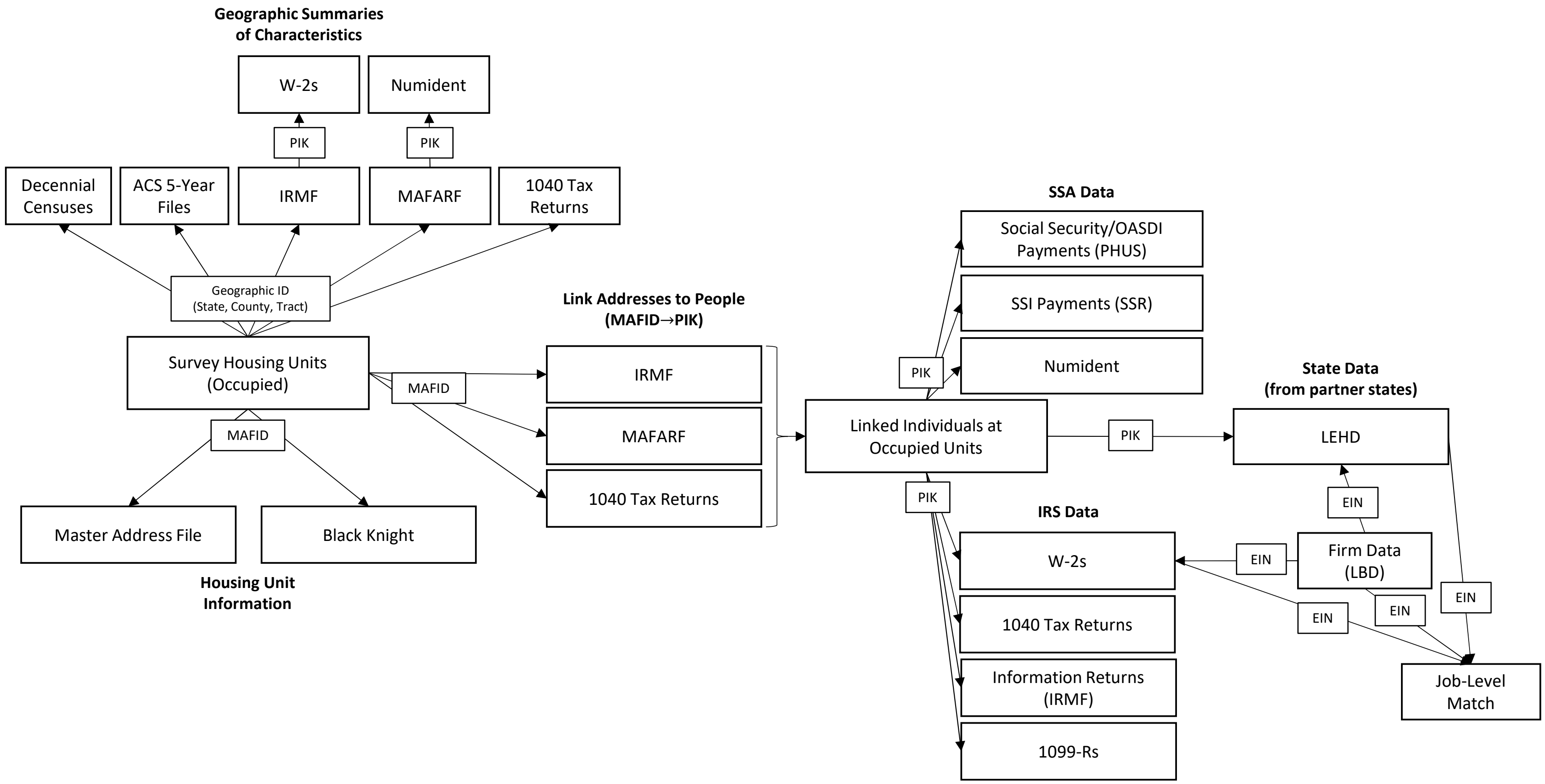
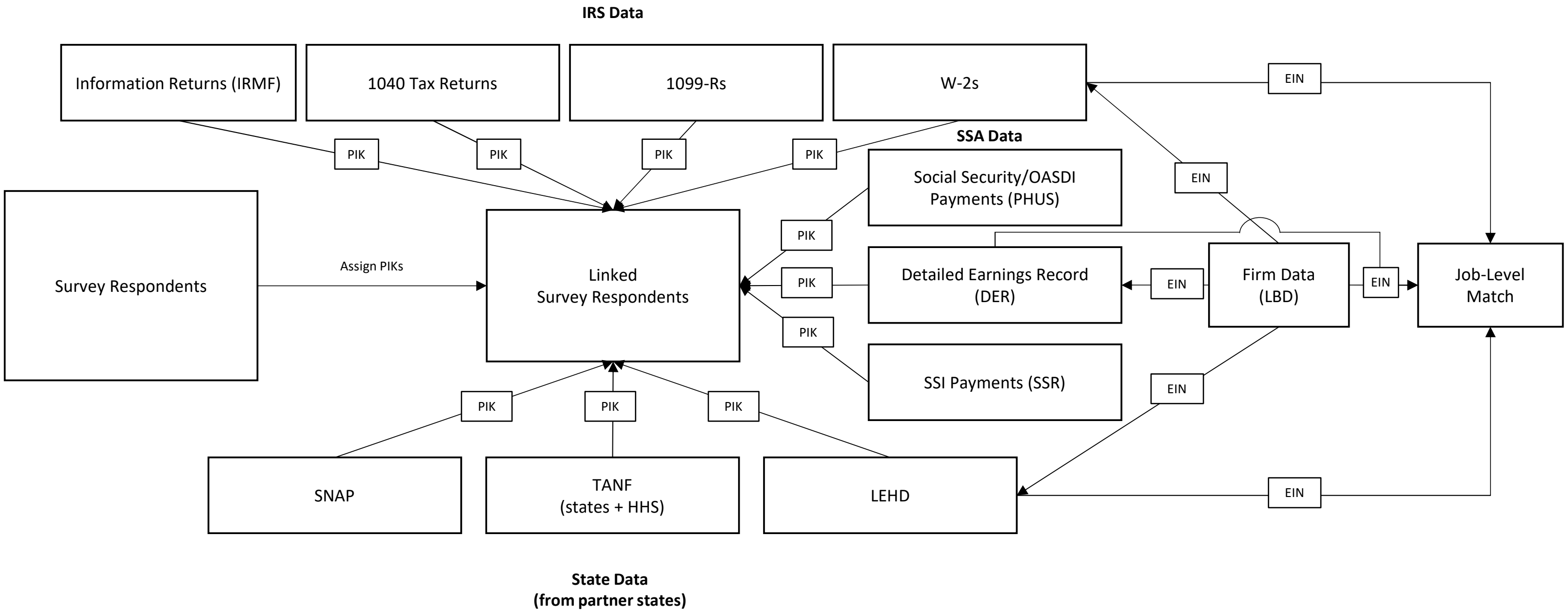
Figure 1: Linkage Diagram for Address File



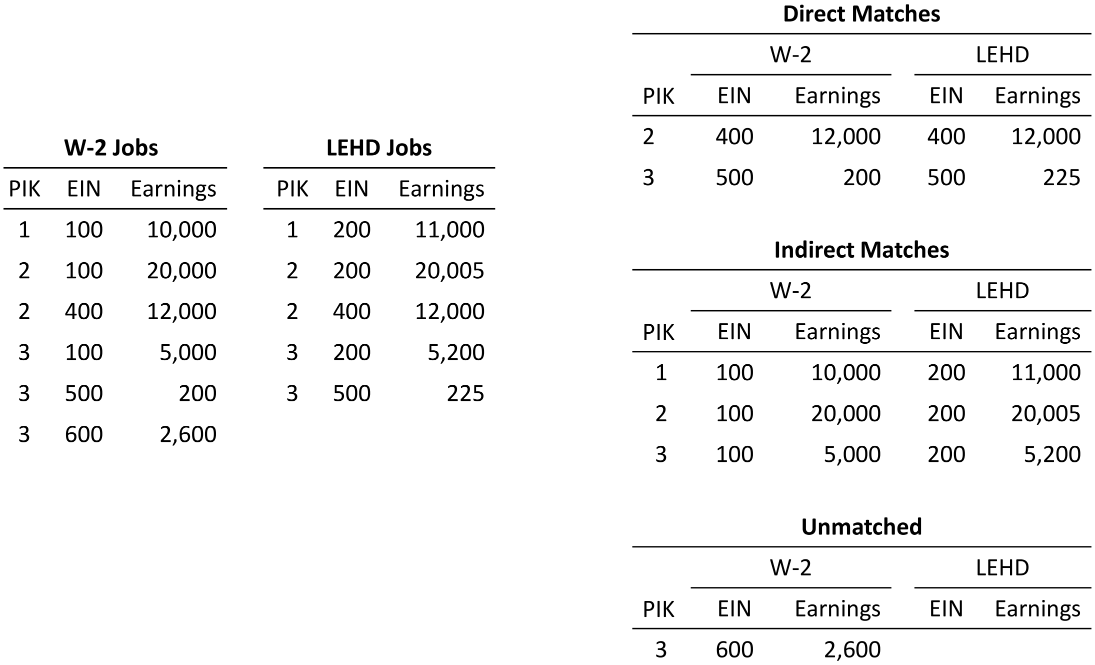
*Notes:* This diagram shows the linkage used to create the address-based extract file used for weighting. The file starts with the set of occupied addresses in the survey. That file is linked to three sets of files: 1) Geographic summaries of characteristics (by state, county, and tract identifiers), 2) housing unit information from the Master Address File and Black Knight data, and 3) files to link the addresses to people living in them (MAFID → PIK). From the third set of files, we create a roster of all individuals found in the occupied surveyed units and link them to the files shown to the right.

Figure 2: Linkage Diagram for Person File



*Notes:* This diagram shows the linkage used to create the person-level extract file. The file starts with the set of respondents in the survey. For those respondents that can be linked to their Social Security Numbers and therefore assigned a Protected Identification Key (PIK), we link them to the administrative records shown.

Figure 3: Simple Job Linkage Example



**W-2 Jobs**

| PIK | EIN | Earnings |
|---|---|---|
| 1 | 100 | 10,000 |
| 2 | 100 | 20,000 |
| 2 | 400 | 12,000 |
| 3 | 100 | 5,000 |
| 3 | 500 | 200 |
| 3 | 600 | 2,600 |

**LEHD Jobs**

| PIK | EIN | Earnings |
|---|---|---|
| 1 | 200 | 11,000 |
| 2 | 200 | 20,005 |
| 2 | 400 | 12,000 |
| 3 | 200 | 5,200 |
| 3 | 500 | 225 |

**Direct Matches**

| | W-2 | | LEHD | |
|---|---|---|---|---|
| PIK | EIN | Earnings | EIN | Earnings |
| 2 | 400 | 12,000 | 400 | 12,000 |
| 3 | 500 | 200 | 500 | 225 |

**Indirect Matches**

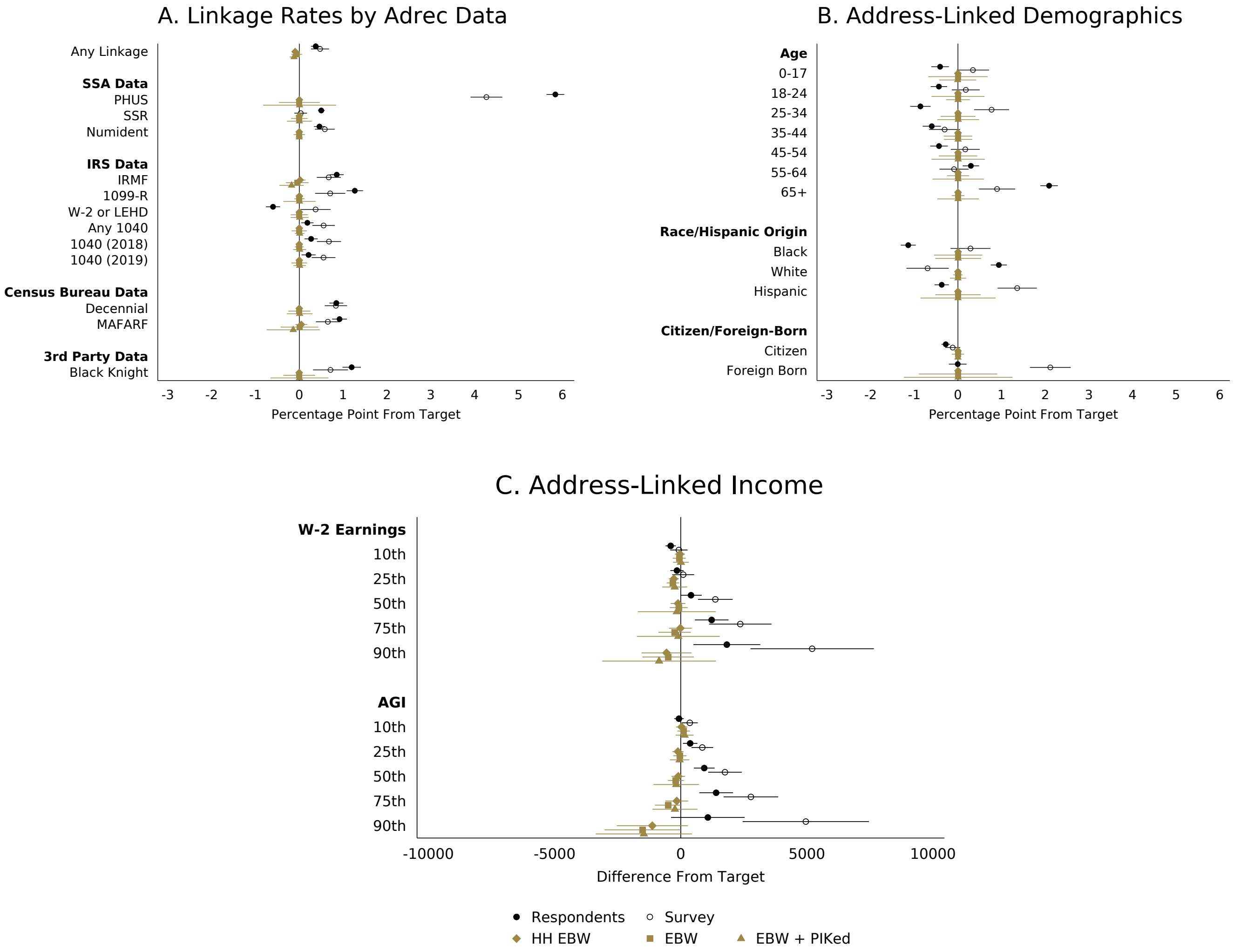
| | W-2 | | LEHD | |
|---|---|---|---|---|
| PIK | EIN | Earnings | EIN | Earnings |
| 1 | 100 | 10,000 | 200 | 11,000 |
| 2 | 100 | 20,000 | 200 | 20,005 |
| 3 | 100 | 5,000 | 200 | 5,200 |

**Unmatched**

| | W-2 | | LEHD | |
|---|---|---|---|---|
| PIK | EIN | Earnings | EIN | Earnings |
| 3 | 600 | 2,600 | | |

*Notes:* This is an example of how jobs are linked between W-2s and the LEHD (all PIKS, earnings, and EINs in the example are made up and do not correspond to actual individuals or firms). First and easiest are the jobs that match on PIK and EIN (same person, same firm identifier), which we call direct matches. Next, we find the indirect matches, where each person has one EIN on the W-2s and another on the LEHD (same person, but different firm identifiers on the two files). In this example, everyone with W-2 EIN = 100 has a job with similar earnings on the LEHD, but with EIN = 200. Finally, there are jobs that remain unmatched and only exist on one file or the other.

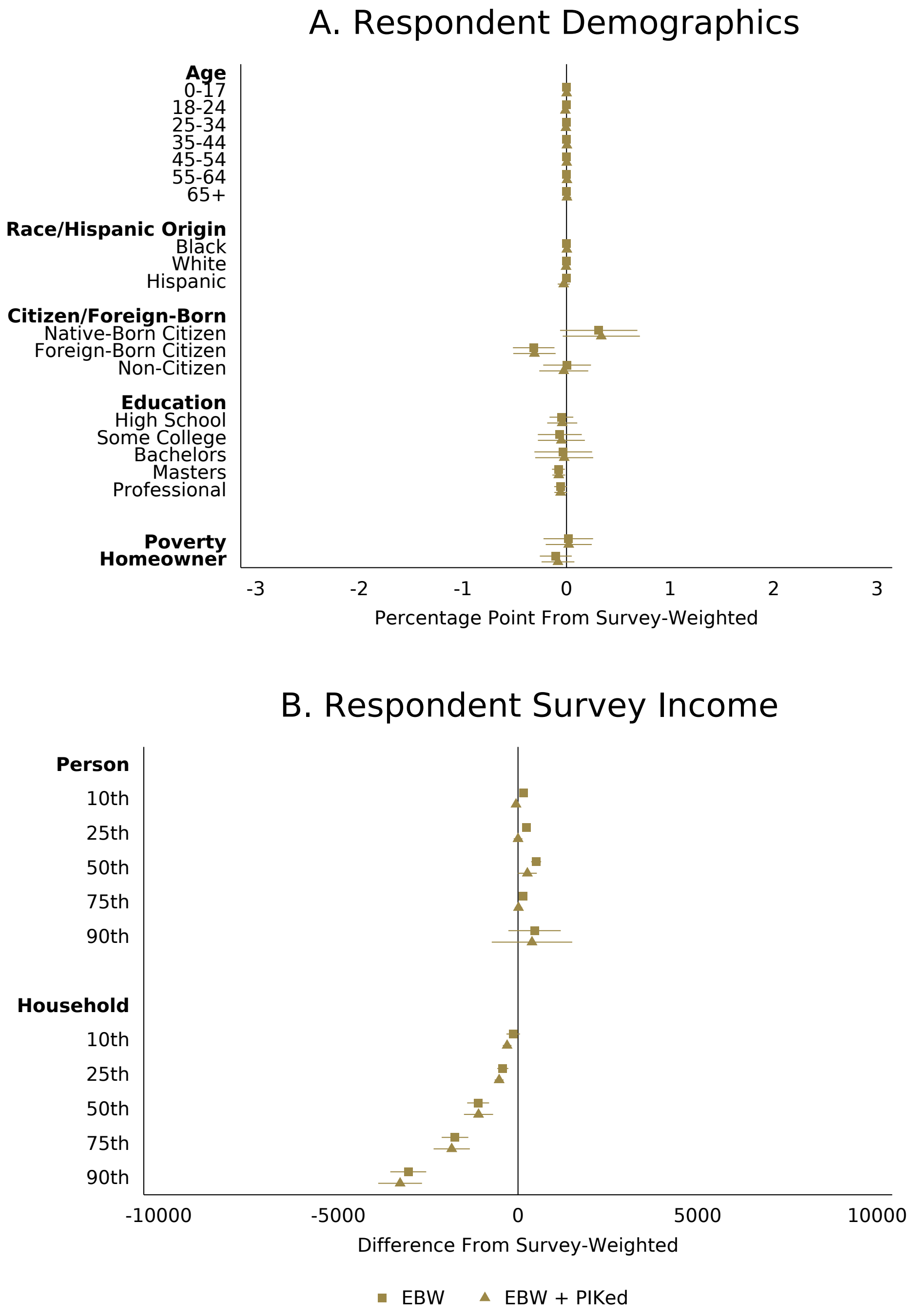
## Figure 4: Comparing Bias in Linked Administrative Characteristics with Different Weights



*Notes:* This figure shows various statistics of address-linked administrative, decennial census, and third-party data (refer to Section 4.1) using different weights compared to the weighting targets (discussed in Sections 5.1 and Appendix B and shown in Table 8). "Respondents" uses the base weights which adjust only for probability of selection into the sample. "Survey" uses the survey weights. "HH EBW" are the Stage 1 weights that adjust for selection into response at the household level. "EBW" are the Stage 2 weights that further adjust to population controls and "EBW + PIKed" are the Stage 3 weights that further adjust for selection into linkage.

