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

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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 tendency has motivated some researchers to exclude imputed earnings 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 document that after controlling for observable heterogeneity imputed survey earnings differ from administrative earnings by more than reported survey earnings do on average, although there is considerable heterogeneity across imputation methods in the degree of concordance between survey and administrative data. We illustrate a wave-like pattern of survey earnings nonresponse over the administrative earnings distribution. Finally, we show that differences between survey and administrative earnings can affect coefficient estimates of earnings regressions, depending upon the regressors of interest. On one hand, the estimated returns to self-employment fall 42.3 log points when replacing all survey earnings with administrative earnings and 24.1 log points when replacing only imputed survey earnings with administrative earnings. On the other hand, the corresponding differences are statistically insignificant for estimates of the gender earnings gap.

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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. One branch of the literature considers the pattern of survey earnings nonresponse, the process by which the Census Bureau imputes earnings data, and implications for how analysts can treat imputed observations. For example, Bollinger et al. (2015a,b) link the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) to the DER. They find that nonresponse follows a "U-shaped" pattern over the DER earnings distribution and that this pattern of nonresponse affects estimates of inequality. Another branch of the literature evaluates survey earnings data quality by comparison to some alternative source, denoting the difference as "measurement error". For example Cristia and Schwabish (2009) 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. Our study will attempt to merge these two strands of the literature to extract new insights.

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 some researchers to question the reliability of imputed labor earnings and to exclude these

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

observations from wage analyses.[2] 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 examine how the relationship between survey data and administrative records varies across demographic groups in order to identify whose earnings data will be impacted most by recent proposals to incorporate administrative data into survey data more extensively.[3] We also characterize survey nonrespondents. These observations are likely to be noisiest for analysts who decide to include imputed observations. Finally we investigate how the coefficient estimates of some key earnings regressions vary with the source of earnings data.

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 $1,413. We find that the difference in imputed survey earnings and administrative exhibits a wider variance than the analogous relationship for reported survey data.[4] We document that some key demographic groups have both larger deviations between survey and administrative data and higher likelihoods of earnings nonresponse relative to other demographic groups. We establish a wave-like pattern of survey earnings nonresponse over the administrative survey distribution. 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. The difference between survey and administrative earnings can alter coefficient estimates, depending upon the regressor of interest. On one hand, the estimated returns to self-employment fall by 42.3 log points when replacing all survey earnings with administrative earnings and 24.1 log points when replacing only imputed survey earnings with administrative earnings. On the other hand, the corresponding differences for estimates of the gender earnings gap are statistically insignificant from 0.

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

[2] Heckman and LaFontaine (2006) argue that the positive wage returns to a General Educational Development (GED) certification in unadjusted CPS data arise in part due to the inclusion of imputed earnings values.

[3] Meyer et al. (2015) provides a description of the advantages and disadvantages of administrative data linked to survey data. They ultimately recommend linking administrative data to survey data and substituting administrative variables for survey questions as a solution to the related problems of rising nonresponse, rising measurement error, and high perceptions of respondent burden.

[4] 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.

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 three well-developed branches of the literature. The first set of studies 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 earnings report matches 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 sample of workers at a large manufacturing company. These studies hypothesize that firms report employees' earnings accurately, and they therefore treat any deviation of employee 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. This work has produced mixed evidence about whether measurement error in earnings affects coefficient estimates of earnings regressions.

A second common validation technique measures the degree to which an employee's earnings report matches 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).[5] 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 present evidence that 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 So-

[5]The CPS Annual Demographic File is often referred to as the March CPS, or more recently as the CPS ASEC.

cial 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 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.

The second, related section of the literature evaluates the quality of imputed earnings data. Like other Census Bureau surveys, SIPP imputes missing data using a "hot-deck" procedure which copies 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. Earnings estimates would be biased if earnings nonresponse is related to earnings itself after conditioning on the match characteristics. Moreover, earnings estimates might be biased even if earnings are conditionally missing at random. One important disadvantage of the hot-deck 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. 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.[6] These two forms of "match bias" have motivated proposals for more 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.

Although analysts' most common approach is to include imputed earnings observations, some empirical researchers have pursued various strategies to remove or mitigate match bias and response bias. One simple approach is to exclude imputed earnings values from analyses. However, this strategy assumes that earnings nonresponse is unrelated to earnings itself ("ignorable"). Bollinger and Hirsch (2013) examine the validity of this assumption, 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

[6] While some more recent research has argued 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.

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.

This paper differs from the pre-existing studies in this branch of the literature by considering survey earnings nonresponse among both survey workers and survey nonworkers. Additionally, we explore the predictors of survey earnings nonresponse, aiming to identify whose survey earnings are likely to be noisier for analysts who decide to include imputed earnings observations. We pair these findings with estimated deviations between survey and administrative earnings conditional on imputation status to infer which coefficient estimates of earnings regressions are likely to be most sensitive to analysts' decisions of how to treat imputed observations.

Third, this paper builds on assessments of the reliability of self-employed individuals' income reports to tax authorities. LaLumia (2009) and Saez (2010) argue that self-employed individuals have greater flexibility to report income strategically in order to optimize after-tax income given the lack of third party reporting and tax withholding. Both studies claim that self-employed incomes are more responsive than wage employment incomes are to the structure of the Earned Income Tax Credit. Internal Revenue Service (2016) estimates that the underreporting of self-employment income accounted for about $125 billion or 27 percent of all tax liability that was not paid voluntarily or timely over the period of 2008 through 2010. Feldman and Slemrod (2007) utilize unaudited tax returns and charitable contributions to conclude that self-employed individuals underreport income to tax authorities by 34.1 percent on average relative to wage and salary workers. While much of this work focuses on self-employed individuals' tendency to underreport income to tax authorities, Hurst et al. (2014) and others contend that self-employed individuals also underreport their income to household surveys. They exploit a methodology proposed by Pissarides and Weber (1989) that uses survey reports of expenditures and incomes to estimate separate Engel curves for wage and salary workers and self-employed workers. Hurst et al. (2014) present evidence that self-employed individuals underreport their incomes to household surveys by about 25%, assuming that these two classes of workers have the same Engel curve. While Pedace and Bates (2000) directly compared individuals' self-employment income across survey and administrative data, we expand on their analysis by examining nonresponse among the self-employed and consider implications for estimates of the returns to self-employment.

3 Data

Next we describe the data sources that we utilize. We begin by discussing the Survey of Income and Program Participation along in section 3.1 and the Detailed Earnings Record alone in 3.2. We then discuss the linked dataset in 3.3.

3.1 Survey of Income and Program Participation

The SIPP is a large, nationally representative 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 insurance, child care, 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 in 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 less comprehensive 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.[7]

In this paper, we use core data from the 2008 panel between May 2008 and March 2013 relating to earnings from a job or self-employed business.[8] SIPP collects detailed data on up to two jobs and two businesses per wave. We also include severance pay and earnings from

[7]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

[8]In the 2008 panel SIPP data which we employ, it is not possible to distinguish between self-employed jobs and self-owned businesses. Consequently, we will use these terms interchangably for the remainder of the paper. Nevertheless, Light and Munk (2015) use data from the 1979 National Longitudinal Survey of Youth to show that 68% of self-employed jobs are not independently reported as self-owned businesses.

"moonlighting," which are collected separately. When studying the predictors of earnings nonresponse our unit of observation is the person-month. When comparing SIPP and DER earnings we aggregate earnings from three or four different waves up to the person-year level (depending on rotation group).[9]

The SIPP reports an "allocation" status flag associated with almost all variables in the survey.[10] 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 the previous month's data.[11] The imputation method depends on the availability of earnings data from the previous month and the likely veracity of any reported earnings data. First, a hot-deck imputation method substitutes the data of a responder to fill in for an item with nonresponse, where the donor is matched cross-sectionally on several observable characteristics. Second, if earnings for a job or business are available in the prior month, 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.[12] We define item nonresponse as any of these three scenarios.

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 classify a person-year observation as imputed in a particular way if any month

[9] 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.

[10] A notable exception is recoded variables. These are transformations of other variables and have no imputation.

[11] 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.

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

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.[13]

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.[14] This flag indicates whether the survey information was obtained from a self-report about the respondent, from a proxy report about the respondent, 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 including allocation flags imputed from a single donor with similar observable characteristics. Similarly, nonrespondents can have all labor force data imputed if an individual declines to provide any employment information. We define unit nonresponse as either of these two scenarios.[15] Among unit nonrespondents, the imputation method depends on the 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 and allocation flags were imputed from a single donor with similar observable characteristics.[16] 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. The process by which the Census Bureau edits responses has also changed significantly for the 2014 panel. Notably, the imputation methodology for Type-Z nonrespondents will no longer have all of their data imputed from the same donor. Consequently, the imputation status

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

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

[15]Note that only the first of these scenarios would correspond to unit nonresponse for the entire survey.

[16]Note that when a donor's allocation flags are copied to a nonrespondent, the nonrespondent's allocation flags will indicate that that the nonrespondent's earnings were reported if the donor's earnings were reported.

of a given variable will be more transparent for the 2014 panel.

3.2 Detailed Earnings Record

The Detailed Earnings Record (DER) is provided to the Census Bureau by the Social Security Administration. The DER includes wage and salary earnings (Box 1), both deferred and nondeferred earnings, and self-employment earnings reported to the IRS.[17] 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) for workers for an employer and at the level of one observation per person-year (1040 Schedule SE) for self-employed individuals. We aggregate both deferred and nondeferred earnings from all jobs and businesses up to the person-year level.

The DER data are processed by the Census Bureau and linked to surveys using a Protected 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 administrative data are available and those for whom they are not. There is also the possibility of other types of errors such as reporting 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 all earnings sources by year. SIPP respondents can report earnings data in a number of ways. Those with a job for an employer

[17] 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.

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.[18]

We categorize an individual as self-employed in a particular year based on SIPP data from that year regardless of whether that individual had 1040 Schedule SE income in the DER for that year. 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.

When comparing SIPP and DER earnings, we restrict our estimation samples to individuals who were assigned a PIK and whose absolute deviation between DER and SIPP earnings is not in the top 1 percent of this distribution. We also drop person-year observations for individuals who were not present in the survey for all 12 months of the calendar year. Consequently, most of our analysis covers the period 2009 through 2012, although we also include survey data from 2008 and 2013 when studying nonresponse in survey data without conditioning on DER variables. Finally, our full sample also includes all individuals aged 15 and older at the end of the calendar year.

Table 1 shows the matched SIPP and DER sample.[19] Person-year observations with either zero earnings in both sources or positive earnings in both sources make up 92.1 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 our full sample and for the members of that sample who had both positive SIPP earnings and positive DER earnings. The second column shows the absolute value of the DER-SIPP difference for the same samples. The magnitude of the differences and the characteristics correlated with larger differences are explored in the next section.

[18]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.

[19]Since Table 1 summarizes the presence of earnings rather than the level of earnings, the sample for this table includes observations that are not present in our full sample due to their extreme absolute difference between DER and SIPP earnings.

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 and by imputation status. We then proceed to regression analysis in section 4.2, which produces estimates of average deviations 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. These observations will be affected most by the recent proposals to incorporate administrative data into survey data more extensively.

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 for our full sample. 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.[20] There are three important trends evident in this figure. Primarily, the bulk of the joint distribution lies within a band around the 45-degree line, where DER earnings equal SIPP earnings.[21] Second, points outside of this band are more likely to have DER earnings in excess of SIPP earnings than vice versa. Third, 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 of the DER-SIPP earnings gap for our full sample. 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.

[20] Note that for this and all other scatterplots we have perturbed each data point by adding spherical random error 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.

[21] 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.

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.[22] The same three inferences that Figure 1 makes apparent also materialize in Figure 2. First, 65.9 percent of the sample has DER earnings within \$5,000 of SIPP earnings. Second, of the remaining 34.1 percent of the joint distribution, 20.7 percent is characterized by DER earnings in excess of SIPP earnings. Finally, 21.0 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. Imputation is one common source of survey earnings data which analysts hypothesize affects data quality. To gauge the validity of this concern, Figure 3 plots the unconditional relationship between administrative earnings and survey earnings by imputation status for our full sample.[23] 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 3 generally appear surprisingly comparable at relatively low earnings levels given the degree of concern that some have expressed about the quality of imputed data. Nevertheless, expanding our focus to higher earnings levels reveals a seemingly higher relative frequency with which SIPP earnings deviate substantially from DER earnings compared to the reported data panel.[24]

Figure 4 presents the same unconditional relationship between administrative earnings and

[22]Since Figure 2 collapses observations with extreme DER-SIPP earnings differences into a single bin, the sample for this figure includes observations that are not present in our full sample due to their extreme absolute difference between DER and SIPP earnings.

[23]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 our full sample. This larger subsample allows for a more complete view of the joint distribution of SIPP and DER earnings by imputation status given the relatively low incidence of earnings imputation.

