Wage Gap Estimation with Proxies and Nonresponse *

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Earnings nonresponse in the Current Population Survey (CPS) is about 30% in the monthly surveys and 20% in the annual March surveys. Half of CPS earnings records rely on "proxy" respondents, among whom nonresponse is particularly high. Even if nonresponse is random, severe bias attaches to wage equation coefficient estimates on non-match (and some imperfectly matched) imputation attributes. If nonresponse is ignorable (i.e., conditional missing at random), unbiased estimates can be achieved by omitting imputed earners. In this paper, we use selection models and longitudinal analysis to examine whether CPS nonresponse is ignorable and how proxy responses affect reported earnings. Based on reasonable instruments to identify selection, we conclude there is negative selection into response for men and, to a far lesser extent, women. Wage equation slope coefficients are affected little by selection but because of intercept shifts, wages for men and to a lesser extent women are understated, as are gender wage gaps. Longitudinal results reinforce the qualitative conclusion that imputation understates earnings, but gender differences are less clear-cut. Cross-sectional estimates of proxy effects on reported earnings suggest large differences in the effects of spouse and non-spouse proxies. These results are driven by heterogeneity, with panel analysis suggesting that both spouse and non-spouse proxy respondents report about 2% less than do self respondents. For most wage equation analyses, response bias and proxy reports are of second order importance and the simple exclusion of imputed earners provides a reasonable first-order approach.

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Introduction

The Current Population Survey (CPS) is used extensively by economists and other social scientists because of its large sample sizes, comprehensiveness, historical continuity, and timeliness. The monthly CPS Outgoing Rotation Group (ORG) files are widely used to analyze hourly earnings for wage and salary workers based on the principal job the previous week, while the American Social and Economic Supplement (ASES) to the March CPS is used to examine earnings reported across all wage and salary jobs during the previous calendar year.

Item nonresponse rates are low for most questions in the CPS, the notable exception being questions on income and earnings. Currently, about 30% of employed wage and salary workers sampled in the CPS-ORG do not provide earnings information. Missing earnings are assigned (allocated) to nonrespondents using a cell hot deck imputation procedure. The procedure matches nonrespondents with the "donor" earnings of the most recent respondent who has an identical set of match characteristics (Hirsch and Schumacher 2004). In the March CPS, about 20% of individuals employed the previous year fail to report annual earnings. Their earnings are assigned using a sequential hot deck procedure. This procedure begins its search for a donor using a relatively detailed set of match attribute values and then, failing to find a match, begins collapsing categories until a matching donor is found (Lillard, Smith, and Welch 1986).

Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006) establish that even if nonresponse is random, the imputation procedure can produce severe "match bias." Wage regression coefficients on attributes that are not match criteria (union, industry, foreign-born, etc.) are biased toward zero by a proportion close to the nonresponse (imputation) rate. Such estimates are widespread in the literature. Coefficients on imperfectly matched attributes such as education can also be severely biased. For example, estimated returns to the GED are overstated because nonrespondents with a GED are typically assigned the earnings of donors with a standard high school degree or some college.[1] Bollinger and Hirsch (2006) examine alternative methods to account for match bias, the simplest of which is to remove imputed earners (nonrespondents) from the estimation sample. But this and other approaches assume that nonresponse is either random or ignorable (i.e., equivalent expected earnings for respondents and nonrespondents, conditional upon regressors in the model). Yet we have surprisingly little information on whether or not earnings nonresponse in the CPS (and other surveys) is ignorable.

The goal of this paper is to address key questions concerning response bias. Is there nonignorable response bias in the ORG and March CPS earnings files? If so, what is its nature and severity? How might researchers correct for the bias? We address the structure and impact of earnings nonresponse using both the CPS ORG and March data files for 1998 through 2008. We employ two principal

[1] For evidence on how earnings imputation affects estimates of the returns to the GED, see Heckman-and LaFontaine (2006) and Bollinger and Hirsch (2006).

approaches. The first focuses on finding appropriate instruments to account for response, thus permitting estimation of selection-adjusted wage equations and measures of bias. The second uses longitudinal analysis. Here the qualitative (and lower-bound quantitative) effects of response bias are inferred based on wage changes among workers who report earnings in one year, but who do not respond and are assigned donor earnings in a second year.

A second and related goal of the paper is to assess the impact of proxy respondents on reported earnings and nonresponse. In the CPS a single household member provides responses for all household members. Thus, roughly half of earnings records are based on self responses and half on the response of a "proxy" household member, often a spouse. It is widely believed that proxy reports have little effect on earnings or wage equation coefficients (e.g., Angrist and Krueger 1999), although the topic has received limited attention. Because earnings nonresponse is more likely when there is a proxy respondent, the proxy status of an earnings record provides a potential instrument to assess whether there exists response bias. Its appropriateness as an instrument depends on whether or not it has an effect on the wage, conditional on other regressors. This question of how proxy response affects reported wages is also important in its own right given the heavy reliance on proxy reported earnings.

Response Bias and Proxy Effects on Earnings: What is Known?

In prior research, validation studies have been used to evaluate the accuracy of measured earnings in the CPS.[2] These studies typically exclude nonrespondents with imputed earnings, but sometime compare reported earnings based on self-response versus a proxy household member. A small number of studies have examined the quality of imputed values and the issue of response bias.

Bound and Kruger (1991) conclude that proxies are about as accurate as self-respondents, based on the 1977 and 1978 March CPS, measuring prior year annual earnings, matched to Social Security earnings records (imputed earners were excluded from the analysis). Mellow and Sider (1983) compare earnings reported in a January 1977 CPS supplement with employer reports on earnings (the survey asked workers the name of their employer) and also conclude that self and proxy reports on earnings are broadly similar.[3] But they are not identical. Proxy reports of wages are lower than self reports (Mellow and Sider 1983, Table 1) and both are lower than are employer reports. Both groups tend to over-report work hours as compared to employer reports, but proxy respondents do so by less than self-respondents.[4]

Even less is known about whether nonresponse in the CPS is ignorable and, similarly, whether imputation does a good job on average in estimating earnings. The little work of which we are aware

[2] For a comprehensive review of studies examining measurement error, see Bound, Brown, and Mathiowetz (2001).
[3] In regressions of the employer-employee difference in reported wages on typical wage determinants, Mellow and Sider (1983) obtain no significant coefficients.
[4] In work using the PSID, Lee and Lee (2009) show that there has been a shift over time toward more women and fewer men providing responses for surveyed households. They show that changes in the unmeasured portion of the gender wage gap may result in part from measurement error associated with this shift in proxy composition.

focuses on the March CPS files measuring annual earnings the previous year and not the monthly earnings ORG files reporting earnings and weeks worked on the principal job during the previous week. Greenlees et al. (1982) examine the March 1973 CPS and compared wage and salary earnings the previous year with 1972 matched income tax records. They restrict their analysis to full-time, full-year heads of households in the private nonagricultural sector whose spouse did not work. They conclude that nonresponse is not ignorable, being negatively related to income (negative selection into response). The authors estimate a standard wage equation using the administrative IRS earnings as the dependent variable. Based on those values they impute earnings for those who are CPS nonrespondents. Their imputations understate administrative wage and salary earnings by .08 log points. The sample included only 561 nonrespondents (imputed values) and earnings were censored at $50,000. Herriot and Spiers (1975) earlier reported similar results with these data, the ratio of CPS respondent to IRS earnings being .98 and of CPS imputed to IRS earnings being .91. Taken together, these figures suggest a downward bias of .07 from nonresponse (relative to responders) since imputed values are based on respondents with similar characteristics. It is not clear whether results from this validation study can be generalized outside this survey and time period. The sequential hot deck procedure used in the March survey at that time was primitive as compared to subsequent methods, for example, failing to use education as a match variable (Lillard et al. 1986). But the findings suggest the importance of the question of whether there exists nonignorable response bias, all the more so given increasing rates of nonresponse.

A more encouraging outcome emerges from the study by David et al. (1986), who examine a similar validation study matching the March 1981 CPS and 1980 IRS reports. They conclude that the Census hot deck does a reasonably good job in predicting earnings as compared to alternative imputation methods. Their results are based on a broader sample and the use of a more detailed Census imputation method than were the imputation methods considered in Greenlees et al. (1982). David et al. note the many difficulties in comparing CPS and IRS measures of income, not regarding either measure as a true measure of earnings. They conclude that nonresponse is not ignorable; the earnings structure for respondents providing an unreliable basis for predicting the earnings of nonrespondents. An interesting finding is that imputations for single returns relative to IRS values are higher than imputed values for joint returns relative to IRS values, suggesting that negative selection into response, and thus imputations understating true earnings, is most serious for married couples. In short, the evidence available suggests that there exists at least modest response bias, that it is likely to reflect negative selection into response, and that it may differ between demographic groups. It is hard to know how results based on March CPS records from more than 25 years ago apply to current CPS earnings and imputation methods or to the CPS ORG earnings files.