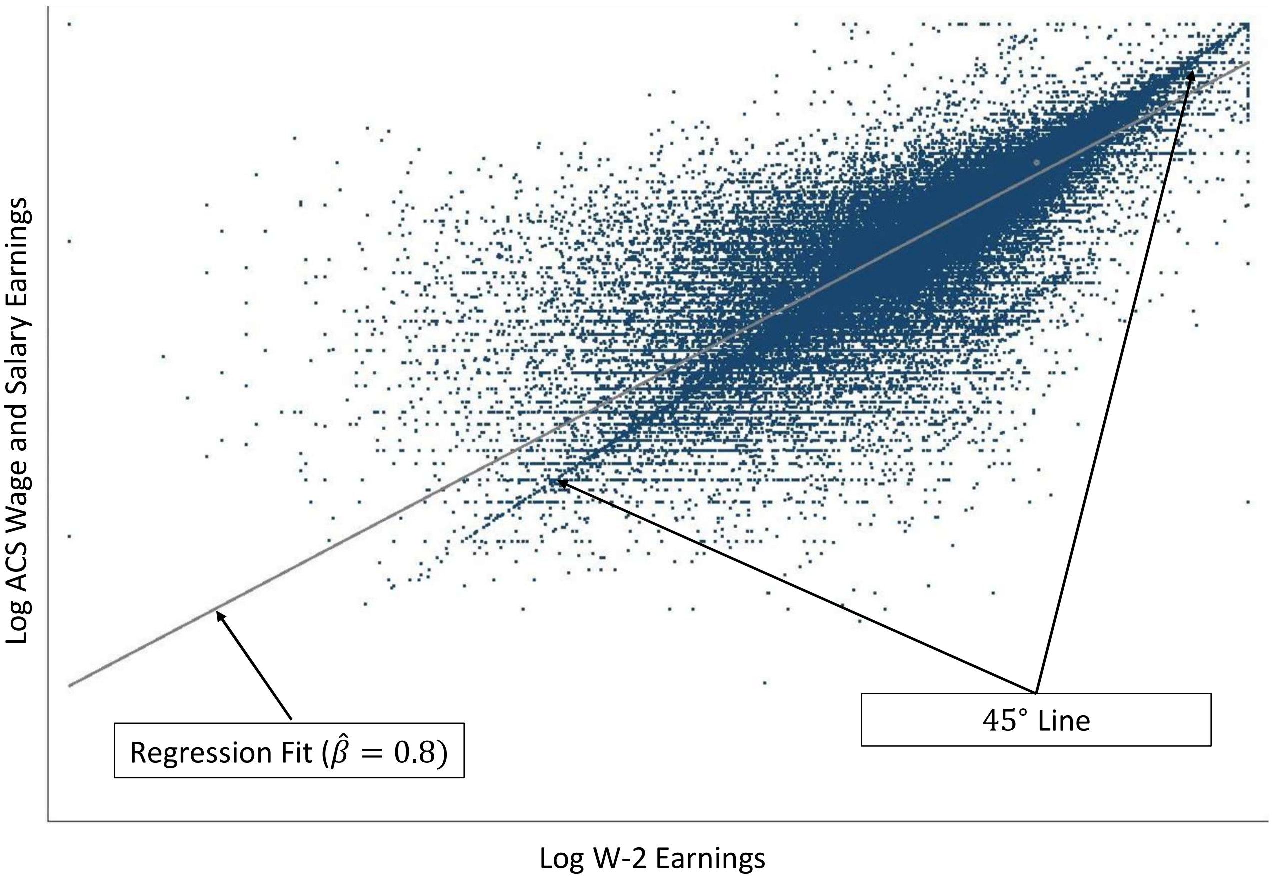
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 5: Comparing Survey Characteristics with Different Weights



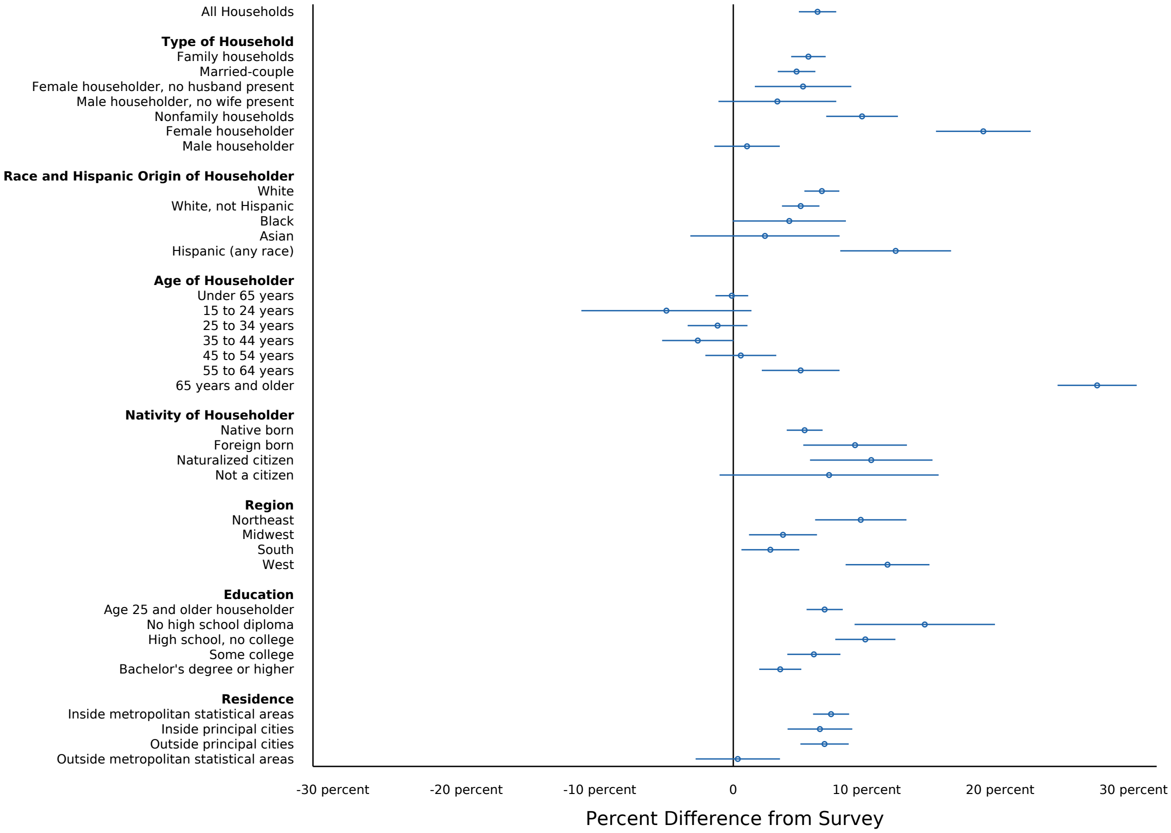
*Notes:* This figure shows various statistics of survey demographics and survey-reported income using the entropy balance weights (discussed in Sections 5.1 and Appendix B) relative to the survey-weighted estimates. "EBW" are the Stage 2 weights that further adjust to population controls and "EBW + PIKed" are the Stage 3 weights that further adjust for selection into linkage.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 6: Intensive Margin Disagreement in Wage and Salary Earnings



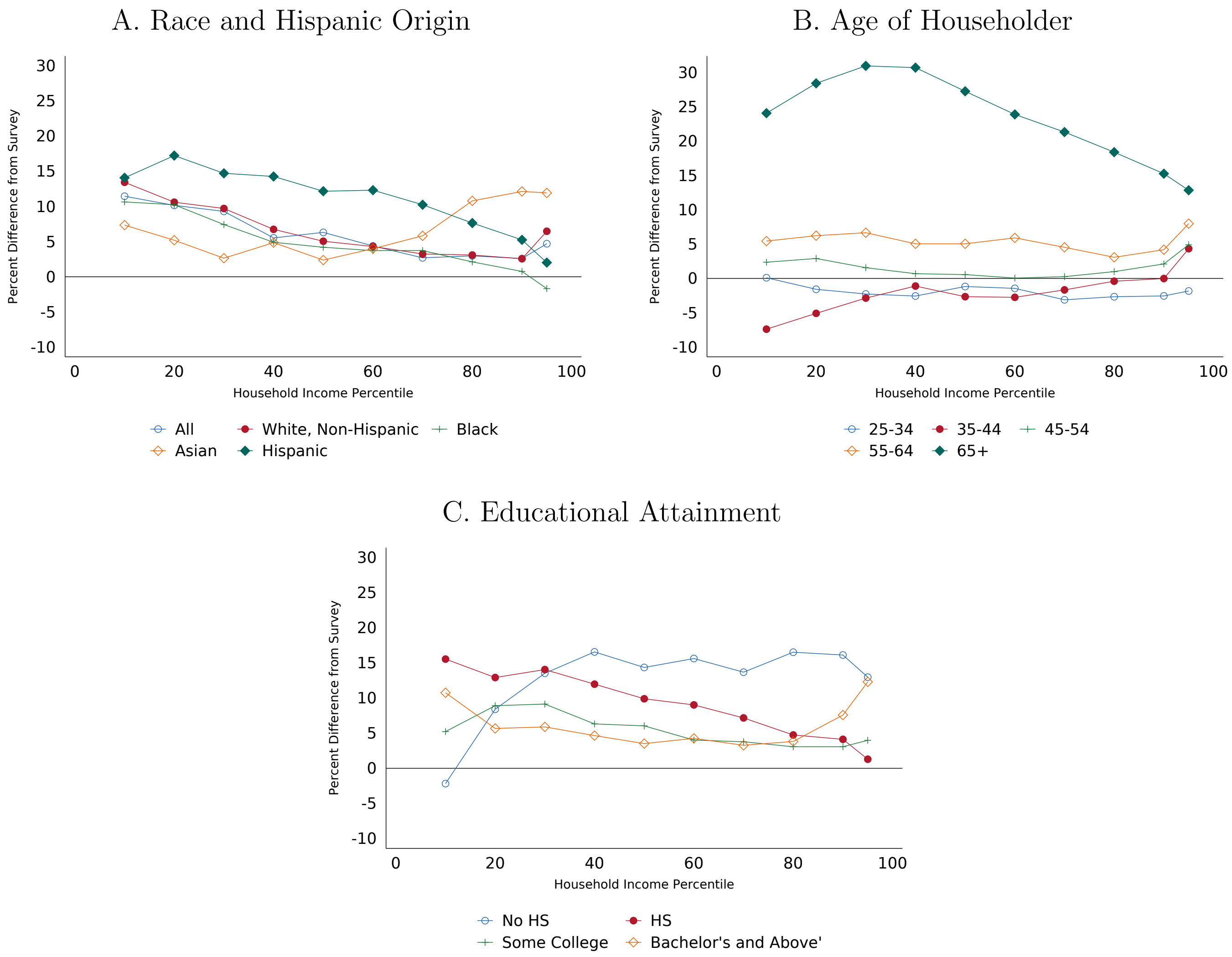
*Notes:* This figure was published in O'Hara, Bee and Mitchell (2017) and is replicated here with permission, as it is no longer possible to disclose scatter plots of individual earnings reports. The figure compares individual survey wage and salary earnings reports to W-2 earnings from the 2011 ACS. The regression fit line is shown and the 45° is visible in the clustering of points below the regression line on the left side of the figure and above the regression fit on the right. While the survey reports cluster around the 45° line, there is considerable noise in the survey relative to the administrative reports, and the figure is consistent with mean-reversion of survey relative to administrative reports (both in the location of points relative to the diagonal and the fact that $\hat{\beta} < 1$). The axes are unlabeled as a condition of the original release. *Source:* O'Hara, Bee and Mitchell (2017) using 2011 American Community Survey data linked to 2010 W-2s.

Figure 7: NEWS Estimate of Median Household Income Relative to Survey in 2018



*Notes:* This figure shows the percent difference between the NEWS estimates of median household income compared to the survey estimates in 2018.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 8: NEWS Estimate of Household Income Relative to Survey by Subgroup in 2018

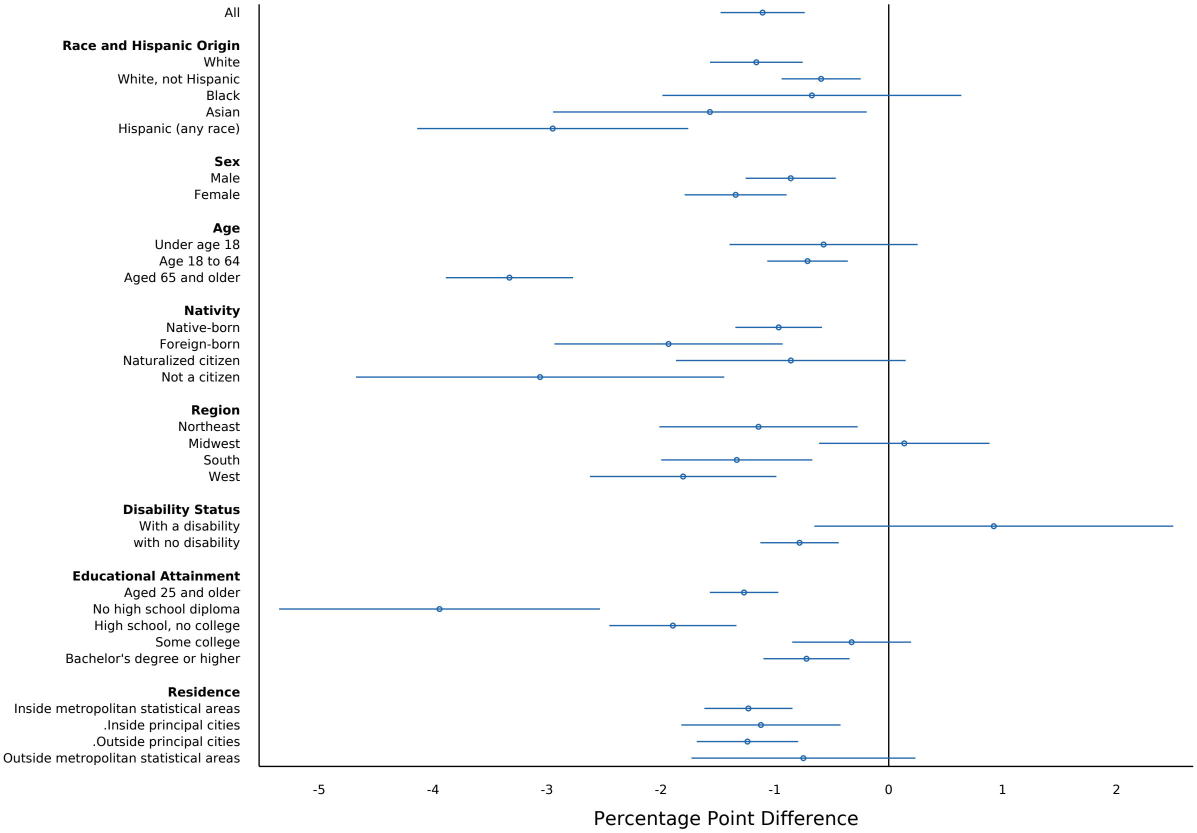

A. Race and Hispanic Origin

B. Age of Householder

C. Educational Attainment

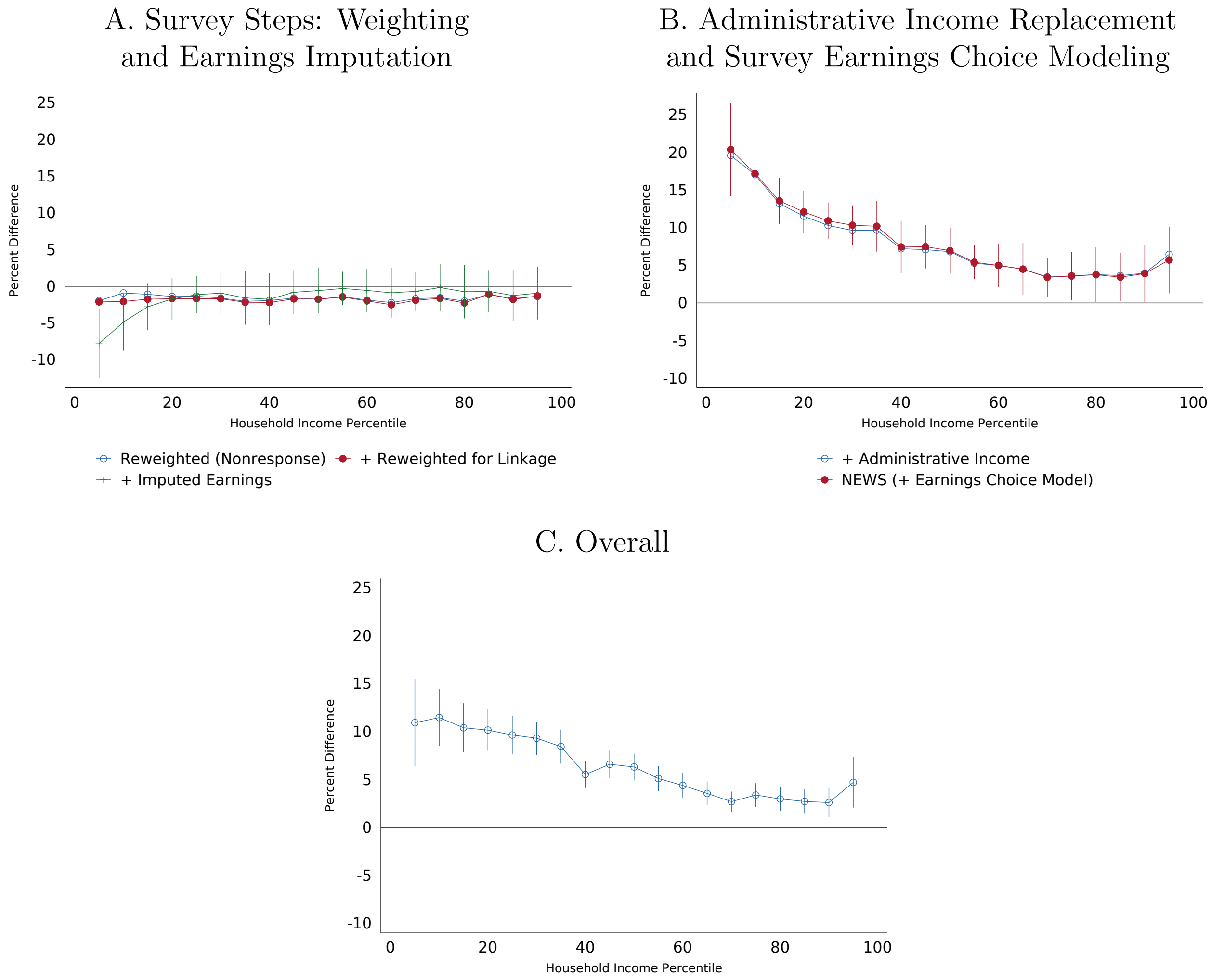
*Notes:* This figure shows the percent difference between the NEWS estimates of household income compared to the survey estimate at the 10th, 25th, 50th, 75th, and 90th percentiles in 2018.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 9: NEWS Estimate of Poverty Relative to Survey in 2018



*Notes:* This figure shows the percentage point difference between the NEWS estimates of poverty compared to the survey estimate in 2018.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 10: Decomposition of NEWS Processing Steps: Household Income


