Do Imputed Earnings Earn Their Keep? Evaluating SIPP Earnings and Nonresponse with Administrative Records*

Rebecca L. Chenevert
Mark A. Klee
Kelly R. Wilkin†

November 2015

Abstract

Recent evidence suggests that labor earnings reported in household surveys compare favorably with labor earnings in administrative records. On the other hand, imputed labor earnings in household surveys seem to match labor earnings in administrative records less closely. This finding has led many researchers to question the reliability of imputed labor earnings and to exclude these observations from wage analyses. However, this strategy might result in sample selection bias if labor earnings are not missing at random. In this paper, we compare reported and imputed labor earnings from the 2008 panel of the Survey of Income and Program Participation to labor earnings from the Social Security Administration's Detailed Earnings Record. We examine how the relationship between survey data and administrative records varies across demographic groups. We also characterize survey nonrespondents in order to improve our understanding of whether and how individuals select out of response on observable dimensions. Finally, we consider implications for estimates of the earnings structure.

*We thank Adam Bee, Gary Benedetto, Molly Heller, and Jonathan Rothbaum for helpful comments. This paper was prepared for the 2015 Federal Committee on Statistical Methodology Research Conference, Washington, DC, December 1-3, 2015. This paper is released to inform interested parties of ongoing research and to encourage discussion of work in progress. The views expressed in this paper are those of the authors and not necessarily those of the U.S. Census Bureau. Any errors are our own.

†*Affiliation:* Social, Economic, and Housing Statistics Division; U.S. Census Bureau. *Address*: Social, Economic, and Housing Statistics Division; U.S. Census Bureau; 4600 Silver Hill Road; Washington, DC 20233. *Chenevert*: rebecca.l.chenevert@census.gov, (301) 763-8538. *Klee*: mark.a.klee@census.gov, (301) 763-4730. *Wilkin*: kelly.r.wilkin@census.gov, (301) 763-7728.

1 Introduction

Historically, survey data have been the main source of information about social and economic characteristics of households in the United States. Labor economists in particular have used survey data to study a variety of topics relating personal and job characteristics to wages and earnings. The Survey of Income and Program Participation (SIPP) is a longitudinal survey with a rich variety of content that provides researchers the opportunity to study a plethora of topics. The focus of the survey is measuring income and participation in government programs, and as such SIPP is of particular interest to researchers studying poverty and public policy among other topics. However, as with many household surveys, SIPP response rates are declining. And, as is also the case with many other surveys, nonresponse rates for earnings and wages are generally higher than would be desired. These questions are regarded as quite sensitive for respondents. Therefore, in this work we aim to study the earnings data of those who respond and those who do not in SIPP. We are able to do this by linking SIPP data to administrative earnings data, the Detailed Earnings Record (DER) from the Social Security Administration.

Several others have looked at similar questions before. Most closely related is the work of Bollinger et al. (2015b), who link the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to the DER. They find that nonresponse is most common in the tails of the earnings distribution, and that nonresponse patterns differ for men and women. Also very closely related is the work of Cristia and Schwabish (2009), who compare earnings from the 1996 SIPP panel to the DER and find earnings under-reported on average, and that factors positively associated with earnings are negatively correlated with measurement error. This study will look at more recent data, and in the future will also compare the redesigned SIPP data as well.

Recent evidence suggests that labor earnings reported in household surveys compare favorably with labor earnings in administrative records.[1] However, imputed labor earnings in household surveys match labor earnings in administrative records less closely. This finding has led many researchers to question the reliability of imputed labor earnings and to exclude these observations from wage analyses. However, this strategy might result in sample selection bias if labor earnings are not conditionally missing at random. In this paper, we compare reported and imputed labor earnings from the 2008 panel of SIPP to labor earnings from the DER. We

[1] For an example, see Abowd and Stinson (2013).

examine how the relationship between survey data and administrative records varies across demographic groups. Finally, we characterize survey nonrespondents in order to improve our understanding of whether and how individuals select out of response on observable dimensions.

Overall, the correlation between the survey reported earnings and administrative reported earnings is encouraging. More than 90 percent of the sample had earnings in neither or both sources, and the mean difference for the matched sample is $969. However, consistent with other studies, we find that the administrative reports of earnings tend to be a little higher on average than the survey reported data. We find that the difference in imputed survey earnings and administrative exhibits a wider variance than the analogous relationship for reported survey data, as expected.[2] We find that nonrespondents are 5.3 percentage points more likely to have positive administrative earnings data than respondents, along with other characteristics that differ between the groups. Therefore, it seems that we may not have values that are missing at random. To test the impact, we look at some basic Mincer earnings regressions and unconditional earnings gaps. Preliminary evidence supports that excluding imputed earnings could introduce bias in estimated coefficients of interest.

The rest of this paper is organized as follows: Section 2 describes the existing literature that this work complements. Section 3 describes the data used. Sections 4 and 5 describe the results of the comparison of the survey and administrative data and the analysis of nonresponse, respectively. Section 6 discusses implications of using imputed data for basic regressions of broad interest to labor economists. Section 7 concludes.

2 Literature Review

This paper builds on a well-developed literature that evaluates the quality of earnings data based on surveys via comparison to some alternate measure of survey respondents' earnings. One common validation technique measures the degree to which an employee's self-reported earnings match an employer's report of that employee's earnings. Mellow and Sider (1983) utilize this strategy in Current Population Survey (CPS) data, while Duncan and Hill (1985) use this strategy in data from a Panel Study of Income Dynamics (PSID) survey instrument for a sam-

[2] All comparisons are statistically significant at the 90 percent level. The estimates in this paper are based on responses from a sample of the population and may differ from actual values because of sampling variability or other factors. As a result, apparent differences between the estimates for two or more groups may not be statistically significant. For more information on the source of the data and the accuracy of the estimates, see http://www.census.gov/programs-surveys/sipp/tech-documentation/source-accuracy-statements.html.

ple of workers at a large manufacturing company. These studies hypothesize that firms report employees' earnings accurately, and they therefore treat any deviation of employee self-reports from employer reports as measurement error. They conclude that measurement error in earnings levels appears low on average, although this obscures larger average absolute differences between earnings reports. In addition, there is mixed evidence about whether these two sources of earnings data yield similar estimates of some aspects of the wage structure.

A second common validation technique measures the degree to which an employee's self-reported earnings match administrative records of that employee's earnings. Both Bound and Krueger (1991) and Bollinger (1998) consider survey earnings from the CPS Annual Demographic File and earnings based on payroll tax records from the Social Security Administration (SSA).[3] These studies hypothesize that firms report employees' earnings accurately for tax purposes, and they therefore treat any deviation of survey data from administrative data as measurement error. They also present evidence that measurement error explains a substantial portion of the overall variance in survey earnings. Moreover, measurement error appears to be negatively correlated with administrative earnings. If administrative earnings represent the truth, then this finding invalidates the common assumption that any measurement error in earnings is "classical". Primarily, respondents with low administrative earnings disproportionately overstate earnings in the CPS Annual Demographic File.

This second validation technique has also been applied using SIPP survey data and Social Security administrative data. Pedace and Bates (2000) explore how well earnings in the 1992 SIPP panel match SSA earnings in the Summary Earnings Record (SER). While they find that SIPP accurately estimates the number of earnings recipients, they join Bollinger (1998) in concluding that respondents at the bottom of the administrative earnings distribution tend to overstate their earnings. They also show evidence that respondents at the top of the administrative earnings distribution tend to understate their earnings, suggesting that earnings data is mean-reverting. Cristia and Schwabish (2009) provide more definitive evidence by comparing the 1996 SIPP panel and the DER. While the SER contains payroll tax records on earnings capped at the taxable maximum, the DER contains uncapped earnings data from payroll tax records. They corroborate the evidence in Pedace and Bates (2000), and also conclude that demographic characteristics that are positively correlated with earnings are negatively correlated

[3]The CPS Annual Demographic File is often referred to as the March CPS, or more recently as the CPS Annual Social and Economic Supplement (CPS ASEC).

with measurement error. Roemer (2002) also uses the DER to report that SIPP represents a respondent's percentile in the wage distribution better than it represents that respondent's wage in dollars. Gottschalk and Huynh (2010) illustrate that this finding bears important implications for estimates of inequality. They show that mean-reverting measurement error yields considerably lower estimates of inequality in SIPP data than in DER data. By contrast, they also document that the relatively strong serial correlation in measurement error yields similar estimates of mobility in SIPP and DER data.

This paper strongly resembles Pedace and Bates (2000) and Cristia and Schwabish (2009); we compare earnings data from the 2008 SIPP panel and the DER, and we consider the correlates of the deviation between these measures. However, these papers placed relatively little emphasis on the role of imputed data in explaining the difference between survey earnings and administrative earnings. Recent evidence suggests that labor earnings reported in household surveys compare favorably with labor earnings in administrative records. Abowd and Stinson (2013) argue that reported survey data and administrative data are quite similar in reliability. By contrast, they show that imputed survey earnings appear less reliable than administrative data. Based on this finding, we highlight the role of imputed data in explaining how well labor earnings in household surveys match labor earnings in administrative records.

A related well-developed literature evaluates the quality of imputed earnings data. Like other Census Bureau surveys, SIPP imputes missing data using a "hot-deck" procedure which assigns to the nonrespondent data reported by a "donor" with similar demographic characteristics. This imputation method assumes that earnings data are missing at random, conditional on the characteristics used to match nonrespondents to donors. One important disadvantage of this procedure is that the curse of dimensionality limits the set of characteristics or the values of these characteristics that may be used to match nonrespondents to donors. If some determinant of nonresponse is omitted from the match criteria, earnings estimates will be biased. Moreover, earnings estimates might be biased even if earnings are conditionally missing at random. Hirsch and Schumacher (2004) demonstrate that if some observable characteristic such as union status is not used to match earnings nonrespondents to donors, then coefficient estimates on this characteristic in a wage equation will be attenuated. Relatedly, Bollinger and Hirsch (2006) illustrate that if nonrespondents are matched to donors according to grouped categories of some characteristic, such as education, then coefficient estimates on more detailed measures of this characteristic such as years of education in a wage equation will be attenuated.[4] These two forms of "match bias" often motivate proposals for more parametric, model-based imputation methods. However, Andridge and Little (2010) argue that hot-deck imputation methods perform relatively well along various dimensions compared to model-based imputation methods.

Empirical researchers have pursued various strategies to remove or mitigate match bias and response bias. One common approach is to exclude imputed earnings values from analyses. Bollinger and Hirsch (2013) examine the validity of this technique, and they conclude that omitting imputed earners from OLS wage equations is generally sufficient to avoid major bias in slope estimates. Bollinger et al. (2015b) revisit the question of whether response bias is ignorable by investigating the pattern of nonresponse over the earnings distribution conditional on covariates. They report a U-shaped pattern of nonresponse, implying that response bias is ignorable over most of the distribution, with the exception of the tails. Bollinger et al. (2015a) establish that this nonignorable response bias causes CPS ASEC to understate inequality measures relative to DER data. Hokayem et al. (2015) utilize a second strategy by exploiting administrative data to evaluate the impact of earnings nonresponse on official poverty estimates. They derive a "full response" measure of poverty by assigning nonrespondents' earnings from DER data, accounting for both the likely deviation of survey from administrative earnings and the likely earnings differences among those who can and those who cannot be matched to administrative data. They find evidence that nonresponse leads CPS ASEC to understate the poverty rate by about one percentage point.

3 Data

Next we describe the data sources that we utilize. We begin by detailing in isolation the Survey of Income and Program Participation in section 3.1 and the Detailed Earnings Record in 3.2. We then discuss the linked dataset.

3.1 Survey of Income and Program Participation

The SIPP is a panel study program that began in 1984. SIPP collects data and measures change for many topics, including economic well-being, family dynamics, education, assets, health

[4] While more recent research tends to argue that imputed earnings are unreliable, David et al. (1986) conclude that hot-deck imputed earnings perform favorably relative to both model-imputed earnings and administrative earnings in IRS data.

insurance, childcare, and food security, following respondents for a panel of roughly three to four years. While there have been many changes to the survey over time, the core principle of measuring the dynamics of these topics has remained the same. SIPP has undergone two major redesigns in its time. The first redesign was with the 1996 panel. This changed the structure of the program from having overlapping panels that began different years to having one panel at a time. The 1996 Panel also marked the change from a paper survey instrument to a Computer Assisted Personal Interview (CAPI) survey. There was a somewhat minor redesign with the 2004 panel, which further leveraged the CAPI functionality and increased the use of dependent data.

Since the 1996 redesign, there have been four panels beginning in 1996, 2001, 2004, and 2008. Respondents were surveyed every four months, called waves, and these surveys consisted of two parts. The first part of the survey is the "core," which asks about the same topics every wave. This includes questions on income from all sources for each month, as well as information about changes in household composition, employment, and other topics. The second piece is a "topical module." Topical modules can be either periodic or once per panel. Topical modules can range from asking questions about lifetime fertility or employment history to questions about assets and liabilities, commuting and work schedule, or retirement savings and pensions.[5]

In this paper, we use the data from the core relating to earnings from a job or self-employed business. SIPP collects detailed data on up to two jobs and two businesses per wave. We also include severance pay and earnings from "moonlighting," which are collected separately. We aggregate earnings for the calendar year, which are collected in three or four different waves (depending on rotation group).[6]

The SIPP reports an "allocation" status flag associated with almost all variables in the survey.[7] In the 2008 panel, these allocation flags indicate one of several statuses: a reported value, a value imputed with a hot-deck method, logical imputation, or an imputation using a previous wave's data.[8] These flags allow users to identify unit nonrespondents. Among item nonrespondents, the imputation method depends on the availability of earnings data from the previous

[5]The complete list of topical modules in the 2008 panel is available at www.census.gov/programs-surveys/sipp/tech-documentation/topical-modules/topical-modules-2008.html

[6]In the 2008 panel, SIPP divided the sample into four groups which are interviewed on a rotating basis called "rotation groups." For example, in 2009, the rotation group 1 wave 3 interview was in May about the preceding 4 months (January through April 2009). Rotation group 2 was then interviewed in June about the preceding four months, so wave 3 refers to February through May 2009 for this group.

[7]Exceptions are recodes, which are transformations of other variables, and those that have no editing.

[8]Cold-deck imputation substitutes a value selected by the data editor, not reported data. Cold-deck imputation is not a method that is commonly used, and is not used in the variables of interest for this analysis.

month and the likely veracity of any reported earnings data. A hot-deck imputation method substitutes the data of a responder to fill in for an item with nonresponse, where the donor is matched on several observable characteristics. If earnings on a job or business from the prior month are available, those earnings are included among the match criteria. If the data-generating process for earnings were known, and if all aspects of this process could be incorporated appropriately into the algorithm matching donors to recipients, then this imputation technique would predict earnings exactly. However, the data generating process for earnings is largely unknown, and the curse of dimensionality limits the observable characteristics that feasibly may be incorporated into the matching algorithm. Finally, if earnings from a job are unusually high or if reported earnings are $0 but there is strong reason to believe that the respondent actually earned a positive amount, new earnings data are imputed logically. This logical imputation process assigns earnings to be implied by either the hourly pay rate and hours worked during the month, a reported annual pay rate, or weeks spent away from a job without pay. Logical imputation only occurs if that job has no earnings data available from a previous month.[9]

While the output file is in a person-month format, it is common for all four monthly values for a single wave have the same allocation flag for a respondent. As we are aggregating the data up to the annual level, waves can cross over a calendar year. For the analyses in section 4 of this paper, we classified a person-year observation as imputed in a particular way if any month in that year is imputed. While similar analyses were conducted using the number of imputed months as the independent variable of interest, these results are not reported as they did not show substantive differences.[10]

A commonly overlooked subtlety of SIPP data is that the allocation flags do not identify all imputed data. Each respondent also has a person-level interview-status flag. As this is a person-level variable, it is constant within each wave.[11] This flag indicates whether the survey information was obtained from the respondent himself, from a proxy, or whether the person was a noninterviewed person in a responding household known as "Type Z." A Type-Z individual has all of their data imputed. Similarly, nonrespondents can have all labor force data imputed if a respondent opts out of the entire employment section. We consider both types of individuals to be unit nonrespondents. Among unit nonrespondents, the imputation method depends on the

[9] Refer to U.S. Census Bureau (2001) for an in-depth discussion of these imputation methods.

[10] Estimates are available from the authors upon request.

[11] Note that this will be more transparent in the 2014 redesign, as the allocation flags will identify every imputed value.

availability of employment data from the previous interview. For new sample members and for individuals whose previous wave data do not imply that the respondent was working at the beginning of the reference period, all labor force data including earnings were imputed from a single donor with similar observable characteristics. For individuals whose previous wave data imply that the respondent was working at the beginning of the reference period, labor force data from the previous wave were imputed longitudinally by projecting through the current interview.

The second major redesign takes effect with the 2014 panel. In this paper we focus on the 2008 panel, but in future work we plan to compare the earnings data in a similar fashion as reported here for the 2014 panel. The 2014 redesign increased the recall period from four months to slightly over a calendar year. It also involves changing the structure of the survey instrument and using an Event History Calendar (EHC) to help aid memory. There will no longer be separate core or topical modules to the survey; all questions are in each wave of the panel.

3.2 Detailed Earnings Records

The Detailed Earnings Records (DER) are provided to the Census Bureau by the Social Security Administration. The DER include wage and salary earnings (Box 1), both deferred and nondeferred earnings, and self-employment earnings reported to the IRS.[12] These earnings are not capped at the taxable maximum. The data are provided at the level of one observation per person, year, and job (W-2) or business (1040-SE). We aggregate both deferred and nondeferred earnings from all jobs and businesses to create a total for the person for each year. If someone had self-employment earnings in either the DER or the SIPP, we classify that person as self-employed in the person-year analysis.