We are unaware of prior work examining response bias in the monthly CPS ORG earnings files, which begin in January 1979. The 1973-78 May CPS earnings supplements, a precursor to the ORGs, did not include imputed earnings values.[5] Using recent ORG files, Hirsch and Schumacher (2004, fn. 29) estimate a selection wage equation model in which the proxy variable is used to identify nonresponse.[6] Their concern was not response bias, however, but the problem of "match bias" on union coefficients (or, more generally, for attributes not used in CPS hot deck matching). Based on similar union wage gap estimates from a full-sample selection model and an OLS wage regression on the sample of earnings respondents, the authors concluded that response bias appeared modest.

Korinek, Mistiaen, and Ravallion (2007) examine potential bias from unit nonresponse, rather than from item nonresponse on earnings. CPS weights are designed to account for nonrandom survey nonparticipation. But such weights are predicated on the assumption that response is nonrandom across geographic areas (states) but random within states. Korinek et al. question such an assumption. They show that response rates across states vary inversely with income (and other measurable factors), and apply this relationship to adjust weights within states.[7] It seems reasonable that negative selection in response might apply to item nonresponse as well as unit nonresponse. Our focus is on item nonresponse with respect to earnings, its frequency being substantially greater than unit nonresponse.

Although our summary of the literature is brief, it is fair to characterize evidence on response bias and proxy effects on earnings in the CPS as being limited, as largely using older data, and as being far from conclusive. Proxy effects on earnings are believed to be minor (Angrist and Krueger 1999), although there are hints in prior studies that proxy respondents report somewhat lower earnings than do self-respondents. There is stronger evidence that there exists some level of nonignorable response bias, with negative selection into response. Most of this evidence is based on years when nonresponse was less frequent and overly sparse hot deck procedures were used in the March CPS. There is no study (of which we are aware) of response bias in the CPS ORG, a data source providing several advantages over the March CPS for the study of *wage* determination (e.g., see Lemieux 2006), but which have substantially higher rates of nonresponse than do the March surveys.[8]

CPS Data, Imputation, and Proxy Respondents

Data. Analysis in this paper uses the CPS Outgoing Rotation Group (ORG) monthly earnings files and the March CPS Annual Social and Economic Supplement (ASES, previously known as the Annual Demographic File or ADF). For both data sets, wage level equations are estimated using multiple

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[5] About 20% of the May 1973-78 records have missing earnings values, much of this presumed to be the result of nonresponse (Hirsch and Schumacher 2004).

[6] Hirsch and Schumacher (2004, fn. 29) report a proxy coefficient of -.02 when included in an OLS wage equation.

[7] We subsequently show that earnings nonresponse is substantially lower in rural areas and higher in large metropolitan areas, holding constant other earnings determinants. Thus, the inverse relationship between response and income found by Korinek et al. (2005) may reflect to some degree the substantial earnings differences with respect to area size if unit as well as earnings nonresponse varies with city size.

[8] Many authors have considered various methods to deal with missing data (e.g., Little 1988; Ibrahim and Lipsitz 1996; Durrant and Skinner 2006; and Egel et al. 2008). These approaches often require strong distributional assumptions and may shed little light on whether response bias results from dropping imputed earners in the CPS.

cross sections pooled across years, while wage change equations are estimated using pooled two year panels. Panel data are made possible by the sample design of the CPS. Households are included in the CPS for eight months -- four consecutive months in the survey, followed by eight months out, followed by four months in. Thus, most CPS households are surveyed during the same month in consecutive years.

The monthly ORG files used are for January 1998 through December 2008. In addition to the demographic and employment questions asked of all households in the monthly CPS, an earnings supplement is administered to the quarter sample of employed wage and salary workers in the outgoing 4th and 8th rotation months of the survey. The supplement includes questions on usual earnings in the principal job the previous week, usual hours worked per week in that job, and worker union status in that job. Based on this information, we create a measure of average hourly earnings. Hourly workers report their straight-time wage rate. For hourly workers who do not report tips, overtime pay, or commissions (and without an allocated "paid by the hour" flag), the straight time wage is used as the wage measure. For all other workers, the wage is measured by usual weekly earnings, which includes tips, overtime, and commissions, divided by usual hours worked per week on the principal job.[9] For workers whose weekly earnings are top-coded in the ORGs (at $2,885 or about $150,000 per year), we assign the estimated mean by year and gender above the cap assuming a Pareto distribution above the median.[10]

The March CPS or ASES is used for 1999 through 2008. The March supplement is administered to all CPS rotation groups. Earnings (and income) questions apply not to the previous week, but to the previous calendar year (1998-2007). The March wage measure is calculated as annual earnings for all wage and salary jobs divided by annual hours worked (the product of week worked and hours worked per week). Most of our analysis using the March survey examines earnings among full-time, full-year workers, defined as those who typically work at least 35 hours per week and were employed at least 50 weeks. Industry and occupation designation is based on the longest job held the previous year. Union status is not measured for that job.

In the March CPS and ORGs, we focus on full time workers between the ages of 18 and 65 who are not enrolled in school full time. The restrictions are meant to avoid issues with respect to selection into part time work and retirement. The sample is also similar to those used by many researchers investigating a variety of wage determinants, and hence informs their results. We refer to this sample in table 1 as the "primary" sample. The "full" sample includes part time workers (and part year workers) and no age or enrollment restrictions (apart from age 16 and over).

Rates of earnings nonresponse (or %Imputed) in the CPS are shown in Table 1 (for earlier years of the ORGs, see Hirsch and Schumacher 2004). Due to more intensive efforts to contact and acquire responses for the March surveys, nonresponse rates for the ASES are lower than for the ORG (and, as

[9] For the few workers who do not report an hourly wage and report variable hours, the wage is calculated using hours worked the previous week.
[10] These are compiled by Barry Hirsch and David Macpherson and posted at http://www.unionstats.com.

seen subsequently, are lower during February and March than in other months for the ORG). In recent years nonresponse in the ORG has been about 30% of the sample versus about 20% in the March CPS. Nonresponse rates are about 1 percentage point higher if one applies employment weights to the sample. This difference results from lower response rates in large metropolitan areas than elsewhere, coupled with a smaller proportion of households sampled in such areas (i.e., larger weights).

Imputation. Individuals for whom earnings are not reported have them imputed (i.e., allocated) by the Census. Different imputation procedures are used in the ORG and ASES.[11] Earnings imputation in the CPS-ORG uses a "cell hot deck" method that has had only minor changes over time. For the ORG files during the 1998-2002, the Census created a hot deck or cells containing 14,976 possible combinations based on the product of the following seven categories: gender (2 cells), age (6), race (2), education (3), occupation (13), hours worked – including whether or not hours per week are variable (8), and receipt of tips, commissions or overtime (2). When new occupation and industry codes were adopted in 2003, major occupation categories fell to 10, reducing the number of hot deck combinations to 11,520. Census keeps all cells "stocked" with a single donor, insuring that an exact match is always found. The donor in each cell is the most recent earnings respondent surveyed previously by the Census with that exact combination of characteristics. As each surveyed worker reports an earnings value, the Census goes to the appropriate cell, removes the previous donor value, and "refreshes" the cell with a new respondent earnings value. If a cell is not stocked by a matching donor from the current survey month, Census uses donor earnings obtained in prior survey months (or years).[12]

Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006) provide detailed analyses of coefficient "match bias" using the ORGs. The intuition for match bias is straightforward. Attributes which are not used in the imputation procedure are largely uncorrelated with imputed earnings. The wage equation coefficients estimated for these attributes are thus a weighted average of a value close to zero and the true coefficient, the implicit weights being roughly the respective proportions of observations which are and are not imputed. For example, union status is not an imputation match criterion. Hence, most union nonrespondents are assigned the earnings of nonunion donors and some nonunion nonrespondents are assigned the earnings of union donors. If one estimates the union-nonunion wage gap among those with imputed earnings one obtains a value close to zero (union status may be correlated with wage determinants used as match criteria). Bollinger and Hirsch (2006, Table 2) report for male workers an unbiased .19 estimate for earnings respondents, a .02 estimate among nonrespondents based on imputed earnings, and a biased full-sample union wage gap estimate of .14 log points. Attenuation of the union coefficient in the full sample exceeds 25%, nearly as large as the 28.7% of the sample imputed. Similar attenuation is found for coefficients on foreign born, marriage, Hispanic status, and others, as well

[11] Details on the ORG imputation procedure are provided by Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006). Lillard, Smith, and Welch (1986) provide a detailed discussion of the March imputation method.
[12] For a discussion of "dated donors" and the bias from use of nominal earnings, see Bollinger and Hirsch (2006).

as for dispersion in coefficients for industry, region, and city size dummies. More complex forms of bias are found for coefficients on imperfectly matched attributes such as schooling, age, and occupation.