A. Survey Steps: Weighting and Earnings Imputation

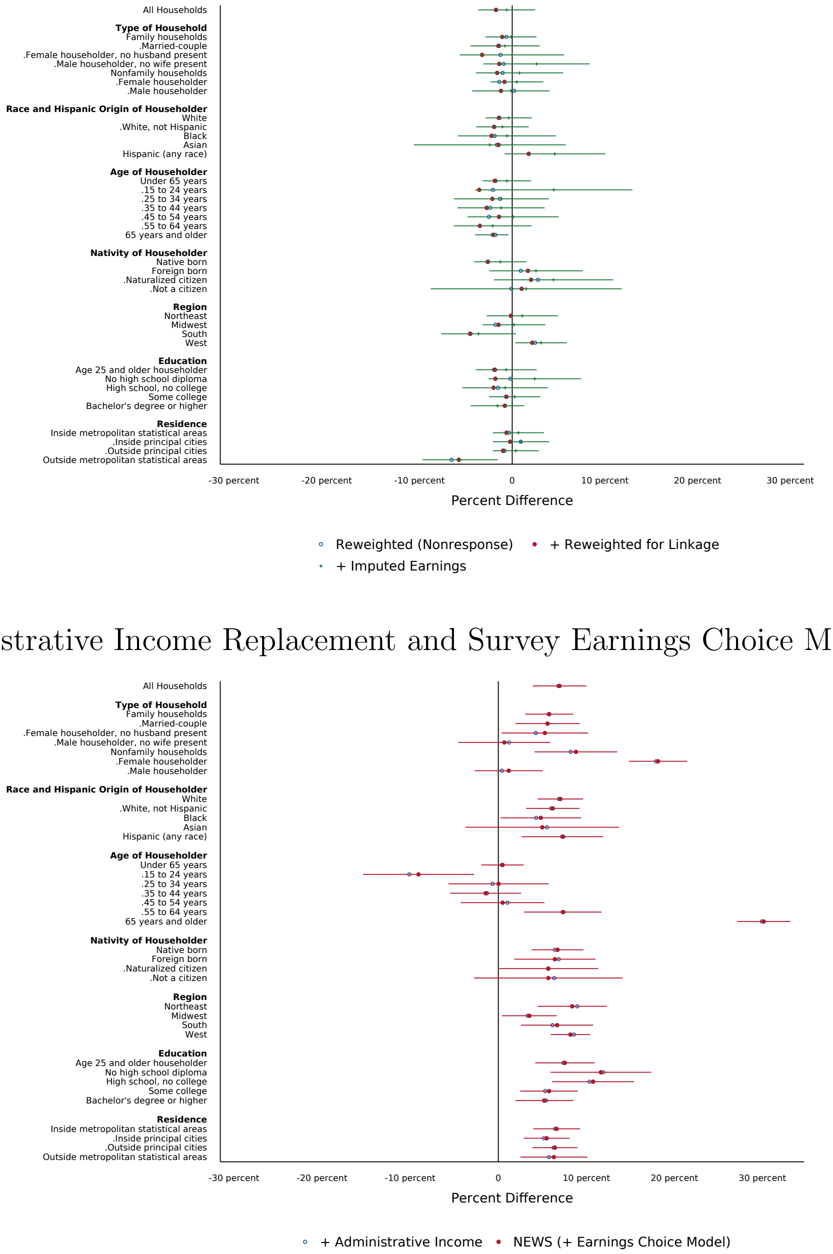
B. Administrative Income Replacement and Survey Earnings Choice Modeling

C. Overall

*Notes:* This figure decomposes the impact of the NEWS processing steps on household income. In Panel A, the figure shows the adjustments made to the survey data, including reweighting and improved earnings imputation comparing household income after the adjustment to the survey estimate. In Panel B, the figure shows impact of replacing survey income responses with administrative income, comparing the estimates after each step to the estimates after reweighting and earnings imputation. The full impact of all adjustments is shown in Panel C. The 95 percent confidence interval for the last step is shown in each: for Panel A comparing the estimate after earnings imputation to the survey estimate and for Panel B comparing the final NEWS estimate to the estimate after earnings imputation.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Figure 11: Decomposition of NEWS Processing Steps By Subgroup: Median Household Income



A. Survey Steps: Weighting and Earnings Imputation

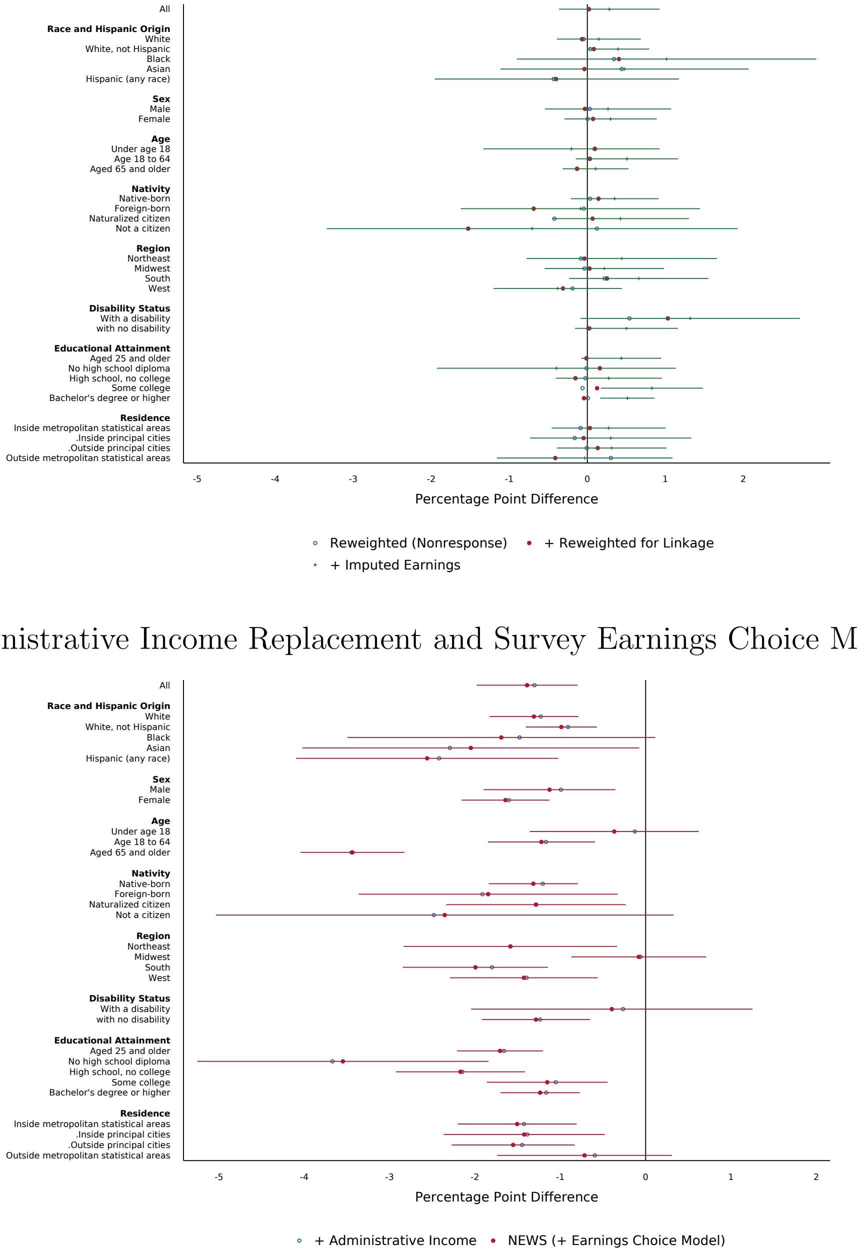
B. Administrative Income Replacement and Survey Earnings Choice Modeling

*Notes:* This figure decomposes the impact of the NEWS processing steps on median household income. In Panel A, the figure shows the adjustments made to the survey data, including reweighting and improved earnings imputation comparing median household income for each group after the adjustment to the survey estimate. In Panel B, the figure shows impact of replacing survey income responses with administrative income, comparing the estimates after each step to the estimates after reweighting and earnings imputation. The 95 percent confidence interval for the last step is shown in each: for Panel A comparing the estimate after earnings imputation to the survey estimate and for Panel B comparing the final NEWS estimate to the estimate after earnings imputation.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 12: Decomposition of NEWS Processing Steps By Subgroup: Poverty

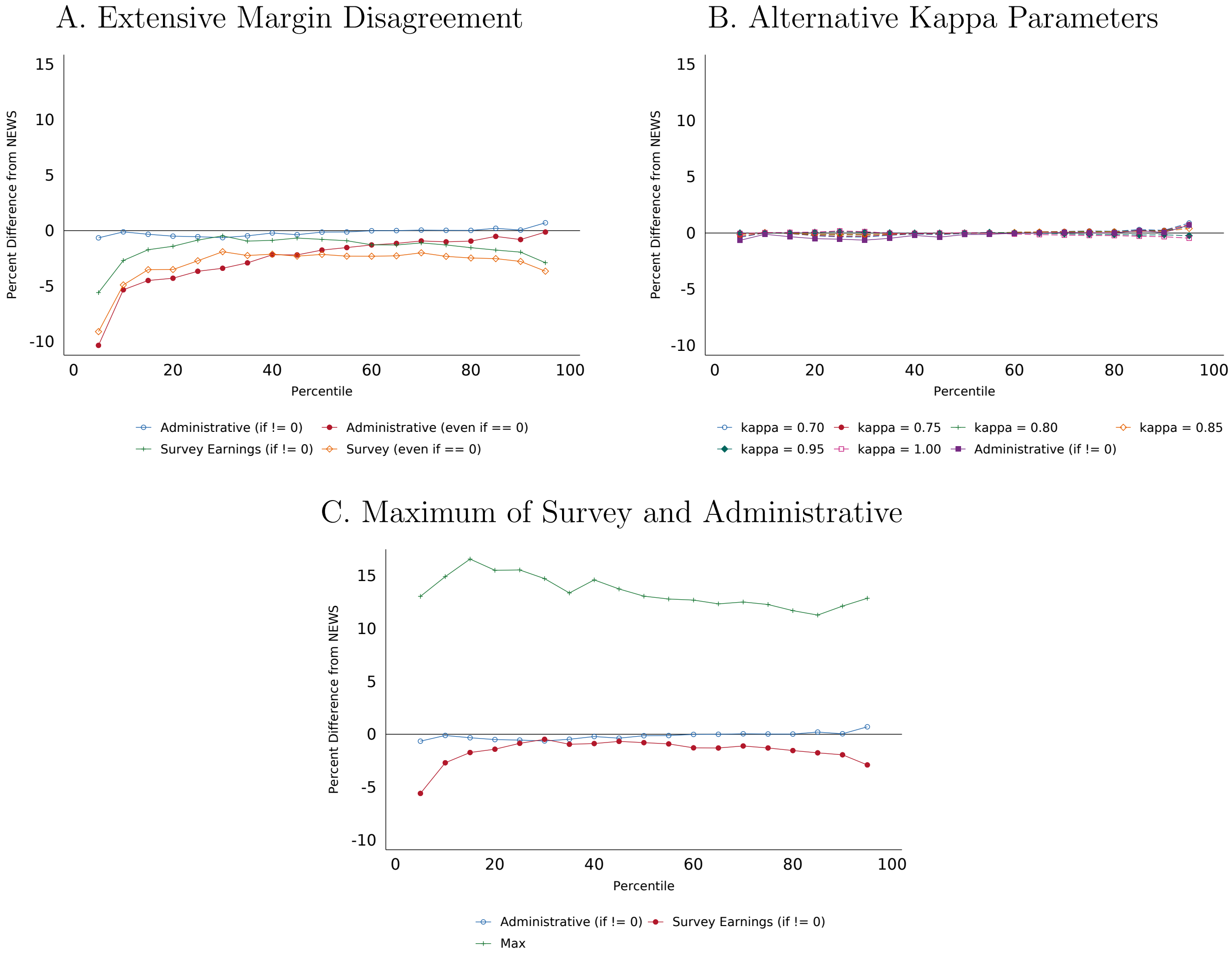
A. Survey Steps: Weighting and Earnings Imputation



B. Administrative Income Replacement and Survey Earnings Choice Modeling

*Notes:* This figure decomposes the impact of the NEWS processing steps on poverty. In Panel A, the figure shows the adjustments made to the survey data, including reweighting and improved earnings imputation comparing poverty for each group after the adjustment to the survey estimate. In Panel B, the figure shows impact of replacing survey income responses with administrative income, comparing the estimates after each step to the estimates after reweighting and earnings imputation. The 95 percent confidence interval for the last step is shown in each: for Panel A comparing the estimate after earnings imputation to the survey estimate and for Panel B comparing the final NEWS estimate to the estimate after earnings imputation.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 13: Alternative Uses of Survey and Administrative Earnings

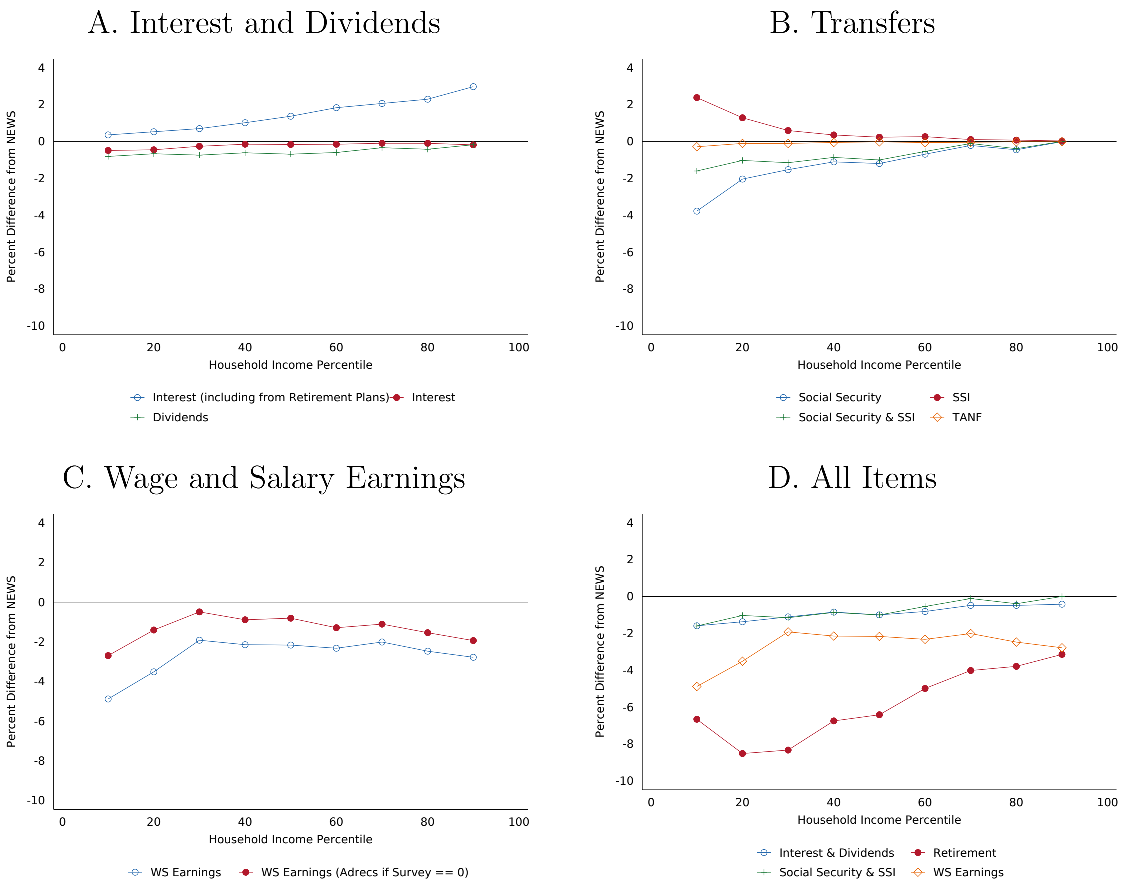

A. Extensive Margin Disagreement

B. Alternative Kappa Parameters

C. Maximum of Survey and Administrative

*Notes:* This figure shows the impact on household income (relative to the baseline NEWS estimates) of alternative uses of survey and administrative earnings in the income estimates. In Panel A, we show how income estimates vary when survey or administrative wage and salary earnings were used for individuals indicated as "Measurement error model" in Table 13. The four options in Panel A include: 1) Administrative earnings if they are not equal to 0, 2) administrative earnings even if they are equal to 0 and survey earnings are positive, 3) survey earnings if they are not equal to 0, and 4) survey earnings even if they are equal to zero and administrative earnings are positive. Panel B shows the impact on household earnings of alternative mean-reversion kappa parameters in the measurement error model (with the share of individual's whose survey earnings are used under each shown in Table 14). Panel B also includes 1) from Panel A, with administrative earnings if they are not equal to 0. Panel C compares the NEWS estimates to simpler uses of survey and administrative earnings, including 1) and 3) from Panel A and using the maximum of administrative and survey earnings.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

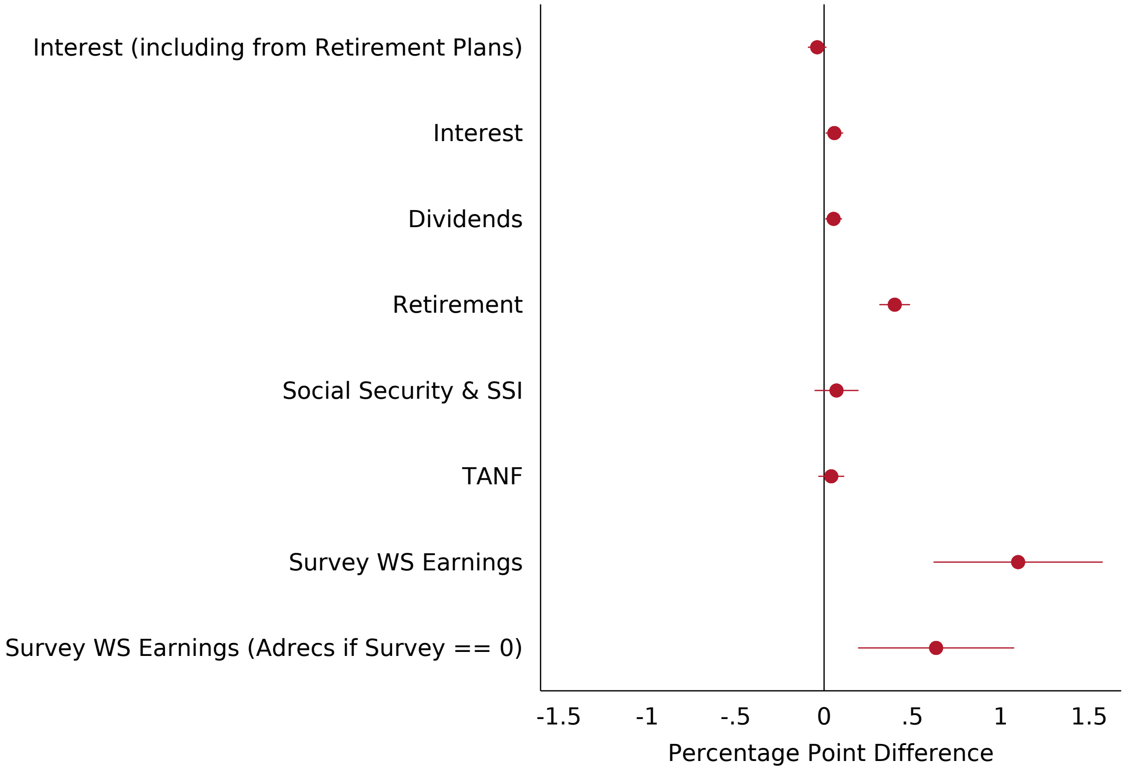
Figure 14: Effect of Removing Individual Administrative Income Items on Household Income