[24]As a means of comparison, Figure A1 plots the unconditional relationship between administrative earnings and survey earnings by proxy interview status for our full sample. 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, leading analysts such as Bollinger and Hirsch (2009) to hypothesize that proxy response affects data quality. To gauge the degree to which proxy interviews influence the unconditional relationship between administrative earnings and survey earnings, the panel on the left displays SIPP earnings and DER earnings for individuals who experienced no 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 A1 generally appear surprisingly comparable given the degree of concern that some have expressed about the quality of proxy-reported data. Nevertheless, even though 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. The correspondence between survey and administrative earnings thus appears generally closer for proxy-reported survey earnings relative to imputed survey earnings.

survey earnings by imputation status in a different format. In particular, this figure separately plots the univariate kernel density estimates of the DER-SIPP earnings difference by imputation status. Our estimation sample for these kernel densities is our full sample, excepting all person-years for which survey earnings differed from administrative earnings by 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 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 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.[25]

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 transfers; NR_{it}

[25]The p-value of this test rounds to 0.000.

is an indicator of nonresponse; u_{it} is a normal error term; and α, β, and γ are parameters to be estimated via ordinary least squares.

In what follows, we define d_{it} as both the absolute value of the difference between SIPP and DER annual earnings ($|DER_{it} - SIPP_{it}|$) and the natural log of that absolute difference.[26] Positive (negative) cofficient estimates in these specifications measure the degree to which regressors on average are associated with DER earnings further from (closer to) SIPP earnings. We use several definitions of NR_{it} to include various types of item and unit nonresponse.

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.[27] We adhere to the latter interpretation when reviewing the estimates of β from equation 1, and thus do not view our results as characterizing measurement error in earnings exactly. Instead, we view our results as speaking to the implications of the increasingly prevalent proposals to incorporate administrative earnings into survey earnings more extensively. Abowd and Stinson (2013) also argue that imputed survey earnings are less reliable than administrative earnings. Based on this evidence, we interpret our estimates of γ from equation 1 as being correlated with the average error in imputed earnings, but not as indicating the average error in imputed earnings exactly.

Table 3 presents the results of equation 1 estimated on our full sample where d_{it} is defined as the absolute difference between DER and SIPP earnings. Each column of Table 3 reflects a different definition of NR_{it} in order to explore whether the different imputation methods yield survey earnings that compare with administrative earnings to varying degrees. Column 1 includes only one nonresponse indicator which takes a value of 1 for person-year observations exhibiting any Census imputed earnings source as part of total SIPP annual earnings.[28] Column

[26]We have also estimated specifications with d_{it} defined as the raw difference rather than the absolute difference between DER and SIPP annual earnings ($DER_{it} - SIPP_{it}$). Estimates are available upon request.

[27]Administrative data might not reflect truth if there are conceptual differences in how earnings are measured in survey and administrative data. Additionally, administrative data are subject to reporting error, although this likely occurs less frequently than it does in survey data. Administrative data would not reflect truth if a sample member's PIK is misassigned. This is especially relevant for the linkage between SIPP and administrative data because some PIKs are assigned to multiple sample members. When this occurs, we match the administrative data associated with that PIK to only one of those sample members. In future work, we plan to explore the robustness of our results to this decision.

[28]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

2 contains two more detailed indicators: one which takes a values of 1 for person-year observations exhibiting unit nonresponse for at least one month of the year, and another which takes a value of 1 for person-year observations exhibiting no unit nonresponse but item nonresponse for at least one earnings source in at least one month of the year. Column 3 offers estimates for our most detailed nonresponse indicators. The Type-Z indicator identifies unit nonrespondents who have all of their labor force data assigned from a single, contemporaneous donor record. The longitudinal labor force imputation indicator identifies the remaining unit nonrespondents whose data are imputed longitudinally by projecting data from the individual's previous interview through the current interview. The hot deck imputation indicator identifies nonrespondents who had no available earnings information from the previous month, for whom missing earnings items are copied from a donor with similar contemporaneous observable characteristics. The logical imputation indicator identifies nonrespondents whose earnings were imputed using data reported elsewhere in the survey to enforce logical consistency. Finally, the imputation based on last month indicators collectively identify nonrespondents who had earnings available from a job or business from the prior month, for whom missing earnings items are copied from a donor with 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. To investigate whether the quality of data imputed according to this technique depends on the source of earnings that initialized this longitudinal chain of imputations, we include separate indicators that identify whether this string of imputed data can be traced back to reported data, cross-sectional hot-deck imputed data, logically imputed data, or Type-Z imputed data.

Column 1 reports that the absolute difference between SIPP and DER earnings is about \$6,821 more on average for person-years with at least some imputed earnings relative to those with no earnings imputation.[29] In column 2, we relax the assumption that the average absolute difference between DER and SIPP earnings is equal for person-years exhibiting any unit nonresponse and person-years exhibiting any item nonresponse. Relative to person-years experiencing no imputed survey earnings, the average absolute difference between DER and SIPP

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.

[29]When we estimate the specification in column 1 using the raw DER-SIPP difference as the dependent variable, we find that SIPP earnings exceed DER earnings by about \$1,421 more on average for person-years with at least some imputed earnings relative to those with no earnings imputation. While the two earnings sources differ substantially more for imputed survey earnings on average when we examine the absolute DER-SIPP earnings gap, the smaller difference in the raw DER-SIPP earnings gap by imputation status suggests that these deviations counterbalance to a large extent.

earnings is about $2,815 larger for unit nonrespondents and about $7,643 larger for item nonrespondents. Column 3 demonstrates the considerable heterogeneity across types of imputation in the degree of concordance between survey and administrative earnings. At one extreme, individuals who experience at least one month of logical imputation have survey earnings that differ from administrative earnings by a statistically insignificant $216 relative to 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 $10,279 larger in absolute value than individuals who experienced no months of imputation. Apart from the logical imputation indicator, the only nonresponse indicator with a coefficient estimate statistically insignificant from zero is the cross-sectional hot-deck imputation indicator.[30]

Table 3 also describes whose survey earnings differ more from administrative earnings, holding nonresponse constant. These findings demonstrate that some of the key relationships documented by Cristia and Schwabish (2009) and Pedace and Bates (2000) are also present in the more recent 2008 panel. For example, individuals who are male, married, and more educated tend to have administrative earnings further from survey earnings. Column 3 shows that the average absolute deviation between DER and SIPP earnings is about $12,085 larger for individuals with a professional degree relative to individuals with only a high school degree or GED. Table 3 also offers estimates for populations of particular interest to many SIPP users. For example, individuals who speak a language other than English in the home have larger average absolute deviations between DER and SIPP earnings than individuals who speak English in the home do. Households that receive means-tested cash or noncash transfers have smaller average absolute deviations between DER and SIPP earnings than nonrecipients of these programs do.[31] Despite these correlations, the relatively low R^2 values reveal that very little of the variability in the difference between SIPP earnings and DER earnings is explained by variability in our observables.

The specifications presented in Table 3 are estimated on our full sample, which includes

[30] As a means of comparison, the coefficient estimate on the regressor indicating person-years that exhibited any proxy response is $273 in column 1, a statistically insignificant $152 in column 2, and a statistically insignificant $24 in column 3.

[31] Examples of means-tested cash transfers include Temporary Assistance for Needy Families (commonly known as welfare) and Supplemental Security Income. Examples of means-tested noncash transfers include Medicaid, Supplemental Nutrition Assistance Program (commonly known as food stamps), and the National School Lunch Program (commonly known as free or reduced-price school meals).

individuals with no annual earnings in either SIPP or DER in order to describe comprehensively how these data sources compare. Analysts commonly consider both workers and nonworkers when studying topics such as poverty and program recipiency. Nevertheless, many analysts consider only the population of workers when studying earnings. To characterize how SIPP earnings compare to DER earnings for this population, Table 4 additionally restricts the estimation sample to individuals who have both positive SIPP earnings and positive DER earnings. This new estimation sample motivates two changes from the model specification in column 3 of Table 3. First, the dependent variable is now the natural log of the absolute difference between DER and SIPP earnings. The observations with zero absolute difference between DER and SIPP earnings which would have been dropped using this transformation overwhelmingly belong to nonworkers in both survey data and administrative data.[32] Second, we now include characteristics of jobs and businesses among our regressors.

Table 4 illustrates that restricting our attention to the sample of workers does affect some qualitative inferences that we can draw about how well SIPP and DER compare on average. On one hand, we find a comparable relative ranking of the degree of correspondence between administrative earnings and survey earnings imputed according to the various methods. 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 administrative earnings furthest from survey earnings, while individuals who experience at least one month of logical imputation have administrative earnings among the closest to survey earnings. On the other hand, we find surprising evidence that individuals who exhibited at least one month of cross-sectional hot deck imputation have administrative earnings closer to survey earnings than do individuals who exhibited no imputed earnings during the year.

Table 4 also demonstrates that including nonworkers in our estimation sample did not influence our qualitative findings by demographic group in general. Workers who are male, married, more educated, nonrecipients of means-tested transfer programs, and non-English speakers in the home tend to have administrative earnings further from survey earnings. There are two notable exceptions. First, the signs of the estimated coefficients on the quartic in age reverse when we exclude nonworkers in survey data or administrative data. Second, the coefficient on the indicator of proxy interview gains statistical significance. This result suggests that proxies report

[32]For comparability, we also estimated a specification with the absolute difference between DER and SIPP earnings as the dependent variable. Refer to Table A1 for results.

other household members' employment status more accurately than they report other household members' earnings conditional on employment.

One of the most striking estimates in this table is that on average self-employed individuals' earnings in SIPP and DER differ by 63.0 log points more than the earnings of workers for an employer do.[33] 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 as Hurst et al. (2014) and other argue.[34] Regardless of which earnings source better reflects truth, Pedace and Bates (2000) offers one important conceptual difference between self-employment earnings amounts in SIPP and DER. As with all earnings, SIPP requests respondents to provide gross self-employment earnings. By contrast, DER earnings from 1040 Schedule SE records capture only self-employment earnings net of business expenses. A second conceptual difference between self-employment earnings amounts in SIPP and DER is that individuals who report self-employment to household surveys might have income reported on tax forms other than 1040 Schedule SE, such as 1099-MISC forms. This income might appear in survey data, but it would not appear in DER data. These conceptual differences might help to explain the relatively large average log absolute difference that we estimate.

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 4 and 5 differ only because the specifications in Table 5 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 4 remain in Table 5, which suggests that these patterns are not solely an artifact of the Census Bureau's imputation techniques. Individuals who are male, married, more educated, nonrecipients of means-tested transfer programs, non-English speakers in the home, proxy respondents, and self-employed have SIPP earnings further from DER earnings than other individuals do.

[33] Note that Hurst et al. (2014) motivate their study by claiming that self-employed individuals may misreport their incomes to household surveys because "if the self-employed have already supplied (or have an intention to supply) a distorted income report to tax authorities, it may be easier to reuse this report for surveys instead of computing a separate and more accurate measure of income." The results in Table 4 imply that on average self-employed individuals are not reusing reports to tax authorities when answering household survey questions. However, it remains unclear whether the separate measure of income that they provide to household surveys is more accurate.

[34] Although we do not present the results in this paper, we also estimated a version of Table 4 with the raw DER-SIPP earnings difference as the dependent variable. We estimate that survey earnings overstate administrative earnings by about $11,864 more for self-employed workers relative to workers for an employer, consistent with the evidence in both Pedace and Bates (2000) and Hurst et al. (2014).

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. This suggests that earnings nonresponse is related to earnings itself, in which case excluding imputed earnings data might bias estimates. 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 6 summarizes the likelihood of nonresponse, both overall and within demographic groups. For people aged 15 and older, 16.1 percent of person-months exhibit any nonresponse. Table 6 also decomposes this overall nonresponse into the unit nonresponse rate and the item nonresponse rate. Unit nonresponse occurs for 7.1 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.[35] 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 6. 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.

Table 6 also begins to characterize survey earnings nonresponse over the administrative earnings distribution. For the full sample, 16.8 percent of person-months in calendar years with positive DER earnings exhibit any nonresponse, while the analogous nonresponse rate is 4.7 percent in calendar years with no DER earnings. However, 13.4 percent of person-months among survey workers in calendar years with positive DER earnings exhibit item nonresponse, while the analogous nonresponse rate is 30.5 percent among survey workers in calendar years

[35] Note that a more standard definition of unit nonresponse would include only sample members who do not answer any question in the survey. We also categorize as unit nonrespondents individuals who do not answer any question in the survey about their employment situation since they resemble Type-Z nonrespondents in their indirect nonresponse to earnings questions.

with no DER earnings. These estimates provide cursory evidence that the pattern of survey earnings nonresponse over the administrative earnings distribution depends upon whether survey nonworkers are included in the estimation sample.