In contrast to the ORG, the ASES use a "sequential" hot deck imputation procedure: Nonrespondents are matched to donors from within the same March survey in sequential steps, each step involving a less detailed match requirement. The procedure first attempts to find a match on the exact combination of variables using the full set of match characteristics (similar to those used in the ORG). Absent a successful match at that level, matching advances to a new step with a less detailed breakdown, for example, broader occupation and age categories. As emphasized by Lillard, Smith, and Welch (1986), the probability of a close match declines the less common an individual's characteristics.

Bollinger and Hirsch (2006) examine alternative estimation procedures to correct for match bias, the simplest being estimation based solely on the sample of respondents. All the suggested corrections, however, rely critically on the assumption that earnings are conditional missing at random (i.e., response bias is ignorable). Thus, an important contribution of this paper will be the guidance it provides on how to deal with imputed earners and match bias. Evidence showing that response bias is not severe or largely ignorable would imply that match bias can be readily addressed. A finding that response bias is nonignorable and severe would produce a more nuanced set of implications, one possible conclusion being that researchers estimating wage equations need to account explicitly for selection into response.

Proxies. The CPS interviews one individual (the "reference" person), typically the household head or co-head, who provides responses for all household members. Thus, roughly half of all individuals have recorded responses that are self-reported and half responses reported by another household member, referred to here as a "proxy" respondent. Among those records based on proxy responses, over half are from a spouse. As reported in Table 2, frequencies of proxy and spouse responses differ by gender. Using our primary sample from the ORG, 57% of male earnings records are based on proxy respondents, 64% of whom are wives. For women, only 40% are based on a proxy, 55% of whom are husbands. Below we examine how nonresponse varies with use of a proxy, along with other worker and location attributes.

Who Fails to Report Earnings?

In this section we examine correlates of earnings nonresponse. We first focus on variables that might identify response in a selection model; i.e., determinants of response unlikely to be correlated with a wage equation error term. For both the ORG and ASES we consider use of the proxy variable. For the ORGs we consider calendar month of the survey and for ASES the CPS rotation group. Nicoletti and Peracchi (2005), based on analysis of the European Community Household Panel, provide evidence justifying inclusion of variables that characterize the data collection process in models of sample response, while excluding the from the outcome model of interest. [13]

Returning to Table 1, nonresponse rates in the ORG are 27.8% among earnings records based on self-reports and 40.5% among records relying on proxies. For the latter group, nonresponse is 34.6% when the proxy is a spouse, but a far higher 49.0% when the proxy is not a spouse. A similar pattern is found in the March supplements, where earnings nonresponse rates are 9 percentage points higher for proxy than self-respondents. This evidence suggests that the proxy measures are strong candidates as selection instruments.

Other potential instruments are identified in Table 1. For the ORGs, we conclude that dummies for survey months February and March are attractive instruments. Nonresponse rates of about 30% seen in the February and March ORG interviews are substantially lower than the 34.9% average rate across the other 10 months (there is little variation in rates across the 10 months). Moreover, earnings are not found to differ in February and March from other months, conditional on other covariates. Discussion with personnel at the Bureau of Labor Statistics revealed that enumerators are evaluated based largely upon interview performance at that time of year. This coincides with the March ASES being in the field and is done to ensure higher responses and more diligence during the ASES. We speculate that enumerators do not distinguish between the various parts of the survey, so additional effort affects response rates for all aspects of the survey. Consistent with this explanation is the higher earnings response rate seen for ASES than for the ORG. Alternative explanations for the February and March differences exist, although we find them less convincing. At that time, household members are more likely aware of income amounts because of tax documents, leading to a higher response rate in the February and March ORG (indeed, ASES is administered in March because it is during tax season). Knowledge of tax documents, however, is less critical for the ORG than for ASES, since ORG questions concern hours worked and rates of pay at the principal job during the prior week and not earnings from the prior calendar year. We also considered whether the ORG response rates might be affected by seasonal factors (i.e., bad weather) that reduce participation costs and improve earnings response during February and March, but monthly response patterns were found to be highly similar across states with very different seasonal weather patterns.

Turning to ASES, we find that households in either their first or fifth month in sample display nonresponse rate about 2 percentage points lower than in the other six months (because of the large samples, the differences are highly significant). The first and fifth month interviews, which take place in the same month one year apart, are typically done in person (CAPI), whereas rotation groups 2-4 and 6-8 in the months directly following the first and fifth month interviews are administered by phone (CATI).

[13] We also examined using as instruments information from CPS supplements on voting behavior and volunteer activity, expecting that "public spirit" might increase the likelihood of survey response but be uncorrelated with the wage. Volunteer activity but not voting was found to be associated with higher earnings response. Each of these potential instruments was significantly correlated with the wage, conditioned on other covariates.

It is reasonable to assume, and generally accepted in the survey literature (see, for example, Lyberg and Kasprzyk, 2004), that use of an in-person interviewer results in higher earnings response.

In an appendix (Table A-1) we present means of typical earnings variables by response status. In general, differences between those who do and do not respond to the earnings questions are not large, but the differences do indicate the correlation with response. In both the ORG and March data, response is less likely for those over 55. Respondents are more likely to have college degrees, while nonrespondents are more likely to be high school graduates. Response among women exceeds that for men. Respondents are more likely to be white, while nonrespondents are more likely Black or Asian. Workers residing outside of metropolitan areas are more likely to be respondents while those who live in the largest metropolitan areas are least likely to respond. Not surprisingly, those who do not report earnings also demonstrate much higher nonresponse rates for such variables as industry, occupation, and union status. The differences in means are not sensitive to the use of sample weights. We use weights so that reported means correspond more closely to population means.

A final piece of "descriptive" data on nonresponse is shown in Figure 1. Here we look at the full sample (all age groups and part-time workers) from the ORG and measure rates of nonresponse by *predicted* earnings, with earnings predicted using coefficients from dense log wage equations among respondents, by gender and pooled. Seeing how response varies with predicted earnings (a weighted mix of measured earnings attributes) may be informative if one makes the strong assumption that unmeasured "skills" among nonrespondents are positively correlated with the earnings attributes measured in a wage equation. Figure 1 shows nonresponse is roughly similar across predicted earnings percentiles, the exception being lower rates of nonresponse in the bottom of the distribution and higher rates of nonresponse in the very top percentiles (a similar pattern is found using ASES). If observed and unobserved earnings attributes are positively correlated, the observed pattern is consistent with negative selection into response, more so for men than for women.

Estimation Models

We begin with a standard log linear model of wages:

$$w = lnWage = X\beta + u$$

Given our large sample we choose a rich set of regressors including fourth order polynomial in potential experience, plus multiple categorical variables for education, marital status, race, immigrant status, metropolitan size, census region, public sector, two digit industry and occupation categories, and, in the ORG, union status. Although genesis of the Mincerian wage equation is as a supply-side human capital model, as employed here it should be regarded as a reduced form equation including demand as well as supply-side wage determinants. In conjunction we posit a threshold crossing model of nonresponse:

$$R = \begin{bmatrix} 1 & \lambda w + Z\delta + v > 0 \\ 0 & otherwise \end{bmatrix},$$

where w is the labor market wage, Z represents all observable characteristics including those in the wage equation, and v are unobservable terms independent of both the determinants of the wage and variables in Z. The term λ allows this model to be linked to the wage equation with either positive (response correlated with high wage) or negative (response correlated with low wage) selection. By substituting the wage equation into the above model we establish a reduced form model for response:

$$R = \begin{bmatrix} 1 & Z\gamma + \varepsilon > 0 \\ 0 & otherwise \end{bmatrix}.$$

The parameter, $\gamma = \lambda\beta + \delta$, while $\varepsilon = \lambda u + v$. We further impose the assumption of standard normality upon ε, and require that ε be strictly independent of components of Z for which the corresponding γ term is not zero. We recognize that these are strong assumptions. Consistent estimation of selection models using Heckman's two step approach typically requires these assumptions. While it may be possible to relax them, the computational burden, given our large sample sizes, becomes problematic. The two-step approach is well known (see Vella 1998) to be less sensitive to violations of normality and strict independence than maximum likelihood approaches.[14]

We first turn to estimates from the reduced form response probits. The marginal effects (evaluated at the mean of all variables) are shown for the variables in *X* in Appendix Table A-2 and for the instruments in *Z* in Table 3. The reported estimates do not use sample weights (differences are minor). Because the weights do not account for sample selection, there is not a strong conceptual argument for using weights in the selection corrected wage equation or in the corresponding first stage probit. Qualitatively, results are largely comparable for men and women and across the ORG and March samples. Some differences are expected because each model includes unique regressors – union status and the February and March dummies in the ORG and an in-coming rotation group dummy (MIS 1 or 5) in the March. Marginal effects are generally larger for the ORG than for the March survey due to higher ORG nonresponse. The information gleaned from Table A-2 is generally similar to that seen previously based on differences in means (Table A-1). Thus, a rundown of results is not warranted. Variables with large marginal effects include Black, Asian, large metro, and selected regions.