*Notes:* In this figure, we replace individual income items from the NEWS estimates with the corresponding survey information and compare the estimate after replacement with the NEWS estimate. An estimate below the zero line indicates that administrative item increases income at that percentile. In Panel A, we replace interest and dividend income with survey responses. For survey interest, we show two measures, including and excluding the survey-reported interest earned in Defined Contribution retirement plans such as 401(k)s. In Panel B, we replace Social Security and SSI separately and together (to address misclassification across programs, as discussed in Bee and Mitchell (2017)) and TANF with survey-reported public assistance income. In Panel C, we replace administrative wage and salary earnings with two survey-based earnings measures. In the first, we use survey responses in all cases where the individual does not have administrative self-employment earnings, even if the individual reported no earnings on the survey. In the second, we only replace administrative wage and salary earnings if the survey report was positive. In Panel D, we show each of the major administrative income items, including interest and dividends, Defined Contribution plan withdrawals, pensions, and survivor and disability pensions (Retirement), Social Security and SSI, and wage and salary earnings.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure 15: Effect of Removing Individual Administrative Income Items on Poverty



*Notes:* In this figure, we replace individual income items from the NEWS estimates with the corresponding survey information, including for interest, dividends, retirement income, Social Security, SSI, TANF, and survey wage and salary earnings. An estimate above the zero line indicates that administrative item decreases overall poverty. For survey interest, we show two measures, including and excluding the interest earned in Defined Contribution retirement plans such as 401(k)s. We replace Social Security and SSI together to address misclassification across programs, as discussed in Bee and Mitchell (2017). We replace administrative wage and salary earnings with two survey-based earnings measures. In the first, we use survey responses in all cases where the individual does not have administrative self-employment earnings, even if the individual reported no earnings on the survey. In the second, we only replace administrative wage and salary earnings if the survey report was positive. Retirement includes Defined Contribution plan withdrawals, pensions, and survivor and disability pensions.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Table 1: Processing and Estimation Steps

| Section | Step | Inputs | Outputs | Notes |
|---|---|---|---|---|
| A. Extracts | 1. Process firm and universe job-level data | LBD | Crosswalk between EINs and LBD firm identifiers (LBDFID) | Some firms have multiple EINs and firm reorganization can change result in within-year changes to EINS. This step maps each EIN to a single firm identifier. For multi-EIN firms, that is the parent firm. For reorganized firms, it is the LBDFID associated with the firm in the subsequent year. |
| | | W-2, DER, LEHD | Crosswalk between jobs in each file | Match LEHD and W-2 jobs where the jobs are not filed under the same EIN in the two data sets. |
| | | LBD, W-2, LEHD | Firm-level summaries | Firm-level summaries of employment and payroll. From the LBD, firm summary of industry codes. |
| | 2. Clean and process SNAP and TANF files | Individual state SNAP and TANF files | Cleaned and standardized versions | |
| | 3. Create extracts of each data set for the address file | Survey housing unit file (with MAFIDs) | Geographic summary extracts at the state, county, and census tract level from decennial census, ACS 5-year files, W-2s, Numident (with MAFID-PIK linked through the MAFARF), and 1040 tax filing | Variables created such as share of the population by race and Hispanic origin, educational attainment, citizenship status, income statistics, poverty, etc. |
| | | | Housing unit information from the MAF and Black Knight data | Information on structure type and home value |
| | | | Roster of individuals (PIKs) observed at these addresses | From the MAFARF, 1040s, and IRMF files |
| | | | For those individuals, extracts from the PHUS, SSR, Numident, W-2s, 1040s, IRMF, and LEHD | |
| | 4. Create extracts of each data set for the person file | PIKed survey respondents | For PIKed respondents, extracts from the PHUS, SSR, DER, Numident, W-2s, 1040s, IRMF, LEHD, SNAP, and TANF files | |
| B. Weighting | 1. Combine the address-file extracts into a merged file | Individual output files from A.3. | Weighting extract | |
| | 2. Run the weighting model | File from B.1. and the person-level respondent file from A.4. | Weights and replicate weights (for standard error estimation) | The final weighted file includes households were all those asked income questions (ages 15+) are PIKed, with the weights adjusted to reflect selection into response and selection into PIK assignment. |
| C. Imputation | 1. Combine the person-file extracts into a merged file | Individual output files from A.4. | Person extract | |
| | 2. Impute missing earnings information | File from C.1. | Independent implicates of imputed earnings information | Impute earnings for 1) survey nonresponse of earnings variables and 2) missing gross earnings (LEHD-equivlent earnings) when missing in administrative data (only taxable earnings + deferred compensation is available) |
| | 3. Impute means-tested program information | Files from C.1. and C.2. | Independent implicates of imputed program participation | Impute SNAP and TANF participation given limited geographic availability of the program data. |
| | 4. Impute non-filer income items available in more detailed administrative data | File from C.1 and parameters estimates on more detailed administrative data. | Independent implicates of imputed unemployment insurance compensation, dividends, and interest income. | |
| D. Estimation | 1. Combine weighting and imputation files and create income estimate file | Files from C.1, C.2, C.3, and C.4. | Income estimates file for each implicate from the imputation process | Income estimates created for various procedures for combining survey and administrative earnings information |
| | 2. Create income and poverty estimates | Files from B.1., C.1., and D.1. | Income and poverty estimates | |

*Notes:* This table describes the steps used in creating the NEWS extracts and income and poverty estimates. In A., we create the address extract (also shown in Figure 1) and the person extract (also shown in Figure 2) by linking survey, decennial census, administrative, and third-party data. In B., we use the address extract to create weights that adjust for unit nonresponse bias and selection into linkage to administrative records, discussed in Section 5.1. In C., we impute missing information in the person extract to address survey item nonresponse and missing administrative data — including gross administrative earnings, means-tested program data, and nonfiler unemployment compensation, interest, and dividends, discussed in Section 5.2. In D., we combine the files from A-C and implement our earnings measurement error model to combine survey and administrative earnings, discussed in Section 6.0.1. We then estimate income and poverty statistics.

## Table 2: Data Sources

| **File** | **Data Source** | **Description** |
|---|---|---|
| Current Population Survey Annual Social and Economic Supplement (CPS ASEC) | Census | Annual survey fielded in February to April with household structure and characteristics at the time of interview and income from the prior calendar year. About 95,000 housing units sampled each year. |
| American Community Survey (ACS) | Census | Rolling survey fielded throughout the year about income from prior 12 months. About 3.5 million housing units sample each year. |
| Short Form Decennial Census | Census | Complete count decennial census data from 2000 and 2010. |
| Master Address File (MAF) | Census | File of residential addresses used to support census survey and decennial operations. Survey samples are drawn from this file for both the CPS ASEC and ACS. |
| Master Address File Auxiliary Reference File (MAFARF) | Census | Comingled file constructed from administrative records, including the IRMF, postal service change of address information, program data, etc. that links individuals (identified by Protected Identification Keys) to addresses in the Master Address File (identified by MAFIDs). |
| Longitudinal Business Database (LBD) | Census | Database of private non-farm establishments with employees from 1976 forward. For each establishment the LBD has information on industry, payroll, employment, and a firm identifier to group establishments into firms. |
| Information Returns Master File (IRMF) | IRS | Universe file with flags for whether an individual received each of the following information returns forms: 1098, 1099-DIV, 1099-INT, 1099-G, 1099-MISC, 1099-R, 1099-S, SSA-1099, and W-2. No income information is available. Also contains address information which has matched to the MAF to get a MAFID for each form. |
| Form 1040 Tax Returns (1040s) | IRS | Universe tax filings with a subset of the information on the complete Form 1040. The extracts provided by the IRS include information on tax-unit wage and salary income, gross rental income, taxable social security income, taxable and tax-exempt interest income, interest income, dividends, Adjusted Gross Income, and a constructed measure of Total Money Income (TMI). TMI is the sum of taxable wage and salary income, interest (taxable and tax-exempt), dividends, gross social security income, unemployment compensation, alimony received, business income or losses (including for partnerships and S-corps), farm income or losses, and net rent, royalty, and estate and trust income. Self-employment income is not available (except as a component of TMI), but flags exist for the filing of different 1040 schedules (such as C, D, E, F, SE). |
| Form W-2 (W-2s) | IRS | Universe data with a subset of information from the Form W-2. The extracts provided by the IRS include select boxes from the form, including wages and salary net of pre-tax deductions for health insurance premiums and deferred compensation (boxes 1 and 5), as well as the total amount of deferred compensation (summed values from Box 12 Codes D-H). Employee and employer pre-tax contributions to health insurance premiums are not available in the W-2 data. |
| Form 1099-R (1099-Rs) | IRS | Universe data with a subset of information from the Form 1099-R. The extracts provided by the IRS include information on amounts of defined-benefit pension payments (including for survivor and disability pensions) and withdrawals from defined-contribution retirement plans. |
| Numerical Identification System (Numident) | SSA | The Numident contains information for anyone ever to have received a Social Security Number. It includes information on date and place of birth, date of death, sex, and some information on citizenship. |
| Payment History Update System (PHUS) | SSA | Monthly Old Age, Survivors, and Disability Insurance (OASDI) payments from 1984 to the present. The PHUS exists for several subsamples of individuals including 1) those receiving payments in 2020 and 2021, 2) CPS ASEC respondents in linked years, and 3) ACS respondents in linked years (currently only 2019). |
| Supplemental Security Record (SSR) | SSA | Monthly Supplemental Security Income (SSI) payments from 1984 to the present for federally SSI and federally administered state SSI. The SSR exists for several subsamples of individuals including 1) those receiving payments in 2020 and 2021, 2) CPS ASEC respondents in linked years, and 3) ACS respondents in linked years (currently only 2019). |
| Detailed Earnings Record (DER) | SSA | Annual job-level income (by Employer Identification Number, EIN) from Form W-2s and annual positive self-employment income (from Form 1040 Schedule SE). The DER exists for several subsamples: 1) CPS ASEC respondents in linked years and 2) ACS respondents in linked years (currently only 2019) |
| Longitudinal Employer Household Dynamics (LEHD) | States | Quarterly job earnings reports from firms to state Unemployment Insurance offices for participating states. For covered jobs, the LEHD includes gross earnings - this includes employee contributions for health insurance premiums not available on the W-2 extracts. Coverage in the LEHD is not complete as many government employees, such as federal civilian employees, postal workers, and Department of Defense employees are not covered by state UI benefits. Some private-sector employees, including those employed by religious organizations, are not covered by UI, and are therefore not present in the LEHD data. |
| Supplemental Nutrition Assistance Program | States | SNAP participant data from partner states. In 2018, SNAP data is available for 17 states. |
| Temporary Assistance for Needy Families (TANF) | States + HHS | TANF participant data from partner states as well as from the Department of Health and Human Services (HHS) for additional states. In 2018, TANF data is available for 36 states. |
| Black Knight Home Value (Black Knight) | Black Knight | Third party data on home values and housing unit characteristics. |

*Notes:* This table describes the data used in this project, including the source of the data and a short description. The name for the data used in Figures 1 and 2 is in parenthesis.

# Table 3: Direct and Indirect Job Linkage Statistics

| | | EIN Matches Only | | EIN and Indirect Matches | |
|---|---|---|---|---|---|
| | All Jobs | Unmatched Jobs | Share of Implied Total | Unmatched Jobs | Share of Implied Total |
| Total Jobs | | | | | |
| W-2 | 256,800,000 | 40,720,000 | 0.146 | 25,680,000 | 0.097 |
| LEHD | 237,900,000 | 21,780,000 | 0.078 | 6,744,000 | 0.026 |
| EIN Matches | 216,100,000 | | 0.776 | | 0.820 |
| Indirect Matches | 15,040,000 | | | | 0.057 |
| Implied Total Jobs | | 278,600,000 | | 263,600,000 | |

*Notes:* This table shows the count of jobs that could be directly linked by Employer Identification Number (EIN) and indirectly linked as discussed in Section 3.3.
*Source:* 2018 W-2 and Longitudinal Employer-Household Dynamics data.

Table 4: Comparing Job-Level LEHD and W-2 Earnings

| | | Health Insurance | | |
|---|---|---|---|---|
| LEHD-W-2 Comparison | All | Yes | No | Yes - No |
| LEHD < W-2 | 8.7 | 9.7 | 3.9 | 5.85*** |
| | (0.2) | (0.2) | (0.3) | (0.30) |
| LEHD ≥ W-2 | | | | |
| 0-1 percent greater | 66.9 | 61.8 | 89.3 | -27.52*** |
| | (0.3) | (0.3) | (0.4) | (0.54) |
| 1-3 percent greater | 6.4 | 7.5 | 2.0 | 5.51*** |
| | (0.1) | (0.2) | (0.2) | (0.26) |
| 3-5 percent greater | 4.9 | 5.8 | 1.3 | 4.50*** |
| | (0.1) | (0.2) | (0.1) | (0.20) |
| 5-10 percent greater | 6.8 | 8.0 | 1.6 | 6.32*** |
| | (0.1) | (0.2) | (0.2) | (0.24) |
| 10+ percent greater | 6.3 | 7.3 | 2.0 | 5.34*** |
| | (0.1) | (0.2) | (0.2) | (0.25) |
| Observations | 47,000 | 39,000 | 8,100 | |

*Notes:* This table shows basic summary statistics on job-level comparisons of LEHD earnings to W-2 earnings (including deferred compensation) for the highest earning job. Jobs are classified by the ratio of LEHD to W-2 earnings. The first category, W-2 > LEHD, indicates that W-2 earnings exceed LEHD earnings by more than a trivial amount ($100). The other categories indicate that LEHD gross earnings exceeded W-2 earnings + deferred compensation by specific percent ranges. Because LEHD gross earnings should exceed W-2 taxable earnings + deferred compensation primarily due to employee pre-tax contributions to health insurance premiums, the sample in this table includes only individuals that responded to the health insurance question in the CPS ASEC, i.e., whose health insurance status was not imputed. The first column shows the share in each LEHD-W-2 bin for all workers with a job in both data sources. The next two columns show estimates for those that reported having and not having private health insurance, respectively. The last column shows the difference between the share in each bin between those having and not having private health insurance. Standard errors in parenthesis. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for differences.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Table 5: Weighted Linkage Rates by Administrative Data Source in the Address Data

| | Target Estimate | Difference from Target | | | |
|---|---|---|---|---|---|
| | Base-Weighted | Base-Weighted | Survey Weighted | EBW-Weighted | |
| | Occupied Units | Respondent Units | Respondent Units | Respondent Units | Respondent + All Adults PIKed Units |
| Any Linkage | 0.932*** | 0.0037*** | 0.0047*** | -0.0006 | -0.0012*** |
| | (0.002) | (0.0006) | (0.0010) | (0.0006) | (0.0005) |
| SSA Data | | | | | |
| PHUS | 0.402*** | 0.0584*** | 0.0427*** | Z | 0.0001 |
| | (0.002) | (0.0010) | (0.0019) | (0.0024) | (0.0043) |
| SSR | 0.050*** | 0.0050*** | 0.0003 | Z | Z |
| | (0.001) | (0.0004) | (0.0007) | (0.0010) | (0.0015) |
| Numident | 0.921*** | 0.0046*** | 0.0058*** | Z | Z |
| | (0.002) | (0.0006) | (0.0012) | (0.0007) | (0.0004) |
| IRS Data | | | | | |
| IRMF | 0.837*** | 0.0085*** | 0.0067*** | -0.0005 | -0.0018 |
| | (0.002) | (0.0008) | (0.0014) | (0.0013) | (0.0014) |
| 1099-R | 0.436*** | 0.0127*** | 0.0070*** | Z | Z |
| | (0.002) | (0.0010) | (0.0018) | (0.0006) | (0.0019) |
| Any 1040 | 0.856*** | 0.0018*** | 0.0055*** | Z | 0.0001 |
| | (0.002) | (0.0007) | (0.0013) | (0.0009) | (0.0006) |
| 1040 (2018) | 0.828*** | 0.0027*** | 0.0068*** | Z | 0.0001 |
| | (0.002) | (0.0008) | (0.0014) | (0.0005) | (0.0008) |
| 1040 (2019) | 0.835*** | 0.0021*** | 0.0055*** | Z | 0.0001 |
| | (0.002) | (0.0008) | (0.0014) | (0.0009) | (0.0007) |
| W-2 or LEHD | 0.751*** | -0.0060*** | 0.0037** | Z | 0.0001 |
| | (0.002) | (0.0008) | (0.0017) | (0.0010) | (0.0010) |
| Census Bureau Data | | | | | |
| Decennial | 0.867*** | 0.0084*** | 0.0083*** | Z | 0.0001 |
| | (0.002) | (0.0008) | (0.0013) | (0.0013) | (0.0015) |
| MAFARF | 0.822*** | 0.0092*** | 0.0065*** | Z | -0.0014 |
| | (0.002) | (0.0009) | (0.0014) | (0.0022) | (0.0031) |
| 3rd Party Data | | | | | |
| Black Knight | 0.644*** | 0.0119*** | 0.0071*** | Z | Z |
| | (0.003) | (0.0011) | (0.0020) | (0.0019) | (0.0034) |

*Notes:* This table shows statistics on selection into response at the household level by data source that can be linked to occupied housing units, as discussed in Section 4.1. The target estimate is calculated on the base-weighted set of all occupied housing units in the March monthly CPS. The other estimates show differences from the target (evidence of selection into the sample unaddressed by weighting if $\neq 0$) for the indicated samples of respondents and weights. Standard errors in parenthesis. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for differences. Z indicates an estimate rounds to zero.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Table 6: Income Type by Source for Filers and Nonfilers

| | **Source** | | |
|---|---|---|---|
| **Income Type** | **Filers** | **Nonfilers** | **Notes** |
| Wage and Salary Earnings | W-2<br>DER<br>LEHD<br>1040 | W-2<br>DER<br>LEHD | Administrative data may miss unreported ”under-the-table” earnings. Current W-2s and DER do not include pre-tax employee contributions to health insurance premiums. LEHD does not have complete coverage. Survey has potential for misreporting and underreporting. |
| Self-Employment Earnings | 1040<br>DER | Survey only | Under-reported substantially on surveys and in administrative records. Considerable disagreement between extensive margin reporting on surveys and administrative data (Abraham et al., 2021). |
| Social Security | 1040<br>PHUS | PHUS | |
| Supplemental Security | SSR | SSR | |
| Unemployment Insurance | 1040 | Survey only | Included in 1040 Total Money Income. Imputed for nonfilers using disclosed results from more detailed 1099-G data. |
| Worker’s Compensation | Survey only | Survey only | Not available federal administrative data. |
| Public Assistance | TANF | TANF | Current data only covers some states. TANF data does not cover all possible cash assistance programs. |
| Veteran’s Benefits | Survey only | Survey only | Potential for VA data use in the future |
| Disability, Survivor, and Retirement Income | 1099-R | 1099-R | |
| Interest | 1040 | Survey only | Imputed for nonfilers using disclosed results from more detailed 1099-INT data. |
| Dividends | 1040 | Survey only | Imputed for nonfilers using disclosed results from more detailed 1099-DIV data. |
| Rent and Royalty Income | 1040 | Survey only | Net rent and royalty income included in 1040 Total Money Income. Gross rent and royalty income available as a separate variable. |
| Educational Assistance | Survey only | Survey only | |
| Financial Assistance | Survey only | Survey only | |
| Alimony | 1040 | Survey only | Included in 1040 Total Money Income |
| Gambling Winnings | 1040 | Survey only | Included in 1040 Total Money Income. Potentially available on survey as ”other income.” |

*Notes:* This table describes the available data sources for the various types of income, including notes about the limitations of various sources. The availability of income varies between filers and nonfilers, with more income sources available in the currently available administrative records for filers.