Figures 5 and 6 explore this dependence in greater detail. Figure 5 depicts the pattern of survey earnings nonresponse over the positive portion of the DER earnings distribution for our full sample. To construct this figure, we rank individuals according to their positive DER earnings.[36] We then compute the average monthly earnings nonresponse rate within each percentile of this distribution. Figure 5 illustrates a wave-like pattern of survey earnings nonresponse over the DER earnings distribution; the likelihood of nonresponse increases with earnings both at the bottom and at the top of the DER earnings distribution.[37]

The wave-like pattern of nonresponse that we document appears to conflict with the U-shaped pattern of CPS ASEC nonresponse over the DER distribution noted by Bollinger et al. (2015b). One important difference between these findings is that we depict the wave-like pattern of nonresponse in a sample of survey workers and survey nonworkers, while Bollinger et al. (2015b) depict the U-shaped pattern of nonresponse in a sample of only survey workers.[38] To determine whether the different estimation samples explain the difference in patterns of survey earnings nonresponse, Figure 6 reproduces Figure 5 on a sample of only survey workers. A U-shaped pattern similar to the one illustrated by Bollinger et al. (2015b) now obtains. One potential explanation for this finding is that among individuals with positive DER earnings, those in the lowest percentiles are the most likely to respond to survey questions about earnings by replying that they did not work and thus had $0 of earnings.[39] This behavior could yield

[36]Note that because our full sample includes data from calendar years 2009 through 2012, we calculate separate DER earnings percentile cutoffs for each calendar year. Although wage inflation has been low over this period, we expect the earnings distribution to be shifted up slightly with each additional year. Moreover, Bollinger et al. (2015b), Meyer et al. (2015), and others have cited a trend of increasing nonresponse over time. Either of these effects could confound our view of the pattern of earnings nonresponse over the DER earnings distribution if we had pooled data for all calendar years and constructed a single set of percentile cutoffs.

[37]Figure A2 shows that the skewness of the DER earnings distribution alters the wave-like pattern of nonresponse when portraying the within-percentile nonresponse rate against the within-percentile median DER earnings. For ease of visualization, we use Lowess to smooth this scatterplot. The difference between within-percentile median DER earnings is large at the top of the DER earnings distribution. The effect is to overemphasize relatively few points at the top of the income distribution. Although the wave-like pattern of nonresponse is still visible, it is less pronounced than it is in the plot of the within-percentile nonresponse rate against DER earnings percentile.

[38]Note that Hokayem et al. (2014), a working paper version of Hokayem et al. (2015), also depicted a wave-like pattern of CPS ASEC nonresponse over the DER earnings distribution in a sample of survey workers and survey nonworkers.

[39]A record of $0 in survey earnings could occur for either of two reasons. First, some sample members report no work during the reference period. Second, some sample members decline to provide any information about their employment situation. The Census Bureau imputes an employment status to these sample members. If they are imputed to have no work during the reference period, we would consider them survey nonworkers who exhibit survey earnings nonresponse.

a U-shaped pattern of survey earnings nonresponse in a sample of only survey workers and a wave-like pattern of survey earnings nonresponse in a sample of both survey workers and survey nonworkers.[40]

The nonresponse rates listed in Table 6 and depicted in Figures 5 and 6 point to characteristics of individuals who are more likely to lack survey earnings reports on average. To investigate further who does not respond, Table 7 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.[41] We estimate the model via ordinary least squares.

The δ coefficients are our parameters of interest, as they point to coefficient estimates which might be biased in earnings regressions that include observations with imputed earnings without accounting for imputation status.[42] When considering a regression of earnings on a set of explanatory covariates, analysts typically assume that any measurement error in the dependent variable is uncorrelated with the explanatory covariates, in which case their coefficient estimates are unbiased. Since X_{im} contains observables that typically appear as explanatory covariates in earnings regressions, the δ coefficients allow for a test of the validity of this assumption. Previous work has argued that earnings data are more mismeasured for imputed survey earnings than for reported survey earnings, and we present additional evidence in support of this claim in Section 4.2. If explanatory covariates of an earnings regression are also correlated with imputation status, then their coefficient estimates might be biased and this bias need not be attenuating.

Column 1 of Table 7 contains the results of the model given by equation 2 estimated on our full sample, where the dependent variable indicates any earnings nonresponse. Many characteristics appear to have a statistically significant relationship to earnings nonresponse owing

[40] Figure A3 shows the U-shaped pattern of nonresponse when portraying the within-percentile nonresponse rate against the within-percentile median DER earnings. For ease of visualization, we use Lowess to smooth this scatterplot.

[41] 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 percent 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.

[42] For this purpose, the correlations that we document in Table 7 need not bear a causal interpretation. We make no claims about which mechanism mediates the correlations between these observable characteristics and earnings nonresponse.

to our very precisely estimated coefficients. We find that men are more likely than women to lack survey earnings reports. Individuals with professional degrees are more likely to be nonrespondents than those in any other education group. Never married individuals and married individuals whose spouses are absent are more likely to suffer nonresponse relative to married individuals whose spouses are present. 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 survey earnings reports. Among populations of particular interest to many SIPP users, recipients of means-tested transfers and individuals who speak a language other than English in the home are less likely to be nonrespondents. Incorporating information about the interview seems to offer insight into the likelihood of earnings nonresponse. The use of proxy interviews appears to be effective at inducing responses to earnings questions, as nonresponse is 4.5 percentage points less likely for person-months characterized by proxy response. Individuals who leave the survey but eventually return are 4.1 percentage points more likely to lack survey earnings reports while in the survey. Similarly, individuals who leave the survey and never return are 3.9 percentage points more likely to lack survey earnings reports 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 7.

Column 1 of Table 7 details that administrative earnings do appear to be predictive of survey earnings nonresponse. Individuals with positive DER earnings are 4.6 percentage points more likely to be nonrespondents relative to individuals with no DER earnings. We observe that a wave-like pattern of nonresponse over the positive portion of the DER distribution persists even after controlling for observable heterogeneity in our full sample. These findings suggest that earnings nonresponse is not independent of administrative earnings. While we do not view administrative earnings as truth per se, we argue that administrative earnings are a generally high quality source of earnings that is available for both survey respondents and survey nonrespondents. If administrative earnings are correlated with true earnings, our results suggest that a strategy of omitting imputed earnings observations will bias coefficient estimates.

Columns 2 through 4 of Table 7 probe the results presented in column 1 by examining the likelihood of unit nonresponse and item nonresponse separately. Column 2 differs from column

1 only in the dependent variable, which indicates unit nonresponse. Many of the predictors of any earnings nonresponse documented in column 1 also predict unit nonresponse.[43] For example, unit nonrespondents in SIPP data are more likely to be male and residing in larger households. Additionally, unit nonresponse is less likely among sample members whose data come from a proxy interview and more likely among individuals who ever leave the sample. However, the pattern of unit nonresponse does deviate from the pattern of any nonresponse in several notable ways. First, individuals with a professional degree are less likely to suffer unit nonresponse relative to individuals with a high school degree or GED only. Second, we cannot reject the null hypothesis that married individuals with an absent spouse are no more or less likely to suffer unit nonresponse relative to married individuals with a present spouse.

Column 2 of Table 7 describes a relatively weak pattern of unit nonresponse over the DER earnings distribution.[44] Individuals with positive DER earnings are 0.2 percentage points less likely to be unit nonrespondents relative to individuals with no DER earnings. Among those with positive DER earnings, the pattern of unit nonresponse is generally decreasing in earnings. Individuals in the bottom DER earnings quintile are 1.1 percentage points more likely to suffer unit nonresponse and individuals in the top quintile are 0.2 percentage points less likely to suffer unit nonresponse, relative to individuals in the middle quintile. By contrast, individuals in the second and fourth quintiles are no more or less likely to exhibit unit nonresponse than those in the middle quintile.[45] The relatively weak relationship between unit nonresponse and administrative earnings conditional on covariates suggests that unit earnings nonresponse might

[43] 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 7 as indicating characteristics of individuals who are more likely to lack survey earnings reports. We also advise caution in interpreting the results presented in column 1 of Table 7, 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.

[44] Note that we observe unit nonrespondents' administrative earnings rather than their donors' administrative earnings. Consequently, we can state how unit nonresponse varies with administrative earnings more confidently than we can state how unit nonresponse varies with other observables.

[45] Although 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 conclude that unit nonresponse is highest among those with low DER earnings, and that unit nonresponse declines gradually and slightly as DER earnings increase. Our results corroborate this evidence.

be roughly close to ignorable.[46]

Column 3 of Table 7 lists the results when the dependent variable indicates item nonresponse. Our sample for this regression is our full sample excluding unit nonrespondents. 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 suffer item nonresponse. Individuals who ever leave the sample are more likely to suffer item nonresponse relative to individuals who remain in sample continuously from 2009 through 2012. However, the pattern of item nonresponse does deviate from the pattern of any nonresponse in several notable ways. First, individuals residing in larger households do not appear more likely to suffer from item nonresponse. Second, individuals who speak a language other than English in the home are no more or less likely to suffer item nonresponse relative to individuals who speak English in the home. Finally, while column 1 of this table showed that proxy responses are effective at reducing earnings nonresponse overall, column 3 shows that proxy respondents are 3.2 percentage points more likely than self-responses to suffer item nonresponse.

The relationship between survey earnings nonresponse and administrative earnings provided in the Column 3 of Table 7 strongly resembles the relationship in column 1. Individuals with no administrative earnings are 4.8 percentage points less likely to decline to provide any earnings data. Among individuals with positive administrative earnings, we observe a wave-line pattern of item nonresponse over the DER earnings distribution after controlling for observable heterogeneity. Sample members in the second administrative earnings quintile are 2.2 percentage points more likely to be item nonrespondents than those in the middle quintile, while sample members in the bottom and fourth administrative earnings quintile are 0.3 and 0.9 percentage points more likely to be item nonrespondents than those in the middle quintile, respectively.

Analysts often restrict their estimation samples to include only employed individuals. To study the estimated patterns of nonresponse in this population, the specification in column 4 of Table 7 differs from the one in column 3 by excluding observations with 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 four notable ways. First, better educated individuals are no longer more likely to be earn-

[46]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.

ings nonrespondents. Second, Black, non-Hispanics are now the most likely racial and ethnic group to be item nonrespondents. Third, individuals in larger households are now less likely to suffer item nonresponse. Fourth, individuals who speak a language other than English in the home are now less likely to suffer item nonresponse. Including characteristics of jobs and businesses among the covariates allows us to draw several new inferences about who is more likely to exhibit item nonresponse. 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. This echoes evidence documented by Hokayem et al. (2014). Another marked effect is that weeks worked per month and hours worked 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.

The estimates in column 4 also demonstrate that dropping survey nonworkers changes our view of the relationship between survey earnings nonresponse and administrative earnings in two salient ways. First, individuals with any administrative earnings record are now 8.2 percentage points less likely to lack earnings data. Second, a U-shaped pattern of item nonresponse similar to the one depicted in Figure 6 appears even after controlling for observable heterogeneity. Respondents in the bottom, second, and top administrative income quintiles are 4.5, 1.9, and 0.5 percentage points more likely to be item nonrespondents than those in the middle quintile, respectively. On the other hand, individuals in the fourth quintile are 0.3 percentage points less likely to be item nonrespondents than those in the middle quintile.

Item nonresponse as referenced in columns 3 and 4 of Table 7 may come from various sources. We consider total earnings to be imputed if any of its components is missing. Table 8 investigates the pattern of item nonresponse by considering how the relationship between observable covariates and item nonresponse varies across earnings sources.[47] To that end, Table 8 presents the results of regressions with the following form:

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

In the equation above, NR_{ijm} indicates earnings nonresponse for individual i working at job or

[47] Note that Table 8 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.

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.[48] Note that X_{ijm} does not include administrative earnings information. We only observe self-employed income from the 1040 Schedule SE at the person level, so we are unable to attribute this income to particular businesses for individuals who own multiple businesses. Although we observe wage and salary income at the job level, we do not observe the mapping of DER jobs to SIPP jobs and constructing that mapping is outside the scope of this analysis.[49] We estimate the model via 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 7. 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 earnings nonresponse on a job to hours worked 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 8 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 reported some information about their labor market situation. The results in column 1 of Table 8 strongly resemble the results in column 4 of Table 7, as most earnings come from jobs

[48] 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 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.

[49] See Abowd and Stinson (2013) and Reeder and Monti (2016) for in depth discussions of mapping SIPP jobs to DER 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 7 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 8 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 7 and column 1 of Table 8 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.[50] We find that these workers are 1.9 percentage points less likely to be item nonrespondents, which is consistent with the hypothesis that gross hourly pay rates are more salient for survey respondents compared to other types of pay.

If workers who are paid by the hour have more salient pay rates, one might wonder whether workers who are paid irregularly have less salient pay rates and are thus more likely to be nonrespondents. Contingent workers tend to have informal, impermanent arrangements to provide a service when, and only for as long as, it is needed. 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. Consequently, column 1 of Table 8 omits this covariate. To understand whether contingent workers are more likely to lack survey earnings reports for jobs at an employer, column 2 of Table 8 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 7.8 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 have larger standard errors 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

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

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 survey earnings reports on jobs for an employer. Individuals who stopped working for their self-employed business are no more or less likely to be nonrespondents, whereas column 1 lists that workers who stopped working for their employer are more likely to be nonrespondents. 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 2.4 percentage points less likely and workers who receive some nonsalary income out of the money that the business brings in are 22.0 percentage points more likely to lack survey earnings reports 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.