The multivariate probit analysis reinforces support for the potential instruments shown previously in Table 1. As evident in Table 3, the proxy and the interview timing variables (MIS for the March ASES and Feb/March for the ORG) are good potential instruments for selection models. Proxy respondents are substantially less likely to respond to the earnings questions. All else constant, a proxy respondent other than a spouse decreases the likelihood of response by about 20%, while a spouse proxy decreases response by somewhat more than 5% in the ORG and less than 5% in the March sample. In the ORG,

[14] Other authors have used selection models to analyze nonresponse data sets other than the CPS. For example, Hamermesh and Donald (2008) consider a selection model for earnings in a survey of college graduates. De Luca and Peracchi (2007) consider a selection model for unit and item nonresponse in a study estimating Engel curves for consumption expenditures. Johansson (2007) considers alternative methods, including sample selection, to address nonresponse in Swedish data.

response rates in February and March are roughly 5 percentage points higher than during the rest of the year. And in the ASES data, response rates are about 2 percentage points higher for people in their first of four months in the survey during each of two years (rotation groups 1 and 5).

Cross Sectional Evidence for Response Selection: Significance and Importance

In order to investigate whether or not there is nonignorable response bias in the CPS, we estimate rich log-linear wage models of the type seen in the literature. As emphasized in Bollinger and Hirsch (2006), inclusion of imputed earners leads to coefficient bias. Although some coefficients are little affected, those attached to attributes that are not imputation match criteria (union status, foreign born, industry, city size, etc.) display attenuation of about 25% in the ORG, while attributes for which there is imperfect matching (e.g., schooling, etc.) display varied forms of bias, some quite serious. If negative selection into response is also a problem, coefficients will be biased further. Including the imputed earners in a wage equation is not a valid solution for response bias since the imputations are simply predicted values from respondents.

Tables 4 and 5 present log-wage estimates for men and women, respectively, based on both the ORG and ASES samples. The first column shown for each data set shows OLS estimates based on respondents only. Full-sample OLS estimates (not shown) differ substantially due to imputation match bias (Bollinger and Hirsch, 2006). The second and third columns present the wage model estimated using the two-step Heckman correction (e.g., Vella 1998), with the coefficient on the inverse Mills ratio reported in the first row. The selection models in all cases rely on the sample-based instruments – February and March dummies for the ORG and a first interview dummy (rotation group 1 or 5) for ASES. The second column results are based on inclusion of the proxy variables in the selection but not wage equation, while the third column includes them in the wage regression. Proxy is such a strong predictor of response that it is natural to consider it as an instrument, especially given that it has no causal impact on realized (as opposed to reported) earnings. Proxy may be correlated with the measured wage equation error term, however, if proxy respondents report higher or lower earnings than do self-respondents.

We first examine the coefficients on the inverse Mills ratio selection terms shown at the top of tables 4 and 5. The Mills ratios for men using the ORG and ASES are negative, highly significant, and quite stable with respect to the inclusion or exclusion of the proxy variables as regressors in the wage equation (i.e., results in columns 2 and 3 are similar). Based on these results, we conclude that men exhibit negative selection into response, consistent with earlier research based on men in the 1973 March CPS matched to 1972 IRS records (Herriot and Spiers, 1975; Greenlees et al. 1982). We also note that non-spouse proxy responses have no apparent correlation with unobservable wage determinants, while proxy reports by wives have a very small positive correlation, reported earnings being about 1% higher.

In contrast to the results for men, the inverse Mills ratios for women, seen in the first row of table 5, are sensitive to the inclusion of the proxy variables in the wage equation. In regressions without the proxy variables included in the wage equation, negative and significant coefficients on the inverse Mills ratios are obtained in both the ORG and ASES. When the proxy variables are included in the wage regression, however, inverse Mills ratio coefficients become insignificant and much smaller in magnitude, although both are still negative. As with men, a spouse proxy response is correlated with a slightly higher reported wage. Its magnitude in the ORG is similar to that for men, while in ASES it is somewhat larger. In contrast to the results for men, for women non-spouse proxy responses are negative and significantly correlated with reported wages. A non-spouse proxy response in the ORG is associated with 3.7% lower reported wage for women, while in the ASES the coefficient is negative but not significantly different than zero. For women, correlation of the proxy variables with the wage appears to be sufficiently strong to reject its use as a selection instrument.

The selection results for men seen in Table 4 appear clear-cut, with negative selection into response appearing to be stable and significant, and differences between self and proxy reports not appearing to be important. For women, evidence for negative selection is weaker and there appear to be differences in reported wages between self and proxy respondents. Later in the paper, we use longitudinal data to examine more directly proxy effects on reported earnings. Panel results show that proxy earnings reports are somewhat lower than self-reported earnings, but the differences between spouse and non-spouse reports seen in cross-section analysis largely reflect unmeasured worker heterogeneity.

We next examine the practical importance of selection on coefficient estimates. Because of large sample sizes, trivial differences in coefficients can be statistically significant. We instead focus on the magnitude of coefficient differences between wage equations with and without an accounting for selection into response. To examine these differences, we return to tables 4 and 5, which allow us to compare coefficients from OLS regressions using respondents only (full sample OLS equations are severely biased) and full-sample selection equations (column 3, using the sampling frame but not proxy dummies as instruments). The key result of these comparisons is that most changes in coefficients are quite minor. For most applications the choice of approach will have little effect on interpretation of coefficients. The possible exceptions are for variables most highly correlated with earnings nonresponse, for example Asian, Black, large metro, and the West-North Central, Mountain, and Pacific regions.

Although response bias has little effect on most wage equation slope estimates, this need not imply selection into response is not substantive. To assess the overall magnitude of response bias, we compare predicted earnings based on both the OLS coefficients for respondents and the selection corrected estimates. These results are seen in Table 6. The first column of table 6 presents overall mean log wages in the ORG and ASES samples, inclusive of the nonrespondents' imputed wages, while the second column presents means for respondents only. The third column presents the mean earnings prediction using coefficients from the OLS respondents models from tables 4 and 5, but for all observations, including non-respondents. The fourth column reports the mean earnings prediction using the selection models reported in column 3 from tables 4 and 5 (for all observations, including non-respondents). The selection term is not used in the prediction, hence this represents the mean of all wages were they to be reported.[15] The difference between these two provides a measure of the magnitude of bias due to selection into response. We find that the difference for men is a sizable -0.09 in both the ORG and ASES. That is, negative selection into response among men is predicted to result in average earnings being understated by (an unrealistically large) 9%. However, downward bias in earnings for women is much smaller, about -.02 log points, as expected given the weak evidence among women of selection bias. Taken at face value, the implication is that conventional estimates of the gender gap in earnings are understated by some 7%. Importantly, whatever the biases due to nonresponse, these show up mainly as differences in the intercepts and not slopes, the latter typically being the principal concern of researchers.

To recap, our conclusion to this point is that selection into response is negative for men, perhaps substantially so, while modest for women. Regression coefficients, apart from intercepts, are not sensitive to selection, with the exception of those on variables highly correlated with nonresponse. We should reiterate a point made earlier. Our OLS estimates are based on earnings respondents. Inclusion of records whose earnings are imputed leads to severe match bias on OLS coefficients, and this will be so even absent response bias. Imputations are generated under the assumption of conditional missing at random – i.e., no response bias. Hence, use of imputations in OLS regressions does not alleviate response bias, while at the same time creating substantial coefficient match bias.

Longitudinal Analysis: Identifying Response Bias as a Fixed Effect?

We provide further investigation of response bias using wage change analysis with CPS panels. Each worker in the CPS panel is observed in the same month in consecutive years. Longitudinal analysis allows us to account (imperfectly) for worker heterogeneity that is a principal source of response bias – unmeasured differences in workers who are and are not likely to report earnings. For workers who respond in one year but not in the adjacent year, we compare their respondent earnings with the earnings imputed to them in the other year. Imputed values on average provide a good measure of the earnings of respondents (donors) with identical measured attributes included as Census match criteria. Individuals who report in one year but not in another are not likely to be fully representative of either those who always report or never report earnings. For this reason, the panel results are likely to provide accurate qualitative but not quantitative estimates of response bias.