The DER data are processed by the Census Bureau and linked to surveys using a Personal Identification Key (PIK). When using survey data linked to administrative data, there are several caveats of which one must be aware. For example, errors in amounts from the administrative data are likely not from the same sources that we think are typical for survey responses. For example, regression to the mean, or the tendency to report closer to the mean than one's actual earnings, is commonly cited as an error prevelant in survey literature. However, there are still likely to be systematic differences between those for whom data are available and those for

[12]The wage and salary earnings recorded in DER stem from both regular sources and irregular sources such as tips, to the extent that these irregular earnings are reported on the W-2.

whom they are not. We expect that those with missing PIK information are different than those without, in ways that are observable to the researcher as well as ways that may not be. There is also the possibility of other types of errors such as clerical or matching errors, which may be difficult to detect.

While many respondents are matched to administrative records, there are differences between those that match and those that do not. Bond et al. (2014) find mobility, lower education, poor English-speaking ability, nonemployed, noncitizens, nonparticipants in programs and minorities are all predictive of those that are not able to match to administrative records. All of the results presented below should be viewed with the caveat that these groups are under-represented in the sample we study. We treat those who have a valid PIK but no record in the DER as having zero earnings in the administrative data.

3.3 SIPP and DER Linked Data

From the SIPP, we aggregate reported earnings data from those who report that they were employed from all earnings sources by year. SIPP respondents can report earnings data in a number of ways. Those with a job for an employer are encouraged to report in the way that is easiest for them to report gross earnings, and these earnings are tied to each job (up to two per wave). Those who are self-employed report their earnings as well as their share of profits for the previous four months, which are spread equally across the weeks that the business was held. However, if someone has only self-employment income of less than $400, we recode that to $0 because self-employment earnings under $400 are not required to be reported for tax filing.[13]

For those who report moonlighting earnings, we do not know with certainty whether these earnings should be classified as wage/salary or self-employment, so we do not treat those with moonlighting earnings as self-employed unless they also report a business or have self-employment earnings in the administrative data.

Table 1 shows the matched SIPP and DER sample. Person-year observations with either zero earnings in both sources or positive earnings in both sources make up 91.9 percent of our sample. Table 2 shows the average unconditional differences in SIPP and DER earnings. The first column shows the mean of DER-SIPP earnings for the whole sample and the sample restricted to those with positive amounts. The second column shows the same information for

[13]Losses are included on the 1040, which is outside the scope of this project. Therefore, losses below any earnings from work for an employer are also recoded to zero earnings.

the absolute value of the difference. The magnitude of the differences and the characteristics correlated with larger differences are explored in the next section.

4 Benchmarking

We begin benchmarking SIPP earnings to DER earnings by describing the data graphically in section 4.1. This allows us to compare data from these two sources overall, by proxy response status, and by imputation status. We then proceed to regression analysis in section 4.2, which produces estimates of average deviations by proxy response status and by imputation status, holding observable characteristics constant. We also address the question of which types of individuals have survey earnings that deviate more from administrative earnings on average.

4.1 Graphical Analysis

Before we analyze the relationship between SIPP earnings and DER earnings conditional on observables, it is important to begin by describing the unconditional version of this relationship. Figure 1 offers a first glance at how well these data sources compare by depicting a scatterplot of SIPP earnings by DER earnings. The sample for this figure is all person-years for individuals aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the year. For ease of visualization, we plot only a random 15 percent of this sample and we additionally narrow our focus to the set of individuals with both SIPP earnings and DER earnings between \$20,000 and \$85,000.[14] The primary takeaway from this figure is that the bulk of the joint distribution lies within a band around the 45-degree line, where DER earnings equal SIPP earnings.[15] A secondary, yet still salient, inference to draw from this figure is that data points outside of this band are more likely to have DER earnings in excess of SIPP earnings than vice versa. A final important lesson to take from this figure is that a nontrivial minority of data points lie relatively far from the 45-degree line.

Figure 2 presents the same unconditional relationship between SIPP earnings and DER earnings in a different format. This figure depicts a histogram for the sample of all person-years for individuals aged 15 and older, who were assigned a PIK, and who were present in the survey for

[14]Note that for this and all other scatterplots we have perturbed each data point by adding spherical random noise in order to avoid disclosing federal tax information. We have also examined the uncensored version of each of these scatterplots. No systematic difference between these sets of figures was apparent.

[15]Note that all comments on scatterplots in this section represent untested observations about our sample. Consequently, the apparent trends that we highlight might not be statistically significant.

all 12 months of the year. Figure 2 places person-years into bins according to the integer portion of the difference between DER earnings and SIPP earnings, in thousands of dollars. Thus, person-years in the "-1" bin have SIPP earnings greater than DER earnings by between \$1,000 and \$1,999. The person-years in the "0" bin have either SIPP earnings greater than DER earnings by no more than \$999 or DER earnings greater than SIPP earnings by no more than \$999. The person-years in the "1" bin have DER earnings greater than SIPP earnings by between \$1,000 and \$1,999. Person-years with SIPP earnings in excess of DER earnings by \$10,000 or more are located in the leftmost bin, while person-years with DER earnings in excess of SIPP earnings by \$10,000 or more are located in the rightmost bin. The same three inferences that Figure 1 makes apparent also materialize in Figure 2. First, 69.3 percent of the sample has DER earnings within \$5,000 of SIPP earnings. Second, of the remaining 30.7 percent of the joint distribution, 25.1 percent is characterized by DER earnings in excess of SIPP earnings. Finally, 18.8 percent of the sample has DER earnings outside of a \$10,000 band around SIPP earnings.

One might wonder how the basic relationship illustrated in Figures 1 and 2 depends on the source of the survey earnings data. For example, analysts often hypothesize that proxy response affects data quality. If a household member is absent at the interview, SIPP allows another household member who is present at the interview to answer on the absentee's behalf. This form of data collection is known as a proxy interview. Proxy respondents might have relatively poor knowledge of other household members' earnings.[16] To gauge the degree to which proxy interviews influence facets of the unconditional relationship between administrative earnings and survey earnings, Figure 3 plots this relationship by proxy interview status. The panel on the left displays SIPP earnings and DER earnings for individuals who experienced no months of proxy response during the year, while the panel on the right displays the corresponding relationship for individuals who experienced at least one month of proxy response during the year. The two portions of Figure 3 generally appear surprisingly comparable given the degree of concern that is often expressed about the quality of proxy-reported data. Nevertheless, two characteristics of this figure suggest that proxy-reported survey data correspond less well to administrative data than self-reported survey data do. First, while both panels illustrate that the bulk of the distribution lies within a band around the 45-degree line, the associated bandwidth appears slightly larger for individuals with at least one month of proxy interview during the year. Second, DER earnings outside of this band appear to exceed SIPP earnings with higher

[16] See Bollinger and Hirsch (2009).

frequency for individuals with at least one month of proxy interview during the year.

Imputation is another common source of survey earnings data which analysts often hypothesize affects data quality. If a survey member declines to provide any data in general or earnings data in particular, Census Bureau surveys fill in the missing data in order to salvage that observation for use in analysis. SIPP uses a nonparametric matching algorithm to identify a survey member with similar observables who did report earnings, and then assigns the nonrespondent's earnings to be the amount reported by this "donor." These limitations have given rise to a natural concern that imputed earnings might deviate considerably from true earnings. Consequently, analysts commonly exclude imputed earnings from their analyses.

To gauge the validity of this concern, Figure 4 plots the unconditional relationship between administrative earnings and survey earnings by imputation status.[17] The panel on the left displays SIPP earnings and DER earnings for individuals who experienced no months of imputed earnings during the year, while the panel on the right displays the corresponding relationship for individuals who experienced at least one month of imputed earnings during the year. The two portions of Figure 4 generally appear surprisingly comparable at relatively low earnings levels given the degree of concern that is often expressed about the quality of imputed data. Nevertheless, expanding our focus to higher earnings levels reveals a relatively high frequency of data points for which imputed SIPP earnings deviate substantially from DER earnings.

Figure 5 presents the same unconditional relationship between administrative earnings and survey earnings by imputation status in a different format. In particular, this figure separately plots the univariate kernel density estimates of the difference between DER earnings and SIPP earnings by imputation status. Our sample for this estimation is all person-years for individuals aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months, and whose survey earnings differed from administrative earnings by no more than $100,000 in absolute value. Points to the right of the "0" mark indicate that DER earnings exceed SIPP earnings, while points to the left of the "0" mark indicate that SIPP earnings exceed DER earnings. The red, dashed line in this figure plots the kernel density for individuals who had no months of imputed data during the year, while the blue, solid line plots the kernel density for individuals who had at least one month of imputed data during the year. Two salient points

[17] Like the other scatterplots described in this section, Figure 4 displays data for the sample of all person-years aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months. This figure differs from the other scatterplots in this section, as it includes data points for a random 25 percent rather than a random 15 percent of this sample.

emerge from this figure. First, the distribution of DER-SIPP differences for individuals with nonimputed survey earnings has more mass located around "0" than does the analogous distribution for individuals with imputed survey earnings. Second, the distribution of differences for individuals with imputed survey earnings has more mass at relatively large amounts than the analogous distribution for individuals with nonimputed survey earnings. This difference is especially visible for negative amounts, which suggests that imputed survey earnings are more likely to overstate administrative earnings by a relatively large amount than reported survey earnings are. These apparent differences are statistically significant, as a Kolmogorov-Smirnov test rejects the null hypothesis that the distribution of differences for imputed survey earnings equals the corresponding distribution for nonimputed survey earnings.[18]

4.2 Regression Analysis

Given the unconditional patterns of DER-SIPP earnings differences established in Section 4.1, we now investigate the effect of non-response on annual earnings estimates in greater detail. In general, our econometric specification takes the form

$$d_{it} = \alpha + \beta Z_{it} + \gamma NR_{it} + u_{it}, \tag{1}$$

where d_{it} is the difference in annual earnings reported in SIPP and the DER for respondent i in reference year t; Z_{it} is a vector of person-level characteristics in year t, including demographics, education, region, English-speaking ability, citizenship status, an indicator for children in the family, metropolitan area size, and an indicator of receipt of any means-tested transfer income; NR_{it} is an indicator of nonresponse; u_{it} is a normal error term; and α, β, and γ are parameters to be estimated.

In what follows, we define d_{it} as both the raw difference between SIPP and DER annual earnings ($DER_{it} - SIPP_{it}$) and the absolute value of that difference ($|DER_{it} - SIPP_{it}|$). Positive (negative) cofficient estimates in the raw-difference specifications measure the degree to which regressors on average are associated with DER earnings in excess of (less than) SIPP earnings, whereas the absolute-deviation specifications estimate the overall magnitudes by which the two earnings measures differ. Further, we use several proxies for NR_{it} to include various types of item and unit nonresponse.

[18]The p-value of this test is 0.000.

Many studies, such as Cristia and Schwabish (2009) and Pedace and Bates (2000), frame this type of analysis as an investigation of measurement error in earnings data, defining administrative earnings as truth. More recent investigations such as Abowd and Stinson (2013) and Hokayem et al. (2015) maintain a more agnostic stance on whether reported survey earnings or administrative earnings better reflect truth. We adhere to the latter interpretation when reviewing the estimates of β from equation 1. As such, our results speak to the implications of increasingly prevalent proposals to replace self-reported survey earnings with administrative earnings in an attempt to reduce respondent burden. Abowd and Stinson (2013) also argue that imputed survey earnings are less reliable than administrative earnings. Based on their evidence, we interpret our estimates of γ from equation 1 as being correlated with the average noise in imputed earnings, but not as indicating the average noise in imputed earnings exactly.

Table 3 presents estimates of equation 1 where NR_{it} is measured as having any Census imputation as part of total SIPP annual earnings.[19] Nonresponse is estimated to have two very different effects depending on how one defines the deviation in earnings. Column 1 lists the results when the dependent variable is the raw difference. The estimate of γ shows SIPP earnings in excess of DER earnings by about \$1,900 more on average for individuals with at least one month of imputed earnings than for those with no earnings imputation. Column 2 lists the results when the dependent variable is the absolute difference. The estimate of γ in this column, however, indicates that SIPP and DER earnings differ by about \$6,900 more on average for individuals with at least one month of imputed earnings than for those with no earnings imputation. While DER earnings differ from SIPP earnings significantly on average when we examine the absolute difference, these deviations counterbalance to a large extent. Consequently, DER earnings are relatively close to SIPP earnings on average when we examine the raw difference.

Table 3 also describes individuals whose survey earnings differ more from administrative earnings, holding nonresponse constant. Like Cristia and Schwabish (2009), we conclude that individuals who are male and more educated tend to have administrative earnings in greater excess of survey earnings. Column 1 also establishes the correspondence of SIPP and DER earnings by race, ethnicity, and nativity. Foreign-born citizens tend to have administrative earn-

[19]The incidence of nonresponse and the correspondence between survey and administrative earnings that we document likely depend upon the particulars of SIPP survey data and DER administrative data to a great extent. Consequently, we question how generalizable our results would be to the nation at large. Instead, our population of interest is participants in the 2008 SIPP panel who have been linked to DER data. Accordingly, we do not apply sample weights to draw inferences about the nation as a whole. Neither do we account for the complex sample design of SIPP in estimating standard errors.

ings in greater excess of survey earnings relative to native-born individuals on average. For populations of particular interest to many SIPP users, individuals who speak a language other than English in the home and individuals who receive means-tested transfer income tend to have survey earnings in greater excess of administrative earnings on average. Despite these correlations, the relatively low R^2 reveals that very little of the variability in the difference between SIPP earnings and DER earnings is explained by variability in our observables. Column 2 shows that individuals who are men, married, more educated, and parents of children under 18 exhibit greater average absolute deviations of survey data from administrative data than other individuals do. On the other hand, individuals who receive means-tested transfer income display smaller average absolute deviations of survey data from administrative data than other individuals do.

Tables 4 and 5 investigate the deviation of imputed survey earnings from administrative earnings in greater detail. Specifically, the model estimated for Table 3 imposes the assumption that unit nonrespondents and item nonrespondents experience the same average deviations of survey earnings from administrative earnings. Table 4 relaxes this assumption. Column 1 shows that survey earnings are not statistically different from administrative earnings for unit nonrespondents on average relative to individuals who reported earnings. By contrast, SIPP earnings exceed DER earnings by about $2,900 more on average for item nonrespondents than for individuals who report earnings. Column 2 illustrates that the imputation process serves to increase the absolute difference between survey earnings and administrative earnings for both unit nonrespondents and item nonrespondents.

Table 5 further relaxes the assumption that all unit nonrespondents and all item nonrespondents experience the same deviations of survey earnings from administrative earnings. Indeed, SIPP pursues different methods of imputing missing earnings data for different types of unit and item nonrespondents. Type-Z individuals have all of their labor force data assigned from a single, contemporaneous donor record. Labor force data are imputed longitudinally for other unit nonrespondents by projecting data from the individual's previous interview through the current interview. Missing earnings items are imputed via a hot deck that assigns earnings to be the amount reported by a donor with similar observable characteristics. If earnings on a job or business from the prior month are available, earnings for the current month are assigned from a donor who had similar earnings last month. However, the item nonrespondent's earnings from the prior month may also have been imputed based on the prior month's earnings. We expect the quality of data imputed according to this technique to depend on whether this string of imputed data was based on reported data, hot-deck imputed data, or logically imputed data initially. Finally, logical imputation uses data reported elsewhere in the survey to enforce logical consistency and fill in items that are missing.

Column 1 of Table 5 demonstrates the considerable heterogeneity across types of imputation in the degree of concordance between survey and administrative earnings. Among unit nonrespondents, individuals who had no prior wave labor force data available display administrative earnings about \$2,240 in greater excess of survey earnings on average than individuals who reported earnings data. By contrast, unit nonrespondents who had prior wave labor force data available display survey earnings about \$3,050 in greater excess of administrative earnings on average than individuals who report earnings data. Among item nonrespondents, standard cross-sectional hot-deck imputation is associated with the largest estimated magnitude of the average DER-SIPP gap. Individuals with at least one month of hot-deck imputation display survey earnings about \$6,625 in greater excess of administrative earnings on average than individuals who report earnings data. Relative to individuals with no months of imputed earnings data during the year, administrative earnings are about \$3,515 greater than survey earnings on average for individuals who experienced at least one month of hot-deck imputation based on prior month data and initially based on earnings that were imputed from a cross-sectional hot deck. Some methods of imputation do produce survey earnings that correspond better to administrative earnings. For example, person-years with at least one month of imputation based on prior month data and initially based on reported earnings exhibit a similar DER-SIPP gap to person-years with no months of imputed earnings.

Column 2 shows that there is also considerable heterogeneity across types of imputation in the degree to which imputation increases the average absolute deviation of survey earnings from administrative earnings. Individuals who experienced at least one month of any type of imputation, with the exception of Type-Z imputation, exhibit greater average absolute differences than individuals who experience no months of imputation during the year. At one extreme, individuals who experience at least one month of logical imputation have survey earnings that differ from administrative earnings by about \$388 more in absolute value than do individuals who experienced no months of imputation. At the opposite extreme, individuals who experienced at least one month of hot-deck imputation based on prior month data and initially based on data that were hot-deck imputed cross-sectionally have an average DER-SIPP earnings gap that is about \$7,322 larger in absolute value than individuals who experienced no months of imputation.