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. We approach this question by estimating various aspects of the earnings structure, including the gender earnings gap, the Black-White earnings gap, the returns to education and the earnings-age profile which we estimate via a Mincer regression, and the returns to self-employment. We estimate each regression using the log of four different earnings sources as 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 log earnings for individuals who experienced no months of imputed survey earnings during the year and DER log earnings for individuals who experienced at least one month of imputed survey earnings during the year.

Table 9 lists the estimates of the gender earnings gap, Table 10 lists the estimates of the Black-White earnings gap, Table 11 lists the estimates of the Mincer regression, and Table 12 lists the estimates of the returns to self-employment. 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 9, 10, 11, and 12, the set of regressors includes only a constant.

The results in Tables 9, 10, 11, and 12 offer insight into four issues that are relevant for data users. First, comparing the estimates in columns 1 and 3 allows us to test the influence of the strategy of excluding observations with imputed earnings. The strategy of restricting the estimation sample to only individuals with reported earnings assumes that earnings response bias is ignorable. Second, comparing the estimates in columns 3 and 4 allows for a test of the validity of this assumption, as DER earnings which the DER-SIPP hybrid includes for survey nonrespondents offer one counterfactual for how nonrespondents would have answered earnings regressions.[51] 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. Third, comparing the estimates in columns 1 and 4 allows for a test of the impact of any additional error in imputed survey earnings relative to administrative earnings. If this error 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. Finally, comparing the estimates in columns 2 and 4 allows for a test of the impact of any additional measurement error in reported survey earnings relative to administrative earnings. If this measurement error does indeed bias regression coefficients, we will reject the null hypothesis that DER earnings and the DER-SIPP hybrid yield equal coefficient estimates.

Table 9 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 34.7 log points less than men on average. A comparison of the estimates in columns 1 and 3 reveals that the strategy of dropping imputed earnings observations yields no statistical difference in the estimated gender earnings gap. However, a comparison of the estimates in columns 3 and 4 shows evidence that including administrative earnings for survey

[51] To the extent that DER earnings and SIPP earnings differ conceptually, DER earnings are not our ideal counterfactual. Rather, we would want to include in the dependent variable the response that nonrespondents would have given to survey earnings questions. We plan to model this counterfactual in future work and to evaluate the implied estimates of the gender earnings gap, Black-White earnings gap, Mincer regression, and returns to self-employment relative to the estimates using the DER-SIPP hybrid presented in this paper.

earnings nonrespondents reduces the estimated gender earnings gap by about 7.4 percent. This result suggests that earnings nonresponse is not ignorable for estimates of the gender earnings gap. A comparison of the estimates in columns 1 and 4 reveals that any additional error 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. Similarly, comparing the estimates in columns 2 and 4 suggests that any additional measurement error that is present in reported survey earnings and not in these individuals' administrative earnings does not affect our estimate of the gender earnings gap. These findings suggest that a strategy of incorporating administrative earnings into survey earnings more extensively would not have a substantial impact on the estimated gender earnings gap.

Table 10 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 27.3 log points more on average than the omitted race group.[52] The implied average Black-White earnings gap is about 25.7 log points. 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. A comparison of the estimates in columns 3 and 4 shows evidence that including administrative earnings for survey earnings nonrespondents has no significant impact on our estimate of the Black-White 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. A comparison of the estimates in columns 1 and 4 reveals that any additional error that is present in imputed survey earnings and not in these individuals' administrative earnings has no statistically significant effect on the estimated Black-White earnings gap. Finally, comparing the estimates in columns 2 and 4 suggests that any additional measurement error that is present in reported survey earnings and not in administrative earnings does not affect our estimate of the Black-White earnings gap. Again, we conclude that a strategy of incorporating administrative earnings into survey earnings more extensively would not substantially

[52]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 in the omitted group. Also contained in the omitted group are individuals whose reported race was either American Indian or Alaska Native or Native Hawaiian or Other Pacific Islander. 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.

influence our estimated Black-White earnings gap.

Table 11 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 13.7 log points in additional earnings on average. The first year of potential experience brings about 8.9 log points 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 3 suggests that the strategy of dropping observations with imputed earnings does increase the magnitude of the estimated returns to education but leaves the estimated returns to potential experience unchanged. A comparison of the estimates in columns 3 and 4 shows that including administrative earnings for survey earnings nonrespondents does significantly increase our estimate of the return to potential experience by 6.1 percent, yet it does not influence our estimate of the return to education. This finding suggests that earnings response bias is ignorable for an estimate of the average return to education but not for an estimate of the average return to potential experience in a Mincer regression. A comparison of the estimates in columns 1 and 4 reveals mixed evidence about the impact of any additional error 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 these models. On the other hand, we do reject the null hypothesis of no difference in the estimated return to potential experience across these models. Finally, comparing the estimates in columns 2 and 4 suggests that any additional measurement error that is present in reported survey earnings relative to administrative earnings has no significant impact on either our estimate of the returns to education or our estimate of the returns to potential experience. Thus, we infer that a strategy of incorporating administrative data into survey data more extensively would not alter our estimate of the returns to education. However, this strategy could change imputed earnings amounts enough to alter our estimate of the returns to potential experience.

Finally, Table 12 summarizes how estimates of the returns to self-employment vary with the source of earnings data. Recall that DER measures self-employment earnings net of business expenses, while SIPP measures gross self-employment earnings. This conceptual difference complicates our interpretation of the estimates in Table 12. In Tables 9, 10, and 11, we view comparisons of the estimates in columns 3 and 4 as a test of the assumption that earnings nonresponse is ignorable. Although we recognize that DER earnings do not represent truth, we argue that they are a generally high quality measure that is present for both respondents and nonrespondents. Consequently, DER earnings might serve as an acceptable counterfactual for how survey nonrespondents would have answered earnings questions. If self-employment earnings in DER and SIPP measure different concepts, it is less clear whether the DER-SIPP hybrid serves as an acceptable counterfactual in the context of Table 12. While it is unclear to what degree Table 12 speaks to the ignorability of earnings nonresponse, these results do offer insight into the implications of incorporating administrative earnings into survey earnings more extensively. A comparison of columns 1 and 2 reveals that replacing all survey earnings with all administrative earnings leads to a decline of 42.3 log points in the estimated returns to self-employment. Thus, the returns to self-employment vary dramatically with the source of earnings that we use to estimate. This gap in point estimates does not appear to be driven disproportionately by imputed survey earnings, as replacing only imputed survey earnings with administrative earnings causes the average returns to self-employment to fall by 24.1 log points. These inferences underscore both the need for a deeper understanding of the informational content of survey and administrative self-employment earnings and the need for caution before incorporating administrative earnings into survey earnings more extensively.

7 Conclusion

In this paper, we explore the relationship between survey and administrative earnings as well as 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. Self-employed workers also have a greater difference in survey and administrative earnings than workers for an employer do, a pattern which Pedace and Bates (2000) also noted. 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 they do for individuals with no months imputed. In examining the predictors of nonresponse, our evidence supports Hokayem et al. (2014) who find that respondents who are self-employed are less likely to respond to earnings questions and that survey earnings nonresponse follows a wave-like pattern over the DER earnings distribution in a sample of survey workers and nonworkers. 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, households that receive means-tested transfers have survey earnings that deviate less from administrative earnings and are less likely to have imputed survey earnings relative to households that 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 characterize how the likelihood of survey earnings nonresponse varies across survey workers and survey nonworkers. For example, individuals with zero DER earnings are 4.6 percentage points less likely in a sample of survey workers and nonworkers and 8.2 percentage points more likely in a sample of only survey workers to suffer earnings nonresponse relative to those with positive DER earnings. We show that earnings response bias affects some estimates of key aspects of the earnings structure, such as the gender earnings gap and the returns to potential experience. Finally, we show that estimates of the returns to self-employment vary dramatically if we replace survey earnings with administrative earnings.

Our paper offers practical insight for analysts studying earnings. Most 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. In an attempt to mitigate bias, some 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 survey nonworkers, 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, and the likelihood of any nonresponse increases both at the bottom and at the top of the DER earnings distribution. This relationship in principle could bias upwards estimates of poverty and program eligibility.

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. 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. Some of the evidence presented here could be used to argue for these proposals. For example, we show that the strategy of replacing survey earnings data with administrative earnings data would alter earnings 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. Additionally, we fail to reject the null hypothesis that replacing SIPP earnings with DER earnings leaves unchanged several key estimates of the earnings structure including the gender earnings gap, the Black-White earnings gap, and the returns to education. By contrast, some of the evidence presented here could be used to argue against proposals to incorporate administrative earnings into survey earnings more fully. For example, our investigation documents that replacing self-reported earnings data with administrative data would considerably affect the average observation for individuals who are male; married; more educated; self-employed; and means-tested transfer recipients. 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 illustrate that this difference in measures can drastically influence the returns to potential experience and especially the returns to self-employment, underscoring the need for caution before incorporating administrative earnings for all survey participants.

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 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.[53] In an attempt to reduce respondent burden, the 2014 SIPP panel does not

[53]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.

convert hourly and bi-weekly reports into more salient amounts for verification.[54] 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 response.

[54]The 2014 panel does ask respondents to verify hourly or bi-weekly amounts that imply unusually large or small pay rates.

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Table 1. The Presence of SIPP Earnings and DER Earnings

	DER= \$0	DER> \$0	Total
SIPP= \$0	28.9%	6.0%	34.9%
SIPP> \$0	1.9%	63.2%	65.1%
Total	30.8%	69.2%	183,385

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 lists the percentage of unweighted person-year cases exhibiting positive SIPP earnings and positive DER earnings. The total number of person-year cases in the sample is 183,385.

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

	(1) DER-SIPP	(2) |DER-SIPP|	Observations
Including Zero Earners	$1,413	$7,156	181,553
Excluding Zero Earners	$2,102	$10,732	114,635

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 no SIPP earnings or no DER earnings. The estimates in column 1 are sample means of the average raw difference between DER earnings and SIPP earnings. A positive raw difference implies that DER earnings exceed SIPP earnings. The estimates in column 2 are sample means of the average absolute difference between DER earnings and SIPP earnings.

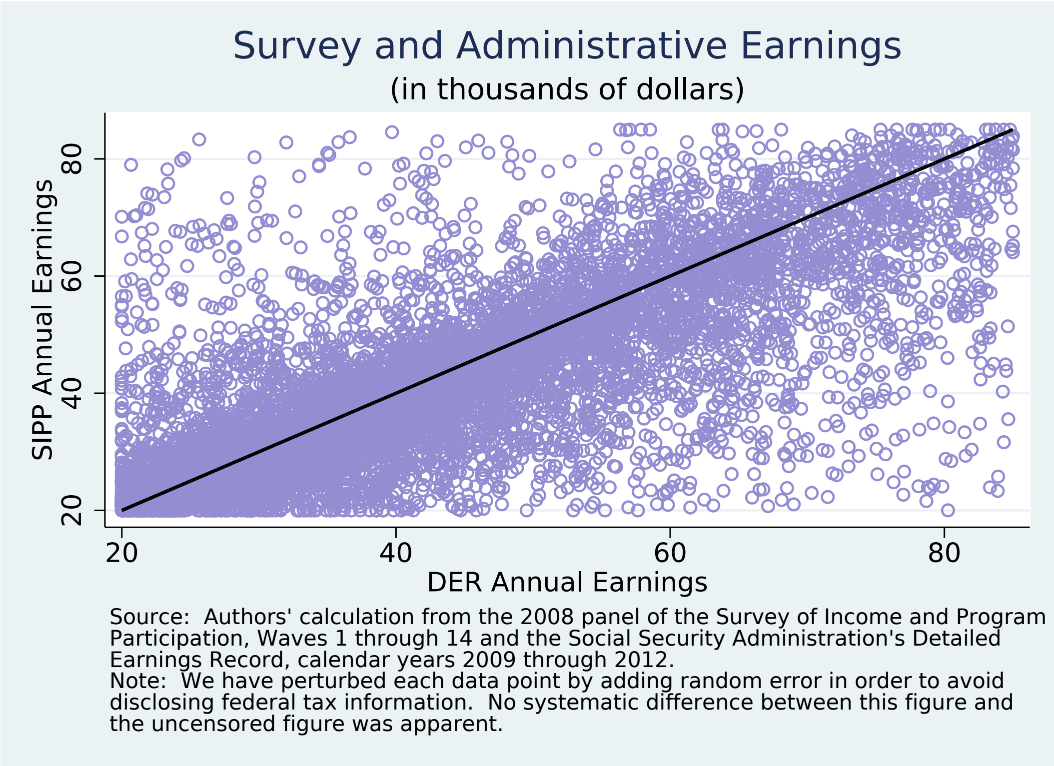

Figure 1. The data points pictured represent a random 15 percent sample of all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. We focus on the set of individuals with both SIPP earnings and DER earnings between \$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 error in order to avoid disclosing federal tax information.