The model and results of wage determination and response presented in the previous section can be extended to provide a starting framework for our longitudinal analysis. Letting *R* designate "*Respond*" and *NR "Not Respond*" the model implies that

$$E[w|X,R] = X\beta + E[u|X,R] \text{ and}$$

[15] The selection predicted means for respondents only, not shown in the table, are highly similar to those shown in column 4.

$$E[w|X, NR] = X\beta + E[u|X, NR].$$

When nonrespondents have their earnings imputed from those of respondents, the difference is on average

$$\mathrm{E}[w - w^{impute}|X, R] = \Delta X\beta + E[u|X, R] - E[u|X, NR] .[$$

In the case of missing at random (as assumed by the imputation process), the two terms $E[u|X,R]$ and $E[u|X,NR]$ would be zero, and the only differences would be in observable X's. On average, one would expect that no differences in means exist, so comparison of aggregate wages between reported and imputed earnings would be roughly zero. Although Bollinger and Hirsch (2006) show that the imputation procedure will produce bias in β, we can assume the term $\Delta X\beta$ would be zero given no differences in means. If, as our results for men suggest above, there is negative selection into response, the term $E[u|X,R]$ would be negative, while the term $E[u|X, NR]$ would be positive. Assuming $\Delta X\beta$ to be zero still, we would expect, on average, that non-respondents' wages are underestimated by the imputation procedure and $E[w\text{-}w^{impute}]$ is positive.

While we cannot observe both reported and imputed wages for anyone in the same period, the panel data allow us to observe wages in consecutive years. For individuals who respond in both periods, the difference is simply the term $\Delta X\beta$. For individuals who do not respond in the first period, but respond in the second period (designated NR/R) we can write their difference as

$$\mathrm{E}[w_2 - w_1^{impute}|X, NR/R] = \Delta X\beta + E[u_2|X, NR/R] - E[u_1|X, R].$$

By comparing the difference between the average change for respondents in both periods and the average change in those who only respond in one period, we can isolate the term $E[u_2|X, NR/R]\text{-}E[u_1|R]$. Interpreting this term as a measure of response bias – as one could if we could compare the actual wage to the imputed wage in the same period – requires two assumptions. The first is that there is no difference, on average, in the wage growth rates of respondents and nonrespondents. That is, the terms $\Delta X\beta$ are approximately the same for both groups. Secondly, the term $E[u_2|X,NR/R]$ must "look like" $E[u|NR]$. That is, those who change from NR to R must have the higher u associated with the initial selection into nonresponse. If these assumptions hold, then individuals whose wages are first imputed, and then who respond in the second period should have higher wage growth than those who respond in both periods. Those who first respond and then are imputed should have lower wage growth. If response switchers have error terms (unobservables) "in between" wage earners who always respond and those who never respond, then longitudinal estimates provide correct qualitative measures of response bias, but understate the magnitude.

We examine wage growth in two ways. First, we compare average annual wage growth rates for the four groups. Table 7 presents these results. The panel sample, like the primary sample in the cross sectional analysis, is composed of workers who are full time in both periods, who are not enrolled in school full time in either period and who are between ages 18 and 65 in both years. The first two columns present the number and percentages of respondents who fall into the four response categories. As one would expect, the majority of respondents respond in both periods: 59% of the ORG sample and 73% of the ASES sample. If response were simply random, based on the marginal rates in the cross section, we would expect 44% and 65% of the panel samples to respond in both periods. The higher rates indicate that there is some persistence in response behavior. Similarly, if response were random, we would expect approximately 12% of the ORG and 4% of the ASES to be nonresponders in both periods, less than observed persistence in nonresponse. In spite of the persistence, there are a large numbers of individuals who switch status, suggesting that they are close to the margin between response and nonresponse.

The average wage growth for responders in the ORG is 2.8%, and in the ASES it is 3.2%. We take this as a baseline growth, which, under our model above, should represent the term $\Delta X\beta$. We note that in both the ORG and the ASES, the aggregate patterns are consistent with the negative selection into response found in the cross sectional data. Compared to the respond–respond wage growth, the respond–impute growth is much lower and impute–respond growth higher. The model above would suggest that the impute–impute group should have roughly the same wage growth as the respond–respond group. This is the case for the ASES but not the ORG panel. This most likely suggests that the $\Delta X\beta$ term is different for non-responders than for responders. We next turn to the results by gender. In the ORG, the wage growth pattern continues to hold, with equal strength, for both women and men. While consistent with the negative selection into response found for men in the prior section, it is not fully consistent with the very weak selection result found previously for women. And it is not clear whether wage growth for the impute–respond group should be compared to that of the respond-respond or impute-impute group. In the ASES we find a less clear-cut pattern; while respond–impute growth for both men and women is smaller than for the respond–respond group, the magnitude is stronger and clearer for women. Further, impute–respond growth for men is slightly smaller than for respond–respond while for women it is much larger.[16]

The panel evidence provides reinforcing evidence that there exists some degree of negative selection into response and that imputations tend to understate earnings. The magnitudes implied by the panel estimates are substantially smaller than those implied by the cross-section selection models (see Table 6), but this is expected given that response "switchers" may be more similar to those who almost always respond than to those who would rarely or never respond. The principal difference in the cross-section and panel results is that the former found at most weak evidence for negative selection among women, whereas the latter results suggest that negative selection is at least as strong among women as men (or more precisely, at least as strong among the population of female versus male response switchers). In short, the strong assumptions necessary for this aggregate analysis are no doubt violated, although we cannot know to what degree. Strong assumptions are also required in the selection model, however. Just as the panel results are identified based on the unrepresentative set of workers who switch

[16] Largely similar results are obtained if we restrict ourselves to the much smaller sample of individuals who did not have proxy reports in either year.

response status, the selection results are effectively identified based on those whose response status is induced by the CPS sample frame instruments (more intensive efforts by or more direct contact with Census enumerators), a group not necessarily representative of the population of workers whose response status is unaffected by these instruments.[17]

Table 8 presents results from three different specifications further examining selection in the panel data. The first column in each panel is a difference specification based upon the linear specification in the previous section. In all specifications, we include changes in education; square, cubic and quartic experience; marital status; citizenship for foreign born; and industry and occupation, plus year dummies. Since industry and occupation categories changed in 2003, we remove the 2002-2003 panel observations from our sample. Other specifications that include 2002-2003 and use various approaches to address the change are not substantively different. We also include changes in the two proxy variables. The selection equation includes these differences plus the indicator variables for the interview timing: February and March for the ORG and month in sample 1/5 for ASES. In both specifications the interview timing variables are highly significant in the selection equation and similar to those found in the level equations. Note that here the selection equation represents selection into response in both periods. This is our preferred specification because it is most comparable to our approach up to this point. We find that in both samples for men and women, the Mills ratio coefficient representing selection is insignificant. In the ORG, the coefficient is negative and small, around -.03 for men and women, suggesting very weak negative selection for each group. However, the coefficient is insignificant and accounting for selection has little impact on the estimated slope coefficients and the intercepts. In the ASES, the Mills ratio coefficient is positive and somewhat larger in magnitude, around .10, but still insignificant.

The second column of Table 8 adds first period level values for all regressors in the original specification to the selection equation. That is, the selection equation in the second column now includes initial year proxy variables, education, experience (as a quartic), marital status, race, metropolitan size, region, industry and occupation in addition to the first differences. The wage equation still includes the first differences only. Under the assumptions that the first difference wage model applies, these are all valid instruments for the selection equation. However, if initial conditions matter in the wage equation, the inclusion of these variables would tend to make it appear that there is selection. In all specifications, the coefficient on the Mills ratio declined in magnitude. It is statistically significant in the ASES women's equation, but has reversed sign compared to the first column. It is also smaller in magnitude than in the level equation, only -.03. It also appears to have little impact on estimated coefficients and intercepts. While the finding of slight negative selection for women in the ASES is consistent with the

[17] Given that the imputation process can generate substantially different wage growth patterns, it is highly questionable to use imputed values in panel analysis. More fundamentally, panel analysis identifies causal effects off of changes in X (e.g., union status), yet the imputation process generally assigns a nonrespondent the reported earnings of an X-stayer rather than an X-switcher (Bollinger and Hirsch, 2006, 485n).

findings in table 7, we are skeptical that this represents the true data generating process, as the assumption that none of the level variables matter in wage growth is strong. It is beyond the scope of this paper to determine if a first differences wage specification is correct.

The final column in table 8 presents the original levels specification estimated on the first year variables in the panel. We present this in order to note that the panel sample appears to be rather different from the cross section sample. In the ORG data, we find statistically significant evidence for negative selection for men but not for women, consistent with the previous findings, except that the male selection coefficient is now much smaller. In the ASES, we find no statistical evidence of selection for either men or women. The coefficient on the inverse Mills ratio for men is negative but close to zero. We conclude that it is not that the selection term "drops out" in the first differences (one possible explanation of the differences between the cross section and panel findings), but more likely a difference in average response behavior between those in the cross section and those in the panel. This finding is consistent with other research on data quality. Bollinger (1998) finds that measurement error in the reporting of annual earnings in the March CPS is reduced (in both mean differences and variance) when the sample is restricted to those that can be matched across years (see also Bound and Krueger (1991) for related findings). Bollinger and David (2005) find that measurement error in food stamp program participation in the SIPP is concentrated among those who later leave the panel for voluntary reasons.