The specifications presented in Tables 3, 4, and 5 include individuals with no annual earnings in either SIPP or DER in order to describe comprehensively how these data sources compare. Often, analysts consider only the population of workers when studying the determinants of earnings. To characterize how SIPP earnings compare to DER earnings for this population, Table 6 additionally restricts the estimation sample to individuals who have both positive SIPP earnings and positive DER earnings. A comparison of column 1 in Tables 5 and 6 reveals that restricting our attention to the sample of workers does affect the qualitative inferences that we can draw about how well SIPP and DER compare on average. First, we now fail to reject the null hypothesis that the DER-SIPP gap is the same for individuals who experienced at least one month of logical imputation and individuals who experienced no months of imputation. Second, the coefficient estimate on imputation based on prior-month data and initially based on reported data is now statistically signficant. In addition, the coefficient estimate on imputation based on prior-month data and initially based on logically imputed data is now statistically significant and positive.

A comparison of column 2 in Tables 5 and 6 reveals that restricting our attention to the sample of workers also affects the qualitative inferences that we can draw when the dependent variable is the absolute difference between SIPP and DER earnings. We now fail to reject the null hypothesis that the average absolute difference between DER and SIPP is different for individuals with no months of imputed data and individuals with either any month of logically imputed data or any month when all labor force data were imputed longitudinally. On the other hand, individuals with any month of Type-Z imputed data now exhibit an absolute difference of SIPP and DER earnings which is about $1,313 larger on average relative to individuals with no months of imputed data. Finally, individuals who experience any month when earnings are hot-deck imputed cross-sectionally now exhibit an average DER-SIPP gap which is about $1,861 smaller in absolute than individuals who experience no months of imputed earnings.

Selecting our sample to include only workers allows us to introduce details about individuals' labor market situation to the set of explanatory variables Z_{it}. Consequently, we learn that self-employed individuals' earnings in SIPP overstate DER earnings by about $11,713 more on average relative to individuals who are not self-employed. This result is consistent with either of two hypotheses. First, surveys might measure self-employed earnings relatively poorly as Pedace and Bates (2000) and others argue. Alternatively, tax records might measure self-employed earnings relatively poorly. Although the average raw difference between SIPP and DER earnings is quite large for self-employed individuals, column 2 reveals that this estimate does not differ statistically in magnitude from the average absolute difference. This suggests that SIPP earnings tend to overstate DER earnings for self-employed individuals to a much greater extent than DER earnings tend to overstate SIPP earnings. By contrast, the average raw difference is considerably smaller in magnitude than the average absolute difference for many highly educated individuals. Indeed, survey earnings and administrative earnings on average disagree more in absolute for individuals with a professional degree than they do for self-employed individuals.

Finally, we explore the extent to which imputed earnings influence the average difference between survey earnings and administrative earnings by demographic groups. To that end, the specifications presented in Tables 6 and 7 differ only because the specifications in Table 7 also restrict the estimation sample to include only individuals who displayed no months of imputed survey data during the year. Many of the key observations from Table 6 remain in Table 7, which suggests that these patterns do not exist solely in imputed data. Column 1 shows that on average individuals who are male; more educated; parents of children under 18; and proxy respondents have reported DER earnings in greater excess of SIPP earnings than other individuals do. We also find that self-employed individuals have SIPP earnings in greater excess of DER earnings on average than workers for an employer do. Column 2 shows that individuals who are male; more educated; married; self-employed; parents of children under 18; and proxy respondents have greater average absolute differences between SIPP earnings and DER earnings than other individuals do.

5 Predictors of Earnings Nonresponse

In the previous section, we illustrated that imputed labor earnings in survey data resemble labor earnings in administrative data worse than reported labor earnings in survey data do on average. If this deviation of imputed survey data from administrative data reflects measurement error, one natural strategy for mitigating bias is to exclude imputed earnings data from analyses. However, this strategy is poorly suited to some analyses, such as when statistical power is an especially acute concern. Moreover, Bollinger et al. (2015a,b) argue that nonrespondents disproportionately fall in the tails of the administrative earnings distribution, which suggests that excluding imputed earnings data might bias estimates to some degree. In this section, we explore the pattern of earnings nonresponse with the aim of understanding the implications of the decision to include imputed earnings data.

To begin, Table 8 summarizes the likelihood of nonresponse, both overall and within demographic groups. For people aged 15 and older, 14.9 percent of person-months exhibit any nonresponse. Table 8 also decomposes this overall nonresponse into the unit nonresponse rate and the item nonresponse rate. Unit nonresponse occurs for 5.8 percent of person-months for people aged 15 and older. For our purposes, unit nonrespondents either do not answer any question in the survey or do not answer any question in the survey about their employment situation. All employment data are imputed for these individuals. Workers provided some information about their employment situation but did not answer questions about earnings for 15.4 percent of person-months according to Table 8. Note that this definition of the item nonresponse rate excludes individuals who did not work and therefore received no earnings questions. Alternatively, the item nonresponse rate for earnings is 9.6 percent if we classify nonemployed individuals as reporting earnings of $0.

The nonresponse rates by demographic group listed in Table 8 suggest characteristics of individuals who are more likely to lack earnings data on average. To investigate further who does not respond, Table 9 presents the results of regressions with the following form:

$$NR_{im} = \zeta + \delta X_{im} + \eta_{im} \tag{2}$$

In equation 2, NR_{im} indicates earnings nonresponse for individual i in month m, X_{im} is a set of observable characteristics, ζ is a constant, and η_{im} is a normally distributed error term.[20] We estimate the model via ordinary least squares.

If earnings data are indeed noisier for imputed values than for reported values conditional on observables as the previous section suggests, the β coefficients shed light on who is likely to exhibit more mismeasured earnings on average. When considering a regression of a mismeasured dependent variable on a set of explanatory covariates, analysts typically assume that the measurement error in the dependent variable is uncorrelated with the explanatory covariates. In

[20] Recall that at each interview, individuals provide details about each of the preceding four months. Consequently, a natural unit of observation for the regression given by equation 2 is the person-month. However, 97.2% of person-wave observations exhibit earnings data that are missing for either no month in that wave or every month in that wave. Rather than collapse our regressions to the person-wave level, we use the person-month as the unit of observation and account for correlations at the person-wave level by clustering standard errors.

this case, the coefficient estimates on the explanatory variables would be unbiased. If instead the measurement error in the dependent variable is correlated with the explanatory covariates, the coefficient estimates on these explanatory variables would be biased. Thus, the β coefficients are our parameters of interest, as they point to coefficient estimates which might be biased in models that include observations with imputed earnings.[21]

Column 1 of Table 9 contains the estimation results of the model given by equation 2, where the dependent variable indicates any earnings nonresponse. Our sample for this regression is all individuals aged 15 and older. Many characteristics appear to have a statistically significant relationship to earnings nonresponse owing to our very precisely estimated coefficients. Among those characteristics with larger point estimates, we find that individuals who are better educated and male are more likely to lack earnings data on average. Household structure also appears related to nonresponse, as individuals who reside in larger households or who have no children under age 18 are also more likely to lack earnings data. Perhaps because our sample treats individuals who report no employment as reporting \$0 in earnings, individuals who report any receipt of means-tested transfer programs are 6.4 percentage points less likely to lack earnings data. Finally, incorporating information about interview status seems to offer insight into the likelihood of earnings nonresponse. The use of proxy interviews appears to be especially effective at inducing responses to earnings questions, as nonresponse is 8.1 percentage points less likely for person-months characterized by proxy response. Individuals who leave the survey but eventually return are 4.5 percentage points more likely to lack earnings data while in the survey. Similarly, individuals who leave the survey and never return are 7.4 percentage points more likely to lack earnings data before they leave the survey. These findings suggest that efforts to interview households that are marginally attached to the survey might be ineffective at reclaiming earnings data, even if they are effective at reclaiming other data. Despite the correlations discussed above, the variation in these observables appears to explain relatively little of the variation in nonresponse, as R^2 is relatively low for all four columns of Table 9.

Columns 2 through 4 of Table 9 probe the results presented in column 1 by examining the likelihood of unit nonresponse and item nonresponse separately. The dependent variable in column 2 indicates unit nonresponse. Our sample for this regression is all individuals aged 15 and older. The pattern of unit nonresponse in this sample strongly resembles the pattern of any

[21]For this purpose, the correlations that we document in Table 9 need not bear a causal interpretation. We make no claims about which mechanism mediates the correlations between these observable characteristics and earnings nonresponse.

earnings nonresponse documented in column 1.[22] For example, unit nonrespondents in SIPP data are more likely to be male and residing in larger households. However, the pattern of unit nonresponse does deviate from the pattern of any nonresponse in several notable ways. First, unit nonresponse appears to be nonmonotonic rather than generally increasing in education. Unit nonrespondents in SIPP data are more likely to have a high school degree only or an Associate's degree only. Second, individuals who received any means-tested transfer payments are only 0.7 percentage points less likely to be unit nonrespondents.

Column 3 of Table 9 lists the results when the dependent variable indicates item nonresponse. Our sample for this regression is all individuals aged 15 and older who provided at least some labor force data. The pattern of item nonresponse in this sample strongly resembles the pattern of any earnings nonresponse along some dimensions. For example, individuals who are male, better educated, and non-recipients of means-tested programs are more likely to respond to some labor force questions but not to earnings questions. However, the pattern of item nonresponse does deviate from the pattern of any nonresponse in several notable ways. First, household structure appears to contribute little to the pattern of any nonresponse established in column 1. Individuals residing in larger households do not appear more likely to suffer from item nonresponse, while individuals with no children are only 1.7 percentage points more likely to suffer from item nonresponse. Additionally, the details of survey participation appear to predict item nonresponse differently than they predict earnings nonresponse overall. While column 1 of this table showed that proxy responses are effective at reducing earnings nonresponse overall, column 3 shows that proxy respondents are 3.7 percentage points more likely than own responses to provide some labor force data but no earnings data. Similarly, eventual attritors are only 2.0 percentage points more likely to be item nonrespondents while in the survey.

Analysts often restrict their estimation samples to include only employed individuals. To

[22] We recommend caution when interpreting estimates of models that feature unit nonresponse as the dependent variable. Bollinger et al. (2015b) explicitly exclude unit nonrespondents from their analysis because neither earnings nor many basic observable characteristics are observed for a unit nonrespondent in CPS ASEC data. Instead, the missing data are replaced with data provided by a single donor with observable characteristics similar to those characteristics that we can observe for the unit nonrespondent. Several facets of SIPP mitigate the impact of this problem for our analysis. First, some individuals whom we treat as unit nonrespondents did provide some demographic data despite declining to provide any labor force data. Second, for the remaining unit nonrespondents, U.S. Census Bureau (2001) details how the longitudinal nature of SIPP enables the imputation of some characteristics such as sex, race, and age by inferring these characteristics from previous responses. For time-varying variables, we conservatively do not interpret the coefficient estimates in column 2 of Table 9 as indicating characteristics of individuals who are more likely to decline to provide all labor force data. We also advise caution in interpreting the results presented in column 1 of Table 9, as the indicator for any nonresponse takes value 1 for unit nonrespondents. Nevertheless, these analyses remain insightful for an investigation of the (potentially imputed) characteristics of individuals whose survey earnings resemble administrative earnings less closely on average.

study the estimated patterns of nonresponse in this population, the specification in column 4 of Table 9 builds on the one in column 3 by excluding individuals who report no employment in SIPP data. This sample selection criterion enables the inclusion of covariates that describe respondents' jobs and businesses. Redefining the sample changes the inferences from column 3 in three notable ways. First, better educated individuals are no longer more likely to be earnings nonrespondents. Second, Black, non-Hispanics are now the most likely racial and ethnic group to be item nonrespondents. Third, program recipients are no longer less likely to be earnings nonrespondents, which suggests that they are relatively more willing to report no employment than they are to report earnings conditional on employment. Including characteristics of jobs and businesses among the covariates allows us to draw several new inferences about who is more likely to provide some labor force data but still lack earnings data. The most stark finding is that self-employed individuals are 19.8 percentage points more likely than workers for an employer to be item nonrespondents. Another marked effect is that "contingent" workers are 12.9 percentage points more likely to be item nonrespondents than individuals who have a regular work arrangement. Third, more weeks worked per month or more worked hours per week are both positively associated with item nonresponse. Finally, individuals who stopped work during a particular wave are more likely to be item nonrespondents.

Item nonresponse as referenced in columns 3 and 4 of Table 9 may come from various sources. We consider total earnings to be imputed if any of its components is missing. Table 10 investigates the pattern of item nonresponse by considering how the relationship between observable covariates and item nonresponse varies across earnings sources.[23] To that end, Table 10 presents the results of regressions with the following form:

$$NR_{ijm} = \zeta + \beta X_{ijm} + \eta_{ijm} \tag{3}$$

In the equation above, NR_{ijm} indicates earnings nonresponse for individual i working at job or business j in month m, X_{ijm} is a set of observable characteristics that includes some details of the job or business, and η_{ijm} is a normally distributed error term.[24] We estimate the model via

[23] Note that Table 10 does not report results when the dependent variable indicates item nonresponse for questions about earnings from moonlighting or severance pay. We also estimated models with these dependent variables. However, we did not find these results to be insightful, perhaps due to the relatively low rates at which sample members receive these types of pay. We therefore exclude them from our discussion. Estimates are available upon request.

[24] Recall that at each interview individuals provide earnings on up to two jobs for an employer and up to two self-employed businesses during each of the preceding four months. Consequently, a natural unit of observation for the regression given by equation 3 is the person-job-month or person-business-month. However, 83.2% of person-month

ordinary least squares.

One central motivation for running person-job-month and person-business-month regressions is improving the estimated coefficients on job characteristics. We considered employment across all jobs when defining these covariates at the person-month level for Table 9. For example, the class of worker variables in the person-month level regression indicate status on any job or business, while weeks worked and hours worked in the person-month level regression measure time worked on all jobs and businesses combined. However, to the extent that individuals provide earnings data on one job or business and decline to provide earnings data on another job or business in the same month, we expect this strategy to yield coefficient estimates that are difficult to interpret. In particular, the mechanism that links hours worked on a job to earnings nonresponse on that job might differ from the mechanism that links earnings nonresponse on that job to hours worked on other jobs or businesses. By running regressions at the person-job-month level and person-business-month level, we can separate out any effect that characteristics of other jobs or businesses might have on earnings nonresponse.

Column 1 of Table 10 reports the results when the dependent variable indicates nonresponse to earnings questions about jobs for an employer. The estimation sample is the set of all individuals aged 15 and older, who worked on a noncontingent basis at a job for an employer, and who provided some information about their labor market situation. The results in column 1 of Table 10 strongly resemble the results in column 4 of Table 9, as a heavy majority of earnings comes from jobs for an employer. Nevertheless, analyzing earnings nonresponse at the person-job-month level rather than the person-month level does yield some different inferences, primarily for the coefficients on job characteristics. For example, while column 4 of Table 9 reports that individuals who work more weeks per month or more hours per week are more likely to be item nonrespondents, column 1 of Table 10 suggests that individuals who work more weeks per month or more hours per week are less likely to be item nonrespondents. A final difference between column 4 of Table 9 and column 1 of Table 10 is that by restricting attention to jobs for an employer we can investigate whether workers who are paid by the hour are differentially likely to be item nonrespondents.[25] We find that these workers are 1.9 percentage points less

observations on which individuals work two jobs for an employer exhibit earnings data that are missing for either no job in that month or both jobs in that month, and 74.4% of person-month observations on which individuals work two self-employed businesses exhibit corresponding behavior. Rather than collapse our regressions to the person-month level, we use person-job-month or person-business-month as the unit of observation and account for arbitrary correlations at the person-wave level when constructing standard errors.

[25] SIPP does not ask respondents if they are paid by the hour on self-employed jobs.

likely to be item nonrespondents, which is consistent with the hypothesis that gross hourly pay rates are more salient for survey respondents than other types of pay.

While column 4 of Table 9 indicates that contingent workers are considerably more likely to be item nonrespondents, column 1 of Table 10 omits this covariate. Due to the irregular nature of contingent work, SIPP attempts to reduce these respondents' burden by skipping questions on dates worked and class of worker status. To understand whether contingent workers are more likely to lack data on earnings for jobs at an employer, column 2 of Table 10 presents the results of a model that omits the class of worker indicators, the weeks worked variable, and the stopped work indicator and includes a contingent worker indicator. Thus, the estimation sample is the set of all individuals aged 15 and older, who worked at a job for an employer, and who provided some information about their labor market situation. We conclude that contingent workers are 5.8 percentage points more likely than workers with a regular arrangement to lack data on earnings for jobs at an employer.

Column 3 reports the results when the dependent variable indicates nonresponse to earnings items about self-employed businesses. The estimation sample is the set of all individuals aged 15 and older, who worked at a self-employed business, and who provided some information about their labor market situation. In general, the coefficient estimates for this model are noisier than the corresponding estimates in columns 1 and 2 due to the lower prevalence of self-employed businesses. Restricting our attention to earnings from businesses also reveals some inferences that were not apparent when we considered earnings from jobs for an employer. For example, individuals who work more weeks per month or more hours per week are more likely to lack earnings data on self-employed businesses, while columns 1 and 2 show that they are no more likely to lack earnings data on jobs for an employer. Finally, by restricting attention to self-employed businesses we can analyze whether workers who receive different types of business earnings are differentially likely to be item nonrespondents. Workers who draw a regular salary from their business are 2.5 percentage points less likely and workers who receive some nonsalary income out of the money that the business brings in are 21.8 percentage points more likely to lack data on earnings from a self-employed business. These findings suggest that regular salary income is more salient for business owners. By contrast, irregular nonsalary income appears to be less salient for business owners.