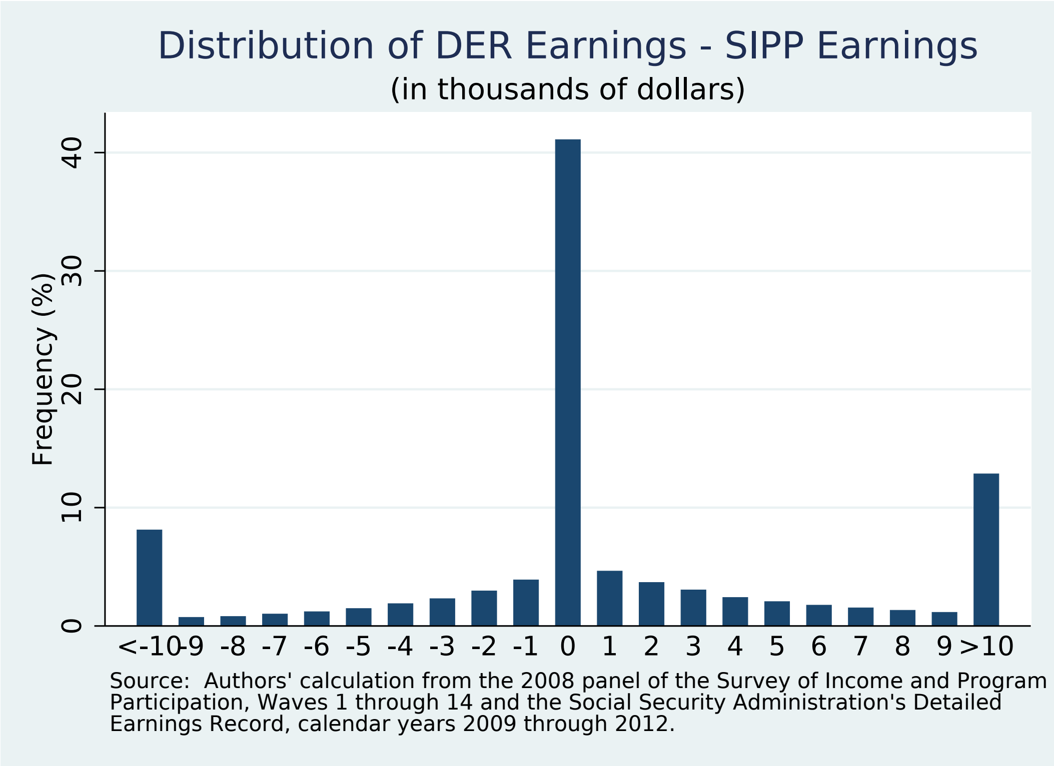

Figure 2. The sample is all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. This figure plots the difference between administrative earnings and survey earnings. Each bin represents the integer portion of the 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.

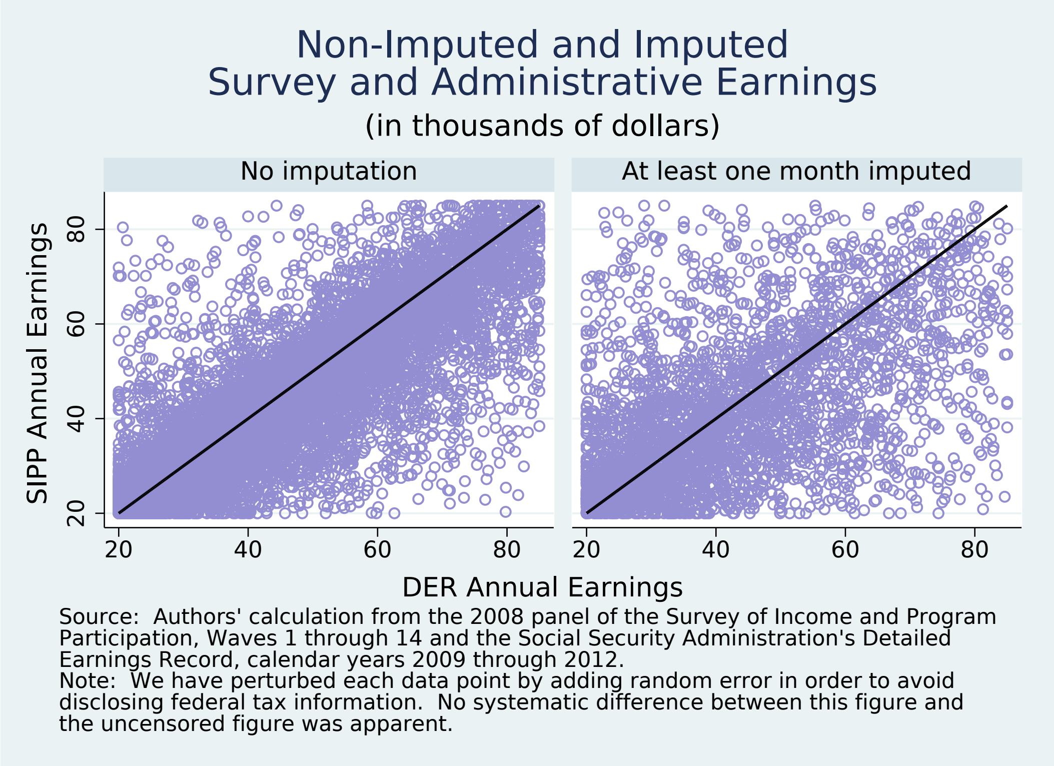

Figure 3. The data points pictured represent a random 25 percent sample of all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. We focus on the set of individuals with both SIPP earnings and DER earnings between \$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 error 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.

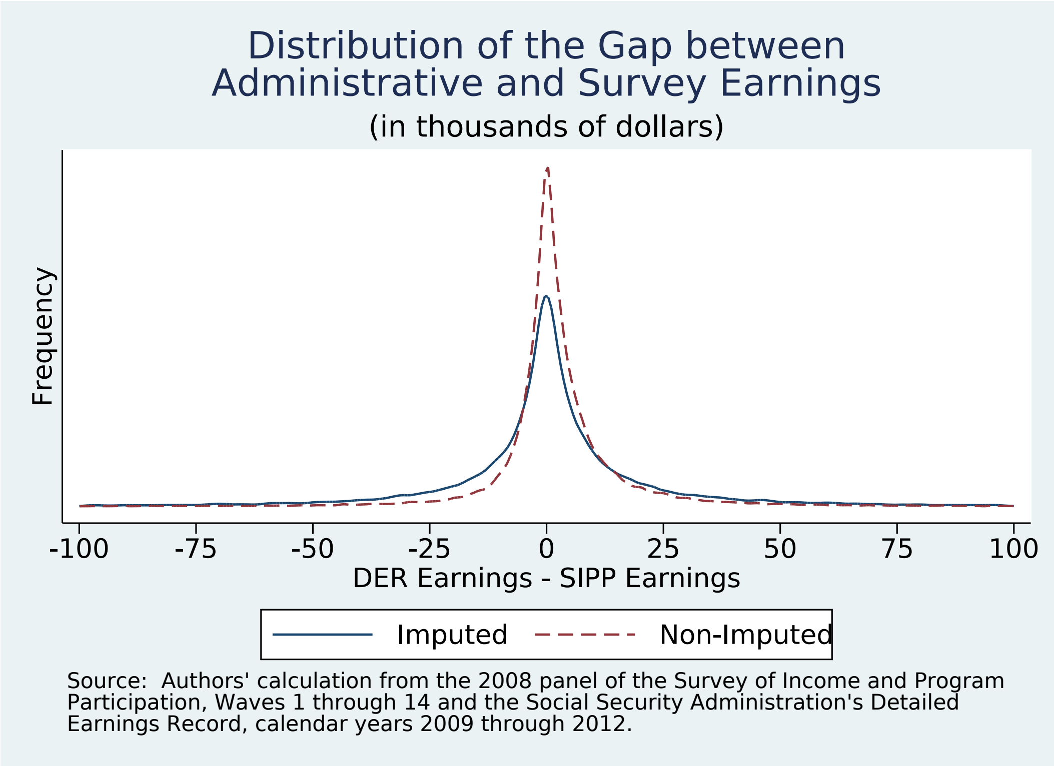

Figure 4. The data points pictured represent all person-years for people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, who had both positive SIPP earnings and positive DER earnings, and who exhibited an absolute difference between survey 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

VARIABLES	(1) |DER-SIPP|	(2) |DER-SIPP|	(3) |DER-SIPP|
Any nonresponse	6,820.738***	—	—
	(113.284)		
Any unit nonresponse	—	2,815.203***	—
		(143.239)	
Any item nonresponse	—	7,642.596***	—
		(137.310)	
Any hot-deck imputation	—	—	256.703
			(246.628)
Any Type-Z imputation	—	—	696.000***
			(143.465)
Any longitudinal labor force imputation	—	—	4,636.319***
			(266.704)
Any imputation based on last month — Reported	—	—	6,327.620***
			(226.592)
Any imputation based on last month — Imputed	—	—	10,279.204***
			(307.640)
Any imputation based on last month — Logical	—	—	7,473.505***
			(739.837)
Any imputation based on last month — Type-Z	—	—	6,380.843***
			(1,346.044)
Any logical imputation	—	—	215.559
			(161.127)
Any proxy response	272.648***	151.806	23.814
	(92.579)	(92.468)	(91.804)
Midwest	-749.845***	-688.889***	-687.911***
	(136.485)	(136.174)	(134.810)
South	-212.608	-186.839	-179.745
	(130.646)	(130.194)	(129.038)
West	-296.600**	-180.701	-100.616
	(151.012)	(150.555)	(149.177)
Number of household members	110.364	220.057***	168.337**
	(75.024)	(75.633)	(75.720)
Number of family members	-30.261	-59.629	-46.453
	(74.788)	(75.456)	(75.432)
Age	-145.761	-240.022**	-285.136***
	(102.287)	(101.751)	(100.529)
Age^2	26.802***	28.804***	29.413***
	(3.292)	(3.274)	(3.232)
Age^3	-0.561***	-0.578***	-0.580***
	(0.043)	(0.043)	(0.042)
Age^4	0.003***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)
Female	-3,382.030***	-3,372.607***	-3,375.490***
	(87.814)	(87.519)	(86.754)

(*continued...*)

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

VARIABLES	(1) |DER-SIPP|	(2) |DER-SIPP|	(3) |DER-SIPP|
(*...continued*)			
Black, non-Hispanic	746.326***	759.970***	651.810***
	(246.099)	(245.021)	(242.153)
Asian, non-Hispanic	920.277**	979.529***	891.091**
	(371.556)	(370.171)	(365.119)
White, non-Hispanic	516.105**	481.038**	440.783**
	(219.907)	(218.969)	(216.087)
Hispanic	594.386**	585.773**	581.802**
	(266.209)	(265.282)	(261.763)
Married, spouse absent	427.047	463.239	388.541
	(433.034)	(431.387)	(425.157)
Never married	-1,143.446***	-1,095.904***	-1,151.213***
	(138.584)	(138.126)	(136.708)
Previously married	-223.459**	-257.894**	-269.035**
	(113.779)	(113.328)	(112.097)
Elementary school	-259.277	-293.534*	-309.088*
	(167.670)	(165.986)	(164.745)
Some high school	-557.524***	-528.563***	-541.689***
	(112.813)	(111.682)	(110.956)
Some college	662.872***	597.970***	613.945***
	(121.683)	(121.436)	(120.520)
Associate's degree	523.621***	517.755***	512.717***
	(114.435)	(113.941)	(112.741)
Bachelor's degree	3,043.554***	2,985.117***	2,976.899***
	(149.228)	(148.823)	(147.221)
Master's degree	4,191.902***	4,120.028***	4,090.433***
	(228.669)	(227.999)	(225.636)
(*continued...*)			

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

VARIABLES	(1) |DER-SIPP|	(2) |DER-SIPP|	(3) |DER-SIPP|
(*...continued*)			
Professional degree	12,204.456***	12,121.903***	12,084.740***
	(753.678)	(749.643)	(744.506)
Doctorate degree	7,265.045***	7,181.250***	7,095.956***
	(663.052)	(659.790)	(657.054)
Foreign-born, citizen	412.598*	414.834*	439.844**
	(220.328)	(219.348)	(217.305)
Foreign-born, non-citizen	305.278	337.621	420.138
	(280.481)	(279.829)	(276.196)
Any children under 18	649.664***	536.862***	589.912***
	(118.595)	(118.150)	(117.176)
Non-English speaker	500.039***	477.086**	457.084**
	(189.246)	(188.377)	(186.464)
Any means-tested transfers	-2,351.855***	-2,267.864***	-2,143.892***
	(84.035)	(83.723)	(82.826)
Observations	181,553	181,553	181,553
R^2	0.172	0.177	0.190