Can Composition Account for Cross Section and Panel Differences?

To further investigate differences between the cross section and panel results, we examine differences in characteristics between the respective samples. In order to create panel samples in the ORG and March CPS, we matched on broad race, gender and age variables to check the standard match based on household and person level identification numbers in the public use files (for discussion of matching in the CPS see Card 1996; Madrian and Lefgren 2000). Matching in general removes individuals who do not live at the same address (the sample unit in the CPS) for over a year, a disproportionate number of whom are young. Clearly, the panel sample in the CPS is not representative of the general population, because it will not, by nature, capture those who move. The matching on demographic characteristics also removes individuals who remain at the same address but provide inconsistent data from period to period. We finally note that our restriction that individuals be full time (full year) in both periods is also restrictive, although consistent with literature using the CPS.

A complete characterization of the differences between the two samples is beyond the scope of this research. Table 9, however, focuses upon an important difference – the relationship to the reference person. We collapse the BLS classifications of relationship into four groups: head or co-head of households (primary individuals, heads with relatives, husbands and wives), children of heads, partners or roommates, and all other relationships. This grouping was done with a focus on the largest categories and their relative imputation rates. We note that head/co-heads have lower nonresponse (imputation) rates than any other group. Although partners and roommates are second, their nonresponse rates are still markedly higher than heads/co-heads. Adult children living with their parents have the highest nonresponse rate, with the other relations a close second. As we move from the cross section to the panel data, using either the ORG or ASES, the data concentrate upon heads/co-heads, whose shares are rising, while shares of the three other categories are falling. These same generalizations hold when we analyze more detailed relationship groups. All groups display a slight decline in the nonresponse rate, but the largest change is for household head/co-heads. Hence, the panel sample concentrates upon primary individuals, reference persons, and the spouses of reference persons who are responders in the first year.

In Table 10 we present selected coefficients from re-estimating the third specification in tables 4 and 5 (the main cross section results), this time using the original cross section sample, minus those who are not either a head of household or spouse of a head. As with table 8, we present only the inverse Mills coefficients and coefficients on the proxy variables. For comparison, we repeat the comparable "full sample" results previously shown in Tables 4 and 5. Comparing these results, what is most notable is that the magnitude of the sample selection coefficients for men in both samples has fallen dramatically. While it is still significant in the ORG for men, it is far smaller. Comparing it to the level equations in table 8 (using the cross sectional sample) it is qualitatively quite similar. It also is consistent with the raw comparisons for the panel in table 7. We note in particular that in the ASES sample, the estimated coefficient on the Mills ratio is nearly the same for men and women, and insignificant. Hence selection on the observables would dominate the overall means. We also note that the coefficients on the proxy variables are closer to the level results in the panel. Of particular note here is that in both samples, selection effects of nonresponse can be reduced by limiting the sample to household heads/co-heads. The panel sample is more concentrated on this group, and hence has less selection as well. The selected sample, however, is no longer representative of the larger working population. For this selected sample, biases from imputations would be smaller than those seen in the full sample, with smaller differences between men and women and less bias in gender wage gap estimates.

Proxy Reports Revisited

The wage change and wage level results previously reported in Table 8 include coefficients on the spouse and non-spouse proxy variables. Longitudinal analysis is well suited to net out fixed worker effects and measure for the same individuals the difference between self-reported and proxy-reported earnings. Recall that in Tables 4 and 5, wage level selection models suggested that workers whose wages are reported by a spouse have wages similar to those who self-report, whereas those with non-spouse proxies have lower reported wages. This same pattern is seen in the Table 8 levels equations, where we restrict the level samples to those included in the panel. For men and women in both the ORG and ASES, spouse reports are similar to self-reports, whereas non-spouse reports are 7-8% less.

In sharp contrast to the cross section findings, however, wage change results (with or without *X*'s in levels included as instruments) clearly show that spouse and non-spouse proxy reports are remarkably similar to each other. In the ORG and ASES samples, men who self-report in one year and have a proxy respondent in the other year display earnings about 2 to 2½% lower when reported by a proxy, either spouse or non-spouse. An identical pattern is seen for women, with spouse and non-spouse reports being about 1 to 2% lower. The systematic difference between wage level and panel results reflects worker heterogeneity. Workers whose earnings are reported by a non-spouse proxy tend to have unmeasured attributes associated with lower wages, whereas those with spouse proxy reports tend to have unmeasured attributes associated with slightly higher wages.

Our results on proxy reports are relatively clear-cut and important. From the perspective of government data collection, the "good" news is that proxy reports differ from self reports only modestly and, on average, non-spouse reports are similar to those of spouses. The "bad" news is that about half of all earnings reports are based on proxy responses and the differences seen in Table 8 are "modest" rather than zero. Implications for researchers are less clear-cut. Inclusion of spouse and non-spouse proxy controls in standard wage level equations may be warranted in order to control for unmeasured heterogeneity, although it is important that they be interpreted as such. A point worth noting is that nothing in our results tells us whether proxy reports on earnings are modestly too low or whether self reports are too high, although older validation studies suggest that it is proxy reports that are too low, if one makes the (strong) assumption that administrative data measure true earnings.

Conclusion

Item nonresponse, earnings imputation, and proxy respondents are common in household surveys, most notably in the CPS. We examine the issue of response bias on earnings and proxy respondents using the CPS ORG monthly earnings files and March CPS ASES for 1998-2008. Although wage studies by labor economists typically include imputed earners and records with proxy respondents, recent research shows clearly that inclusion of imputed earners can introduce substantial bias due to mismatch in the imputation process. Simple corrections for match bias, including removal of imputed earners from the estimation sample, largely eliminate the first-order distortions resulting from imperfect matching. But this and other approaches to correct for match bias (see Bollinger and Hirsch 2006) rest on the important assumption that nonrespondents are conditional missing at random (ignorable response bias). Including imputed earners in an OLS wage equation not only introduces coefficient match bias, but also fails to offer a solution for response bias given that the earnings assigned to nonrespondents are obtained from respondents. This paper explores the nature of response bias in the CPS, along with the related issue of how proxy responses (a strong correlate of nonresponse) affect reported earnings.

Using selection wage equations in which selection is identified by measures on the timing of the surveys (and, in some specifications, proxy respondents), we find clear-cut evidence of negative selection into response among men, but weak evidence among women. Importantly, the response bias appears to be largely a fixed effect, introducing bias into estimates of wage equation intercepts but not slopes, with the exception of a few attributes highly correlated with nonresponse. Understatement of men's earnings due to nonresponse coupled with a small effect on women's earnings will result in an understatement of the gender wage gap.

Panel analysis leads to a more complex and nuanced assessment of response bias. Comparing workers' reported earnings with the same workers' imputed value in the previous or subsequent year should provide a correct qualitative and lower-bound quantitative measure of response bias. Consistent with the evidence of negative selection into response, we find that imputations understate reported earnings, albeit by modest amounts. Such evidence is found for men and women, with estimates for women at least as large as for men. Further exploration shows that the sample composition of CPS panels differs from cross section samples, with household heads and spouses overrepresented in a panel. We then go back to our full cross-section sample and restrict it to household heads (married or single) and spouses. When we re-estimate the selection models, the selection bias found for men in the head/co-head sample is substantially lower than seen in the full sample, explaining some of the discrepancy between our original selection results and the panel analysis. These results complement prior literature (Bollinger 1998) suggesting that respondents who remain in a panel often provide more reliable data than those who exit. We add to that finding by showing that limiting a sample to heads of household or their spouses, bias from nonresponse is reduced as well.

Finally, we examine the effect of proxy reports on earnings, which account for half of all earnings records in the CPS. In standard cross section earnings equations, proxy reports are about 2-3% lower than self-reports. What these results mask is a substantive difference between spouse and non-spouse proxies. For both men and women in the CPS and ASES, non-spouse proxies report earnings lower than self reports, whereas spouse proxy reports are close to self reports. Panel analysis, however, reveals that the non-spouse reporting effects are not due to reporting error but to unobserved heterogeneity, including that associated with response bias since proxy is a strong correlate of response. Based on the panel analysis, proxy effects on reported earnings tend to be negative, modest in size (about 2%), and not greatly different for spouse and non-spouse proxies. The "problem" is not the use by Census of proxy respondents, who provide reasonably accurate (and low cost) reports on earnings but, rather, worker heterogeneity correlated with both the proxy variable and nonresponse.