So far, this section has discussed the correlates of nonresponse, thereby pointing to coefficient estimates that are more likely on average to be biased in earnings regressions as a result of mismeasured earnings. One natural strategy to mitigate this bias would be to omit observations with imputed earnings. However, this strategy assumes that earnings nonresponse is ignorable. In an attempt to test this assumption, we include administrative earnings information in the model given by equation 2. If we reject the null hypothesis that earnings nonresponse is unrelated to true earnings conditional on covariates, then earnings nonresponse is not ignorable. While we do not view administrative earnings as truth per se, we argue that administrative earnings are closely correlated with true earnings.

Column 1 of Table 11 contains the estimation results of this model, where the dependent variable indicates any earnings nonresponse. Our sample for this regression is all individuals aged 15 and older who were assigned a PIK. We also exclude all person-month observations during a year when that individual leaves the sample either temporarily or permanently. These additional regressors and sample selection criteria leave many results unchanged qualitatively from column 1 of Table 9. For example, the relatively low R^2 suggests that much of the variation in earnings nonresponse remains unexplained, even though many of the same observables appear to be correlated with nonresponse. Nevertheless, some inferences do appear different in column 1 of Table 11 relative to column 1 of Table 9. First, some covariates no longer appear correlated with earnings nonresponse, including: number of family members; the Asian, non-Hispanic indicator; the White, non-Hispanic indicator; and the indicator that the respondent does not speak English in the home. Additionally, some covariate estimates appear to have reversed sign, including the indicator for a change in family composition and the associate's degree indicator. As a result of these differences, the generally increasing relationship between earnings nonresponse and education from Table 9 is no longer apparent. Finally, many coefficient estimates are attenuated in column 1 of Table 11 relative to column 1 of Table 9, including: number of household members; the female indicator; the proxy response indicator; the attritor indicator; the indicator for any children under 18; and the indicator for any means-tested transfer income.

The coefficient estimates based on administrative earnings data that are reported in column 1 of Table 11 reveal several interesting patterns about the ignorability of earnings nonresponse. First, individuals who work for more employers are more likely to lack earnings data. Second, individuals with positive administrative earnings are 5.3 percentage points more likely to suffer from missing earnings data than individuals with no administrative earnings records. Among respondents with positive administrative earnings, those in the bottom and fourth quintiles are

0.6 and 1.0 percentage points less likely to exhibit earnings nonresponse than individuals in the middle quintile, respectively. Those in the second quintile are 1.5 percentage points more likely to display earnings nonresponse than individuals in the middle quintile. This pattern poses a stark contrast to the U-shaped pattern of earnings nonresponse documented for workers who are item nonrespondents by Bollinger et al. (2015b). To explore the sources of this difference, we analyze how administrative earnings are related to unit nonresponse and item nonresponse separately.

Column 2 of Table 11 contains the estimation results of this model, where the dependent variable indicates unit earnings nonresponse. Our sample for this regression is all individuals aged 15 and older who were assigned a PIK. We also exclude all person-month observations during a year when that individual leaves the sample either temporarily or permanently. There are two salient differences between the findings presented in column 2 and the corresponding figures in column 1. First, while individuals with positive administrative earnings are more likely to decline to provide any labor force data, the differential of 1.0 percentage points is smaller than the corresponding differential in the likelihood of any nonresponse.[26] Second, the likelihood of unit nonresponse displays a relatively weak pattern over the administrative earnings distribution. Individuals in the bottom two quintiles are 0.4 percentage points and 0.5 percentage points less likely to lack all labor force data than those in the middle quintile. By contrast, individuals in the top two quintiles are no more or less likely to lack all labor force data than those in the middle quintile.[27] The relatively weak relationship between unit nonresponse and administrative earnings conditional on covariates suggests that unit earnings nonresponse might be roughly close to ignorable.[28]

Column 3 of Table 11 lists the results when the dependent variable indicates item nonresponse. Our sample for this regression is all individuals aged 15 and older, who provided at least some labor force data, and who were assigned a PIK. We also exclude all person-month

[26]Note that we observe unit nonrespondents' administrative earnings rather than their donors' administrative earnings. Nevertheless, we suggest caution in interpreting the estimates reported in column 2. As stated previously, some of the explanatory covariates in the model do not represent the characteristics of unit nonrespondents but rather the characteristics of their donors.

[27]While Bollinger et al. (2015b) explicitly exclude unit nonrespondents from their primary analysis, they do study the pattern of unit nonresponse over the administrative earnings distribution for men and women. They find that unit nonrespondents have lower and more dispersed administrative earnings than those who provide at least some CPS ASEC data. By contrast, we find that individuals with low administrative earnings are less likely to suffer from unit nonresponse.

[28]This conclusion corroborates the findings of Bee et al. (2015). They exploit 1040 records matched to CPS ASEC unit nonrespondents to show that the income distribution is quite similar for those who participate in the supplement and those who do not participate.

observations during a year when that individual leaves the sample either temporarily or permanently. Column 3 of Table 11 shows that individuals with no administrative earnings are 4.4 percentage points less likely to decline to provide any earnings data. Among individuals with positive administrative earnings, the pattern of item nonresponse resembles the U-shaped pattern documented by Bollinger et al. (2015b). Sample members in the second administrative earnings quintile are 2.0 percentage points more likely to be item nonrespondents than those in the middle quintile, while sample members in the fourth administrative earnings quintile are 0.9 percentage points more likely to be item nonrespondents than those in the middle quintile. However, we find no significant difference between the average tendency to lack earnings data for individuals in the bottom or top quintiles and those in the middle quintile. By contrast, Bollinger et al. (2015b) showed that individuals in the tails of the administrative earnings distribution are more likely to lack earnings data than those in the middle of this distribution.

One potential explanation for these different patterns of nonresponse at the bottom and top of the administrative earnings distribution is that our estimation sample in column 3 included individuals with no survey earnings, while Bollinger et al. (2015b) considered only those with positive survey earnings. To evaluate the influence of SIPP respondents who report no employment, the estimation sample in column 4 further restricts the sample to only SIPP employed individuals. This additional sample selection criterion impacts the inferences from column 3 in two important ways. First, the sign of the coefficient on the indicator of positive administrative earnings surprisingly reverses. Individuals with any administrative earnings record are now 7.8 percentage points less likely to lack earnings data. Second, the U-shaped pattern of item nonresponse over the administrative earnings distribution that Bollinger et al. (2015b) illustrated for CPS ASEC now becomes apparent in SIPP data. Respondents in the bottom, second, and top administrative income quintiles are 4.5, 1.8, and 0.4 percentage points more likely to be item nonrespondents than those in the middle quintile, respectively. On the other hand, individuals in the fourth quintile display no significantly different tendency to respond to earnings items than those in the middle quintile.

6 Implications for Estimates of the Earnings Structure

Now that we have characterized how the relationship between survey earnings and administrative earnings varies by whether survey earnings were imputed and who is more likely to have imputed data, one might wonder what implications these relationships have for estimates that are of broad interest to labor economists. We approach this question by estimating various aspects of the earnings structure, including the gender earnings gap, the Black-White earnings gap, and the return to education which we estimate via a Mincer regression. We estimate each regression for four different dependent variables: SIPP earnings, DER earnings, only reported SIPP earnings, and a hybrid of SIPP and DER earnings. This DER-SIPP hybrid is defined as SIPP earnings for individuals who experienced no months of imputed survey earnings during the year and DER earnings for individuals who experienced at least one month of imputed survey earnings during the year.

Table 12 lists the estimates of the gender earnings gap, Table 13 lists the estimates of the Black-White earnings gap, and Table 14 lists the estimates of the Mincer regression. In each table, the dependent variable is SIPP earnings, DER earnings, only reported SIPP earnings, and the DER-SIPP hybrid in columns 1, 2, 3, and 4, respectively. The estimation sample in columns 1, 2, and 4 includes all person-years for individuals aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the year, and who displayed both positive SIPP earnings and positive DER earnings. The estimation sample in column 3 also excludes person-year observations that experienced at least one month of imputed survey earnings during the year. Aside from the covariates listed in Tables 12, 13, and 14, the set of regressors includes only a constant.

The results in Tables 12, 13, and 14 offer insight into four issues that are relevant for data users. First, comparing the estimates in columns 1 and 4 allows for a test of the impact of any additional noise in imputed survey earnings relative to administrative earnings. If this noise does indeed bias regression coefficients, we will reject the null hypothesis that SIPP earnings and the DER-SIPP hybid yield equal coefficient estimates as dependent variables. Second, comparing the estimates in columns 1 and 3 allows us to test the efficacy of the strategy of excluding observations with imputed earnings. If any additional noise in imputed earnings does indeed bias regression coefficients, we will reject the null hypothesis that all SIPP earnings and only reported SIPP earnings yield equal coefficient estimates. The strategy of restricting the estimation sample to only individuals with reported earnings assumes that earnings response bias is ignorable. Comparing the estimates in columns 3 and 4 allows for a test of the validity of this assumption. If earnings response bias is indeed ignorable, we will be unable to reject the null hypothesis that the DER-SIPP hybrid and only reported SIPP earnings yield equal coefficient estimates. Finally, comparing the estimates in columns 2 and 4 allows for a test of the impact of any additional noise in reported survey earnings relative to administrative earnings. If this noise does indeed bias regression coefficients, we will reject the null hypothesis that DER earnings and the DER-SIPP hybrid yield equal coefficient estimates.

Table 12 details how estimates of the gender earnings gap depend on the source of earnings data. When the dependent variable is both reported and imputed SIPP earnings, we see that women earn approximately $12,035 less than men on average. A comparison of the estimates in columns 1 and 4 reveals that any additional noise that is present in imputed survey earnings and not in these individuals' administrative earnings has no statistically significant impact on the estimated gender earnings gap. However, a comparison of the estimates in columns 1 and 3 reveals that the strategy of dropping imputed earnings observations yields a larger estimated gender earnings gap. This result suggests that any additional noise in imputed survey earnings relative to reported survey earnings attenuates the estimated gender earnings gap. Nevertheless, a comparison of the estimates in columns 3 and 4 shows evidence that including administrative earnings for survey earnings nonrespondents does not significantly impact our estimate of the gender earnings gap. Thus, we join Bollinger and Hirsch (2013) and Bollinger et al. (2015b) in concluding that earnings response bias is ignorable for an estimate of average earnings differences even though nonresponse does appear to depend on administrative earnings in the tails of the distribution. Finally, comparing the estimates in columns 2 and 4 suggests that replacing reported survey earnings with administrative earnings yields a larger estimate of the gender earnings gap. This finding does not support the claim of Abowd and Stinson (2013) that reported survey earnings are similar to administrative earnings in reliability.

Table 13 describes how estimates of the Black-White earnings gap depend on the source of earnings data. When the dependent variable is both reported and imputed SIPP earnings, we see in column 1 that White workers earn approximately $8,173 more on average than the omitted race group.[29] The implied average Black-White earnings gap is about $9,278. A comparison of the estimates in columns 1 and 4 reveals that any additional noise that is present in imputed survey earnings and not in these individuals' administrative earnings has no statistically significant impact on the estimated Black-White earnings gap. The implied Black-White earnings

[29]SIPP gives respondents the option of reporting more than one race. The indicators in this table define racial groups to include individuals who reported only one race. Individuals who reported multiple races are included in the omitted group. Note that the race indicators in this model are not interacted with ethnicity indicators. Consequently, the set of workers who report Black alone may include both Hispanic and non-Hispanic individuals.

gap grows by about \$325, so we fail to reject the null hypothesis of an equal Black-White earnings gap across models that use all SIPP earnings and the DER-SIPP earnings hybrid as the dependent variable. A comparison of the estimates in columns 1 and 3 reveals that the strategy of dropping imputed earnings observations has no statistically significant impact on the estimated Black-White earnings gap. While the point estimate on Black alone grows in magnitude, the implied point estimate of the Black-White earnings gap grows by a statistically insignificant \$1,281. A comparison of the estimates in columns 3 and 4 shows evidence that including administrative earnings for survey earnings nonrespondents does not significantly impact our estimate of the Black-White earnings gap. Again, we conclude that earnings response bias is ignorable for an estimate of average earnings differences by race even though nonresponse does appear to depend on administrative earnings in the tails of the distribution. Finally, comparing the estimates in columns 2 and 4 suggests that replacing reported survey earnings with administrative earnings does not significantly impact our estimate of the Black-White earnings gap. This strategy reduces our point estimate of the gap by only about \$177.

Finally, Table 14 reports how estimates of the returns to education and potential experience depend on the source of earnings data. Column 1 lists the coefficient estimates of a basic Mincer regression when the dependent variable is both reported and imputed SIPP earnings. Each additional year of education delivers about $\$4,953$ in additional earnings on average. The first year of potential experience bring about $\$1,899$ in additional earnings on average, although the average earnings gain that comes from each subsequent year of potential experience declines over time. A comparison of the estimates in columns 1 and 4 reveals mixed evidence about the impact of any additional noise that is present in imputed survey earnings and not in these individuals' administrative earnings. On one hand, we fail to reject the null hypothesis of no difference in the estimated return to education across models that use all SIPP earnings and the DER-SIPP earnings hybrid as the dependent variable. On the other hand, we do reject the null hypothesis of no difference in the estimated return to potential experience across these models. A comparison of the estimates in columns 1 and 3 suggests that the strategy of dropping observations with imputed earnings does indeed increase the magnitude of the estimated returns to education and potential experience. A comparison of the estimates in columns 3 and 4 shows that including administrative earnings for survey earnings nonrespondents does significantly impact our estimate of the return to education, yet not our estimate of the return to potential experience. This finding suggests that although earnings nonresponse does appear related to administrative earnings in the tails of the administrative earnings distribution, earnings response bias is ignorable for an estimate of the average return to potential experience in a Mincer regression. Nevertheless, this pattern of earnings nonresponse does imply non-ignorable response bias for an estimate of the average return to education. Finally, comparing the estimates in columns 2 and 4 suggests that replacing reported survey earnings with administrative earnings does not significantly impact our estimates of the return to education, while this strategy does significantly affect our estimates of the return to potential experience.

7 Conclusion

In this paper, we explore the impact of the decision to include observations with imputed earnings in regressions. Our findings corroborate many conclusions of the existing literature. We join Cristia and Schwabish (2009) in concluding that men and better educated individuals have a greater difference in survey and administrative earnings. We echo Abowd and Stinson (2013) in showing that earnings for individuals with at least one month imputed compare less favorably to administrative data than for individuals with no months imputed. After controlling for observables, individuals with any nonresponse display a DER-SIPP gap that is about $\$1,920$ smaller on average than respondents, while the absolute value of this gap is about $\$6,898$ larger on average for individuals with any nonresponse. When attempting to understand the implications of this result for regression estimates, our evidence supports Hokayem et al. (2014) who find that respondents who are self-employed or Black, non-Hispanic are less likely to respond to earnings questions. We document a U-shaped pattern of earnings nonresponse over the administrative earnings distribution among survey members who worked, similar to Bollinger et al. (2015b). Finally, our result that earnings unit nonresponse is roughly close to ignorable is consistent with results of Bee et al. (2015).

We also contribute to the literature by documenting relationships that have not yet been established to our knowledge. First, there appears to be considerable heterogeneity in the proximity of survey earnings to administrative earnings across different imputation methodologies. For example, survey earnings that are imputed based on last month's survey earnings deviate from administrative earnings by substantially more than other methods if this imputation was initially based on an imputed earnings value. Second, individuals who receive means-tested transfer income have survey earnings that deviate from administrative earnings by less than individuals who do not receive transfer income. Additionally, we report that individuals who ever leave the survey either temporarily or permanently are more likely to be earnings nonrespondents while in the survey. Next, the details of how sample members are paid appear to matter for whether they respond to earnings questions. Individuals who receive regular hourly or salaried pay appear more likely to respond to these questions. Similarly, those who receive more erratic pay, such as contingent workers and business owners who receive nonsalary income, appear less likely to respond to earnings questions. Next, we show evidence of a novel pattern of nonresponse over the administrative earnings distribution. Bollinger et al. (2015b) show a U-shaped pattern of nonresponse over this distribution, excluding from their analysis unit nonrespondents and individuals with either no survey earnings or no administrative earnings. By contrast, we find that individuals with no administrative earnings are 5.3 percentage points less likely to lack reported earnings data. Similarly, when we include individuals who have no survey earnings in our nonresponse regressions, individuals whose administrative earnings lie in the bottom quintile of the distribution appear no more likely to lack survey earnings data than individuals in the middle quintile of this distribution. When we include unit nonrespondents, the U-shaped pattern of nonresponse disappears as the likelihood of nonresponse is smaller for individuals in the bottom quintile of the earnings distribution than for individuals in the middle quintile of this distribution. Finally, we show that earnings response bias and any additional measurement error that might exist in survey earnings relative to administrative earnings do affect some estimates of key aspects of the earnings structure.

Our paper offers practical insight for analysts studying earnings. Often analysts include observations with imputed earnings in their analyses, for example when sample size is an especially acute concern. Given the conclusion that imputed survey earnings are less reliable than administrative earnings, estimated regression coefficients on observable characteristics that are correlated with earnings nonresponse are susceptible to bias. Nevertheless, observable characteristics explain relatively little of the variation in nonresponse, so the risk of bias may be small. In an attempt to mitigate bias, many researchers exclude observations with imputed earnings from analyses. This tactic will yield unbiased estimates if the likelihood of earnings nonresponse is unrelated to earnings itself. However, our investigation reveals a complex relationship between earnings nonresponse and administrative earnings. Among a sample of workers, we report that earnings response bias due to item nonresponse is nonignorable in the tails of the distribution. Bollinger et al. (2015a) argue that this condition biases inequality estimates. When we also include nonemployed individuals and unit nonrespondents, we find that earnings nonresponse remains correlated with administrative earnings, although the pattern differs. Individuals with no administrative earnings are less likely to exhibit any nonresponse. This relationship in principle could bias upwards estimates of means-tested program eligibility. Among individuals with positive administrative earnings, those in the bottom quintile are less likely to exhibit any nonresponse than those in the middle quintile. Thus, including unit nonrespondents could mitigate the earnings response bias in the left tail of the administrative earnings distribution documented by Bollinger et al. (2015b).