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 4. Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) ln |DER-SIPP|
Any hot-deck imputation	-0.113***
	(0.019)
Any Type-Z imputation	0.117***
	(0.025)
Any longitudinal labor force imputation	0.348***
	(0.030)
Any imputation based on last month — Reported	0.348***
	(0.018)
Any imputation based on last month — Hot-deck imputed	0.879***
	(0.020)
Any imputation based on last month — Logical	0.713***
	(0.049)
Any imputation based on last month — Type-Z	0.495***
	(0.108)
Any logical imputation	0.055***
	(0.018)
Any proxy response	0.094***
	(0.012)
Midwest	-0.102***
	(0.018)
South	-0.015
	(0.017)
West	0.013
	(0.020)
Number of household members	0.023**
	(0.010)
Number of family members	-0.003
	(0.011)
Age	0.204***
	(0.032)
Age^2	-0.006***
	(0.001)
Age^3	0.000***
	(0.000)
Age^4	-0.000***
	(0.000)
Female	-0.269***
	(0.013)
Black, non-Hispanic	0.193***
	(0.038)
Asian, non-Hispanic	0.112**
	(0.048)
White, non-Hispanic	0.061*
	(0.034)
Hispanic	0.127***
	(0.040)
Married, spouse absent	0.129**
	(0.055)

(*continued...*)

Table 4 (continued). Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) ln |DER-SIPP|
(*...continued*)	
Never married	-0.060***
	(0.019)
Previously married	-0.059***
	(0.018)
Elementary school	-0.100**
	(0.044)
Some high school	-0.127***
	(0.027)
Some college	0.051***
	(0.018)
Associate's degree	0.059***
	(0.017)
Bachelor's degree	0.248***
	(0.019)
Master's degree	0.393***
	(0.026)
Professional degree	0.744***
	(0.050)
Doctorate degree	0.523***
	(0.055)
Private	-0.050
	(0.039)
Federal government	0.109**
	(0.044)
State government	-0.187***
	(0.041)
Local government	-0.212***
	(0.040)
Self-employed	0.630***
	(0.021)
Hours worked	0.016***
	(0.000)
(*continued...*)	

Table 4 (continued). Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) ln |DER-SIPP|
(*...continued*)	
Foreign-born, citizen	0.041
	(0.026)
Foreign-born, non-citizen	0.025
	(0.032)
Any children under 18	0.020
	(0.015)
Non-English speaker	0.067***
	(0.024)
Any means-tested transfers	-0.178***
	(0.014)
Observations	106,378
R^2	0.181

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 logged 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, logically imputed, or Type-Z imputed. Other controls include CBSA size indicators 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 5. Deviation of Positive Reported SIPP Earnings from Positive DER Earnings

VARIABLES	(1) ln |DER-SIPP|
Any proxy response	0.147***
	(0.015)
Midwest	-0.080***
	(0.023)
South	0.003
	(0.022)
West	0.043*
	(0.024)
Number of household members	0.030**
	(0.014)
Number of family members	-0.010
	(0.014)
Age	0.189***
	(0.042)
Age^2	-0.005***
	(0.001)
Age^3	0.000***
	(0.000)
Age^4	-0.000***
	(0.000)
Female	-0.258***
	(0.017)
Black, non-Hispanic	0.250***
	(0.049)
Asian, non-Hispanic	0.122**
	(0.060)
White, non-Hispanic	0.059
	(0.044)
Hispanic	0.154***
	(0.051)
Married, spouse absent	0.160**
	(0.072)
Never married	-0.035
	(0.023)
Previously married	-0.061***
	(0.022)
Elementary school	-0.060
	(0.055)
Some high school	-0.090***
	(0.035)
Some college	0.063***
	(0.023)
Associate’s degree	0.075***
	(0.021)
Bachelor’s degree	0.243***
	(0.024)

(*continued...*)

Table 5 (continued). Deviation of Positive Reported SIPP Earnings from Positive DER Earnings

VARIABLES	(1) ln |DER-SIPP|
(*...continued*)	
Master's degree	0.399***
	(0.032)
Professional degree	0.713***
	(0.062)
Doctorate degree	0.507***
	(0.068)
Private	-0.030
	(0.059)
Federal government	0.102
	(0.064)
State government	-0.165***
	(0.061)
Local government	-0.185***
	(0.060)
Self-employed	0.750***
	(0.031)
Hours worked	0.019***
	(0.001)
Foreign-born, citizen	0.041
	(0.032)
Foreign-born, non-citizen	0.026
	(0.040)
Any children under 18	0.048**
	(0.019)
Non-English speaker	0.083***
	(0.029)
Any means-tested transfers	-0.189***
	(0.018)
Observations	72,359
R^2	0.135

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 logged absolute difference between DER earnings and SIPP earnings. Other controls include CBSA size indicators 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 6. Characteristics of Nonrespondents

CHARACTERISTICS	Any Non-response (percent)	Obs	Unit Non-response (percent)	Obs	Item Non-response (percent)	Obs
Overall	16.1	3,849,934	7.1	3,849,934	15.4	1,901,584
Gender						
Female	14.1	2,031,415	6.4	2,031,415	14.6	932,365
Male	18.3	1,818,519	8.0	1,818,519	16.2	969,219
Education						
Less than HS	12.0	639,948	7.6	639,948	13.6	161,106
HS or some college	17.0	2,260,485	7.9	2,260,485	15.5	1,124,426
Bachelor's or postgraduate	16.7	949,501	5.1	949,501	15.6	616,052
Race and ethnicity						
White, non-Hispanic	15.8	2,668,350	6.3	2,668,350	15.7	1,353,303
Black, non-Hispanic	18.2	450,707	9.3	450,707	18.4	195,612
Asian, non-Hispanic	17.8	159,323	9.1	159,323	14.6	82,403
Hispanic	15.3	448,721	8.9	448,721	11.3	216,157
Marital status						
Married	15.4	1,994,751	5.3	1,994,751	15.3	1,104,901
Divorced or separated	15.2	479,649	5.9	479,649	14.7	256,386
Never married or widowed	17.4	1,375,534	10.3	1,375,534	15.9	540,297
Age						
Under 25	20.3	643,615	13.7	643,615	17.1	221,015
25 — 34	18.6	576,752	8.8	576,752	13.0	378,841
35 — 44	17.6	615,278	6.3	615,278	14.2	426,805
45 — 54	18.2	698,198	6.1	698,198	15.6	464,865
55 — 64	15.8	612,212	5.1	612,212	17.0	321,179
65 or older	7.0	703,879	3.5	703,879	20.4	88,879
Family Structure						
No children under 18	15.9	2,541,950	7.1	2,541,950	16.2	1,167,701
Any children under 18	16.4	1,307,984	7.3	1,307,984	14.0	733,883
Hours worked						
Less than 20 hours	—	—	—	—	18.5	125,134
20 — 34 hours	—	—	—	—	16.7	267,817
35 or more hours	—	—	—	—	14.9	1,508,633
Employer type						
Job for Employer	—	—	—	—	13.6	1,741,738
Self-Employed	—	—	—	—	35.3	159,846
DER earnings						
DER$=$ \$0	4.7	666,554	3.0	666,554	30.5	18,900
DER$>$ \$0	16.8	1,530,205	5.0	1,530,205	13.4	1,169,692

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. Additionally, the last two rows restrict the sample to person-months for individuals who were assigned a PIK and who were present in the survey for all 12 months of the calendar year. Family structure includes only children living in the household. Hours worked includes usual weekly hours of work at all jobs and businesses. Employer type indicates whether an individual worked on a self-employed job during the wave. Usual weekly hours worked and employer type data are imputed for all unit nonrespondents. DER earnings indicates whether an individual had an administrative earnings record for the relevant calendar year.

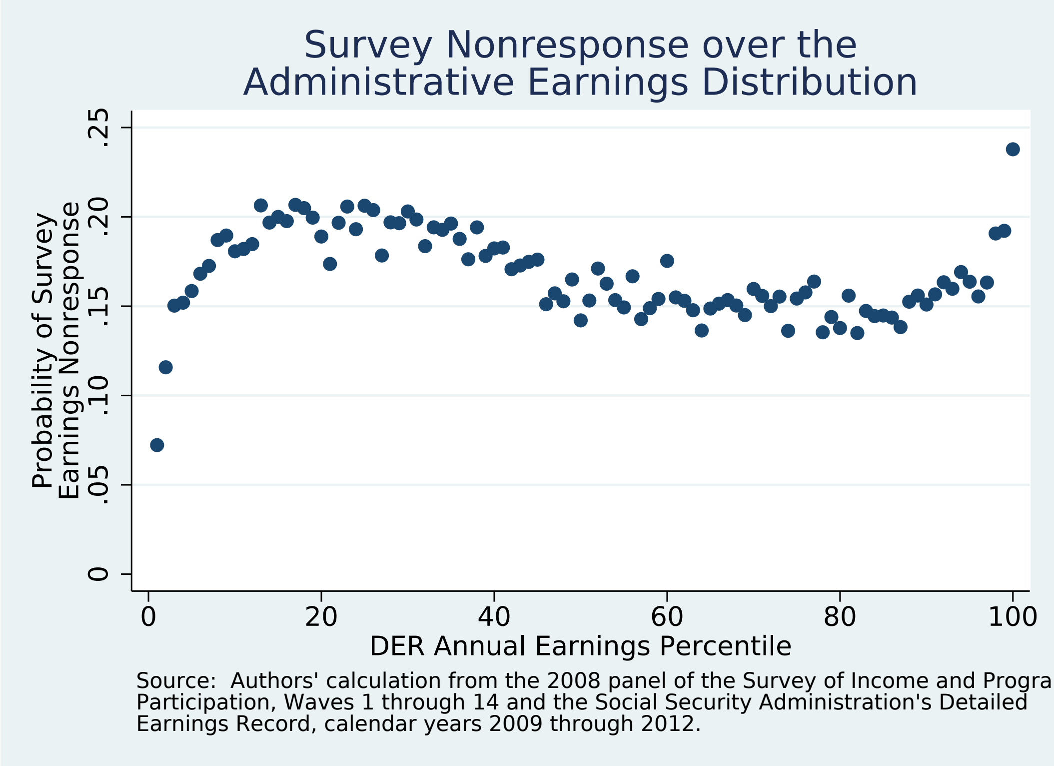

Figure 5. The data points pictured represent people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and who had positive DER earnings during that calendar year. This figure plots the fraction of person-months exhibiting either unit or item earnings nonresponse (on the vertical axis) within each percentile of the DER earnings distribution (on the horizontal axis). We construct percentile cutoffs separately for each year of DER earnings data to avoid conflating the effects of calendar year on nonresponse and the effects of DER earnings percentile on nonresponse.

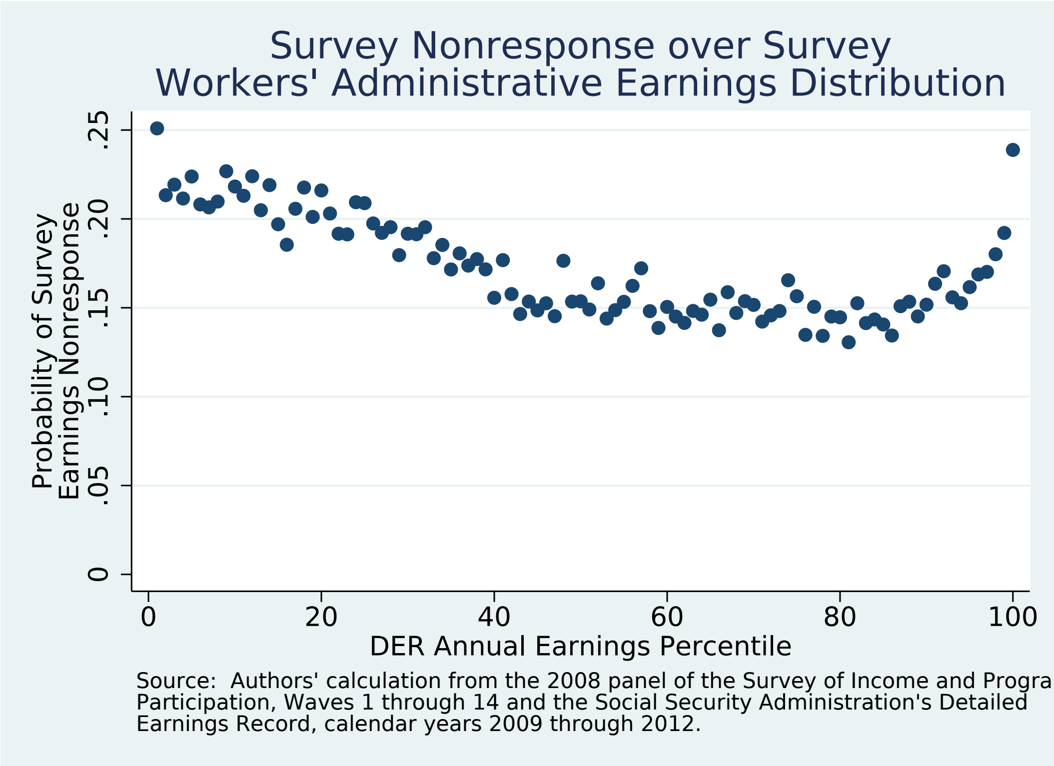

Figure 6. The data points pictured represent people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and who had both positive DER earnings and positive SIPP earnings during that calendar year. This figure plots the fraction of person-months exhibiting either unit or item earnings nonresponse (on the vertical axis) within each percentile of the DER earnings distribution (on the horizontal axis). We construct percentile cutoffs separately for each year of DER earnings data to avoid conflating the effects of calendar year on nonresponse and the effects of DER earnings percentile on nonresponse.