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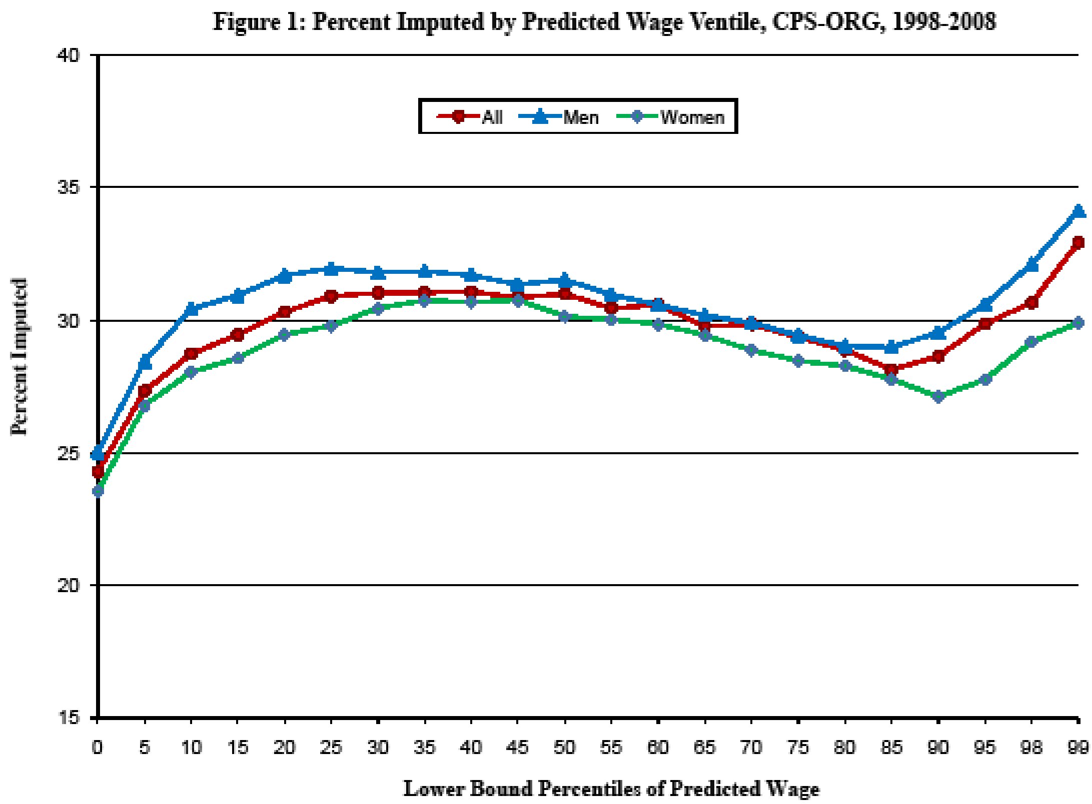
Figure 1: Percent Imputed by Predicted Wage Ventile, CPS-ORG, 1998-2008
All
Men
Women
Percent Imputed
40
35
30
25
20
15
0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95 98 99
Lower Bound Percentiles of Predicted Wage

Table 1: CPS Imputation Rates by Sample, Wage Measure, Year, Survey Frame, and Proxy Status

	ORG		March surveys	
Sample or Year	N	%Imputed	N	%Imputed
Full sample, unweighted	1,867,388		782,095	18.1%
Wage based on weekly earnings		29.8%	n.a	n.a
Wage based on hourly & weekly earnings		31.9%	n.a	n.a
Full sample, weighted	1,867,388		782,095	18.9%
Wage based on weekly earnings		31.2%	n.a	n.a
Wage based on hourly & weekly earnings		33.3%	n.a	n.a
Primary sample, unweighted	1,499,630		564,722	18.7%
Wage based on weekly earnings		30.4%	n.a	n.a
Wage based on hourly & weekly earnings		32.7%	n.a	n.a
Primary sample, weighted	1,499,630		564,722	19.6%
Wage based on weekly earnings		31.8%	n.a	n.a
Wage based on hourly & weekly earnings		34.1%	n.a	n.a
Primary sample, all years, weighted	**1,499,630**	**34.1%**	**564,722**	**19.6%**
1998	120,905	27.2%	40,464	17.2%
1999	126,269	31.0%	41,526	16.6%
2000	128,580	33.3%	40,779	19.6%
2001	136,088	35.0%	65,807	20.2%
2002	145,147	35.0%	63,757	21.4%
2003	142,438	36.4%	62,442	20.7%
2004	139,802	36.1%	61,878	20.8%
2005	141,171	35.7%	62,327	19.1%
2006	141,412	35.7%	62,749	20.0%
2007	139,990	34.9%	62,993	20.1%
2008	137,828	34.6%	n.a	n.a
Self Report	756,693	27.8%	281,887	15.2%
Proxy Report*	742,937	40.5%	282,835	24.0%
Spouse	452,234	34.6%	185,813	18.6%
Nonspouse	290,703	49.0%	97,022	32.9%
February	123,985	30.5%	n.a	n.a
March	122,831	29.6%	n.a	n.a
January, April-December	1,252,828	34.9%	n.a	n.a
First Interview	n.a	n.a	142,330	17.8%
Later Interview	n.a	n.a	422,392	20.2%

Full Sample includes all persons working during the earnings reference period. Primary Sample restricted to persons ages 18 to 65 working full time (year round in ASES) and not enrolled full time in school.
*Proxy information not available in 1998 March CPS.

Table 2: Self Reports and Proxy Earnings Responses, by Gender and Marital Status

	ORG Sample			March ASES Sample		
	All	Men	Women	All	Men	Women
Self Reports	50.5%	42.9%	59.8%	49.9%	42.8%	59.1%
Proxy	49.5%	57.1%	40.2%	50.1%	57.2%	40.9%
Spouse	30.2%	36.6%	22.2%	32.9%	39.5%	24.5%
Non-spouse	18.4%	20.5%	18.0%	17.2%	17.8%	16.4%
%Proxies who are spouse	60.9%	64.1%	55.3%	65.7%	68.9%	59.9%

All results computed without sample weight using the primary sample (see Table 1),

Table 3: Estimated Marginal Effects of Potential Instruments in Probit Response Model

	ORG		ASES	
	Male	Female	Male	Female
Non-spouse Proxy	-0.238**	-0.254**	-0.201**	-0.195**
Spouse Proxy	-0.0618**	-0.0818**	-0.0385**	-0.0196**
February	0.0434**	0.0411**	n.a.	n.a.
March	0.0500**	0.0461**	n.a.	n.a.
Month in Sample 1 or 5	n.a.	n.a.	0.0229**	0.0251**
Sample Size	827,531	672,099	318,119	246,603

Dependent variable = 1 if respondent. Unweighted estimates shown (weighted estimates available by request). Other variables and coefficients included are shown in Appendix A-2.
*significant at 5%
**significant at 1%.