Our investigation also points to the potential consequences of the increasingly prevalent proposals to utilize administrative records more extensively in the production of household survey data.[30] In the extreme, these proposals call for data producers to reduce respondent burden by removing earnings questions from surveys and to replace self-reported earnings data with transformed administrative earnings data. Our investigation documents that this strategy would alter self-reported earnings data considerably on average for individuals who are male; married; more educated; self-employed; Black, non-Hispanic; and Asian, non-Hispanic. Abowd and Stinson (2013) and Bollinger et al. (2015b) argue that this change would offer a different measure of earnings, though not necessarily an improved measure. We also show that the strategy of replacing survey earnings data with administrative earnings data would alter the data considerably on average for individuals who experienced at least one month of imputed earnings during the year. The results of Abowd and Stinson (2013) would suggest that this change represents an improvement, underscoring the need for future research to explore how administrative earnings records might be incorporated into the process of imputing earnings data for public use. Accordingly, we reject the null hypothesis that replacing imputed SIPP earnings with DER earnings leaves several key estimates of the earnings structure unchanged. Nevertheless, this strategy does not appear to alter other key estimates of the earnings structure significantly.

The scope for future work remains tremendous given several changes to SIPP earnings data collection implemented by the forthcoming 2014 panel. First, in an effort to reduce the cost of the survey, SIPP reduced the frequency with which it conducts interviews. Survey participants in the 2008 panel were interviewed three times per year, each time providing information about the preceding four months. One commonly cited advantage of the 2008 SIPP panel is

[30] See Meyer et al. (2015) for one recent, prominent example that proposes more comprehensive use of administrative data to improve survey data on program participation and income from programs and to mitigate the increasing trend of nonresponse.

the relatively high interview frequency, which might serve to reduce recall bias in earnings reports. By contrast, survey participants in the 2014 panel are interviewed once per year, each time providing information about the preceding calendar year. Second, earnings questions in the 2008 panel primed respondents by reminding them of the amount reported at the last interview and offered respondents the opportunity to report the same amount at the current interview. By contrast, earnings questions in the 2014 panel neither "feed back" the amount that was reported at the last interview nor offer the opportunity to report no change in earnings since the last interview. Similarly, the 2008 panel imputed some missing earnings data conditional on earnings in the previous wave, while this option was not available for the 2014 panel. Third, the 2014 SIPP panel requires some individuals to aggregate earnings amounts manually before reporting, whereas the 2008 panel allowed these individuals to report each payment received. For example, individuals who received tips must report this income as a monthly amount in the 2014 panel, while the 2008 panel allowed them to report up to five separate payments received in each month. Similarly, individuals who receive highly variable pay other than tips, bonuses, commisions, and overtime must report this income as a monthly average that pertains to multiple months potentially in the 2014 panel. The 2008 panel allowed these sample members to report up to five separate payments received in each month. Fourth, the 2008 SIPP panel attempted to minimize earnings nonresponse by offering some sample members who decline to provide earnings data different methods of reporting this same data. On the other hand, the 2014 SIPP panel pursues a different approach by offering some individuals the opportunity to report earnings in a range when they initially decline to provide earnings data. Fifth, in an attempt to reduce measurement error during the interview the 2008 SIPP panel converted reported hourly and bi-weekly amounts into more salient amounts that respondents verified and had the opportunity to correct.[31] In an attempt to reduce respondent burden, the 2014 SIPP panel does not convert hourly and bi-weekly reports into more salient amounts for verification.[32] Finally, the 2014 panel offers respondents more flexibility in accounting periods when reporting earnings in the hope of inducing individuals to report in the most accurate and least burdensome way. The 2014 panel will offer a natural experiment to evaluate whether these changes increase earnings volatility, the deviation of survey earnings from administrative earnings, or the likelihood of

[31] For example, individuals who reported an hourly amount were prompted with a bi-weekly paycheck amount, and individuals who reported a bi-weekly amount were prompted with a monthly take home pay amount.

[32] The 2014 panel does ask respondents to verify hourly or bi-weekly amounts that imply unusually large pay amounts assuming that this pay rate is received by a full-time, full-year worker.

response.

References

Abowd, J. M. and M. H. Stinson (2013). Estimating measurement error in annual job earnings: A comparison of survey and administrative data. *The Review of Economics and Statistics 95*(5), 1451–1467.

Andridge, R. R. and R. J. A. Little (2010). A review of hot deck imputation for survey nonresponse. *International Statistical Review 78*(1), 40–64.

Bee, C. A., G. Gathright, and B. D. Meyer (2015). Bias from unit non-response in the measurement of income in household surveys.

Bollinger, C. R. (1998). Measurement error in the Current Population Survey: A nonparametric look. *Journal of Labor Economics 16*(3), 576–594.

Bollinger, C. R. and B. T. Hirsch (2006). Match bias from earnings imputation in the Current Population Survey: The case of imperfect matching. *Journal of Labor Economics 24*(3), 576–594.

Bollinger, C. R. and B. T. Hirsch (2009, November). Wage gap estimation with proxies and nonresponse.

Bollinger, C. R. and B. T. Hirsch (2013). Is earnings nonresponse ignorable? *The Review of Economics and Statistics 95*(2), 407–416.

Bollinger, C. R., B. T. Hirsch, C. M. Hokayem, and J. P. Ziliak (2015a, June). Measuring levels and trends in earnings inequality with nonresponse, imputations, and topcoding.

Bollinger, C. R., B. T. Hirsch, C. M. Hokayem, and J. P. Ziliak (2015b, April). Trouble in the tails? Earnings nonresponse and response bias across the distribution.

Bond, B., J. D. Brown, A. Luque, and A. O'Hara (2014). The nature of bias when studying only linkable person records: Evidence from the American Community Survey. Technical Report 2014-08, U.S. Census Bureau.

Bound, J. and A. B. Krueger (1991). The extent of measurement error in longitudinal earnings data: Do two wrongs make a right? *Journal of Labor Economics 9*(1), 1–24.

Cristia, J. and J. A. Schwabish (2009). Measurement error in the SIPP: Evidence from administrative matched records. *Journal of Economic and Social Measurement 34*(1), 1–17.

David, M., R. J. A. Little, M. E. Samuhel, and R. K. Triest (1986). Alternative methods for CPS income imputation. *Journal of American Statistical Association 81*(393), 29–41.

Duncan, G. J. and D. H. Hill (1985). An investigation of the extent and consequences of measurement error in labor-economic survey data. *Journal of Labor Economics 3*(4), 508–532.

Gottschalk, P. and M. Huynh (2010). Are earnings inequality and mobility overstated? The impact of nonclassical measurement error. *The Review of Economics and Statistics 92*(2), 302–315.

Hirsch, B. T. and E. J. Schumacher (2004). Match bias in wage gap estimates due to earnings imputation. *Journal of Labor Economics 22*(3), 689–722.

Hokayem, C., C. Bollinger, and J. P. Ziliak (2014, April). The role of CPS nonresponse on the level and trend in poverty. Technical Report DP2014-05, University of Kentucky Center for Poverty Research.

Hokayem, C., C. Bollinger, and J. P. Ziliak (2015). The role of CPS nonresponse on the level and trend in poverty. *Journal of the American Statistical Association*, forthcoming.

Mellow, W. and H. Sider (1983). Accuracy of response in labor market surveys: Evidence and implications. *Journal of Labor Economics 1*(4), 331–344.

Meyer, B. D., W. K. C. Mok, and J. X. Sullivan (2015). Household surveys in crisis. *Journal of Economic Perspectives*, forthcoming.

Pedace, R. and N. Bates (2000). Using administrative records to assess earnings reporting error in the Survey of Income and Program Participation. *Journal of Economic and Social Measurement 26*(3,4), 173–192.

Roemer, M. (2002, November). Using administrative earnings records to assess wage data quality in the March Current Population Survey and the Survey of Income and Program Participation. Technical Report TP-2002-22, U.S. Census Bureau.

U.S. Census Bureau (2001). *Survey of Income and Program Participation Users' Guide* (Third Edition ed.). Washington, DC: U.S. Census Bureau. United States Department of Commerce.

Table 1. The Presence of SIPP Earnings and DER Earnings

	No DER earnings	Positive DER earnings	Total
No SIPP earnings	56,410	8,216	64,626
Positive SIPP earnings	4,745	90,394	95,139
Total	61,155	98,610	159,765

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample for this table is all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the year. This table counts the number of unweighted person-year cases exhibiting positive SIPP earnings and positive DER earnings.

Table 2. The Average Deviation of SIPP Earnings and DER Earnings

	(1) DER-SIPP	(2) $\lvert$DER-SIPP$\rvert$	Observations
Including Zero Earners	$969	$6,277	158,168
Excluding Zero Earners	$1,928	$10,429	89,418

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample for this table is all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the year, and whose absolute deviation of survey from administrative earnings was in the bottom 99 percent of this distribution. The sample in the second row additionally exclude person-years that displayed either zero SIPP earnings or zero DER earnings. The estimates in column 1 are sample means of the average raw difference between DER earnings and SIPP earnings. The estimates in column 2 are sample means of the average absolute difference between DER earnings and SIPP earnings.

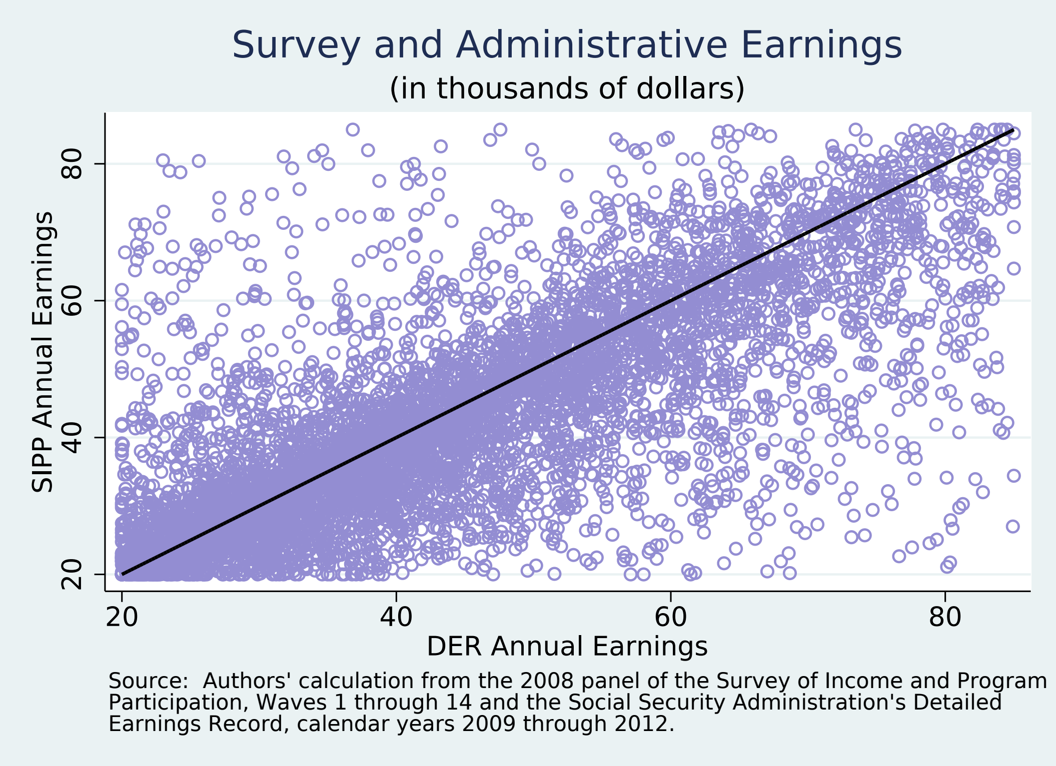

Figure 1. The data points pictured represent a random 15 percent sample of all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. We focus on the set of individuals with both SIPP earnings and DER earnings between \$20,000 and \$85,000 for ease of visualization. This figure plots the relationship between administrative earnings (on the horizontal axis) and survey earnings (on the vertical axis). We perturb each data point by adding spherical random noise in order to avoid disclosing federal tax information.

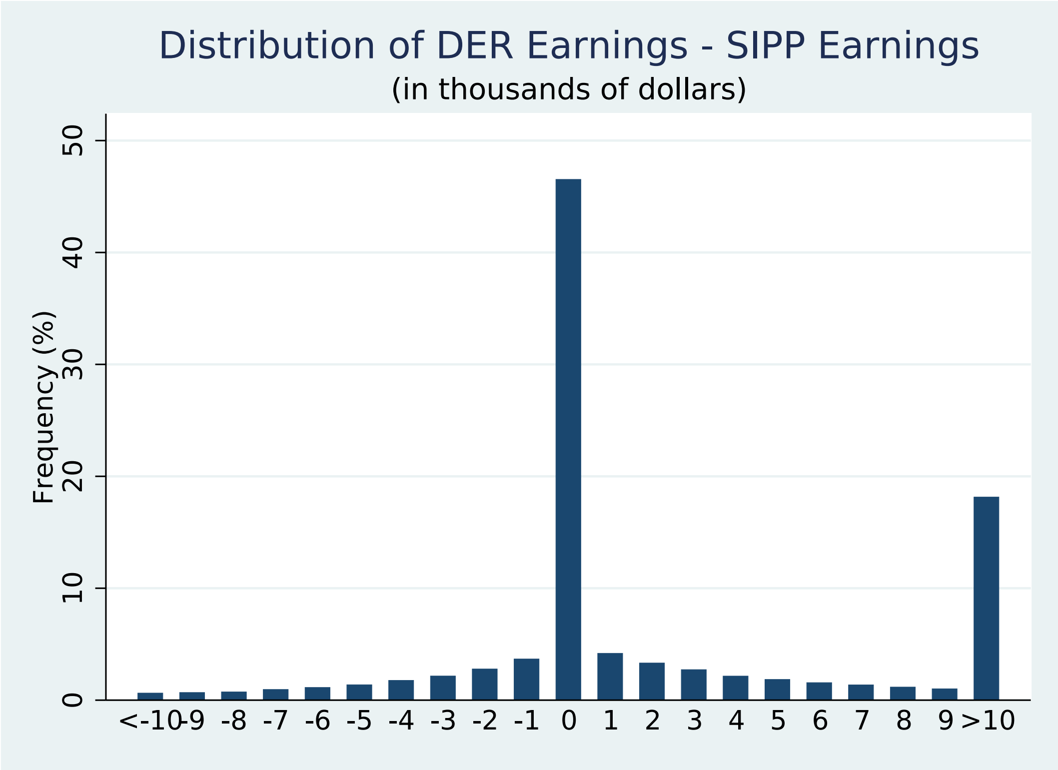

Figure 2. The sample is all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. This figure plots the difference between administrative earnings and survey earnings. Each bin represents the integer portion of the difference between DER earnings and SIPP earnings, in thousands of dollars. For example, the bin labeled "-1" includes person-years for which SIPP earnings exceed DER earnings by some amount between $1,000 and $1,999, inclusive; the bin labeled "0" includes person-years for which either SIPP earnings exceed DER earnings by up to and including $999 or DER earnings exceed SIPP earnings by up to and including $999; and the bin labeled "1" includes person-years for which DER earnings exceed SIPP earnings by some amount between $1,000 and $1,999, inclusive. The bin labeled "< -10" includes person-years for which SIPP earnings exceed DER earnings by $10,000 or more. The bin labeled "> 10" includes person-years for which DER earnings exceed SIPP earnings by $10,000 or more.