Table 7. Ignorability of Earnings Nonresponse

VARIABLES	(1) Any Non- response	(2) Unit Non- response	(3) Item Non- response	(4) Item Non- response
Midwest	-0.012***	0.005***	-0.018***	-0.030***
	(0.001)	(0.001)	(0.001)	(0.002)
South	0.009***	0.005***	0.004***	0.009***
	(0.001)	(0.001)	(0.001)	(0.002)
West	-0.022***	0.006***	-0.029***	-0.048***
	(0.001)	(0.001)	(0.001)	(0.002)
Number of household members	0.023***	0.026***	-0.001	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Number of family members	0.001	-0.003***	0.005***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
Age	-0.023***	-0.022***	-0.001	0.011***
	(0.001)	(0.001)	(0.001)	(0.003)
Age^2	0.001***	0.001***	0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Age^3	-0.000***	-0.000***	-0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Age^4	0.000***	0.000***	0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.028***	-0.013***	-0.017***	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Black, non-Hispanic	0.005	0.002	0.003	0.019***
	(0.003)	(0.002)	(0.002)	(0.004)
Asian, non-Hispanic	-0.005	0.003	-0.008***	-0.002
	(0.004)	(0.002)	(0.003)	(0.005)
White, non-Hispanic	0.004*	-0.004**	0.008***	0.007*
	(0.003)	(0.002)	(0.002)	(0.003)
Hispanic	-0.025***	-0.007***	-0.020***	-0.019***
	(0.003)	(0.002)	(0.002)	(0.004)
Married, spouse absent	0.012***	0.003	0.010***	0.021***
	(0.005)	(0.003)	(0.004)	(0.006)
Never married	0.018***	0.011***	0.009***	0.018***
	(0.002)	(0.001)	(0.001)	(0.002)
Previously married	-0.006***	-0.012***	0.005***	0.008***
	(0.001)	(0.001)	(0.001)	(0.002)
Elementary school	-0.012***	-0.009***	-0.004***	-0.011**
	(0.002)	(0.001)	(0.002)	(0.004)
Some high school	-0.021***	-0.011***	-0.011***	-0.017***
	(0.002)	(0.001)	(0.001)	(0.003)
Some college	-0.010***	-0.009***	-0.002	-0.012***
	(0.001)	(0.001)	(0.001)	(0.002)
Associate’s degree	-0.004***	-0.005***	0.001	-0.008***
	(0.001)	(0.001)	(0.001)	(0.002)
Bachelor’s degree	-0.000	-0.010***	0.009***	-0.003
	(0.001)	(0.001)	(0.001)	(0.002)
Master’s degree	-0.004*	-0.012***	0.008***	-0.003
	(0.002)	(0.001)	(0.002)	(0.003)

(*continued...*)

Table 7 (continued). Ignorability of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
(*...continued*)				
Professional degree	0.028***	-0.010***	0.038***	-0.010*
	(0.004)	(0.002)	(0.004)	(0.005)
Doctorate degree	-0.006	-0.015***	0.008**	-0.027***
	(0.004)	(0.002)	(0.004)	(0.005)
Private	—	—	—	0.061***
				(0.004)
Federal government	—	—	—	0.085***
				(0.005)
State government	—	—	—	0.046***
				(0.005)
Local government	—	—	—	0.045***
				(0.004)
Self-employed	—	—	—	0.198***
				(0.003)
Weeks worked	—	—	—	0.018***
				(0.000)
Hours worked	—	—	—	0.001***
				(0.000)
Foreign-born, citizen	0.016***	0.008***	0.010***	0.011***
	(0.002)	(0.001)	(0.002)	(0.003)
Foreign-born, non-citizen	0.009***	0.011***	-0.001	-0.002
	(0.003)	(0.002)	(0.002)	(0.003)
Proxy response	-0.045***	-0.084***	0.032***	0.055***
	(0.001)	(0.001)	(0.001)	(0.001)
Number of interviews	0.002***	0.001***	0.002***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
Any sample gaps	0.041***	0.021***	0.023***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)
Attritor	0.039***	0.030***	0.012***	0.022***
	(0.001)	(0.001)	(0.001)	(0.002)
Any children under 18	-0.047***	-0.039***	-0.012***	-0.022***
	(0.001)	(0.001)	(0.001)	(0.002)
Change in family composition	-0.022***	-0.015***	-0.009***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.003)
Non-English speaker	-0.005***	-0.005***	-0.001	-0.007***
	(0.002)	(0.001)	(0.002)	(0.002)
Any means-tested transfers	-0.007***	0.007***	-0.015***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.002)
Stopped work	—	—	—	0.028***
				(0.002)
Any admin records	0.046***	-0.002**	0.048***	-0.082***
	(0.002)	(0.001)	(0.002)	(0.005)
Number of admin records	0.040***	0.012***	0.032***	0.024***
	(0.001)	(0.001)	(0.001)	(0.001)
(*continued...*)				

Table 7 (continued). Ignorability of Earnings Nonresponse

VARIABLES	(1) Any Non-response	(2) Unit Non-response	(3) Item Non-response	(4) Item Non-response
(*...continued*)				
Bottom admin earnings quintile	0.006***	0.011***	-0.003*	0.045***
	(0.002)	(0.001)	(0.002)	(0.003)
Second admin earnings quintile	0.020***	-0.001	0.022***	0.019***
	(0.002)	(0.001)	(0.002)	(0.002)
Fourth admin earnings quintile	-0.009***	-0.000	-0.009***	-0.003*
	(0.002)	(0.001)	(0.002)	(0.002)
Top admin earnings quintile	-0.004**	-0.002**	-0.002	0.005**
	(0.002)	(0.001)	(0.002)	(0.002)
Observations	2,196,759	2,196,759	2,099,877	1,318,449
R^2	0.054	0.069	0.047	0.078

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 8. Predictors of Earnings Item Nonresponse

VARIABLES	(1) Jobs	(2) Jobs	(3) Businesses
Midwest	-0.019***	-0.018***	-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)	(0.002)	(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***	0.007
	(0.001)	(0.001)	(0.005)
Age	0.002	0.002	0.034***
	(0.002)	(0.002)	(0.011)
Age^2	-0.000	-0.000	-0.001***
	(0.000)	(0.000)	(0.000)
Age^3	0.000	0.000	0.000**
	(0.000)	(0.000)	(0.000)
Age^4	-0.000*	-0.000*	-0.000**
	(0.000)	(0.000)	(0.000)
Female	-0.006***	-0.006***	0.007
	(0.001)	(0.001)	(0.005)
Black, non-Hispanic	0.028***	0.028***	0.022
	(0.003)	(0.003)	(0.017)
Asian, non-Hispanic	0.007*	0.007*	0.023
	(0.004)	(0.004)	(0.018)
White, non-Hispanic	0.006**	0.006**	-0.013
	(0.003)	(0.003)	(0.014)
Hispanic	-0.008***	-0.008***	-0.004
	(0.003)	(0.003)	(0.017)
Married, spouse absent	0.030***	0.029***	0.020
	(0.005)	(0.005)	(0.021)
Never married	0.021***	0.021***	0.028***
	(0.002)	(0.002)	(0.008)
Previously married	0.015***	0.014***	0.010
	(0.002)	(0.001)	(0.007)
Elementary school	-0.023***	-0.022***	-0.012
	(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***	-0.003
	(0.002)	(0.002)	(0.007)
Bachelor's degree	-0.006***	-0.007***	-0.007
	(0.002)	(0.002)	(0.007)
Master's degree	-0.010***	-0.011***	0.022**
	(0.002)	(0.002)	(0.009)
Professional degree	-0.010**	-0.011**	-0.001
	(0.004)	(0.004)	(0.014)

(*continued...*)

Table 8 (continued). Predictors of Earnings Item Nonresponse

VARIABLES	(1) Jobs	(2) Jobs	(3) Businesses
(*...continued*)			
Doctorate degree	-0.028***	-0.027***	-0.005
	(0.004)	(0.004)	(0.016)
Federal government	-0.009***	—	—
	(0.003)		
State government	-0.011***	—	—
	(0.002)		
Local government	-0.015***	—	—
	(0.002)		
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***	-0.005
	(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***	0.106***
	(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***	-0.023***
	(0.001)	(0.001)	(0.006)
Change in family composition	0.007***	0.006**	0.014
	(0.002)	(0.002)	(0.011)
Non-English speaker	-0.012***	-0.012***	-0.008
	(0.002)	(0.002)	(0.008)
Any means-tested transfers	-0.001	-0.001	-0.003
	(0.001)	(0.001)	(0.006)
Stopped work	0.045***	—	0.003
	(0.002)		(0.018)
Paid hourly	-0.019***	-0.018***	—
	(0.001)	(0.001)	
(*continued...*)			

Table 8 (continued). Predictors of Earnings Item Nonresponse

VARIABLES	(1) Jobs	(2) Jobs	(3) Businesses
(*...continued*)			
Contingent worker	—	0.078***	—
		(0.027)	
Salaried	—	—	-0.024***
			(0.004)
Other income	—	—	0.220***
			(0.014)
Observations	1,699,807	1,743,220	200,577
R^2	0.027	0.025	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 9. Impact of Earnings Nonresponse: Gender Earnings Gap Estimates

	(1)	(2)	(3)	(4)
VARIABLES	SIPP	DER	Reported SIPP	SIPP-DER Hybrid
Female	-0.347***	-0.337***	-0.366***	-0.339***
	(0.010)	(0.011)	(0.011)	(0.010)
Observations	114,063	114,063	76,079	114,063
R^2	0.026	0.021	0.032	0.022

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 10. Impact of Earnings Nonresponse: Racial Earnings Gap Estimates

VARIABLES	(1) SIPP	(2) DER	(3) Reported SIPP	(4) SIPP-DER Hybrid
Black alone	0.016	0.055	-0.039	0.033
	(0.031)	(0.034)	(0.035)	(0.032)
White alone	0.273***	0.282***	0.236***	0.289***
	(0.027)	(0.030)	(0.031)	(0.029)
Asian alone	0.415***	0.473***	0.390***	0.445***
	(0.037)	(0.040)	(0.041)	(0.039)
Observations	114,063	114,063	76,079	114,063
R^2	0.008	0.006	0.009	0.008

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 11. Impact of Earnings Nonresponse: Mincer Regression Estimates

VARIABLES	(1) SIPP	(2) DER	(3) Reported SIPP	(4) SIPP-DER Hybrid
Years of education	0.137***	0.136***	0.142***	0.138***
	(0.002)	(0.002)	(0.002)	(0.002)
Potential experience	0.091***	0.099***	0.092***	0.098***
	(0.001)	(0.001)	(0.001)	(0.001)
Potential experience squared	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	114,063	114,063	76,079	114,063
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 * $p < 0.1$.

Table 12. Impact of Earnings Nonresponse: Returns to Self-Employment Estimates

VARIABLES	(1) SIPP	(2) DER	(3) Reported SIPP	(4) SIPP-DER Hybrid
Self-employed	0.036**	-0.387***	0.008	-0.205***
	(0.016)	(0.020)	(0.025)	(0.019)
Observations	114,063	114,063	76,079	114,063
R-squared	0.000	0.010	0.000	0.003

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. The self-employment indicator takes value 1 for individuals who received non-zero income or profit from self-employment during the calendar year. Standard errors are clustered at the person level and are listed in parentheses. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * $p < 0.1$.