Table 7: Linkage Rates by Administrative Data Source in the Person Data

| | Full Sample | | NEWS Sample (All Survey-Adults in HH Assigned PIK) | |
|---|---|---|---|---|
| | Survey-Adults (15+) | Survey-Children (<15) | Survey-Adults | Survey-Children |
| Assigned PIK | 85.8 | 79.4 | 100.0 | 89.4 |
| | (0.18) | (0.33) | | (0.30) |
| Any Adrec Linked to Address | | | | |
| If Assigned PIK | 94.7 | 95.6 | 93.9 | 95.0 |
| | (0.15) | (0.22) | (0.16) | (0.26) |
| If Not Assigned PIK | 89.9 | 92.6 | | 92.3 |
| | (0.40) | (0.48) | | (0.88) |
| Present In \| Assigned PIK | | | | |
| Any Administrative Record | 98.1 | 85.2 | 98.0 | 87.4 |
| | (0.05) | (0.30) | (0.07) | (0.33) |
| IRS Data | | | | |
| Tax Filing (1040) | 84.6 | 83.2 | 84.4 | 85.6 |
| | (0.17) | (0.30) | (0.19) | (0.34) |
| IRMF | 89.4 | 7.8 | 88.2 | 7.5 |
| | (0.10) | (0.22) | (0.12) | (0.24) |
| W-2 | 64.3 | 1.0 | 63.9 | 1.0 |
| | (0.16) | (0.07) | (0.17) | (0.08) |
| 1099-R | 21.1 | 0.1 | 20.1 | Z |
| | (0.14) | (0.02) | (0.13) | (0.02) |
| SSA Data | | | | |
| DER | 67.6 | 0.3 | 67.2 | 0.3 |
| | (0.16) | (0.04) | (0.17) | (0.05) |
| PHUS | 37.8 | 3.9 | 35.2 | 3.5 |
| | (0.16) | (0.16) | (0.16) | (0.16) |
| SSR | 3.6 | 1.3 | 3.4 | 1.2 |
| | (0.09) | (0.10) | (0.09) | (0.10) |
| State Data | | | | |
| LEHD | 64.3 | 1.0 | 63.9 | 1.0 |
| | (0.16) | (0.07) | (0.17) | (0.08) |

*Notes:* This table shows statistics on the individuals that can be assigned a PIK as well as the households in which those 15 and over (survey-adults) can be assigned a PIK. For all households and the 82 percent of households with all survey-adults assigned a PIK (the NEWS analysis sample), we show the share of survey-adults and survey-children that can be linked to various data sets. Estimates and standard errors that are 0 by construction are omitted. Z indicates an estimate rounds to zero. Standard errors in parenthesis.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Table 8: Entropy Balance Reweighting Procedure

| **Stage/Step** | **Moment Variables** | **Moment Sample** | **Reweighted Sample** |
|---|---|---|---|
| **1. Housing-unit level** | Linked survey, administrative, and census variables | Non-vacant housing units in March Basic CPS (respondents and nonrespondents) | Respondent housing units |
| **2. Person level** | | | |
| A. Preserve distribution of housing unit characteristics | Linked survey, administrative, and census variables | Householders and householder-partners, using the housing-unit level weights from Stage 1 | Householders and householder partners |
| B. Spousal equivalence | Linked survey, administrative, and census variables | Married couples and cohabiting partners | Married couples and cohabiting partners |
| C. External population targets | State-level population estimates by race, Hispanic-origin, gender, and age | External population estimates | All individuals |
| D. Match distribution of household characteristics in March Basic Sample | Subset of linked survey, administrative, and census variables and state-level population controls | Householders and householder partners in the March Basic File | Householders and householder partners in the full CPS ASEC sample |
| **3. Address Selection into PIK assignment (for all adults in HH)** | | | |
| A. Preserve distribution of respondent and housing unit characteristics | Linked survey, administrative, and census variables.<br>Additional moments for survey-only and linked survey-administrative characteristics from full respondent sample | Respondent sample with weights from step 2. | Households where all individuals asked income questions (age 15+) are linked to a PIK. |
| B. External population targets | State-level population estimates by race, Hispanic-origin, gender, and age | External population estimates | |

*Notes:* This table describes the entropy balance reweighting procedure. In the first stage, respondent housing units are reweighted to control for selection into response. This is done by reweighting them to match the characteristics of the target population – all nonvacant housing units in sample. In the second stage, we estimate individual weights that preserve the distribution of housing-unit characteristics from the first stage, while also matching external population totals and approximating the spousal equivalence of weights that are a part of the existing CPS ASEC weights, as in Rothbaum and Bee (2022). To address selection into PIK assignment (and the availability of administrative data), we add a third-stage weighting adjustment.

## Table 9: Rates of Missing Data for Imputed Income Items

| | Missingness Rate |
|---|---|
| Survey | |
| Earnings from Primary Job | 0.456 |
| | (0.003) |
| Earnings from Other Employers | |
| Wage and Salary | 0.367 |
| | (0.007) |
| Self Employment | 0.445 |
| | (0.014) |
| Farm Self Employment | 0.574 |
| | (0.020) |
| Usual Hours Worked Per Week | 0.260 |
| | (0.003) |
| Weeks Worked Last Year | 0.250 |
| | (0.003) |
| Administrative | |
| Job 1 LEHD (gross earnings) missing \| W-2 or DER not missing | 0.080 |
| | (0.001) |
| or large disagreement between LEHD and W-2 | 0.178 |
| | (0.002) |
| Job 2 LEHD (gross earnings) missing \| W-2 or DER not missing | 0.120 |
| | (0.002) |
| or large disagreement between LEHD and W-2 | 0.184 |
| | (0.003) |
| SNAP administrative data unavailable | 0.695 |
| | (0.001) |
| TANF administrative data unavailable | 0.474 |
| | (0.001) |

*Notes:* This table shows the share of the 2019 CPS ASEC sample that is missing information for the various items imputed in this work, as discussed in Section 5.2. Standard errors in parenthesis.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Table 10: Imputation Summary Statistics: Survey Earnings

| | | | Imputed Estimate | | SRMI - Survey |
|---|---|---|---|---|---|
| | W-2 Earnings | Respondents | Survey | SRMI | (Percent difference for dollar values) |
| Has Survey Earnings | = 0 | 0.181 | 0.282 | 0.230 | -0.052*** |
| | | | | | (0.007) |
| | != 0 | 0.908 | 0.860 | 0.907 | 0.046*** |
| | | | | | (0.005) |
| | q = 1 | 0.676 | 0.623 | 0.706 | 0.083*** |
| | | | | | (0.014) |
| | q = 2 | 0.924 | 0.842 | 0.921 | 0.079*** |
| | | | | | (0.009) |
| | q = 3 | 0.967 | 0.928 | 0.961 | 0.033*** |
| | | | | | (0.008) |
| | q = 4 | 0.984 | 0.960 | 0.978 | 0.018*** |
| | | | | | (0.006) |
| | q = 5 | 0.985 | 0.960 | 0.973 | 0.013** |
| | | | | | (0.006) |
| Average Wage and Salary Earnings | = 0 | 45,760 | 43,550 | 40,440 | -0.071 |
| (from main job) | | | | | (0.061) |
| | != 0 | 55,520 | 52,470 | 53,330 | 0.016 |
| | | | | | (0.047) |
| | q = 1 | 11,960 | 22,010 | 20,840 | -0.053 |
| | | | | | (0.084) |
| | q = 2 | 23,540 | 29,810 | 26,300 | -0.118* |
| | | | | | (0.055) |
| | q = 3 | 37,750 | 43,950 | 37,910 | -0.137** |
| | | | | | (0.045) |
| | q = 4 | 57,340 | 62,050 | 56,790 | -0.085 |
| | | | | | (0.058) |
| | q = 5 | 120,300 | 100,000 | 124,900 | 0.248*** |
| | | | | | (0.061) |
| Median Wage and Salary Earnings | = 0 | 25,900 | 30,210 | 31,360 | 0.038 |
| (from main job) | | | | | (0.092) |
| | != 0 | 41,200 | 37,690 | 37,090 | -0.016 |
| | | | | | (0.047) |
| | q = 1 | 6,747 | 12,400 | 13,780 | 0.111 |
| | | | | | (0.158) |
| | q = 2 | 20,720 | 24,660 | 22,160 | -0.102 |
| | | | | | (0.055) |
| | q = 3 | 35,630 | 36,250 | 33,570 | -0.074 |
| | | | | | (0.055) |
| | q = 4 | 55,350 | 51,490 | 52,060 | 0.011 |
| | | | | | (0.045) |
| | q = 5 | 100,300 | 78,690 | 97,460 | 0.238** |
| | | | | | (0.073) |

*Notes:* This table shows basic summary statistics of survey wage and salary earnings conditional on W-2 earnings (having a W-2 and by W-2 earnings quintile for q = 1,2,3,4,5). Each row shows the relevant survey wage and salary earnings statistic for survey earnings respondents, imputed as part of regular survey production and by SRMI, as discussed in Section 5.2. Standard errors in parenthesis. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for differences.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Table 11: Imputation Summary Statistics: Means-Tested Benefits

| | Administrative Data Available? | | Difference | Diff in Diff |
|---|---|---|---|---|
| | Yes | No | No - Yes | (Adrec - Survey) and (No - Yes) |
| TANF | | | | |
| Survey | | | | |
| Receipt | 1.03 | 1.05 | 0.02 | 0.17 |
| | (0.08) | (0.08) | (0.11) | (0.20) |
| Amount | 3,054 | 3,937 | 882** | -975** |
| | (205) | (331) | (391) | (471) |
| Administrative | | | | |
| Receipt | 0.78 | 0.97 | 0.19 | |
| | (0.06) | (0.16) | (0.18) | |
| Amount | 2,604 | 2,511 | -93 | |
| | (168) | (244) | (293) | |
| SNAP | | | | |
| Survey | | | | |
| Receipt | 9.85 | 9.28 | -0.57* | -0.42 |
| | (0.32) | (0.22) | (0.38) | (0.51) |
| Amount | 2,363 | 2,345 | -18 | 73 |
| | (70) | (51) | (87) | (120) |
| Administrative | | | | |
| Receipt | 16.11 | 15.12 | -0.99* | |
| | (0.44) | (0.39) | (0.58) | |
| Amount | 2,807 | 2,862 | 55 | |
| | (60) | (80) | (100) | |

*Notes:* This table shows basic summary statistics of means-tested benefits imputed for incomplete state-level administrative data. For both TANF and SNAP, the first rows show how survey responses vary across states with and without administrative records and the next set of rows show the administrative and imputed estimates. For each, we then compare the states without administrative data (No) to the states with (Yes) and take the difference in difference by comparing the administrative (No - Yes) to the survey (No - Yes). The means-tested benefit imputation is discussed in Section 5.2. Standard errors in parenthesis. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for differences.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Table 12: Sources of Administrative and Survey Earnings

A. All Individuals

| Administrative Earnings Sources | | | | Share with Unimputed Survey: | |
|---|---|---|---|---|---|
| W-2 | DER | LEHD | N | Wage and Salary Earnings | Self-Employment Earnings |
| X | X | X | 72,000 | 0.887 | 0.029 |
| | | | | (0.002) | (0.001) |
| X | X | | 5,900 | 0.704 | 0.033 |
| | | | | (0.010) | (0.003) |
| X | | X | 400 | 0.105 | 0.034 |
| | | | | (0.018) | (0.011) |
| X | | | 300 | 0.804 | 0.024 |
| | | | | (0.036) | (0.011) |
| | X | X | 30 | 1.000 | Z |
| | | | | Z | Z |
| | X | | <15 | Z | Z |
| | | | | Z | Z |
| | | X | 500 | 0.244 | 0.058 |
| | | | | (0.026) | (0.016) |
| | | | 75,000 | 0.045 | 0.027 |
| | | | | (0.001) | (0.001) |

B. Citizenship and DER Earnings

| | | | N | | Share Reporting | | | |
|---|---|---|---|---|---|---|---|---|
| Administrative Earnings Sources | | | (Survey Earnings Respondents Only) | | Wage and Salary Earnings | | Self-Employment Earnings | |
| W-2 | DER | LEHD | In Numident | Not In Numident | In Numident | Not In Numident | In Numident | Not In Numident |
| X | X | Yes or No | 47,000 | <15 | 0.874 | Z | 0.029 | Z |
| | | | | | (0.002) | Z | (0.001) | Z |
| X | | Yes or No | 350 | 200 | 0.093 | 0.847 | 0.035 | 0.023 |
| | | | | | (0.018) | (0.033) | (0.011) | (0.011) |

*Notes:* This table shows the counts and share of adults with each possible administrative earnings data source (W-2, DER, and LEHD) as well as the share in each group that reported survey earnings (among those that responded to the survey earnings questions). Panel A shows the estimates for all individuals in the CPS ASEC. Panel B shows how the presence or absence of DER earnings given W-2 earnings is related to differential probability of reporting survey earnings for individuals who can be assigned PIKs that have SSNs (In Numident) and do not (Not In Numident). Z indicates an estimate rounds to zero. Standard errors in parenthesis.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

# Table 13: Combining Survey and Administrative Earnings

## A. By Reported Earnings Type and Source

| Survey | | Administrative | | Rule | | Percent of Sample | |
|---|---|---|---|---|---|---|---|
| Wage and Salary | Self Employment | Wage and Salary | Self Employment | Wage and Salary | Self Employment | All Adults | Any Earnings |
| X | X | X | X | Job-level administrative | 1040 (from TMI) | 0.4 | 0.6 |
| | X | X | X | Job-level administrative | 1040 (from TMI) | 0.4 | 0.6 |
| X | | X | X | Job-level administrative | 1040 (from TMI) | 4.1 | 5.7 |
| | | X | X | Job-level administrative | 1040 (from TMI) | 0.4 | 0.5 |
| X | X | | X | None (administrative) | 1040 (from TMI) | 0.7 | 1.0 |
| | X | | X | None | 1040 (from TMI) | 1.5 | 2.1 |
| X | | | X | None (administrative) | 1040 (from TMI) | 1.3 | 1.7 |
| | | | X | None | 1040 (from TMI) | 1.2 | 1.7 |
| X | X | X | | Measurement error model | Survey | 1.8 | 2.4 |
| | X | X | | Measurement error model | | 0.8 | 1.1 |
| X | | X | | Measurement error model | None | 50.5 | 70.1 |
| | | X | | Job-level administrative | None | 5.6 | 7.7 |
| X | X | | | Survey | Survey | 0.8 | 1.1 |
| | X | | | None | Survey | 1.0 | 1.4 |
| X | | | | Survey | None | 1.6 | 2.3 |
| | | | | None | None | 28.0 | |

## B. By Combination Rule

| | Percent of Sample | |
|---|---|---|
| Combination Rule | All Adults | Any Earnings |
| Simple - no earnings or only earnings in one source | 38.6 | 14.7 |
| Earnings Choice | 53.0 | 73.6 |
| Default to administrative data due to data issues (potential misclassification, missing self-employment, etc.) | 8.4 | 11.7 |

*Notes:* This table describes the possible combinations of survey and administrative reports of wage and salary and self-employment earnings as well as our rules for when we use survey and administrative reports for each. If the administrative wage and salary earnings on the 1040 is positive but there are no reported job-level administrative earnings, then we use the 1040 value when the rule indicates use of the job-level data. "All adults" includes anyone 15 or over as they are asked survey earnings questions. The sample only includes individuals in the NEWS sample.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Table 14: Combining Administrative and Survey Earnings: Share with Survey Earnings by Mean Reversion Parameter Kappa

| | Share |
|---|---|
| Kappa | Survey Earnings |
| 0.7 | 5.8 |
| | (1.1) |
| 0.75 | 8.4 |
| | (1.5) |
| 0.8 | 11.8 |
| | (2.0) |
| 0.85 | 16.0 |
| | (2.3) |
| 0.9 | 20.6 |
| (NEWS) | (2.7) |
| 0.95 | 25.8 |
| | (3.4) |
| 1 | 30.9 |
| | (3.8) |

*Notes:* This table shows how variation in the mean-reversion kappa parameter in the measurement error model affect the share of individuals whose survey wage and salary earnings are used. Figure 13 shows how the household income distribution differs under these alternatives. Standard errors in parenthesis.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Table 15: Combining Administrative and Survey Earnings: Use of Survey Earnings by Group

A. Race and Hispanic Origin

| | Share Survey Earnings | |
|---|---|---|
| Race/Hispanic Origin | Overall | Relative to Average |
| All | 20.6 | Z |
| | (2.7) | (0.2) |
| Black | 13.8 | -6.8* |
| | (2.9) | (3.1) |
| Hispanic | 22.1 | 1.5 |
| | (2.9) | (1.2) |
| White Non-Hispanic | 22.6 | 2.0 |
| | (3.0) | (1.2) |

B. Age

| | Share Survey Earnings | |
|---|---|---|
| Age | Overall | Relative to Average |
| 18-24 | 6.3 | -14.3** |
| | (1.4) | (3.5) |
| 25-34 | 29.0 | 8.4** |
| | (4.4) | (2.5) |
| 34-44 | 26.8 | 6.3** |
| | (3.5) | (1.8) |
| 45-54 | 20.5 | -0.1 |
| | (4.1) | (2.1) |
| 55-64 | 16.2 | -4.3* |
| | (3.3) | (2.1) |
| 65+ | 8.7 | -11.9*** |
| | (2.6) | (2.4) |

C. Occupation

| | Share Survey Earnings | |
|---|---|---|
| Occupation (Last Week) | Overall | Relative to Average |
| Unemployed | 14.2 | -6.4 |
| | (5.3) | (6.0) |
| Management | 30.3 | 9.7** |
| | (5.6) | (3.0) |
| Business and Financial Operations | 25.2 | 4.6 |
| | (2.8) | (3.2) |
| Computer and Mathematical | 41.5 | 20.9** |
| | (7.2) | (6.9) |
| Architecture and Engineering | 52.3 | 31.7*** |
| | (4.0) | (2.9) |
| Life, Physical, and Social Science | 9.1 | -11.5*** |
| | (2.1) | (2.2) |
| Community and Social Services | 3.1 | -17.5*** |
| | (1.8) | (3.3) |
| Legal | 11.0 | -9.6 |
| | (11.0) | (8.5) |
| Education, Training, and Library | 8.8 | -11.8*** |
| | (4.2) | (2.5) |
| Arts, Design, Entertainment, Sports, and Media | 7.5 | -13.1** |
| | (2.7) | (3.5) |
| Healthcare Practitioners and Technical | 21.9 | 1.3 |
| | (3.8) | (2.0) |
| Healthcare Support | 4.1 | -16.4*** |
| | (1.6) | (3.8) |
| Protective Service | 15.4 | -5.2 |
| | (3.5) | (5.8) |
| Food Preparation and Serving Related | 10.2 | -10.4 |
| | (9.8) | (7.7) |
| Building and Grounds Cleaning and Maintenance | 15.1 | -5.5 |
| | (6.1) | (3.9) |
| Personal Care and Service | 8.8 | -11.8* |
| | (4.0) | (4.8) |
| Sales and Related | 11.9 | -8.7*** |
| | (1.2) | (1.8) |
| Office and Administrative Support | 16.9 | -3.7 |
| | (1.9) | (1.9) |
| Farming, Fishing, and Forestry | 61.1 | 40.5 |
| | (24.3) | (22.3) |
| Construction Trades and Extraction Workers | 42.2 | 21.6 |
| | (11.3) | (10.6) |
| Installation, Maintenance, and Repair Workers | 38.4 | 17.8** |
| | (4.6) | (5.9) |
| Production Occupations | 20.5 | -0.1 |
| | (5.1) | (3.7) |
| Transportation | 11.9 | -8.7** |
| | (2.6) | (2.7) |
| Material Moving | 29.9 | 9.3* |
| | (5.4) | (4.2) |