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

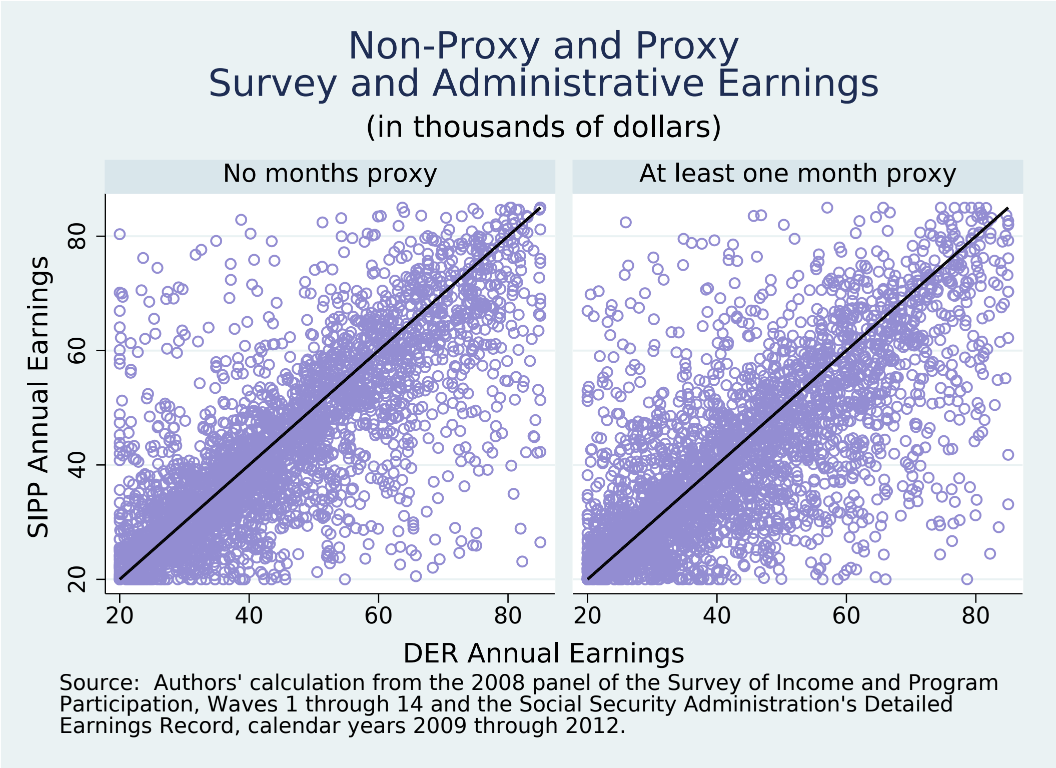

Figure 3. The data points pictured represent a random 15 percent sample of all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. We focus on the set of individuals with both SIPP earnings and DER earnings between $20,000 and $85,000 for ease of visualization. This figure plots the relationship between administrative earnings (on the horizontal axis) and survey earnings (on the vertical axis) by proxy interview status. We perturb each data point by adding spherical random noise in order to avoid disclosing federal tax information. The scatterplot on the left includes only individuals whose data were not provided by proxy response for any month of the year. The scatterplot on the right includes only individuals whose data were provided by proxy response for at least one month of the year.

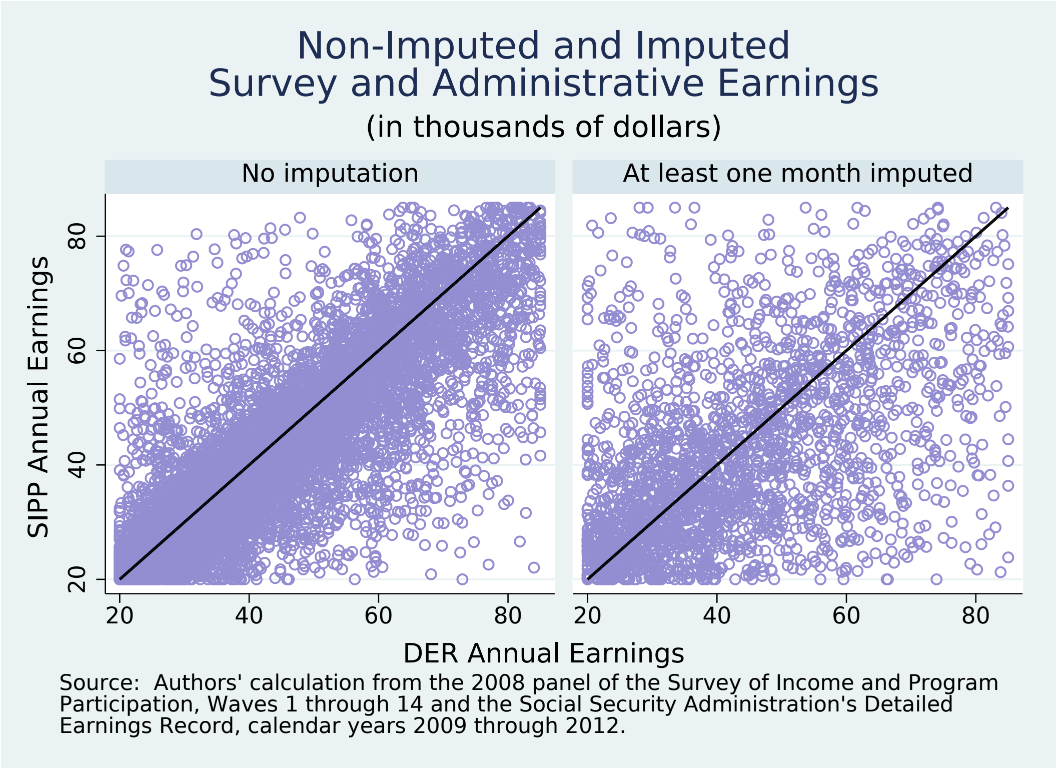

Figure 4. The data points pictured represent a random 25 percent sample of all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. We focus on the set of individuals with both SIPP earnings and DER earnings between \$20,000 and \$85,000 for ease of visualization. This figure plots the relationship between administrative earnings (on the horizontal axis) and survey earnings (on the vertical axis) by imputed earnings status. We perturb each data point by adding spherical random noise in order to avoid disclosing federal tax information. The scatterplot on the left includes only individuals whose survey earnings data were not imputed for any month of the year. The scatterplot on the right includes only individuals whose survey earnings data were imputed for at least one month of the year.

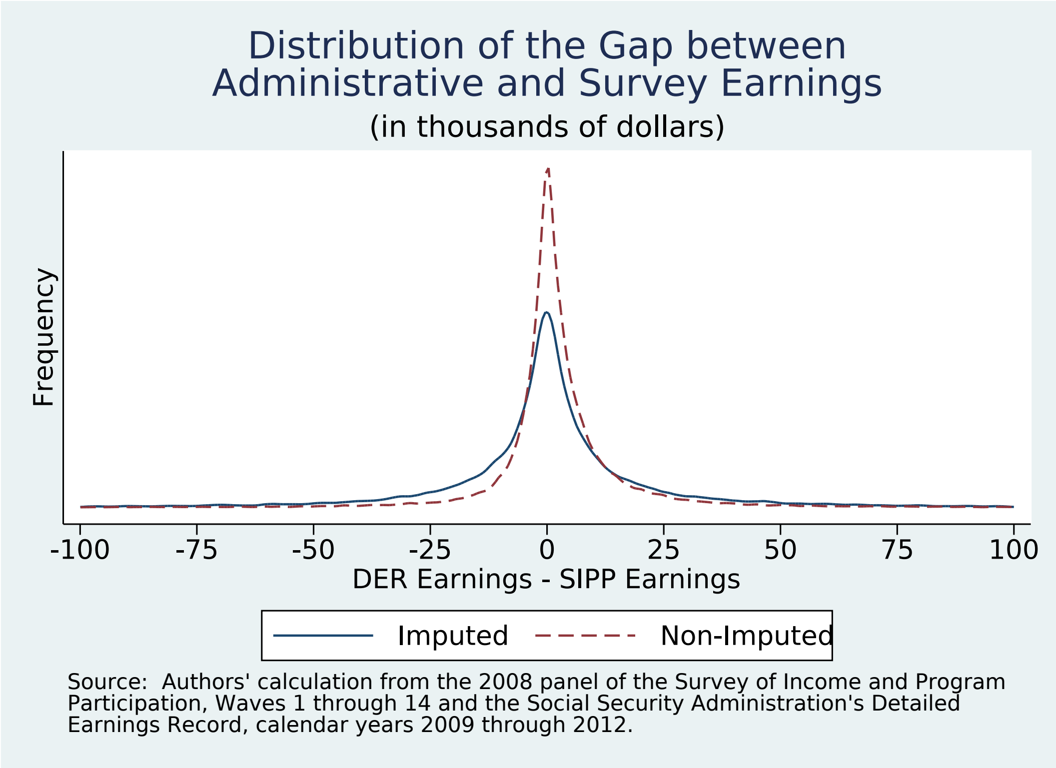

Figure 5. The data points pictured represent all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, who had both positive SIPP earnings and positive DER earnings, and who exhibited an absolute difference between survey and administrative earnings that did not exceed $100,000. This figure plots the univariate kernel density estimates of the difference between DER earnings and SIPP earnings by imputed earnings status. Points to the left of the "0" label exhibited SIPP earnings larger than DER earnings, while points to the right of the "0" label exhibited DER earnings larger than SIPP earnings. The red, dashed line plots estimates only for individuals whose survey earnings data were not imputed for any month of the year. The blue, solid line plots estimates only for individuals whose survey earnings data were imputed for at least one month of the year.

Table 3. Deviation of SIPP Earnings from DER Earnings: Any Nonresponse

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
Any nonresponse	-1,919.228***	6,898.190***
	(150.799)	(115.027)
Midwest	-287.687*	-576.744***
	(152.410)	(124.611)
South	-111.066	-162.447
	(147.172)	(119.463)
West	-288.231*	-213.553
	(168.233)	(137.777)
Number of household members	7.261	-173.747***
	(81.387)	(65.229)
Number of family members	62.347	56.399
	(82.686)	(66.248)
Age	-488.859***	-113.168
	(111.729)	(93.510)
Age squared	21.680***	24.635***
	(3.595)	(3.024)
Female	-681.496***	-3,068.404***
	(98.211)	(80.129)
Black, non-Hispanic	633.243**	512.375**
	(266.341)	(221.789)
Asian, non-Hispanic	1,278.955***	791.004**
	(423.778)	(341.302)
White, non-Hispanic	367.802	626.719***
	(234.691)	(196.445)
Hispanic	1,344.658***	347.540
	(293.877)	(241.735)
Married, spouse absent	324.423	-83.473
	(486.621)	(413.664)
Never married	-405.975**	-1,326.117***
	(159.209)	(130.404)
Previously married	-104.780	-502.648***
	(121.522)	(100.496)
Elementary school	-275.560	-114.830
	(173.785)	(151.381)
Some high school	23.273	-483.810***
	(118.404)	(102.572)
Some college	327.244**	651.344***
	(138.261)	(113.620)
Associate's degree	291.111**	532.843***
	(130.985)	(106.704)
Bachelor's degree	975.960***	2,909.959***
	(172.324)	(139.714)
Master's degree	1,239.306***	4,055.376***
	(272.902)	(218.525)

(*continued...*)

Table 3 (continued). Deviation of SIPP Earnings from DER Earnings: Any Nonresponse

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Professional degree	2,851.460***	10,487.695***
	(869.993)	(693.289)
Doctorate degree	2,214.646***	6,993.012***
	(781.239)	(630.579)
Foreign-born, citizen	740.761***	431.731**
	(252.788)	(202.519)
Foreign-born, non-citizen	186.290	260.202
	(328.652)	(261.065)
Any children under 18	-97.282	583.070***
	(139.758)	(111.768)
Non-English speaker	-463.043**	343.148**
	(218.351)	(173.367)
Any transfer income	-730.051***	-3,050.304***
	(92.119)	(94.792)
Observations	158,168	158,168
R^2	0.010	0.182

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012. Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and whose absolute deviation of survey from administrative earnings was in the bottom 99 percent of this distribution. The estimates in this table result from OLS regressions. The dependent variable in column 1 is the difference between DER earnings and SIPP earnings. The dependent variable in column 2 is the absolute difference between DER earnings and SIPP earnings. Any nonresponse indicates whether earnings data were imputed for any reason, which may reflect either unit nonresponse or item nonresponse. Unit nonresponse occurs in two scenarios: when non-interviewed individuals reside with interviewed individuals and when interviewed individuals decline to provide any information about their labor market situations. Item nonresponse occurs when interviewed individuals provide some information about their labor market situations but decline to provide information about either earnings from a job for an employer, earnings from a self-employed business, earnings from moonlighting, or severance pay. Other controls include CBSA size indicators, cubic age, and quartic age. Non-English speaker indicates individuals who speak a language other than English in the home. All time-varying explanatory variables are defined as of December for each year. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 4. Deviation of SIPP Earnings from DER Earnings: Unit and Item Nonresponse

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
Any unit nonresponse	4.553	5,128.445***
	(204.553)	(162.857)
Any item nonresponse	-2,896.065***	7,796.814***
	(192.934)	(143.902)
Midwest	-307.679**	-558.353***
	(152.074)	(124.354)
South	-121.716	-152.649
	(146.967)	(119.158)
West	-332.794**	-172.558
	(167.870)	(137.501)
Number of household members	-71.120	-101.642
	(81.027)	(65.307)
Number of family members	80.495	39.704
	(82.527)	(66.375)
Age	-454.320***	-144.942
	(111.784)	(93.193)
Age squared	21.064***	25.202***
	(3.594)	(3.013)
Female	-690.149***	-3,060.444***
	(98.056)	(79.910)
Black, non-Hispanic	614.426**	529.686**
	(265.456)	(221.091)
Asian, non-Hispanic	1,263.419***	805.296**
	(423.408)	(340.345)
White, non-Hispanic	403.357*	594.011***
	(233.854)	(195.877)
Hispanic	1,365.633***	328.245
	(293.092)	(241.055)
Married, spouse absent	273.262	-36.408
	(486.750)	(412.292)
Never married	-469.064***	-1,268.079***
	(158.815)	(130.012)
Previously married	-114.835	-493.398***
	(121.341)	(100.143)
Elementary school	-242.832	-144.937
	(172.991)	(150.334)
Some high school	23.979	-484.459***
	(118.468)	(101.825)
Some college	360.295***	620.939***
	(137.994)	(113.361)
Associate's degree	286.993**	536.631***
	(130.886)	(106.372)
Bachelor's degree	1,014.509***	2,874.496***
	(172.137)	(139.419)
Master's degree	1,289.614***	4,009.096***
	(272.617)	(218.389)

(*continued...*)

Table 4 (continued). Deviation of SIPP Earnings from DER Earnings: Unit and Item Nonresponse

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Professional degree	2,911.722***	10,432.258***
	(867.134)	(691.560)
Doctorate degree	2,260.214***	6,951.093***
	(780.115)	(628.231)
Foreign-born, citizen	729.819***	441.797**
	(252.630)	(201.830)
Foreign-born, non-citizen	167.903	277.116
	(328.162)	(260.785)
Any children under 18	-16.553	508.805***
	(139.482)	(111.468)
Non-English speaker	-462.908**	343.024**
	(217.922)	(172.861)
Any transfer income	-783.509***	-3,001.126***
	(91.796)	(93.965)
Observations	158,168	158,168
R^2	0.012	0.184

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.
Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and whose absolute deviation of survey from administrative earnings was in the bottom 99 percent of this distribution. The estimates in this table result from OLS regressions. The dependent variable in column 1 is the difference between DER earnings and SIPP earnings. The dependent variable in column 2 is the absolute difference between DER earnings and SIPP earnings. Unit nonresponse occurs in two scenarios: when non-interviewed individuals reside with interviewed individuals and when interviewed individuals decline to provide any information about their labor market situations. Item nonresponse occurs when interviewed individuals provide some information about their labor market situations but decline to provide information about either earnings from a job for an employer, earnings from a self-employed business, earnings from moonlighting, or severance pay. Other controls include CBSA size indicators, cubic age, and quartic age. Non-English speaker indicates individuals who speak a language other than English in the home. All time-varying explanatory variables are defined as of December for each year. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 5. Deviation of SIPP Earnings from DER Earnings: Detailed Nonresponse Type

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
Any hot-deck imputation	-6,625.835***	3,681.865***
	(317.637)	(243.895)
Any Type-Z imputation	2,239.581***	162.567
	(193.760)	(170.977)
Any longitudinal labor force imputation	-3,051.713***	2,686.463***
	(448.855)	(343.988)
Any imputation based on last month — Reported	408.605	2,269.766***
	(309.375)	(232.990)
Any imputation based on last month — Imputed	3,515.126***	7,322.420***
	(431.305)	(313.817)
Any imputation based on last month — Logical	1,264.158	5,519.321***
	(828.437)	(624.038)
Any proxy response	663.439***	192.747**
	(105.395)	(87.480)
Any logical imputation	861.216***	387.869**
	(210.012)	(163.952)
Midwest	-242.947	-527.380***
	(151.169)	(125.598)
South	-137.319	-113.122
	(145.999)	(120.468)
West	-246.388	-104.167
	(166.834)	(138.628)
Number of household members	-110.245	-99.125
	(81.124)	(65.655)
Number of family members	49.995	19.150
	(82.354)	(66.543)
Age	-353.291***	-199.345**
	(111.733)	(94.431)
Age squared	18.471***	27.193***
	(3.584)	(3.047)
Female	-657.707***	-3,155.184***
	(98.411)	(81.507)
Black, non-Hispanic	589.069**	422.143*
	(264.124)	(222.568)
Asian, non-Hispanic	1,227.504***	700.237**
	(419.560)	(340.854)
White, non-Hispanic	408.323*	569.125***
	(231.986)	(197.436)
Hispanic	1,329.741***	252.460
	(290.758)	(242.839)
Married, spouse absent	329.051	-6.163
	(483.246)	(413.319)
Never married	-392.356**	-1,263.864***
	(159.063)	(131.828)

(*continued...*)

Table 5 (continued). Deviation of SIPP Earnings from DER Earnings: Detailed Nonresponse Type

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Previously married	75.194	-453.692***
	(123.523)	(103.957)
Elementary school	-150.105	-157.089
	(171.942)	(151.558)
Some high school	127.941	-464.543***
	(117.814)	(103.425)
Some college	396.103***	663.960***
	(137.428)	(115.034)
Associate's degree	336.891***	616.686***
	(130.146)	(108.013)
Bachelor's degree	1,061.617***	2,947.809***
	(171.100)	(141.157)
Master's degree	1,342.442***	4,088.196***
	(270.686)	(219.819)
Professional degree	3,036.228***	10,754.686***
	(863.685)	(701.219)
Doctorate degree	2,365.209***	7,087.293***
	(777.970)	(639.216)
Foreign-born, citizen	663.093***	517.547**
	(251.161)	(203.632)
Foreign-born, non-citizen	150.369	371.979
	(325.031)	(261.043)
Any children under 18	157.235	542.436***
	(139.970)	(113.302)
Non-English speaker	-382.178*	357.823**
	(216.416)	(174.164)
(*continued...*)		