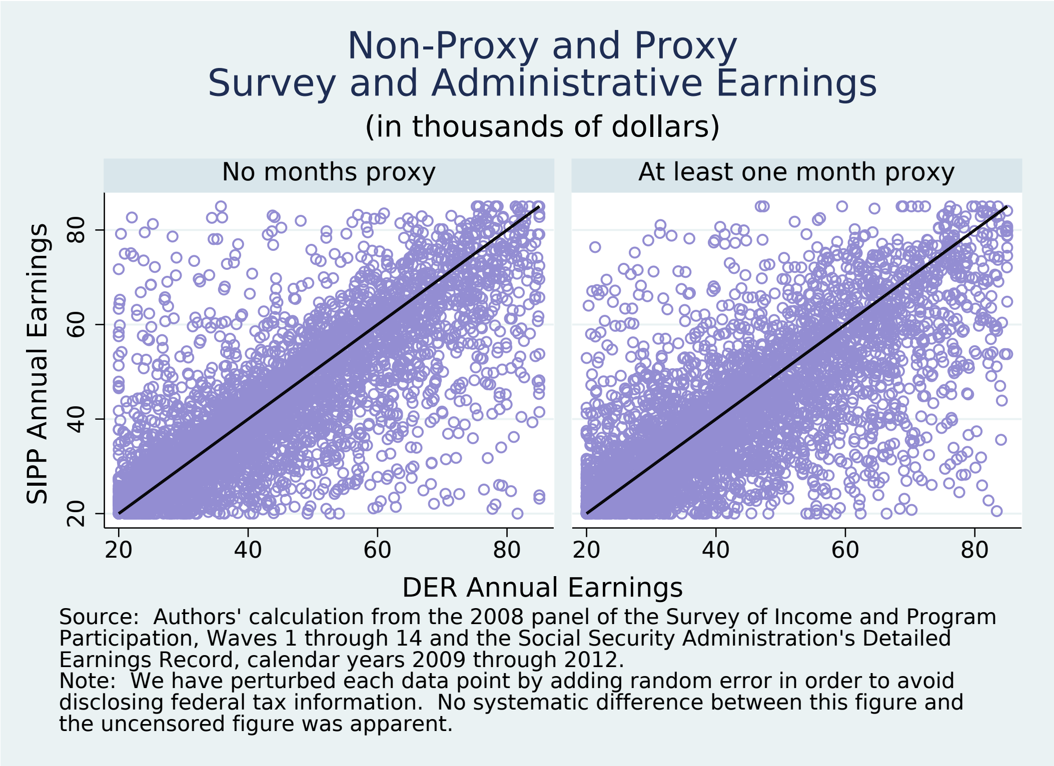

Figure A1. The data points pictured represent a random 15 percent sample of all person-years for people aged 15 and older, who were assigned a PIK, and who were present in the survey for all 12 months of the calendar year. We focus on the set of individuals with both SIPP earnings and DER earnings between $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 error 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.

Table A1. Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) |DER-SIPP|
Any hot deck imputation	-3,320.531***
	(327.177)
Any type z imputation	1,169.374***
	(244.695)
Any longitudinal labor force imputation	3,027.264***
	(346.724)
Any imputation based on last month — Reported	3,915.485***
	(269.161)
Any imputation based on last month — Imputed	11,627.975***
	(391.468)
Any imputation based on last month — Logical	8,520.343***
	(871.400)
Any imputation based on last month — Type z	6,724.942***
	(1,696.539)
Any logical imputation	229.413
	(174.532)
Any proxy response	455.498***
	(141.977)
Midwest	-1,429.590***
	(217.417)
South	-565.240***
	(212.161)
West	10.190
	(245.131)
Number of household members	10.107
	(101.721)
Number of family members	156.514
	(99.921)
Age	-654.443**
	(309.522)
Age^2	29.849***
	(11.096)
Age^3	-0.447***
	(0.166)
Age^4	0.002**
	(0.001)
Female	-3,184.135***
	(160.066)
Black, non-Hispanic	808.886*
	(423.703)
Asian, non-Hispanic	1,482.618**
	(593.224)
White, non-Hispanic	373.677
	(386.461)
Hispanic	628.374
	(446.389)
Married, spouse absent	1,563.434**
	(744.854)

(*continued...*)

Table A1 (continued). Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) |DER-SIPP|
(*...continued*)	
Never married	-798.257***
	(193.592)
Previously married	-823.874***
	(195.604)
Elementary school	-841.320*
	(442.769)
Some high school	-498.405**
	(239.988)
Some college	706.118***
	(184.367)
Associate's degree	366.149**
	(179.096)
Bachelor's degree	3,240.046***
	(233.347)
Master's degree	5,226.135***
	(356.184)
Professional degree	14,275.393***
	(1,038.989)
Doctorate degree	7,608.256***
	(835.931)
Private	-305.576
	(454.385)
Federal government	362.584
	(530.317)
State government	-2,871.224***
	(475.238)
Local government	-2,454.282***
	(456.303)
Self-employed	9,440.083***
	(356.884)
Hours worked	140.724***
	(5.752)
(*continued...*)	

Table A1 (continued). Deviation of Positive SIPP Earnings from Positive DER Earnings

VARIABLES	(1) |DER-SIPP|
(*...continued*)	
Foreign-born, citizen	-121.148
	(330.588)
Foreign-born, non-citizen	-212.393
	(402.456)
Any children under 18	657.244***
	(172.609)
Non-English speaker	243.836
	(298.991)
Any means-tested transfers	-1,437.010***
	(144.280)
Observations	106,491
R^2	0.189

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 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, logically imputed, or Type-Z imputed. Other controls include CBSA size indicators 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 A2. Deviation of Positive Reported SIPP Earnings from Positive DER Earnings

VARIABLES	(1) |DER-SIPP|
Any proxy response	914.161***
	(149.803)
Midwest	-1,015.941***
	(228.385)
South	-364.577
	(226.444)
West	449.073*
	(264.718)
Number of household members	20.769
	(113.696)
Number of family members	185.476*
	(109.597)
Age	-751.375**
	(346.700)
Age^2	32.639***
	(12.386)
Age^3	-0.493***
	(0.185)
Age^4	0.002**
	(0.001)
Female	-2,672.932***
	(169.377)
Black, non-Hispanic	1,301.671***
	(465.212)
Asian, non-Hispanic	1,352.950**
	(651.454)
White, non-Hispanic	205.679
	(423.366)
Hispanic	790.582
	(483.480)
Married, spouse absent	2,156.591**
	(894.103)
Never married	-483.022**
	(200.628)
Previously married	-755.502***
	(196.433)
Elementary school	-402.574
	(471.777)
Some high school	-250.320
	(257.158)
Some college	699.186***
	(190.917)
Associate's degree	398.238**
	(182.971)
Bachelor's degree	2,775.837***
	(238.189)

(*continued...*)

Table A2 (continued). Deviation of Positive Reported SIPP Earnings from Positive DER Earnings

	(1)
VARIABLES	|DER-SIPP|
(*...continued*)	
Master's degree	4,428.673***
	(368.683)
Professional degree	12,507.406***
	(1,177.320)
Doctorate degree	6,689.883***
	(935.606)
Private	-148.593
	(549.031)
Federal government	67.070
	(626.038)
State government	-2,558.700***
	(564.662)
Local government	-2,171.068***
	(540.079)
Self-employed	9,770.620***
	(521.129)
Hours worked	141.875***
	(6.841)
Foreign-born, citizen	-131.695
	(347.213)
Foreign-born, non-citizen	-190.795
	(428.648)
Any children under 18	625.446***
	(184.632)
Non-English speaker	274.977
	(313.558)
Any means-tested transfers	-1,388.721***
	(152.261)
Observations	72,466
R^2	0.142

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 absolute difference between DER earnings and SIPP earnings. Other controls include CBSA size indicators 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 * $p < 0.1$.

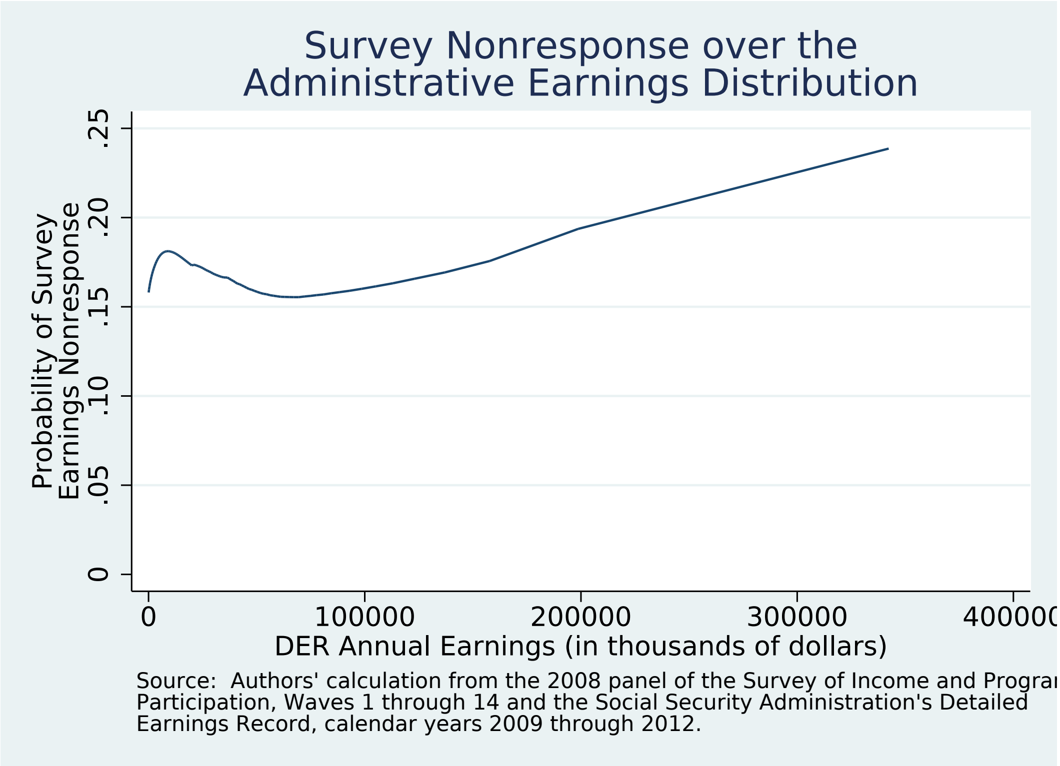

Figure A2. The data points pictured represent people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and who had positive DER earnings during that calendar year. This figure plots the Lowess smoothed scatterplot of the fraction of person-months exhibiting either unit or item earnings nonresponse (on the vertical axis) within each percentile of the DER earnings distribution. We plot this nonresponse rate against the median DER earnings within each percentile of the DER earnings distribution (on the horizontal axis). We construct percentile cutoffs separately for each year of DER earnings data to avoid conflating the effects of calendar year on nonresponse and the effects of DER earnings percentile on nonresponse.

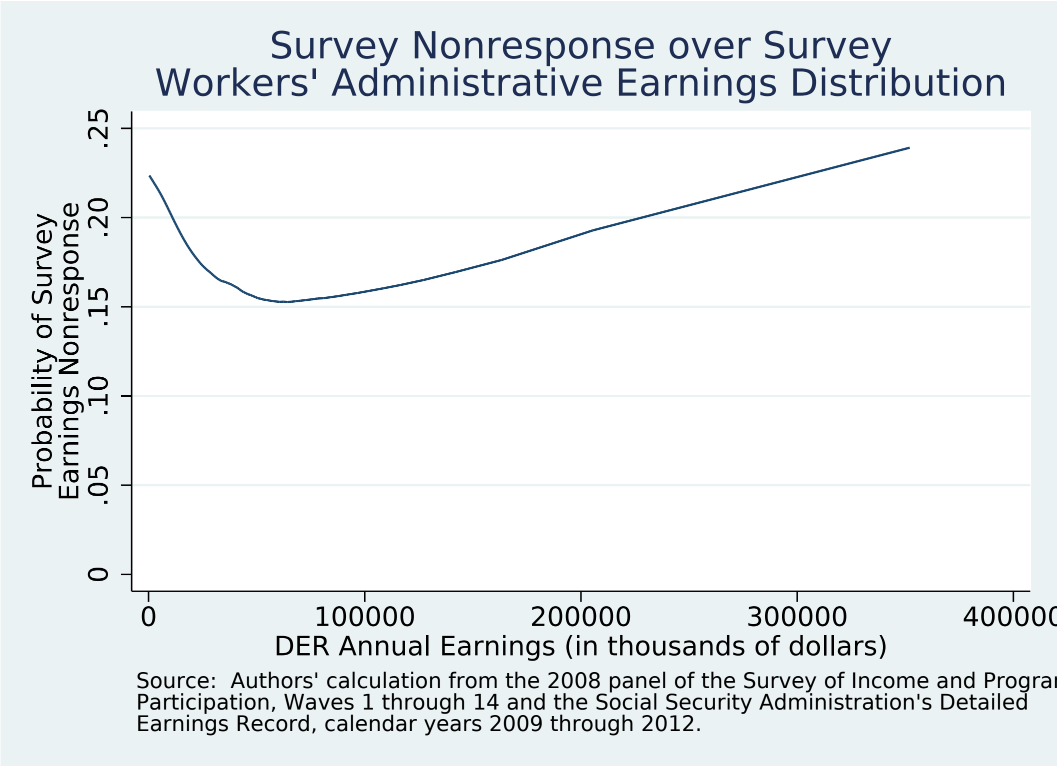

Figure A3. The data points pictured represent people aged 15 and older, who were assigned a PIK, who were present in the survey for all 12 months of the calendar year, and who had both positive DER earnings and positive SIPP earnings during that calendar year. This figure plots the Lowess smoothed scatterplot of the fraction of person-months exhibiting either unit or item earnings nonresponse (on the vertical axis) within each percentile of the DER earnings distribution. We plot this nonresponse rate against the median DER earnings within each percentile of the DER earnings distribution (on the horizontal axis). We construct percentile cutoffs separately for each year of DER earnings data to avoid conflating the effects of calendar year on nonresponse and the effects of DER earnings percentile on nonresponse.