D. Industry

| | Share Survey Earnings | |
|---|---|---|
| Industry (Last Week) | Overall | Relative to Average |
| Unemployed | 14.2 | -6.4 |
| | (5.3) | (6.0) |
| Agriculture, Forestry, Fishing, and Hunting | 64.1 | 43.5 |
| | (30.7) | (28.7) |
| Mining | 29.2 | 8.6 |
| | (11.2) | (8.8) |
| Construction | 58.6 | 38.0** |
| | (12.1) | (11.5) |
| Manufacturing | 18.9 | -1.7 |
| | (6.6) | (5.1) |
| Wholesale Trade | 13.5 | -7.1 |
| | (7.6) | (8.4) |
| Retail Trade | 4.2 | -16.4*** |
| | (1.5) | (2.8) |
| Transportation and Warehousing | 17.2 | -3.4 |
| | (6.6) | (5.8) |
| Utilities | 6.8 | -13.8* |
| | (5.9) | (6.4) |
| Information | 23.9 | 3.3 |
| | (8.4) | (8.2) |
| Finance and Insurance | 43.8 | 23.2* |
| | (8.1) | (10.2) |
| Real Estate and Rental and Leasing | 79.0 | 58.4*** |
| | (11.3) | (11.7) |
| Professional, Scientific, and Technical Services | 36.2 | 15.7 |
| | (11.6) | (11.1) |
| Management of companies and enterprises | 2.0 | -18.6*** |
| | (3.6) | (4.5) |
| Administrative and support and waste management services | 22.8 | 2.2 |
| | (11.2) | (9.2) |
| Educational Services | 9.8 | -10.8*** |
| | (3.7) | (2.1) |
| Health Care and Social Assistance | 10.9 | -9.7*** |
| | (2.3) | (1.7) |
| Arts, Entertainment, and Recreation | 39.3 | 18.7 |
| | (24.6) | (23.7) |
| Accommodation and Food Service | 14.4 | -6.2 |
| | (14.5) | (12.4) |
| Other Services | 27.0 | 6.4 |
| | (9.3) | (10.4) |
| Public Administration | 7.4 | -13.2 |
| | (4.7) | (7.0) |

*Notes:* This table shows the share of individuals in each subgroup where survey earnings are used from the measurement error model for choosing survey or administrative earnings discussed in Section 6.0.1 and in more detail in Bee et al. (2023) Standard errors in parenthesis. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for differences relative to average.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Table 16: NEWS Median Household Income Estimates Compared to Survey in 2018

| | Survey | | | NEWS | | | | |
|---|---|---|---|---|---|---|---|---|
| | Number | Median Income (dollars) | | Number | Median Income (dollars) | | Percent Difference (NEWS - Survey) | |
| **Characteristic** | **(thousands)** | **Estimate** | **95 percent CI** | **(thousands)** | **Estimate** | **95 percent CI** | **Estimate** | **95 percent CI** |
| **HOUSEHOLDS** | | | | | | | | |
| All Households | 128,600 | 63,180 | 823 | 133,700 | 67,170 | 962 | 6.3*** | 1.4 |
| **Type of Household** | | | | | | | | |
| Family households | 83,480 | 80,660 | 791 | 85,840 | 85,210 | 1,221 | 5.6*** | 1.3 |
| .Married-couple | 61,960 | 93,650 | 1,340 | 63,950 | 98,100 | 1,402 | 4.7*** | 1.4 |
| .Female householder, no husband present | 15,040 | 45,130 | 1,329 | 15,250 | 47,490 | 1,754 | 5.2*** | 3.6 |
| .Male householder, no wife present | 6,480 | 61,520 | 1,485 | 6,644 | 63,550 | 2,798 | 3.3 | 4.4 |
| Nonfamily households | 45,100 | 38,120 | 983 | 47,890 | 41,800 | 846 | 9.6*** | 2.7 |
| .Female householder | 23,510 | 32,010 | 794 | 24,860 | 38,010 | 1,201 | 18.7*** | 3.6 |
| .Male householder | 21,580 | 45,750 | 1,034 | 23,030 | 46,230 | 1,212 | 1.0 | 2.5 |
| **Race and Hispanic Origin of Householder** | | | | | | | | |
| White | 100,500 | 66,940 | 769 | 104,000 | 71,390 | 984 | 6.6*** | 1.3 |
| ..White, not Hispanic | 84,730 | 70,640 | 777 | 87,370 | 74,210 | 1,166 | 5.1*** | 1.4 |
| Black | 17,170 | 41,360 | 1,079 | 18,290 | 43,100 | 2,058 | 4.2* | 4.3 |
| Asian | 6,981 | 87,190 | 3,342 | 7,019 | 89,270 | 5,614 | 2.4 | 5.6 |
| Hispanic (any race) | 17,760 | 51,450 | 876 | 18,400 | 57,710 | 2,314 | 12.2*** | 4.2 |
| **Age of Householder** | | | | | | | | |
| Under 65 years | 94,420 | 71,660 | 683 | 99,370 | 71,580 | 1,001 | -0.1 | 1.2 |
| ..15 to 24 years | 6,199 | 43,530 | 3,204 | 6,961 | 41,350 | 2,245 | -5.0 | 6.4 |
| ..25 to 34 years | 20,610 | 65,890 | 1,281 | 22,080 | 65,110 | 1,764 | -1.2 | 2.3 |
| ..35 to 44 years | 21,370 | 80,740 | 1,276 | 22,490 | 78,600 | 2,390 | -2.7* | 2.7 |
| ..45 to 54 years | 22,070 | 84,460 | 2,198 | 23,000 | 84,940 | 2,017 | 0.6 | 2.7 |
| ..55 to 64 years | 24,170 | 68,950 | 1,720 | 24,840 | 72,430 | 1,975 | 5.0*** | 2.9 |
| 65 years and older | 34,160 | 43,700 | 972 | 34,360 | 55,610 | 1,370 | 27.3*** | 3.0 |
| **Nativity of Householder** | | | | | | | | |
| Native born | 108,600 | 64,240 | 848 | 114,100 | 67,680 | 981 | 5.3*** | 1.3 |
| Foreign born | 20,020 | 58,780 | 1,891 | 19,670 | 64,140 | 2,322 | 9.1*** | 3.9 |
| ..Naturalized citizen | 11,040 | 65,520 | 2,682 | 10,480 | 72,290 | 2,877 | 10.3*** | 4.6 |
| ..Not a citizen | 8,976 | 51,940 | 1,254 | 9,193 | 55,670 | 4,458 | 7.2* | 8.3 |
| **Region** | | | | | | | | |
| Northeast | 22,050 | 70,110 | 2,247 | 22,840 | 76,810 | 2,876 | 9.6*** | 3.4 |
| Midwest | 27,690 | 64,070 | 1,722 | 28,730 | 66,460 | 1,726 | 3.7*** | 2.5 |
| South | 49,740 | 57,300 | 978 | 52,470 | 58,890 | 1,418 | 2.8** | 2.2 |
| West | 29,100 | 69,520 | 1,900 | 29,700 | 77,560 | 2,366 | 11.6*** | 3.1 |
| **Residence** | | | | | | | | |
| Inside metropolitan statistical areas | 110,800 | 66,160 | 725 | 112,600 | 71,010 | 1,049 | 7.3*** | 1.4 |
| ..Inside principal cities | 42,980 | 59,360 | 1,457 | 43,040 | 63,210 | 1,653 | 6.5*** | 2.4 |
| ..Outside principal cities | 67,810 | 70,930 | 902 | 69,520 | 75,780 | 1,522 | 6.8*** | 1.8 |
| Outside metropolitan statistical areas | 17,790 | 49,870 | 1,941 | 21,170 | 50,040 | 1,722 | 0.3 | 3.2 |
| **Education** | | | | | | | | |
| Age 25 and Above | 122,400 | 64,760 | 806 | 126,800 | 69,200 | 963 | 6.8*** | 1.4 |
| No HS | 11,230 | 28,330 | 1,260 | 11,850 | 32,400 | 1,599 | 14.4*** | 5.3 |
| HS | 31,810 | 46,070 | 870 | 33,270 | 50,630 | 999 | 9.9*** | 2.3 |
| Some College | 33,940 | 60,940 | 918 | 35,090 | 64,620 | 1,432 | 6.0*** | 2.0 |
| Bachelor's and Above | 45,410 | 101,800 | 1,135 | 46,550 | 105,400 | 1,940 | 3.5*** | 1.6 |

*Notes:* This table compares the NEWS median household income estimates to the survey estimates by subgroup in 2018. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for percent differences.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Table 17: NEWS Poverty Estimates Compared to Survey in 2018

| | **Survey** | | **NEWS** | | **Change in poverty (NEWS - Survey)** | |
|---|---|---|---|---|---|---|
| **Characteristic** | **Percent** | **95 percent CI** | **Percent** | **95 percent CI** | **Difference** | **95 percent CI** |
| **PEOPLE** | | | | | | |
| **....Total** | **11.78** | **0.29** | **10.67** | **0.39** | **-1.11***** | **0.37** |
| **Race and Hispanic Origin** | | | | | | |
| White | 10.07 | 0.30 | 8.91 | 0.40 | -1.16*** | 0.41 |
| ...White, not Hispanic | 8.07 | 0.28 | 7.48 | 0.35 | -0.59*** | 0.35 |
| Black | 20.77 | 1.16 | 20.10 | 1.46 | -0.67 | 1.31 |
| Asian | 10.10 | 0.94 | 8.52 | 1.41 | -1.57** | 1.38 |
| Hispanic (any race) | 17.56 | 0.80 | 14.61 | 1.14 | -2.95*** | 1.19 |
| **Sex** | | | | | | |
| Male | 10.57 | 0.32 | 9.71 | 0.40 | -0.86*** | 0.40 |
| Female | 12.94 | 0.33 | 11.59 | 0.48 | -1.34*** | 0.45 |
| **Age** | | | | | | |
| Under 18 years | 16.20 | 0.67 | 15.62 | 0.86 | -0.57 | 0.83 |
| 18 to 64 years | 10.68 | 0.29 | 9.97 | 0.37 | -0.71*** | 0.35 |
| 65 years and older | 9.75 | 0.46 | 6.42 | 0.45 | -3.33*** | 0.56 |
| **Nativity** | | | | | | |
| Native-born | 11.45 | 0.31 | 10.48 | 0.40 | -0.97*** | 0.38 |
| Foreign-born | 13.79 | 0.67 | 11.86 | 0.97 | -1.93*** | 1.01 |
| ...Naturalized citizen | 9.93 | 0.75 | 9.07 | 0.99 | -0.86* | 1.01 |
| ...Not a citizen | 17.46 | 1.01 | 14.40 | 1.59 | -3.06*** | 1.63 |
| **Region** | | | | | | |
| Northeast | 10.28 | 0.66 | 9.14 | 0.86 | -1.14** | 0.87 |
| Midwest | 10.37 | 0.66 | 10.51 | 0.83 | 0.14 | 0.75 |
| South | 13.57 | 0.55 | 12.24 | 0.66 | -1.33*** | 0.66 |
| West | 11.22 | 0.64 | 9.41 | 0.83 | -1.80*** | 0.83 |
| **Residence** | | | | | | |
| Inside metropolitan statistical areas | 11.34 | 0.32 | 10.11 | 0.43 | -1.23*** | 0.39 |
| ...Inside principal cities | 14.59 | 0.65 | 13.47 | 0.74 | -1.12*** | 0.70 |
| ...Outside principal cities | 9.42 | 0.40 | 8.18 | 0.47 | -1.24*** | 0.45 |
| Outside metropolitan statistical areas | 14.68 | 0.99 | 13.93 | 1.14 | -0.75 | 0.98 |
| **Disability Status** | | | | | | |
| ....Total, aged 18 to 64 | 10.68 | 0.29 | 9.97 | 0.37 | -0.71*** | 0.35 |
| With a disability | 25.72 | 1.32 | 26.64 | 1.66 | 0.92 | 1.58 |
| With no disability | 9.46 | 0.25 | 8.68 | 0.36 | -0.78*** | 0.35 |
| **Educational Attainment** | | | | | | |
| ....Total, aged 25 and older | 9.90 | 0.24 | 8.62 | 0.32 | -1.27*** | 0.30 |
| No high school diploma | 25.90 | 1.05 | 21.96 | 1.36 | -3.94*** | 1.41 |
| High school, no college | 12.73 | 0.47 | 10.83 | 0.56 | -1.90*** | 0.56 |
| Some college | 8.38 | 0.38 | 8.05 | 0.51 | -0.33 | 0.52 |
| Bachelor's degree or higher | 4.37 | 0.32 | 3.65 | 0.33 | -0.72*** | 0.38 |

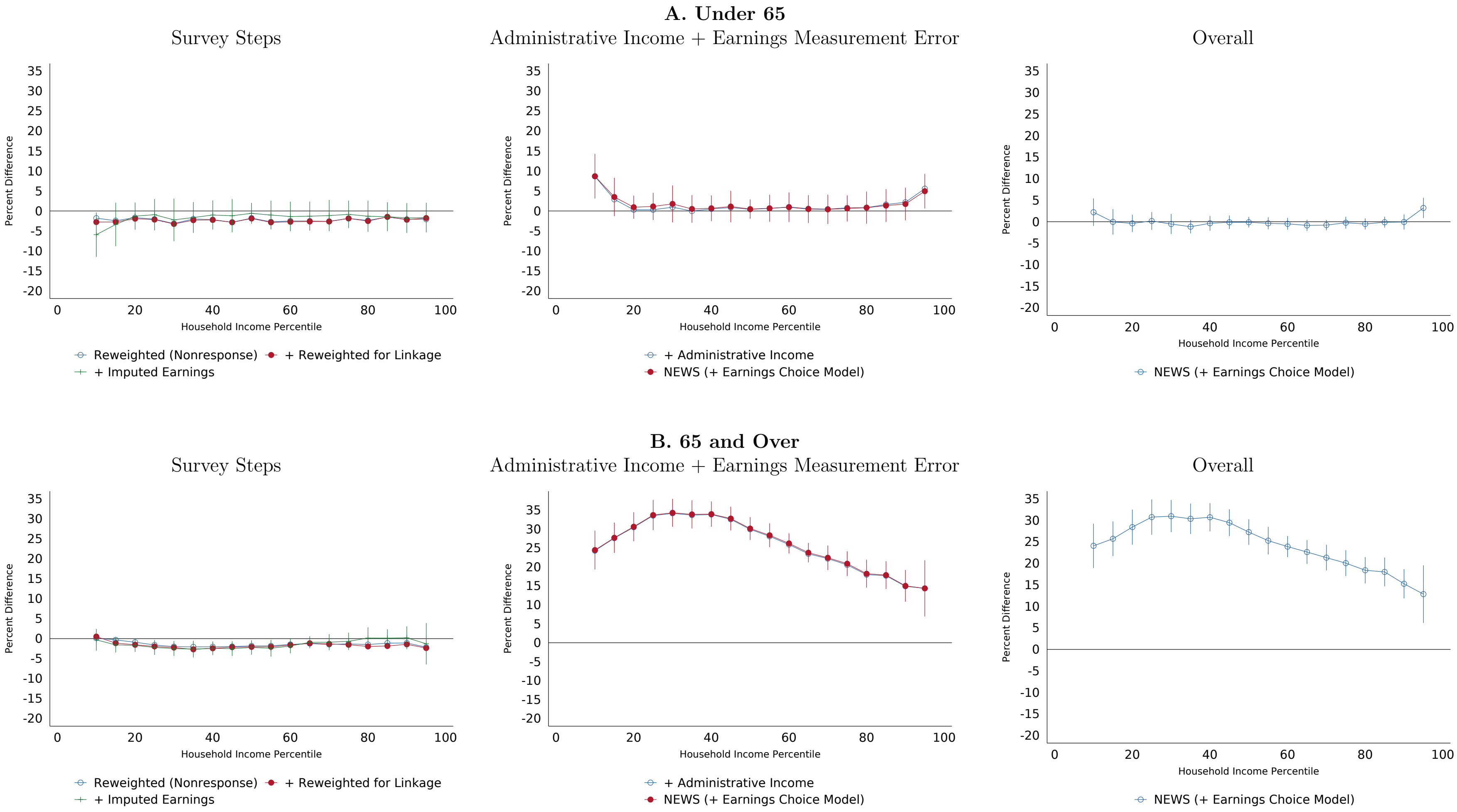
*Notes:* This table compares the NEWS poverty estimates to the survey estimates by subgroup in 2019. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for percent differences.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Table 18: NEWS Inequality Estimates Compared to Survey in 2018

|  |  |  |  |  | **Percent Difference** |  |
|---|---|---|---|---|---|---|
|  | **Survey** |  | **NEWS** |  | **(NEWS - Survey)** |  |
| **Measure** | **Estimate** | **95 percent CI** | **Estimate** | **95 percent CI** | **Estimate** | **95 percent CI** |
| Shares of Aggregate Income |  |  |  |  |  |  |
| 1st Quintile | 0.036 | 0.001 | 0.037 | 0.001 | 0.001 | 0.001 |
| 2nd Quintile | 0.091 | 0.001 | 0.089 | 0.002 | -0.002* | 0.002 |
| 3rd Quintile | 0.148 | 0.001 | 0.142 | 0.003 | -0.005*** | 0.003 |
| 4th Quintile | 0.227 | 0.002 | 0.215 | 0.004 | -0.012*** | 0.004 |
| 5th Quintile | 0.498 | 0.004 | 0.516 | 0.009 | 0.018*** | 0.008 |
| Top 5 Percent | 0.218 | 0.005 | 0.252 | 0.012 | 0.034*** | 0.012 |
| Summary Measures |  |  |  |  |  |  |
| Gini Index | 0.459 | 0.004 | 0.476 | 0.009 | 0.017*** | 0.009 |
| 90/10 percentile ratio | 12.52 | 0.34 | 11.52 | 0.36 | -1.00*** | 0.35 |
| 90/50 percentile ratio | 2.92 | 0.04 | 2.82 | 0.04 | -0.10*** | 0.05 |
| 50/10 percentile ratio | 4.29 | 0.10 | 4.09 | 0.10 | -0.20*** | 0.11 |

*Notes:* This table compares NEWS inequality statistics to the survey estimates in 2018. ***, **, and * indicate significance at the 1, 5, and 10 percent levels and are only shown for percent differences.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

Figure A1: Decomposition of NEWS Processing Steps By Age: Distribution of Household Income



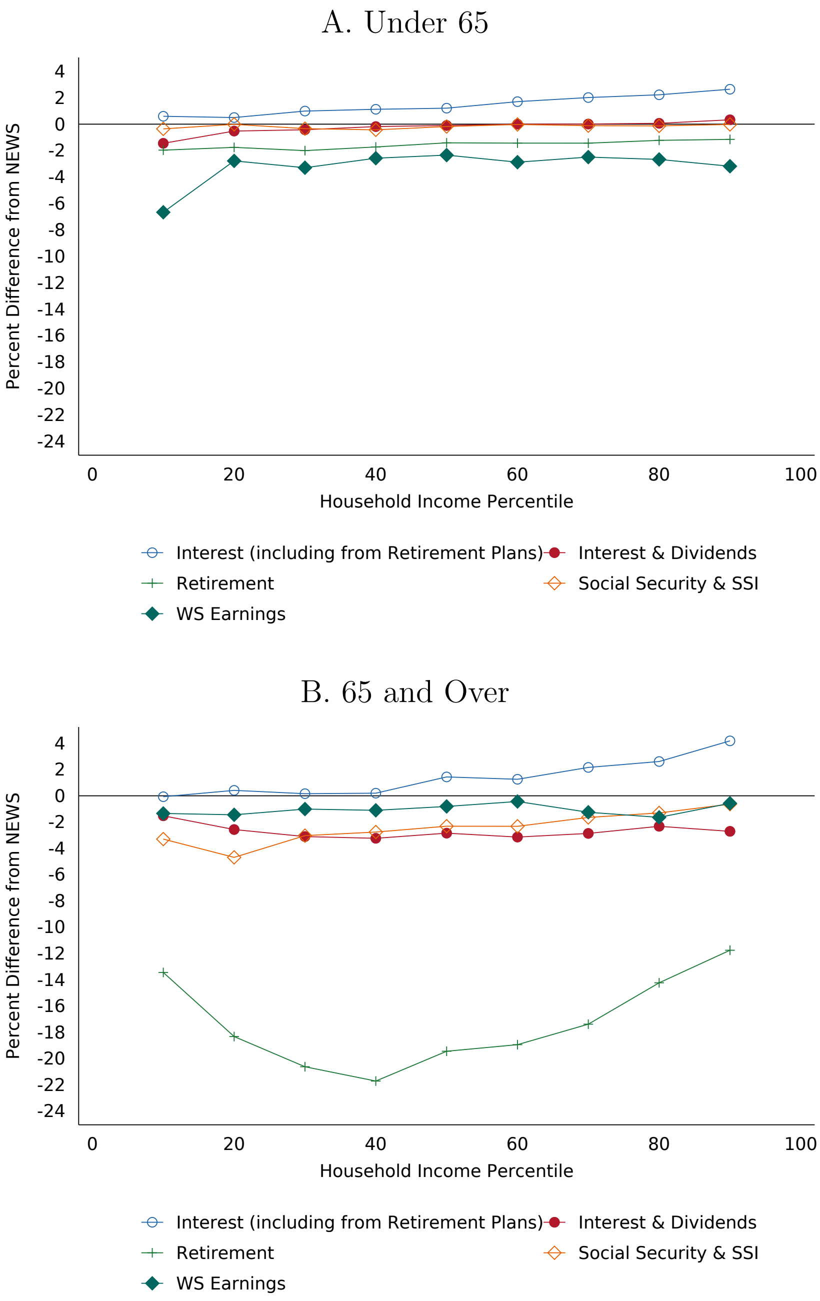
A. Under 65

B. 65 and Over

*Notes:* This figure decomposes the impact of the NEWS processing steps on household income. In the first column, the figures show the adjustments made to the survey data, including reweighting and improved earnings imputation comparing household income after the adjustment to the survey estimate. In the second column, the figures show impact of replacing survey income responses with administrative income, comparing the estimates after each step to the estimates after reweighting and earnings imputation. The full impact of all adjustments is shown in the third column. The 95 percent confidence interval for the last step is shown in each: for A comparing the estimate after earnings imputation to the survey estimate and for B comparing the final NEWS estimate to the estimate after earnings imputation.