Table 5 (continued). Deviation of SIPP Earnings from DER Earnings: Detailed Nonresponse Type

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Any transfer income	-641.890***	-3,163.082***
	(92.211)	(94.931)
Observations	158,168	158,168
R^2	0.019	0.177

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and whose absolute deviation of survey from administrative earnings was in the bottom 99 percent of this distribution. The estimates in this table result from OLS regressions. The dependent variable in column 1 is the difference between DER earnings and SIPP earnings. The dependent variable in column 2 is the absolute difference between DER earnings and SIPP earnings. Type-Z imputation was performed for non-interviewed individuals who reside with interviewed individuals. Longitudinal labor force imputation was performed for interviewed individuals who decline to provide any information about their labor market situations when information about this situation was available last wave. Hot deck imputation was performed when some component of earnings is missing and no information about this income is available from a previous month. Imputation based on last month was performed when some component of earnings is missing and information about this earnings is available from a previous month. This previous earnings may also have been imputed based on last month's data. The initial month's earnings that is used to impute subsequent months' earnings was either reported, hot-deck imputed, or logically imputed. Other controls include CBSA size indicators, cubic age, and quartic age. Non-English speaker indicates individuals who speak a language other than English in the home. All time-varying explanatory variables are defined as of December for each year. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 6. Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
Any hot-deck imputation	-2,373.737***	-1,860.838***
	(454.256)	(335.743)
Any Type-Z imputation	3,218.760***	1,312.942***
	(655.354)	(504.035)
Any longitudinal labor force imputation	-2,671.681***	-493.946
	(776.459)	(589.965)
Any imputation based on last month — Reported	-603.211*	2,826.048***
	(361.068)	(272.946)
Any imputation based on last month — Imputed	1,844.083***	11,223.464***
	(569.619)	(410.897)
Any imputation based on last month — Logical	2,918.492***	6,352.998***
	(972.768)	(750.235)
Any proxy response	847.808***	657.701***
	(187.429)	(146.315)
Any logical imputation	267.916	247.797
	(239.249)	(185.570)
Midwest	-615.439**	-1,349.044***
	(279.660)	(218.516)
South	-443.940	-417.339*
	(279.527)	(216.850)
West	-235.521	20.172
	(318.280)	(246.939)
Number of household members	-106.104	-213.291**
	(133.669)	(102.711)
Number of family members	45.234	171.977*
	(134.928)	(103.311)
Female	-1,024.383***	-3,073.664***
	(208.986)	(164.882)
Black, non-Hispanic	195.201	728.434*
	(533.986)	(435.294)
Asian, non-Hispanic	1,475.102*	1,398.454**
	(759.755)	(602.822)
White, non-Hispanic	471.783	450.169
	(473.633)	(392.899)
Hispanic	1,147.854**	373.014
	(551.438)	(452.080)
Married, spouse absent	1,169.394	1,228.840
	(995.899)	(783.567)
Never married	-223.674	-939.063***
	(256.934)	(202.702)
Previously married	-41.703	-1,036.111***
	(257.049)	(200.273)
Elementary school	436.021	-953.262**
	(529.826)	(435.697)
Some high school	619.300**	-407.339*
	(302.389)	(241.937)

(*continued...*)

Table 6 (continued). Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Some college	653.364***	776.717***
	(241.446)	(189.188)
Associate's degree	370.267	444.024**
	(232.189)	(181.709)
Bachelor's degree	1,848.996***	3,291.526***
	(302.260)	(237.072)
Master's degree	2,829.393***	5,192.329***
	(470.571)	(363.572)
Professional degree	7,759.132***	14,005.270***
	(1,438.533)	(1,063.294)
Doctorate degree	5,447.593***	7,402.478***
	(1,093.960)	(856.368)
Private	89.980	-364.557
	(624.663)	(474.257)
Federal government	-498.744	293.206
	(737.221)	(550.200)
State government	-2,538.030***	-2,741.091***
	(658.282)	(499.733)
Local government	-1,472.692**	-2,336.468***
	(623.949)	(473.924)
Self-employed	-11,713.350***	11,776.952***
	(503.736)	(375.654)
Hours worked	47.637***	142.884***
	(7.779)	(6.035)
Foreign-born, citizen	854.224*	-28.569
	(443.535)	(338.278)
Foreign-born, non-citizen	708.273	-99.991
	(530.578)	(413.882)
Any children under 18	578.220**	737.791***
	(225.810)	(177.320)
Non-English speaker	-271.529	198.085
	(394.104)	(305.255)
(*continued...*)		

Table 6 (continued). Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Any transfer income	-665.609	-900.633**
	(450.033)	(360.909)
Observations	82,936	82,936
R^2	0.057	0.184

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, whose absolute deviation of survey from administrative earnings was in the bottom 99 percent of this distribution, and who had both positive SIPP earnings and positive DER earnings. The estimates in this table result from OLS regressions. The dependent variable in column 1 is the difference between DER earnings and SIPP earnings. The dependent variable in column 2 is the absolute difference between DER earnings and SIPP earnings. Type-Z imputation was performed for non-interviewed individuals who reside with interviewed individuals. Longitudinal labor force imputation was performed for interviewed individuals who decline to provide any information about their labor market situations when information about this situation was available last wave. Hot deck imputation was performed when some component of earnings is missing and no information about this income is available from a previous month. Imputation based on last month was performed when some component of earnings is missing and information about this earnings is available from a previous month. This previous earnings may also have been imputed based on last month's data. The initial month's earnings that is used to impute subsequent months' earnings was either reported, hot-deck imputed, or logically imputed. Other controls include CBSA size indicators, a quartic in age, and 2-digit occupational affiliation according to the 2000 Census occupation classification system. Class of worker (*i.e.* private, federal, state, local, self-employed) and occupation indicate characteristics of employment on any job or business. Hours worked measures time worked on all jobs and businesses combined. Non-English speaker indicates individuals who speak a language other than English in the home. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 7. Deviation of Positive Reported SIPP Earnings from Positive DER Earnings

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
Any proxy response	1,038.594***	987.877***
	(185.045)	(151.796)
Midwest	-718.202***	-1,124.216***
	(271.745)	(228.361)
South	-893.430***	-381.083
	(276.719)	(232.126)
West	-216.262	318.130
	(318.415)	(263.952)
Number of household members	-152.016	-226.755**
	(136.433)	(108.334)
Number of family members	189.845	240.225**
	(136.546)	(108.274)
Female	-1,314.574***	-2,638.019***
	(207.542)	(171.934)
Black, non-Hispanic	795.904	1,030.500**
	(559.961)	(463.345)
Asian, non-Hispanic	1,449.170*	1,402.569**
	(779.660)	(653.376)
White, non-Hispanic	40.039	121.738
	(502.879)	(417.214)
Hispanic	1,273.960**	581.252
	(574.097)	(476.995)
Married, spouse absent	2,223.700*	1,596.311*
	(1,147.519)	(968.613)
Never married	213.401	-637.329***
	(246.565)	(205.548)
Previously married	-6.247	-946.922***
	(236.052)	(193.536)
Elementary school	-47.112	-459.205
	(534.686)	(453.559)
Some high school	-96.796	-234.877
	(292.804)	(244.986)
Some college	536.609**	779.355***
	(236.675)	(193.370)
Associate's degree	211.984	437.447**
	(222.384)	(181.181)
Bachelor's degree	1,695.431***	2,740.109***
	(285.425)	(236.162)
Master's degree	2,250.369***	4,512.895***
	(463.938)	(377.410)
Professional degree	6,309.052***	12,114.769***
	(1,464.160)	(1,182.989)
Doctorate degree	4,945.980***	6,820.032***
	(1,130.617)	(946.786)
Private	214.250	-332.997
	(658.468)	(526.334)

(*continued...*)

Table 7 (continued). Deviation of Positive Reported SIPP Earnings from Positive DER Earnings

VARIABLES	(1) DER-SIPP	(2) |DER-SIPP|
(*...continued*)		
Federal government	-730.979	53.019
	(776.244)	(607.078)
State government	-2,441.784***	-2,658.389***
	(686.309)	(551.760)
Local government	-1,633.911**	-2,156.390***
	(649.778)	(517.038)
Self-employed	-12,065.154***	9,625.887***
	(691.173)	(531.082)
Hours worked	45.289***	135.401***
	(8.478)	(6.920)
Foreign-born, citizen	772.930*	-203.578
	(433.429)	(352.287)
Foreign-born, non-citizen	593.051	-181.629
	(530.455)	(438.146)
Any children under 18	549.427**	679.849***
	(222.427)	(186.333)
Non-English speaker	-200.662	93.211
	(386.737)	(317.399)
Any transfer income	-369.979	-685.672*
	(445.473)	(379.950)
Observations	57,580	57,580
R^2	0.054	0.141

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, whose absolute deviation of survey from administrative earnings was in the bottom 99 percent of this distribution, who had both positive SIPP earnings and positive DER earnings, and who had no months of imputed earnings during the year. The estimates in this table result from OLS regressions. The dependent variable in column 1 is the difference between DER earnings and SIPP earnings. The dependent variable in column 2 is the absolute difference between DER earnings and SIPP earnings. Other controls include CBSA size indicators, a quartic in age, and 2-digit occupational affiliation according to the 2000 Census occupation classification system. Class of worker (*i.e.* private, federal, state, local, self-employed) and occupation indicate characteristics of employment on any job or business. Hours worked measures time worked on all jobs and businesses combined. Non-English speaker indicates individuals who speak a language other than English in the home. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 8. Characteristics of Nonrespondents

CHARACTERISTICS	Any Non-response (percent)	Obs	Unit Non-response (percent)	Obs	Item Non-response (percent)	Obs
Overall	14.9	3,849,934	5.8	3,849,934	15.4	1,915,457
Gender						
Female	12.9	2,031,415	5.2	2,031,415	14.6	939,035
Male	17.0	1,818,519	6.5	1,818,519	16.2	976,422
Education						
Less than HS	10.5	639,948	6.0	639,948	13.7	163,309
HS or some college	15.7	2,260,485	6.5	2,260,485	15.6	1,132,641
Bachelor's or postgraduate	15.8	949,501	4.0	949,501	15.6	619,507
Race and ethnicity						
White, non-Hispanic	14.6	2,668,350	5.1	2,668,350	15.7	1,362,327
Black, non-Hispanic	16.9	450,707	7.9	450,707	18.4	197,373
Asian, non-Hispanic	16.6	159,323	7.8	159,323	14.7	82,903
Hispanic	13.8	448,721	7.3	448,721	11.4	218,223
Marital status						
Married	14.4	1,994,751	4.2	1,994,751	15.3	1,110,916
Divorced or separated	14.1	479,649	4.7	479,649	14.8	258,318
Never married or widowed	15.8	1,375,534	8.6	1,375,534	16.0	546,223
Age						
Under 25	18.0	643,615	11.3	643,615	17.2	224,395
25 — 34	17.2	576,752	7.2	576,752	13.1	381,774
35 — 44	16.5	615,278	5.0	615,278	14.2	429,509
45 — 54	17.2	698,198	4.9	698,198	15.6	467,505
55 — 64	14.8	612,212	4.1	612,212	17.1	322,871
65 or older	6.4	703,879	2.8	703,879	20.4	89,403
Family Structure						
No children under 18	14.8	2,541,950	5.9	2,541,950	16.3	1,176,222
Any children under 18	15.0	1,307,984	5.8	1,307,984	14.1	739,235
Usual weekly hours worked						
Less than 20 hours	—	—	—	—	18.5	127,605
20 — 34 hours	—	—	—	—	16.7	271,208
35 or more hours	—	—	—	—	15.0	1,516,644

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14.
Note: Sample for "Any Nonresponse" and "Unit Nonresponse" columns is all person-months for people aged 15 and older. Sample for "Item Nonresponse" column is all person-months for people aged 15 and older who worked on a job or business, moonlighted, or earned severance pay. We also restrict this sample to people who were not imputed to work unpaid at a family business and people who provided some information about their job, business, moonlighting, or severance pay. Family structure includes only children living in the household. Usual hours worked includes work at all jobs and businesses. Usual weekly hours worked data are imputed for all unit nonrespondents.

Table 9. Predictors of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
Midwest	-0.007***	0.008***	-0.015***	-0.030***
	(0.001)	(0.001)	(0.001)	(0.002)
South	0.005***	0.007***	-0.002**	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
West	-0.021***	0.007***	-0.030***	-0.049***
	(0.001)	(0.001)	(0.001)	(0.002)
Number of household members	0.039***	0.046***	-0.003***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Number of family members	-0.011***	-0.016***	0.005***	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
Age	-0.023***	-0.027***	0.004***	0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Age squared	0.001***	0.001***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.040***	-0.020***	-0.023***	-0.005***
	(0.001)	(0.000)	(0.001)	(0.001)
Black, non-Hispanic	0.021***	0.011***	0.012***	0.029***
	(0.002)	(0.001)	(0.002)	(0.003)
Asian, non-Hispanic	0.008***	0.012***	-0.004*	0.007*
	(0.003)	(0.002)	(0.002)	(0.004)
White, non-Hispanic	0.015***	0.003***	0.013***	0.006**
	(0.002)	(0.001)	(0.002)	(0.003)
Hispanic	-0.013***	-0.006***	-0.008***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.003)
Married, spouse absent	0.030***	0.017***	0.016***	0.028***
	(0.004)	(0.003)	(0.003)	(0.005)
Never married	0.021***	0.016***	0.007***	0.020***
	(0.001)	(0.001)	(0.001)	(0.002)
Previously married	-0.006***	-0.013***	0.006***	0.013***
	(0.001)	(0.001)	(0.001)	(0.001)
Elementary school	-0.037***	-0.021***	-0.018***	-0.015***
	(0.002)	(0.001)	(0.001)	(0.003)
Some high school	-0.050***	-0.025***	-0.028***	-0.022***
	(0.001)	(0.001)	(0.001)	(0.002)
Some college	-0.008***	-0.015***	0.006***	-0.011***
	(0.001)	(0.001)	(0.001)	(0.002)
Associate's degree	0.019***	0.016***	0.005***	-0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
Bachelor's degree	0.010***	-0.010***	0.019***	-0.004**
	(0.001)	(0.001)	(0.001)	(0.002)
Master's degree	0.007***	-0.015***	0.022***	-0.003
	(0.002)	(0.001)	(0.001)	(0.002)
Professional degree	0.045***	-0.009***	0.056***	-0.009**
	(0.004)	(0.002)	(0.003)	(0.004)

(*continued...*)

Table 9 (continued). Predictors of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
(*...continued*)				
Doctorate degree	0.013***	-0.016***	0.028***	-0.027***
	(0.004)	(0.002)	(0.003)	(0.004)
Private	—	—	—	0.070***
				(0.003)
Federal government	—	—	—	0.085***
				(0.004)
State government	—	—	—	0.053***
				(0.004)
Local government	—	—	—	0.051***
				(0.003)
Self-employed	—	—	—	0.198***
				(0.002)
Weeks worked	—	—	—	0.018***
				(0.000)
Hours worked	—	—	—	0.000***
				(0.000)
Foreign-born, citizen	0.029***	0.017***	0.014***	0.009***
	(0.002)	(0.001)	(0.001)	(0.002)
Foreign-born, non-citizen	0.024***	0.028***	-0.002	-0.003
	(0.002)	(0.001)	(0.001)	(0.002)
Proxy response	-0.081***	-0.128***	0.037***	0.064***
	(0.001)	(0.001)	(0.001)	(0.001)
Number of interviews	0.002***	0.001***	0.001***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Any sample gaps	0.045***	0.019***	0.030***	0.052***
	(0.001)	(0.001)	(0.001)	(0.001)
Attritor	0.074***	0.061***	0.020***	0.037***
	(0.001)	(0.001)	(0.001)	(0.001)
Any children under 18	-0.066***	-0.056***	-0.017***	-0.024***
	(0.001)	(0.001)	(0.001)	(0.001)
Change in family composition	0.011***	0.011***	0.001	0.004*
	(0.002)	(0.001)	(0.001)	(0.002)
Non-English speaker	-0.013***	-0.010***	-0.004***	-0.010***
	(0.001)	(0.001)	(0.001)	(0.002)
Any transfer income	-0.064***	-0.007***	-0.061***	-0.007
	(0.001)	(0.001)	(0.001)	(0.004)
Stopped work	—	—	—	0.035***
				(0.002)

(*continued...*)

Table 9 (continued). Predictors of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
(*...continued*)				
Contingent worker	—	—	—	0.129***
				(0.007)
Observations	3,849,934	3,849,934	3,625,618	2,160,613
R^2	0.058	0.124	0.032	0.069

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14.

Note: Sample for columns 1 and 2 is all person-months for people aged 15 and older. Sample for column 3 is all person-months for people aged 15 and older who provided at least some data about their labor market situation. Sample for column 4 is all person-months for people aged 15 and older who provided at least some data about their labor market situation and who worked on a non-contingent job or business. We also restrict the sample in columns 3 and 4 to people who were not imputed to work unpaid at a family business. The estimates in this table result from OLS regressions. The dependent variable in column 1 indicates any earnings nonresponse. The dependent variable in column 2 indicates unit earnings nonresponse. Unit nonresponse occurs in two scenarios: when non-interviewed individuals reside with interviewed individuals and when interviewed individuals decline to provide any information about their labor market situations. Item nonresponse occurs when interviewed individuals provide some information about their labor market situations but decline to provide information about either earnings from a job for an employer, earnings from a self-employed business, earnings from moonlighting, or severance pay. Other controls in columns 1 through 4 include CBSA size indicators, cubic age, and quartic age. Other controls in column 4 also include 2-digit occupational affiliation according to the 2000 Census occupation classification system. In column 4, class of worker (*i.e.* private, federal, state, local, self-employed), stopped work, and occupation indicate characteristics of employment on any job or business. Hours worked and weeks worked measure time worked on all jobs and businesses combined. Any sample gaps indicates individuals who leave the survey and later return to the survey. Non-English speaker indicates individuals who speak a language other than English in the home. Standard errors are clustered at the person-wave level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 10. Predictors of Earnings Item Nonresponse

VARIABLES	(1) Jobs	(2) Jobs	(3) Businesses
Midwest	-0.019***	-0.019***	-0.026***
	(0.002)	(0.001)	(0.007)
South	0.004***	0.003**	-0.014**
	(0.001)	(0.001)	(0.006)
West	-0.036***	-0.035***	-0.090***
	(0.002)	(0.001)	(0.007)
Number of household members	-0.003***	-0.003***	-0.010**
	(0.001)	(0.001)	(0.005)
Number of family members	0.010***	0.009***	0.008
	(0.001)	(0.001)	(0.005)
Age	0.001	0.002	0.034***
	(0.002)	(0.002)	(0.011)
Age squared	-0.000	-0.000	-0.001***
	(0.000)	(0.000)	(0.000)
Female	-0.006***	-0.006***	0.007
	(0.001)	(0.001)	(0.005)
Black, non-Hispanic	0.029***	0.028***	0.023
	(0.003)	(0.003)	(0.017)
Asian, non-Hispanic	0.007*	0.008**	0.023
	(0.004)	(0.004)	(0.018)
White, non-Hispanic	0.006**	0.006**	-0.012
	(0.003)	(0.003)	(0.014)
Hispanic	-0.009***	-0.008***	-0.004
	(0.003)	(0.003)	(0.016)
Married, spouse absent	0.030***	0.030***	0.019
	(0.005)	(0.005)	(0.020)
Never married	0.021***	0.021***	0.028***
	(0.002)	(0.001)	(0.008)
Previously married	0.015***	0.014***	0.010
	(0.002)	(0.001)	(0.007)
Elementary school	-0.023***	-0.022***	-0.011
	(0.003)	(0.003)	(0.013)
Some high school	-0.029***	-0.027***	-0.003
	(0.002)	(0.002)	(0.011)
Some college	-0.015***	-0.015***	0.003
	(0.002)	(0.002)	(0.008)
Associate’s degree	-0.016***	-0.016***	-0.002
	(0.002)	(0.002)	(0.007)
Bachelor’s degree	-0.006***	-0.007***	-0.006
	(0.002)	(0.002)	(0.007)
Master’s degree	-0.010***	-0.011***	0.024***
	(0.002)	(0.002)	(0.009)
Professional degree	-0.010**	-0.011**	0.001
	(0.004)	(0.004)	(0.014)
Doctorate degree	-0.027***	-0.027***	-0.005
	(0.004)	(0.004)	(0.016)
Private	0.009***	—	—
	(0.003)		

(*continued...*)

Table 10 (continued). Predictors of Earnings Item Nonresponse

VARIABLES	(1) Jobs	(2) Jobs	(3) Businesses
(*...continued*)			
State government	-0.002	—	—
	(0.003)		
Local government	-0.006**	—	—
	(0.003)		
Weeks worked	-0.003***	—	0.013***
	(0.001)		(0.004)
Hours worked	-0.001***	-0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Foreign-born, citizen	0.008***	0.008***	-0.004
	(0.002)	(0.002)	(0.009)
Foreign-born, non-citizen	-0.006**	-0.005**	0.008
	(0.002)	(0.002)	(0.011)
Proxy response	0.055***	0.054***	0.105***
	(0.001)	(0.001)	(0.005)
Number of interviews	0.001***	0.001***	0.005***
	(0.000)	(0.000)	(0.001)
Any sample gaps	0.053***	0.052***	0.040***
	(0.001)	(0.001)	(0.005)
Attritor	0.037***	0.036***	0.027***
	(0.001)	(0.001)	(0.005)
Any children under 18	-0.025***	-0.024***	-0.024***
	(0.001)	(0.001)	(0.006)
Change in family composition	0.007***	0.005**	0.013
	(0.002)	(0.002)	(0.011)
Non-English speaker	-0.012***	-0.011***	-0.008
	(0.002)	(0.002)	(0.008)
Any transfer income	-0.009**	-0.008*	-0.013
	(0.005)	(0.004)	(0.021)
Stopped work	0.045***	—	0.003
	(0.002)		(0.018)
Paid hourly	-0.019***	-0.018***	—
	(0.001)	(0.001)	
Contingent worker	—	0.058***	0.205***
		(0.007)	(0.058)
Salaried	—	—	-0.025***
			(0.004)

(*continued...*)

Table 10 (continued). Predictors of Earnings Item Non-response

VARIABLES	(1) Jobs	(2) Jobs	(3) Businesses
(*...continued*)			
Other income	—	—	0.218***
			(0.014)
Observations	1,703,276	1,756,622	201,191
R^2	0.027	0.026	0.039

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14.

Note: Sample for column 1 is all person-job-months for people aged 15 and older, who worked on a non-contingent basis at a job for an employer, and who provided some information about their labor market situation. Sample for column 2 is all person-job-months for people aged 15 and older, who worked at a job for an employer, and who provided some information about their labor market situation. Sample for column 3 is all person-business-months for people aged 15 and older, who worked at a self-employed business, and who provided some information about their labor market situation. We also restrict the sample in columns 1 and 2 to people who were not imputed to work unpaid at a family business. The estimates in this table result from OLS regressions. The dependent variable in columns 1 and 2 indicates item nonresponse to questions about earnings at a job for an employer. The dependent variable in column 3 indicates item nonresponse to questions about earnings at a self-employed business. Other controls in all columns include CBSA size indicators, cubic age, and quartic age, and 2-digit occupational affiliation according to the 2000 Census occupation classification system. A federal government worker indicator is also among the controls in columns 1 and 2. Class of worker indicators (*i.e.* private, federal, state, local, self-employed), stopped work indicator, occupation indicators, usual weekly hours worked, weeks worked, hourly pay indicator, salaried indicator, and other income indicator are all defined separately for each person-job-month or person-business month observation based on the characteristics of each job or business. Any sample gaps indicates individuals who leave the survey and later return to the survey. Non-English speaker indicates individuals who speak a language other than English in the home. Standard errors are clustered at the person-wave level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 11. Ignorability of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
Midwest	-0.011***	0.003***	-0.014***	-0.029***
	(0.001)	(0.001)	(0.001)	(0.002)
South	0.007***	0.004***	0.004***	0.008***
	(0.001)	(0.001)	(0.001)	(0.002)
West	-0.021***	0.003***	-0.025***	-0.045***
	(0.001)	(0.001)	(0.001)	(0.002)
Number of household members	0.021***	0.026***	-0.003***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Number of family members	0.001	-0.004***	0.006***	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
Age	-0.023***	-0.021***	-0.003***	0.008**
	(0.001)	(0.001)	(0.001)	(0.003)
Age squared	0.001***	0.001***	0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.028***	-0.012***	-0.017***	-0.004**
	(0.001)	(0.001)	(0.001)	(0.001)
Black, non-Hispanic	0.005*	0.003*	0.002	0.019***
	(0.003)	(0.002)	(0.002)	(0.004)
Asian, non-Hispanic	-0.003	0.003	-0.005*	-0.000
	(0.004)	(0.002)	(0.003)	(0.005)
White, non-Hispanic	0.004	-0.006***	0.010***	0.008**
	(0.003)	(0.002)	(0.002)	(0.004)
Hispanic	-0.026***	-0.010***	-0.017***	-0.017***
	(0.003)	(0.002)	(0.002)	(0.004)
Married, spouse absent	0.009**	0.001	0.008**	0.021***
	(0.005)	(0.003)	(0.004)	(0.007)
Never married	0.017***	0.011***	0.008***	0.018***
	(0.002)	(0.001)	(0.001)	(0.002)
Previously married	-0.007***	-0.012***	0.004***	0.007***
	(0.001)	(0.001)	(0.001)	(0.002)
Elementary school	-0.012***	-0.008***	-0.004***	-0.012**
	(0.002)	(0.001)	(0.001)	(0.005)
Some high school	-0.018***	-0.009***	-0.010***	-0.016***
	(0.002)	(0.001)	(0.001)	(0.003)
Some college	-0.008***	-0.008***	-0.000	-0.010***
	(0.001)	(0.001)	(0.001)	(0.002)
Associate's degree	-0.002*	-0.004***	0.002	-0.008***
	(0.001)	(0.001)	(0.001)	(0.002)
Bachelor's degree	0.000	-0.011***	0.011***	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Master's degree	-0.003	-0.013***	0.009***	-0.002
	(0.002)	(0.001)	(0.002)	(0.003)
Professional degree	0.031***	-0.008***	0.040***	-0.010*
	(0.005)	(0.002)	(0.004)	(0.006)
Doctorate degree	-0.005	-0.013***	0.007	-0.030***
	(0.005)	(0.002)	(0.004)	(0.006)

(*continued...*)

Table 11 (continued). Ignorability of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
(*...continued*)				
Private				0.066***
				(0.004)
Federal government	—	—	—	0.086***
				(0.005)
State government	—	—	—	0.052***
				(0.005)
Local government	—	—	—	0.050***
				(0.005)
Self-employed	—	—	—	0.197***
				(0.003)
Weeks worked	—	—	—	0.018***
				(0.000)
Hours worked	—	—	—	0.001***
				(0.000)
Foreign-born, citizen	0.015***	0.008***	0.008***	0.008***
	(0.002)	(0.001)	(0.002)	(0.003)
Foreign-born, non-citizen	0.008***	0.009***	-0.001	-0.001
	(0.003)	(0.002)	(0.002)	(0.003)
Proxy response	-0.050***	-0.086***	0.030***	0.054***
	(0.001)	(0.001)	(0.001)	(0.001)
Number of interviews	0.003***	0.001***	0.002***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)
Any sample gaps	0.035***	0.019***	0.019***	0.034***
	(0.001)	(0.001)	(0.001)	(0.002)
Attritor	0.039***	0.030***	0.012***	0.023***
	(0.001)	(0.001)	(0.001)	(0.002)
Any children under 18	-0.042***	-0.035***	-0.010***	-0.021***
	(0.001)	(0.001)	(0.001)	(0.002)
Change in family composition	-0.023***	-0.017***	-0.008***	-0.014***
	(0.002)	(0.001)	(0.002)	(0.003)
Non-English speaker	-0.003	-0.004***	0.001	-0.006**
	(0.002)	(0.001)	(0.002)	(0.003)
Any transfer income	-0.027***	-0.006***	-0.022***	-0.008
	(0.002)	(0.001)	(0.001)	(0.006)
Stopped work	—	—	—	0.028***
				(0.003)
Contingent worker	—	—	—	0.107***
				(0.010)
Any admin records	0.053***	0.010***	0.044***	-0.078***
	(0.002)	(0.001)	(0.002)	(0.004)
Number of admin records	0.031***	0.002***	0.031***	0.024***
	(0.001)	(0.001)	(0.001)	(0.001)
Bottom admin earnings quintile	-0.006***	-0.004***	-0.002	0.045***
	(0.002)	(0.001)	(0.002)	(0.003)
Second admin earnings quintile	0.015***	-0.005***	0.020***	0.018***
	(0.002)	(0.001)	(0.002)	(0.002)

(*continued...*)

Table 11 (continued). Ignorability of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
(*...continued*)				
Fourth admin earnings quintile	-0.010***	-0.002	-0.009***	-0.003
	(0.002)	(0.001)	(0.002)	(0.002)
Top admin earnings quintile	-0.002	-0.002	-0.001	0.004**
	(0.002)	(0.001)	(0.002)	(0.002)
Observations	1,910,102	1,910,102	1,843,692	1,055,629
R^2	0.054	0.076	0.048	0.080

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample for columns 1 and 2 is all person-months for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. Sample for column 3 is all person-months for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the year, and who provided at least some data about their labor market situation. Sample for column 4 is all person-months for people aged 15 and older; who were assigned a PIK; who were present in the survey for all 12 months of the year; who provided at least some data about their labor market situation; and who worked on a non-contingent job or business. We also restrict the sample in columns 3 and 4 to people who were not imputed to work unpaid at a family business. The estimates in this table result from OLS regressions. The dependent variable in column 1 indicates any earnings nonresponse. The dependent variable in column 2 indicates unit earnings nonresponse. Unit nonresponse occurs in two scenarios: when non-interviewed individuals reside with interviewed individuals and when interviewed individuals decline to provide any information about their labor market situations. Item nonresponse occurs when interviewed individuals provide some information about their labor market situations but decline to provide information about either earnings from a job for an employer, earnings from a self-employed business, earnings from moonlighting, or severance pay. Other controls in columns 1 through 4 include CBSA size indicators, cubic age, and quartic age. Other controls in column 4 also include 2-digit occupational affiliation according to the 2000 Census occupation classification system. In column 4, class of worker (*i.e.* private, federal, state, local, self-employed), stopped work, and occupation indicate characteristics of employment on any job or business. Hours worked and weeks worked measure time worked on all jobs and businesses combined. Any sample gaps indicates individuals who leave the survey and later return to the survey. Non-English speaker indicates individuals who speak a language other than English in the home. We constructed the distribution of person-year level, positive administrative earnings without sample weights. Standard errors are clustered at the person-wave level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 12. Impact of Earnings Nonresponse: Gender Earnings Gap Estimates

	(1)	(2)	(3)	(4)
VARIABLES	SIPP	DER	Reported SIPP	SIPP-DER Hybrid
Female	-12,035.131***	-13,288.647***	-13,032.824***	-12,242.796***
	(317.847)	(359.504)	(380.895)	(331.799)
Observations	88,971	88,971	60,994	88,971
R^2	0.035	0.035	0.041	0.034

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.
Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, who exhibited both positive SIPP earnings and positive DER earnings, and whose SIPP and DER earnings were both in the bottom 99 percent of their respective distributions. Column 3 also restricts the sample to individuals who had no imputed earnings data for any month of the year. The estimates in this table result from OLS regressions. The dependent variable in columns 1 and 3 is SIPP earnings. The dependent variable in column 2 is DER earnings. The dependent variable in column 4 is a hybrid, which is defined as SIPP earnings for people who had no month of imputed SIPP earnings data during the year and DER earnings for people who had at least one month of imputed SIPP earnings data during the year. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 13. Impact of Earnings Nonresponse: Racial Earnings Gap Estimates

VARIABLES	(1) SIPP	(2) DER	(3) Reported SIPP	(4) SIPP-DER Hybrid
Black alone	-1,105.722	-717.163	-2,533.923**	-1,070.417
	(845.454)	(955.005)	(1,049.950)	(878.123)
White alone	8,172.760***	8,708.535***	8,024.679***	8,532.522***
	(775.277)	(880.799)	(967.692)	(808.963)
Asian alone	14,448.261***	17,454.187***	15,590.213***	15,526.097***
	(1,212.485)	(1,407.620)	(1,490.752)	(1,270.065)
Observations	88,971	88,971	60,994	88,971
R^2	0.011	0.011	0.014	0.012

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, who exhibited both positive SIPP earnings and positive DER earnings, and whose SIPP and DER earnings were both in the bottom 99 percent of their respective distributions. Column 3 also restricts the sample to individuals who had no imputed earnings data for any month of the year. The estimates in this table result from OLS regressions. The dependent variable in columns 1 and 3 is SIPP earnings. The dependent variable in column 2 is DER earnings. The dependent variable in column 4 is a hybrid, which is defined as SIPP earnings for people who had no month of imputed SIPP earnings data during the year and DER earnings for people who had at least one month of imputed SIPP earnings data during the year. SIPP gives respondents the option of reporting more than one race. The indicators in this table define racial groups to include individuals who reported only one race. Individuals who reported multiple races are included in the omitted group. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

Table 14. Impact of Earnings Nonresponse: Mincer Regression Estimates

VARIABLES	(1) SIPP	(2) DER	(3) Reported SIPP	(4) SIPP-DER Hybrid
Years of education	4,952.861***	5,206.608***	5,367.872***	5,082.564***
	(69.979)	(80.359)	(83.510)	(73.782)
Potential experience	1,899.405***	2,182.327***	1,987.822***	2,034.514***
	(29.135)	(32.053)	(35.487)	(29.938)
Potential experience squared	-31.146***	-35.785***	-32.806***	-33.304***
	(0.608)	(0.665)	(0.723)	(0.626)
Observations	88,971	88,971	60,994	88,971
R^2	0.233	0.222	0.252	0.236

Source: Authors' calculation from the 2008 panel of the Survey of Income and Program Participation, Waves 1 through 14 and the Social Security Administration's Detailed Earnings Record, calendar years 2009 through 2012.

Note: Sample includes all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, who exhibited both positive SIPP earnings and positive DER earnings, and whose SIPP and DER earnings were both in the bottom 99 percent of their respective distributions. Column 3 also restricts the sample to individuals who had no imputed earnings data for any month of the year. The estimates in this table result from OLS regressions. The dependent variable in columns 1 and 3 is SIPP earnings. The dependent variable in column 2 is DER earnings. The dependent variable in column 4 is a hybrid, which is defined as SIPP earnings for people who had no month of imputed SIPP earnings data during the year and DER earnings for people who had at least one month of imputed SIPP earnings data during the year. When education was reported in a range, years of education is defined as the midpoint of that range. Potential experience is defined as age minus years of education minus 5. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.