*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

## Figure A2: Effect of Removing Individual Administrative Income Items on Household Income by Householder Age



A. Under 65

B. 65 and Over

*Notes:* In this figure, we replace individual income items from the NEWS estimates with the corresponding survey information and compare the estimate after replacement with the NEWS estimate. An estimate below the zero line indicates that administrative item increases income at that percentile. We show each of the major administrative income items, including 1) interest (including and excluding the interest earned in Defined Contribution, DC, retirement plans such as 401(k)s), 2) interest (without DC plan interest) and dividends, 3) DC plan withdrawals, pensions, and survivor and disability pensions (Retirement), 4) Social Security and SSI, and 5) wage and salary earnings.
*Source:* 2019 Current Population Survey Annual Social and Economic Supplement linked to administrative, decennial census, and third-party data.

# Appendices

# A Job Matching

To create the crosswalk of indirect matches, we first match the W-2 and LEHD universe files by PIK-EIN. We identify all direct matches in both data sets and remove them from the file. We then take the remaining jobs in each file and create the Cartesian product of all possible jobs matches for each PIK. In the example in Figure 3, Person 3 has two W-2 jobs (EINs 100 and 600) and one LEHD job (EIN 200) after the direct match on PIK = 3, EIN = 500 is removed. There would be two rows for person 3 in the set of all possible matches: 1) W-2 EIN = 100 with earnings = \$5,000, LEHD EIN = 200 with earnings = \$5,200 and 2) W-2 EIN = 600 with earnings = \$2,600 and LEHD EIN = 200 with earnings = \$5,200. We can then classify jobs as likely matches if their earnings values are close as annual gross earnings from the LEHD and annual taxable earnings + deferred compensation from W-2s should have similar or identical values for a given job. We can then collapse the possible W-2 EIN-LEHD EIN combinations in the data to see which combinations are associated with likely matches. This helps separate true matches from spurious ones that result from within-year job-to-job transitions or possible matches to uncovered jobs. In the example, Person 3 has two possible matches, but the W-2 EIN = 100 to LEHD EIN = 200 is more likely correct as the earnings values are close for that match (\$5,000 to \$5,200) but not for the spurious W-2 EIN = 600 to LEHD EIN = 200 match (\$2,600 to \$5,200). Additionally, two other workers have W-2 EIN = 100 to LEHD EIN = 200 matches, each with similar earnings on the W-2 and LEHD.

From this intuition, we develop our approach to linking the unmatched jobs. We use three indicators (decision rules) to identify jobs matches:

1. How close are the earnings reported on the W-2 and LEHD for the possible job match,
2. What share of jobs in the W-2 EIN match to the same LEHD EIN and what share of jobs from the LEHD EIN match to the same W-2 EIN, and
3. How many likely matches from a W-2 EIN to an LEHD EIN do we need to be confident in

the match.

For the first rule, we can identify matches as likely if the W-2 and LEHD earnings are within some percent of each other. For the second, we can only keep matches in the crosswalk if many or most of the jobs in a W-2 or LEHD EIN are identify as likely matches to a single EIN on the other file. For the third, we may be more confident of a possible match if 100 jobs are all flagged as likely matches than if two are.

We create an iterative process to create our indirect matches where we set the thresholds for each of these three possible rules to identify likely matches. We identify the W-2 EIN-LEHD EIN combinations that match under these thresholds, add those combinations to our crosswalk and then remove the matched jobs from our possible match dataset. The removed jobs include all jobs with those pairs of EINs, not just the ones flagged as likely matches by our percent difference cutoff. We then repeat the process with the remaining jobs after adjusting the thresholds used to identify possible matches. The goal of the iterative process is to first add the matches we are sure of from the set of unmatched jobs (large firms, for example) before we match jobs from smaller firms or with larger differences in earnings across the files.

For example, in the first pass at identifying indirect matches, we flag jobs as likely matches if the W-2 and LEHD earnings are within 10 percent of each other. We then keep the W2 EIN-LEHD EIN combinations where 50 percent or more of them match in one direction or the other - i.e., 50 percent of jobs at a W-2 EIN match to the same LEHD EIN or 50 percent of jobs at the LEHD EIN match to the same W-2 EIN. Finally, we only keep EIN matches for the crosswalk if at least 5 jobs match.

In the example in Figure 3, there are three jobs at W-2 EIN = 100 and LEHD EIN = 200 that are within 10 percent of each other and flagged as likely matches. All jobs in W-2 EIN = 100 match to LEHD EIN = 200 (and vice versa). This combination meets the first two conditions. However, the number of matches is 3, which is less than the threshold of 5 so this combination of EINs would not be flagged as a match. These jobs would be kept in the set of unmatched jobs for the next round of the process.

In subsequent rounds, we can 1) increase the tolerance on likely matches (i.e., from 10 to 20 percent difference in earnings), 2) reduce the share matched needed within W-2 or LEHD EINs (i.e., from 50 percent to 25 percent), or 3) lower the threshold of likely matches needed to confirm a match (i.e., from 5 to 3). From Figure 3, if we lowered the number of likely matches to 3, then we would count W-2 EIN = 100, LEHD EIN = 200 as an indirect match, add that match to our crosswalk, and remove the matches under Indirect Matches from the set of unmatched jobs.[53]

Finally, we do a series of additional steps to match the remaining set of jobs. First, we try to find jobs that have multiple EINs in the LEHD but one EIN in the W-2s, for example if a firm changed EIN mid-year for any reason (restructuring, acquisition, etc.). In that case, the LEHD might have multiple EINs during the year as the firm filed its quarterly reports, but only one EIN for the workers' W-2s. We then flag remaining unmatched jobs as ad hoc likely matches if their earnings are within a certain percent of each other, but they were not matched by the iterative process.

# B Weighting

In this section, we discuss our weighting approach in greater detail. As in the main text, the discussion in this section follows Rothbaum and Bee (2022) closely.

Suppose we have $n$ observations, where $i = 1, 2, \ldots, n$ with base weights based on sampling probabilities of $q = \{q_1, q_2, \ldots, q_n\}$. Entropy balancing estimates weights $w = \{w_1, w_2, \ldots, w_n\}$ that solve the following minimization problem:

$$\min_{w} \sum_{i=1}^{n} w_i \log(\frac{w_i}{q_i}) \tag{B.1}$$

subject to several sets of constraints. First, we have $p$ moment conditions. Let $X = \{X_1, \ldots, X_p\}$ be a matrix of observable characteristics. For characteristic $j$, the moment conditions are defined

[53] In practice, we first increase the earnings percent difference threshold for likely matches from 10 percent to 20 percent to 25 percent. We also decrease the share of matches within an EIN that must match from 50 percent to 25 percent to 10. Finally, we also decrease the minimum number of matches from 5 to 2 to 1. We make each of these changes separately from the initial thresholds and then change them simultaneously.

to match a vector of pre-specified constants $\bar{c}_j$, where:

$$\sum_{i=1}^{n} w_i c_j(X_{i,j}) = \bar{c}_j. \tag{B.2}$$

$c_j(\cdot)$ can be any arbitrary function.

Second, we have constraints on the weights themselves:

$$\begin{aligned} \sum_{i=1}^{n} w_i &= \bar{w} \\ w_i \geq 0, i &= 1, \ldots, n \end{aligned} \tag{B.3}$$

which ensure that the weights sum to some pre-specified total weight $\bar{w}$, which can be the population count or 1. The value of $\bar{w}$ does not affect the relative weights of each observation.

As such the weights can be adjusted to match pre-specified moments such as population means, variances, higher-order moments, moments of any transformed distribution of $X_(i,j)$, etc. In summary, entropy balancing adjusts the weights according to (B.1), subject to the constraints in (B.2) and (B.3).[54]

Entropy balancing was developed as an application of empirical calibration to balance treatment and control groups when estimating causal treatment effects in observational studies. Zhao and Percival (2017) show that, in that context, entropy balancing is equivalent to estimating a logistic model for the propensity score and a linear regression model for the outcome, conditional on the covariates used in the moment conditions. They find that entropy balancing is doubly robust - if at least one of the two models is correctly specified, the estimated population average treatment effect on the treated (PATT) is consistent.[55] Using the notation of that literature, let $\gamma$ be the PATT, $Y$ be an outcome of interest where $Y(1)$ is the outcome if treated and $Y(0)$ is the outcome if untreated, then:

[54] In practice, as is not necessarily possible to satisfy all constraints simultaneously through weighting adjustment, the analyst sets a tolerance level for the moment constraints. The weighting algorithm adjusts the weights iteratively until all constraints are satisfied subject to the specified tolerance.

[55] Double robustness is not a panacea. Kang and Schafer (2007) show via simulation that doubly robust models for missingness can perform poorly when neither model is correctly specified, or as they write, "in at least some settings, two wrong models are not better than one."

$$\gamma = E[Y(1)|T = 1] - E[Y(0)|T = 1]. \tag{B.4}$$

In the causal inference literature, the challenge is that $E[Y(0)|T = 1]$ is not observed. Under entropy balancing, given $\sum_{i=1}^{n} q_i = \bar{q}$, the PATT is estimated as:

$$\hat{\gamma}_{ebw} = \frac{1}{\bar{q}} \sum_{T_i=1} q_i Y_i - \frac{1}{\bar{w}} \sum_{T_i=0} w_i Y_i. \tag{B.5}$$

In the case of survey weights, the "treatment" is nonresponse, and the double robustness result applies. Entropy balancing reweights the sample so that the estimate of $Y$ for the weighted respondents is equal to the estimate of $Y$ for the population,[56] or:

$$E[Y] = \frac{1}{\bar{w}} \sum_{i=1}^{n} w_i Y. \tag{B.6}$$

We would like to reweight the respondent sample so that its distribution of characteristics matches the target population from which the sample was drawn. However, some characteristics are not observable for all housing units with the available linked census, survey, and administrative data. For example, we do not observe any demographic information for housing units that are not linked to an information return in the IRMF file, as the IRMF provides the identifier needed (PIK) to link individuals to all other data sources. Therefore, we use a second source of data for our reweighting – the aforementioned external estimates of population by geography. For both the linked data and the external population estimates, we can specify a set of moment conditions, which are intended to capture the distribution of characteristics in the target population. In the language of our MAR assumption, we are concerned that $f(R|A) \neq f(R|X)$ and that we need $X_O$ (the demographic information) in the weighting model as well, such that $f(R|A, X_O) = f(R|X)$.

Our data have one additional complication – the target moments are at separate levels of aggregation. Estimates from the linked administrative and census data are at the housing unit level

[56] Conditional on strong ignorability ($Y(0), Y(1) \perp T|X$) and overlap ($0 < P(T = 1|X) < 1$), from Rosenbaum and Rubin (1983), as well as the proper specification of the moment conditions required for the Zhao and Percival (2017) double robustness result.

whereas the external state-level population moments are at the individual level. Entropy balancing is not amenable to matching moments at different levels of aggregation. Therefore, we proceed with a multi-stage reweighting procedure, which we discuss below and summarize in Table 8. This is analogous to two-step calibration, as discussed in Estevao and Säarndal (2006).

In the first stage, we adjust the household base weights for nonresponse, controlling to moments estimated from the linked administrative and census data. The target distribution is estimated using the nonvacant housing units in the March Basic CPS Sample, which includes both respondent and nonrespondent housing units. Given the known probability of inclusion in the sample (from the base weights), these are estimates of the underlying population moments for each of the included characteristics. The moments include housing-unit-level summary statistics on race, Hispanic origin, age, marital status, income, sources of income (through information return dummies), citizenship, and nativity.

Entropy balancing adjusts the housing unit weights so that the weighted estimates from respondent units match the moments estimated from all nonvacant households. Let us designate the housing-unit moment constraint variables as $X^L_{i,j}$, where $L$ indicates linked data. Let $w^1_i$ be the output weights of the first-stage reweighting. Given $n$ respondent households, and a set of nonvacant (occupied) households $NV$, where $i = 1, 2, \dots, n_{NV}$ with survey base weights $q_i$, the moment conditions are of the form:

$$\sum_{i=1}^{n} w^1_i c_j(X^L_{i,j}) = \sum_{i=1}^{n_{NV}} q_1 c_j(X^L_{i,j}). \tag{B.7}$$

With these moment conditions, we estimate $w^1_i$ for each household using entropy balancing.

In the second stage, we would like to create weights (denoted $w^2_{m,i}$) for each individual $m$ and household $i$, where $m = 1, 2, \dots, M$, that adjust to external population controls while maintaining the household weighting adjustment from the first stage. We do so by simultaneously matching to three sets of target moments (2A-C in in Table 8):

A Preserve the distribution of housing unit characteristics

B Spousal equivalence

C External population targets

In the first set of constraints (A), we calculate person-weighted moments from the stage-1 weights. Given the number of people in household $i$, $n_i^{HH}$, we define the moment conditions using the stage-1 weights as follows:

$$\sum_{m=1}^{M} w_{m,i}^2 \frac{1}{n_i^{HH}} c_j(X_{i,j}^L) = \sum_{i=1}^{n} w_i^1 c_j(X_{i,j}^L). \tag{B.8}$$

This ensures that if we take the average weight of household members in household $i$ ($HH_i$) as $\bar{w}_i^2 = 1/n_i^{HH} \sum_{p \in HH_i} w_{m,i}^2$ , the following condition will be satisfied:

$$\sum_{i=1}^{n} \bar{w}_i^2 c_j(X_{i,j}^L) = \sum_{i=1}^{n} w_i^1 c_j(X_{i,j}^L). \tag{B.9}$$

This does not require that $\bar{w}_i^2$ is equal to $w_i^1$ for any household $i$, but rather that the specified constraints from stage one hold in the final entropy-balance weights, when the final weights are averaged across all household members. This procedure of dividing the household moments equally among the family members helps ensure that each person contributes to satisfying the moments from the linked administrative and decennial census data, which should reduce the variability of weights among household members. It is particularly important for person-level statistics, such as poverty or health insurance status, that are functions of household or family characteristics. For example, poverty status (poor/non-poor) is defined at an aggregated level (the family), but the share in poverty is estimated from individual weights. By having each household member be part of the moment conditions for the linked data, administrative income affects each member's weight, which affects the poverty estimate.

For the second set of moments in the second-stage reweighting (2.B. in Table 8), we approximate the spousal equalization that is part of existing CPS ASEC weights. We include this set of conditions because household- and family-level statistics should also be invariant to which spouse's weight is used as the family or household weight. Let $S = \{0, 1, 2\}$, where $S = 0$ if an individual is unmarried, 1 if the individual is the first spouse or cohabiting partner on the file, and 2 if the individual is the second spouse or partner on the file. Given an indicator function $I(\cdot)$, the spousal equivalence moment condition for a given characteristic in the linked data is:

$$\sum_{i=m}^{M} \left[ I(S=1) w_{i,m}^{2} c_j(X_{i,j}^{L}) - I(S=2) w_{i,m}^{2} c_j(X_{i,j}^{L}) \right] = 0. \tag{B.10}$$

This does not require that each individual's weight be equal to their partner's, as that would require a separate moment condition for each couple. Instead, it requires that the characteristics of the households of partners in the linked data be balanced.

The third set of moment conditions (2.C. in Table 8) reweight the individual observations to match the age by race/Hispanic-origin/gender cells for each state and the District of Columbia, as noted above. These conditions have the simple form of equation (B.2).

With these three sets of conditions, we reweight the March Basic CPS sample to simultaneously match the household-level linked administrative data and the individual-level state population targets. For each individual, the initial weights for the stage 2 reweighting are the household weights from the stage 1 reweighting ($w_i^1$), so that the minimization from (B.1) becomes:

$$\min_{w^2} \sum_{i=1}^{n} w_i^2 \log(\frac{w_i^2}{w_i^1}). \tag{B.11}$$

However, for the full CPS ASEC sample, there is one more complication. The full sample includes groups that were oversampled based on characteristics reported in earlier survey responses, including Hispanic origin and the presence of children. Therefore, in the full sample, the weights for these oversampled individuals and households need to be adjusted to reflect their prevalence in the population and characteristics. To do this, we add a fourth set of moment conditions (2.D. in Table 8). We create these conditions from the entropy-balance weighted March Basic sample, because it is a stratified random sample that is not affected by oversampling based on observable characteristics from prior survey responses. Let $w_{i,m}^{2,March}$ be the second-stage weights from the March Basic Sample, $w_{i,m}^{2,Full}$ be the second-stage weights from the full CPS ASEC sample, and $M_{Full}$ and $M_{March}$ be the number of individuals in the full and March Basic CPS samples. This fourth set of conditions has the form:

$$\sum_{m=1}^{m_{Full}} w_{i,m}^{2,Full} c_j(X_{i,k}) = \sum_{m=1}^{m_{March}} w_{i,m}^{2,March} c_j(X_{i,k}). \tag{B.12}$$

This fourth set of moments includes information on race, Hispanic origin, income (from the linked administrative data), and the number of adults and children in the household. Without this set of conditions, estimates of the number of households by type (especially for oversampled groups) differ between the full and March Basic CPS ASEC samples. Additionally, without these constraints, observables-based oversampling in the full CPS ASEC biases estimates for oversampled subgroups relative to estimates from the March Basic sample. Although we focus on the estimates from the full CPS ASEC sample in this paper, we present the results from the Basic March sample in the Appendix as well, because it is a stratified random sample with no oversampling based on observable characteristics from earlier survey responses.

At this point, the weights would adjust for selection into response. However, because we are using administrative data to address survey misreporting, inclusion in our sample is also conditional on linkage to a PIK as that is the key to linking each individual to *every* source of administrative data. We therefore include in our sample only those households in which all those old enough to receive survey income questions (15+) are assigned a PIK. To address this selection, we add a third stage to the entropy balancing weight procedure used in Rothbaum and Bee (2022), as shown in Table 8, Stage 3.

Stages 3A and 3B have the same form as 2A and 2C, but add additional moments to the already specified ones from the linked data and external population controls. In adjusting for selection into linkage, we include moments on survey-reported income, administrative income, and survey poverty status by survey reported demographics such as race, Hispanic-origin, citizenship, and age.

The weights after this third-stage adjustment should adjust the sample for both selection into survey response and selection into linkage, to the extent possible given the observable survey and linked administrative data.

For valid inference, we repeat the above two-stage reweighting procedure 160 additional times using the baseline successive difference replicate factors created during the sampling process, which are available for all households regardless of response status. These replicate factors account for the sampling design of the monthly Basic CPS and CPS ASEC. Also, the first-stage target moments from the March Basic CPS sample are estimates and thus subject to sampling error. By repeating the procedure with the base weights and replicate factors, the target moments for each replicate will vary, and variation in the final weights across the replicates will reflect the uncertainty in our linked data estimates. All standard errors reported using EBW are calculated with these 160 replicate-factor EBW.[57]

As noted in Rothbaum et al. (2021), in addition to changing point estimates, improved weights can also affect standard errors. It is generally understood that increased variability among the survey weights can increase the standard errors, so weighting adjustments aimed at reducing bias are often done at the expense of increasing variance. However, Little and Vartivarian (2005) show that this may not hold true if variables used to adjust for nonresponse are correlated with survey variables of interest, a property they call "super-efficiency." This also has implications for how weighting models should be constructed, as including variables that are not strongly predictive of response, but are correlated with outcomes of interest can reduce variance of an estimate even if they do not affect its bias.

# C Imputation

In Section 5.2, we discussed imputation SRMI generally, but did not elaborate on SRMI and left unspecified the particular imputation model used to estimate $f(Y|O,\theta)$. In this section, we discuss those details.

SRMI is an iterative resampling technique to estimate $f(Y|O,\theta)$ while imposing fewer strong parametric assumptions on the joint conditional distribution $f$. Under SRMI imputation, We estimate the model for each $Y_j$ iteratively as follows. In the first iteration, $Y_1$ is regressed on $O$ and the missing values are imputed. Any imputation model can be used to impute values for each $Y_j$,

[57] Refer to "Estimating ASEC Variances with Replicate Weights" (U.S. Census Bureau, 2009) for a discussion of successive difference replication in the CPS ASEC. Note also that at present we do not include uncertainty in the external population targets, but we hope to explore how best to account for that uncertainty in the weights as well in future research.

such as a regression model, a hot deck, or predictive mean matching, with their attendant assumptions about $f(Y|O,\theta)$. Let $Y_1^{(1)}$ denote the filled-in version of the variable $Y_1$ from the first iteration. Now $Y_2$ is imputed using $(O, Y_1^{(1)}$ as covariates to generate $Y_2^{(1)}$, the filled in version of $Y_2$ from the first iteration. This process continues until the missing values in $Y_p$ are imputed using $(O, Y_1^{(1)}, Y_2^{(1)}, \ldots, Y_{p-1}^{(1)})$ as predictors.

We cannot stop at iteration 1 because the imputation of $Y_1^{(1)}$, for example, fails to exploit the observed information from $(Y_2, Y_3, \ldots, Y_p)$. Iterations $t = 2, 3, \ldots$ proceed in the same manner except that all other variables (with some filled at the current and the rest in the previous iterations) are used in imputing each variable. Specifically, at iteration 2, $Y_1$ is re-imputed using $(O, Y_2^{(1)}, Y_3^{(1)}, \ldots, Y_p^{(1)})$ as predictors; $Y_2$ is re-imputed using $(O, Y_1^{(1)}, Y_3^{(1)}, \ldots, Y_p^{(1)})$ as predictors, etc. In each iteration, we are updating our predictions of $\theta$ as well as $Y$.

In general, at iteration $t > 1$, $Y_j$ is re-imputed using $(O, Y_1^{(t)}, Y_2^{(t)}, \ldots, Y_{j-1}^{(t)}, Y_{j+1}^{(t-1)}, \ldots, Y_p^{(t-1)})$ as predictors. The iterations are continued several times in order to fully use the predictive power of the rest of the variables when imputing each variable. Empirical analysis has shown that fewer than 20 and generally as few as 5 to 10 iterations are sufficient to condition the imputed values in any variable on all other variables (Ambler, Omar and Royston, 2007; Van Buuren, 2007; He et al., 2010). By repeating the imputation process in each iteration, SRMI is akin to a Gibbs or MCMC resampling technique that should iteratively converge to the true conditional joint density (if the model is properly specified).

As noted in Section 5.2, we impute survey earnings, job-level administrative gross earnings (or LEHD-equivalent earnings), and missing state-level means-tested program data. For survey earnings, we impute extensive margin earnings receipt and intensive margin earnings amounts for all earnings variables. In the CPS ASEC this includes the variables ern_yn (earnings receipt?), ern_srce (primary job earnings source - wage and salary, self employment, or farm self employment), ern_val (earnings amount from primary job), ws_yn, se_yn, and frm_yn (secondary wage and salary, self employment, for farm self employment earnings?), and ws_val, se_val, and frm_val (amount of secondary earnings in each category). We also impute upstream variables that are highly predictive of earnings, including weeks worked last year (wkswork) and hours worked per week last year (hrswork).

For gross earnings by job (for the two highest earning jobs for each worker), we impute several variables to simplify the imputations and capture important features in the data. First, we impute a dummy variable for whether gross earnings $\approx$ taxable earnings + deferred compensation, which is true for a large share of workers. For those where gross earnings > taxable earnings + deferred compensation, we then impute a series of dummies for whether gross earnings/(taxable earnings + deferred compensation) falls in several bins, including 1.1 and above, [1.05, 1.1), [1.03, 1.05), [1.02, 1.03), [1.01, 1.02), and (1, 1.01). After assigning each job to a gross earnings/(taxable earnings + deferred compensation) bins, we then impute the amount of gross earnings for each job. We chose this approach because many variables (such as survey-reported private health insurance coverage) are good predictors of whether gross earnings/(taxable earnings + deferred) compensation exceeds specific thresholds while not necessarily being good predictors of the exact value of gross earnings/(taxable earnings + deferred).

For each earning variable, we have separate imputation models by spouse (by sex if an opposite-sex couple, by order on the file if a same-sex couple). This allows for a more flexible imputation model and allows us to condition on spousal income in the SRMI.

For state-level means-tested program data, we impute program receipt ({Program}_YN) and, conditional on receipt, the amount received ({Program}_val) for each program at the household level.

As discussed in Hokayem, Raghunathan and Rothbaum (2022), there are a number of challenges to implementing SRMI in this context. First, many income types do not follow a normal distribution. Second, we must select predictors for the modelling of each income variable from a very large set of possible covariates. Third, we must properly account for uncertainty in our estimates of the parameters in $\theta$. Included in this uncertainty is the selection of variables for our imputation models because when we select predictors for our models, we are imposing the assumption that there is no relationship between the excluded variables and the variable being imputed conditional on the included variables. Next, we discuss how we address each of these issues.

To address non-normality, we transform each continuous variable using the inverse hyperbolic sine, which allows us to include negative values, as in Fox et al. (2022).[58]. As the inverse hyperbolic sine is nearly perfectly correlated with the natural log over most of the defined range of the natural log, one can interpret the regression coefficients of continuous variables as elasticities (for continuous dependent variables) or semi-elasticities (for binary dependent variables).

As a practical matter, there are too many potential variables in $O$ to be used in our model. We reduce the set of variables to be used to impute each $Y_j$ in two stages, both using the Least Absolute Shrinkage Operator (LASSO, Friedman, Hastie and Tibshirani (2010)). In the first stage, we take all of the possible interaction terms we specify in $O$ and use LASSO to prune the list to $\hat{O}_j$ that predict $Y_j$ (including all non-interacted terms in $\hat{O}_j$). The set of variables in $\hat{O}_j$ will generally be large (hundreds of variables and interactions, if the regression sample size is large). In terms of the general notation $f(Y|O,\theta)$, this process places constraints on $\theta$.[59].

During the imputation process, we have a second-stage of regularization when we estimate the values in $\hat{\theta}$. As $\hat{\theta}$ is a set of unknown parameters, we also must incorporate the uncertainty in $\hat{\theta}$ into the imputation process – the third challenge noted above. We do this as follows. In each implicate $c$ (independent run of the imputation model), we start by taking a Bayesian Bootstrap of the sample, we then do a second-stage variable selection process to further reduce the number of variables in $\hat{O}_j$ to $\hat{O}_{j,c}$, again using LASSO regularization.[60] From the regression of $Y_j$ on $\hat{O}_{j,c}$, we estimate $\hat{\theta}_{j,c}$. Doing this on a Bayesian Bootstrap sample enables us to account for the uncertainty present in each step of this process, including which variables are used as model predictors ($\hat{O}_{j,c}$) and to draw

[58] Hokayem, Raghunathan and Rothbaum (2022) tested alternative transformations, such as Tukey's gh transformation (He and Raghunathan, 2006) and an empirical normal transformation (Woodcock and Benedetto, 2009). However, as in Fox et al. (2022), they found the inverse hyperbolic sine performed well, and we use that transformation here.

[59] This is primarily done for practical speed considerations. Reducing the number of candidate variables upfront considerably speeds up the process of imputation for each variable in each implicate.

[60] The Bayesian Bootstrap (Rubin, 1981) is the Bayesian analogue of the bootstrap. Each observation is drawn (with replacement) with an expected probability of $1/n$, but with variability. The probabilities of being drawn are defined by taking $n-1$ draws from the uniform distribution (0,1), ordering draws from lowest to highest, where $u = u_0, u_1, u_2, \ldots, u_n$ given $u_0 = 0$ and $u_n = 1$. The probability of being drawn for each observation $i$ is based on the gaps between each adjacent value in $u$, so that for observation $i$ the probability of being drawn is $g_i = u_i - u_{i-1}$. As noted in Benedetto, Stinson and Abowd (2013), using the Bayesian Boostrap adds additional variability to the imputation process to account for the fact that the sample distribution may not be the same as the population distribution. Without the use of the Bayesian Bootstrap, the confidence intervals would not be proper.

from the distribution of parameters values $\hat{\theta}_{j,c}$. This resampling approach to estimating uncertainty in regression-based imputation has been taken in other data products and research, including SIPP topic flag imputation (Benedetto, Motro and Stinson, 2016), the SIPP Gold Standard and SIPP Synthetic Beta (Benedetto, Stinson and Abowd, 2013), and imputation research on missing income in the CPS ASEC (Hokayem, Raghunathan and Rothbaum, 2022).

With the transformed continuous variables, regularization, and Bayesian Bootstrap-based estimation of the uncertainty of $\hat{\theta}$, we are almost ready to impute missing values. We must also specify the functional form of our imputation models (parametrizing $f(Y|O,\theta)$). Unless otherwise indicated, we use predictive means matching (PMM) to impute both binary and continuous dependent variables.

For binary dependent variables, we use a Linear Probability Model (LPM), regressing the dependent variable on the model selected using the LASSO on the Bayesian Bootstrap sample. We then predict the vector $\hat{p}_j(Y = 1|X, \hat{\theta_j})$, which includes the estimated probability for all individuals in sample whether $R_j = 0$ or $R_j = 1$. We then take a random draw for each unit $i$ where $R_{i,j} = 0$ from the ten nearest units $k$ where $R_{k,j} = 1$ to assign $Y_{i,j}$ values. We use LPM rather than a logit or probit model as the LPM model more predictor variables. Although LPM does not impose $0 \leq \hat{p}_{i,j} \leq 1$, the $Y_{i,j}$ draws must equal 0 or 1. Fox et al. (2022) used the same approach for imputing SNAP receipt and showed that this PMM model performed well for several conditional and unconditional statistics ($Q$'s such as SNAP receipt, SNAP receipt conditional on earnings and demographics, for example).

For continuous dependent variables, we use Ordinary Least Squares (OLS), regressing the dependent variable on the model selected using the LASSO on the Bayesian Bootstrap sample. We then predict the vector $\hat{Y}_j(Y_{-j}, X, \hat{\theta}_j)$ where $Y_{-j}$ is the matrix $Y$ excluding $Y_j$, again for all individuals in sample whether $R_j = 0$ or $R_j = 1$. We then take a random draw for each unit $i$ where $R_{i,j} = 0$ from the ten nearest units $k$ where $R_{k,j} = 1$ to assign $Y_{i,j}$ values.

For survey wage and salary earnings from the longest job (ern_val if ern_srce == 1), rather than using PMM, we use a two-stage model that incorporates OLS and quantile regressions. As before, we first use OLS to predict $\hat{Y}_j(Y_{-j}, X, \hat{\theta}_j)$ after LASSO regularization. We then use quantile regression to regress $Y_j$ on binned $\hat{Y_j}$ and several variables from $O$, including race and Hispanic origin, age, education, and hours worked. We do this for each 5th percentile from the 5th to the 95th. This gives us an estimate for $\hat{Y_{j,i,q}}$ for each individual $i$ at each quantile $q$.[61]. From the values of $\hat{Y}_{j,i,q}$, we have a posterior predictive distribution (PPD) of $Y_{j,i}$ for each individual $i$ (after interpolation using Schmidt et al. (2022)). For each individual, we then draw a percentile value from 0 to 1 to impute $Y_{j,i}$ from the PPD. [62]

Using quantile regression to estimate the PPD is useful if there is potential heterogeneity in the relationship between specific variables in $O$ and $Y_j$. For example, suppose the average relationship between education and earnings reflects a bigger right tail for college graduates (more very high earners), the PMM-based estimate would not necessarily reflect that in the resulting imputes. However, the quantile regression-based PPD would. However, more data (a large sample) is required to use quantile regressions to reliably estimate the PPD. Because of the possibility of heterogeneity and the greater data needs, we implement this approach from survey wage and salary earnings from the primary job (the largest single source of survey income, covering almost 70 percent of total income).

For the means-tested program variables imputed at the household level, we recode the data to summarize the information of household members (such as presence of members by race, total household earnings, etc.) and household head variables (such as education, race, etc.) to use as predictors and then impute receipt and amounts using PMM as discussed above.

For nonfilers, we observe whether they received several information returns, including Forms 1099-G, 1099-INT, and 1099-DIV in the IRMF. From these we have information on whether they received UI compensation, interest income, and dividends, respectively. Each of these are vastly underreported on surveys (Rothbaum, 2015). Rothbaum (2023) has been working with more detailed data

[61] The regressions do not impose monotonicity, i.e., it does not ensure that for two quantiles $q$ and $r$ where $r > q$, $\hat{Y}_{j,i,r} > \hat{Y}_{j,i,q}$ (the quantile crossing problem). Following Chernozhukov, Fernández-Val and Galichon (2010), we rearrange the curve by sorting the $\hat{Y}_{j,i,q}$ values from lowest to highest and assigning them to the corresponding position's $q$ value. As Chernozhukov, Fernández-Val and Galichon (2010) show, the rearranged curve is closer to the true quantile curve than the original curve in finite samples.

[62] If any part of this process fails (such as from nonconvergence in a quantile regression estimate), we impute using PMM. This is unusual, but possible, in an automated process like SRMI that runs many regressions per iteration repeated across implicates.

available under a separate agreement between the Census Bureau and IRS, for limited use. In that work, the 1099-G, 1099-INT, and 1099-DIV data is available, including income amounts. Rothbaum (2023) released coefficients that can be used to impute these amounts for nonfilers conditional on survey responses and the administrative data used in this project.

To release this statistics, Rothbaum (2023) estimated models for the synthesis of four variables:

1. UI compensation receipt conditional on receipt of a Form 1099-G
2. UI compensation amount conditional on receipt of UI compensation
3. Interest income amount conditional on receipt of a Form 1099-INT
4. Dividend income amount conditional on receipt of a Form 1099-DIV

In order to allow the creation of synthetic data to correct for survey underreporting, Rothbaum (2023) released three sets of results for each variable.

For UI compensation receipt, they estimate a Linear Probability Model (LPM) of UI compensation receipt conditional on receiving a Form 1099-G. Individuals receive a 1099-G for various government payments, including 1) UI compensation, 2) state or local income tax refunds, credits, or offsets, 3) reemployment trade adjustment assistance payments, 4) taxable grants, and 5) agricultural payments. This model is estimated as described above using the two-stage LASSO feature selection, with the second stage estimated on a Bayesian Bootstrap. As such, the released parameters are effectively a draw from the distribution of possible parameter estimates that could be used to predict nonfiler UI receipt.

With these regression coefficients, we can estimate the expected probability of UI receipt for each nonfiler ($\hat{p}_j(Y = 1|X, \hat{\theta_j})$) on a separate sample (or the data without access to the more detailed 1099-G data). However, as they were estimated using a LPM, we cannot directly use them to synthesize UI receipt data (as the $\hat{p}_j(Y = 1|X, \hat{\theta_j})$ can be $< 0$ or $> 1$, which PMM addresses by taking a random draw from individuals with similar $\hat{p}_j(Y = 1|X, \hat{\theta_j})$, but with observed values for $Y_j$. Instead, Rothbaum (2023) then separate the expected probability space into bins and released the boundaries between those bins and the empirical probability that an observation received UI compensation in each bin. For example, the top quintile of observations has an expected probability of receipt of 0.87 or higher (the boundary). Within that bin of observations with an expected probability of 0.87 or higher that received UI compensation was 0.98 (the empirical probability in the bin), then we can impute UI receipt for this group by drawing a random number between 0 and 1 and assigning receipt if it is $\leq 0.98$.

By releasing regression coefficients, bin boundaries, and empirical probabilities, Rothbaum (2023) implement a semiparametric imputation technique that is similar to the binned imputation proposed by Bondarenko and Raghunathan (2007).

For the income variables – UI compensation, interest income, and dividends – the approach is slightly different. The first step is the same as above for continuous variables – estimate an OLS model to predict expected income amounts conditional on the available information. Again, the models are estimated using the two-stage LASSO feature selection, with the second stage estimated on a Bayesian Bootstrap. The coefficients from this model are released so that the expected income amount can be estimated on a separate sample ($\hat{y}_{i,j}$). To allow the synthesis of continuous variables, Rothbaum (2023) release two set of variables. First, they partition $\hat{y}_{i,j}$ into bins. Then, using quantile regression at various percentiles, the regress income amounts on bin dummies. As with ern_val above, these regression coefficients can be used to estimate a PPD for each individual. By drawing a value from 0 to 1, we can impute income amounts from these PPDs.

In summary, for each income amount synthesized, Rothbaum (2023) release three sets of statistics, regression coefficients, bin boundaries and quantile regression coefficients to enable relatively low dimensional data to be used to synthesize or impute UI compensation amounts, interest income, and dividends.

Finally, We repeat this process five times, to create the five independent implicates. In each implicate, we use SRMI to impute the survey and gross earnings variables, followed, in a separate step, by the imputation of means-tested program variables. For any statistic or parameter estimate, we can account for the uncertainty in the imputation process (Rubin, 1976). To do so, we calculate the total variance by combining the within-implicate variation (for example, the standard error of an estimate in one implicate) with the between-implicate variation (the variance of the estimates for that parameter across the five implicates).