Table 4: Male Wage Equation Estimates: OLS and Selection Models

	ORG			ASES		
	OLS	Selection	Selection	OLS	Selection	Selection
Inverse Mills ratio	n.a	-0.167**	-0.166**	n.a	-0.267**	-0.276**
Non-spouse proxy	n.a	n.a	-0.002	n.a	n.a	0.00008
Spouse proxy	n.a	n.a	0.008**	n.a	n.a	0.012**
Elementary school	-0.174**	-0.181**	-0.181**	-0.210**	-0.211**	-0.211**
Grade 9	-0.144**	-0.154**	-0.154**	-0.188**	-0.193**	-0.193**
Grade 10	-0.122**	-0.129**	-0.129**	-0.161**	-0.161**	-0.161**
Grade 11	-0.099**	-0.105**	-0.105**	-0.145**	-0.145**	-0.145**
Grade 11+ (no HS diploma)	-0.076**	-0.075**	-0.074**	-0.120**	-0.116**	-0.116**
Some college, no degree	0.086**	0.080**	0.080**	0.090**	0.084**	0.084**
Associates degree	0.127**	0.120**	0.120**	0.140**	0.134**	0.134**
Bachelor's degree	0.301**	0.294**	0.294**	0.316**	0.313**	0.313**
Masters degree	0.424**	0.412**	0.412**	0.453**	0.451**	0.451**
Professional degree	0.737**	0.740**	0.739**	0.818**	0.833**	0.834**
Ph.D.	0.653**	0.642**	0.641**	0.680**	0.682**	0.682**
Potential experience (age-schooling-6)	0.045**	0.043**	0.043**	0.061**	0.056**	0.056**
Experience-squared/100	-0.169**	-0.154**	-0.153**	-0.247**	-0.208**	-0.207**
Experience-cubed/10,000	0.290**	0.245**	0.244**	0.450**	0.351**	0.349**
Experience-quartic/1,000,000	-0.207**	-0.162**	-0.162**	-0.324**	-0.237**	-0.237**
Married	0.135**	0.124**	0.119**	0.174**	0.156**	0.148**
Previously married	0.040**	0.030**	0.030**	0.058**	0.044**	0.044**
Black	-0.155**	-0.135**	-0.135**	-0.146**	-0.131**	-0.130**
Asian	-0.039**	-0.025**	-0.024**	-0.032**	-0.016**	-0.014*
Other	-0.059**	-0.061**	-0.061**	-0.073**	-0.069**	-0.069**
Hispanic	-0.131**	-0.134**	-0.134**	-0.119**	-0.119**	-0.119**
Foreign born, not citizen	-0.169**	-0.173**	-0.172**	-0.195**	-0.197**	-0.196**
Foreign born, citizen	-0.0701**	-0.068**	-0.067**	-0.070**	-0.071**	-0.070**
Metro under 250M	0.061**	0.060**	0.060**	0.065**	0.060**	0.060**
Metro 250K-500K	0.082**	0.087**	0.087**	0.085**	0.086**	0.086**
Metro 500K-1M	0.113**	0.119**	0.119**	0.116**	0.122**	0.123**
Metro 1M-2.5M	0.142**	0.148**	0.148**	0.147**	0.149**	0.150**
Metro 2.5M - 5M	0.241**	0.252**	0.252**	0.220**	0.225**	0.225**
Metro 5M +	0.236**	0.253**	0.253**	0.228**	0.243**	0.243**
Mid Atlantic	-0.032**	-0.023**	-0.023**	-0.007	-0.004	-0.004
East North Central	-0.043**	-0.044**	-0.044**	-0.030**	-0.031**	-0.031**
West North Central	-0.085**	-0.104**	-0.104**	-0.087**	-0.107**	-0.108**
South Atlantic	-0.053**	-0.053**	-0.053**	-0.058**	-0.059**	-0.059**
East South Central	-0.090**	-0.086**	-0.086**	-0.083**	-0.086**	-0.086**
West South Central	-0.113**	-0.124**	-0.124**	-0.110**	-0.129**	-0.129**
Mountain	-0.023**	-0.046**	-0.046**	-0.027**	-0.053**	-0.054**
Pacific	0.027**	0.016**	0.017**	0.013**	0.0002	-0.000009
Federal government	0.051**	0.058**	0.058**	0.078**	0.067**	0.0674**
State government	-0.200**	-0.200**	-0.200**	-0.090**	-0.101**	-0.101**
Local government	-0.177**	-0.176**	-0.175**	-0.034**	-0.043**	-0.043**
Union member	0.173**	0.170**	0.170**	n.a	n.a	n.a
Intercept	2.436**	2.515**	2.516**	1.802**	1.921**	1.925**
Respondent sample size	553,727	553,727	553,727	258,552	258,552	258,552
Full sample size	553,727	827,531	827,531	258,552	318,119	318,119

Estimates are unweighted. The wage equations also include industry, occupation, and year dummies. All industry categories exclude government workers. All racial categories exclude Hispanics.
* significant at 5%
** significant at 1%

Table 5: Female Wage Equation Estimates: OLS and Selection Models

	ORG			ASES		
	OLS	Selection	Selection	OLS	Selection	Selection
Inverse Mills ratio	n.a	-0.114**	-0.034	n.a	-0.142**	-0.062
Non-spouse proxy	n.a	n.a	-0.037**	n.a	n.a	-0.024
Spouse proxy	n.a	n.a	0.010**	n.a	n.a	0.023**
Elementary school	-0.167**	-0.173**	-0.166**	-0.179**	-0.180**	-0.176**
Grade 9	-0.159**	-0.166**	-0.160**	-0.180**	-0.184**	-0.180**
Grade 10	-0.145**	-0.149**	-0.146**	-0.163**	-0.165**	-0.164**
Grade 11	-0.115**	-0.119**	-0.116**	-0.148**	-0.153**	-0.150**
Grade 11+ (no HS diploma)	-0.072**	-0.072**	-0.072**	-0.106**	-0.106**	-0.105**
Some college, no degree	0.092**	0.088**	0.090**	0.095**	0.091**	0.093**
Associates degree	0.172**	0.168**	0.169**	0.185**	0.181**	0.183**
Bachelor's degree	0.331**	0.326**	0.327**	0.367**	0.363**	0.364**
Masters degree	0.495**	0.486**	0.489**	0.538**	0.533**	0.533**
Professional degree	0.759**	0.753**	0.753**	0.788**	0.786**	0.785**
Ph.D.	0.723**	0.713**	0.716**	0.763**	0.758**	0.757**
Potential experience (age-schooling-6)	0.048**	0.047**	0.045**	0.066**	0.063**	0.064**
Experience-squared/100	-0.235**	-0.225**	-0.217**	-0.346**	-0.325**	-0.328**
Experience-cubed/10,000	0.537**	0.503**	0.486**	0.823**	0.762**	0.772**
Experience-quartic/1,000,000	-0.467**	-0.430**	-0.419**	-0.728**	-0.668**	-0.679**
Married	0.031**	0.028**	0.018**	0.048**	0.044**	0.030**
Previously married	0.004*	-0.003	-0.002	0.015**	0.008*	0.009*
Black	-0.091**	-0.077**	-0.088**	-0.083**	-0.074**	-0.079**
Asian	-0.019**	-0.008*	-0.015**	-0.015*	-0.004	-0.010
Other	-0.039**	-0.037**	-0.038**	-0.031**	-0.030**	-0.030**
Hispanic	-0.097**	-0.098**	-0.097**	-0.096**	-0.095**	-0.095**
Foreign born, not citizen	-0.162**	-0.165**	-0.161**	-0.175**	-0.178**	-0.176**
Foreign born, citizen	-0.055**	-0.054**	-0.054**	-0.056**	-0.059**	-0.057**
Metro under 250M	0.064**	0.063**	0.064**	0.0778**	0.075**	0.076**
Metro 250K-500K	0.092**	0.095**	0.093**	0.099**	0.099**	0.098**
Metro 500K-1M	0.122**	0.127**	0.123**	0.138**	0.142**	0.140**
Metro 1M-2.5M	0.157**	0.161**	0.158**	0.167**	0.167**	0.167**
Metro 2.5M - 5M	0.255**	0.263**	0.258**	0.252**	0.255**	0.253**
Metro 5M +	0.254**	0.267**	0.258**	0.256**	0.264**	0.259**
Mid Atlantic	-0.033**	-0.026**	-0.031**	-0.019**	-0.017**	-0.018**
East North Central	-0.062**	-0.062**	-0.063**	-0.056**	-0.056**	-0.056**
West North Central	-0.091**	-0.103**	-0.095**	-0.088**	-0.098**	-0.093**
South Atlantic	-0.066**	-0.065**	-0.066**	-0.063**	-0.063**	-0.063**
East South Central	-0.119**	-0.117**	-0.118**	-0.095**	-0.098**	-0.096**
West South Central	-0.141**	-0.147**	-0.143**	-0.133**	-0.142**	-0.137**
Mountain	-0.049**	-0.062**	-0.053**	-0.050**	-0.063**	-0.055**
Pacific	0.022**	0.017**	0.021**	0.011*	0.005	0.009
Federal government	0.092**	0.097**	0.094**	0.156**	0.148**	0.152**
State government	-0.159**	-0.157**	-0.158**	-0.067**	-0.070**	-0.068**
Local government	-0.190**	-0.190**	-0.189**	-0.066**	-0.071**	-0.068**
Union member	0.119**	0.113**	0.117**	n.a	n.a	n.a
Intercept	2.310**	2.355**	2.345**	1.673**	1.728**	1.711**
Respondent sample size	454,991	454,991	454,991	200,826	200,826	200,826
Full sample size	454,991	672,099	672,099	200,826	246,603	246,603

Estimates are unweighted. The wage equations also include industry, occupation, and year dummies. All industry categories exclude government workers. All racial categories exclude Hispanics.
* significant at 5%
** significant at 1%

Table 6: Mean Log Wage Differences from OLS versus Selection Estimates

	(1) Full sample means	(2) Respondent means	(3) Full sample means, OLS respondent coeff.	(4) Selection coefficient means	(5) Overall bias (3) - (4)
CPS ORG:					
Male	2.973	2.982	2.981	3.069	-0.088
Female	2.781	2.792	2.790	2.806	-0.016
M-F difference	0.192	0.190	0.191	0.262	-0.072
CPS ASES:					
Male	3.014	3.021	3.016	3.105	-0.089
Female	2.769	2.780	2.776	2.795	-0.019
M-F difference	0.244	0.242	0.240	0.310	-0.070

See the text for explanation of each of the column measures.

Tabel 7: Panel Wage Growth among Respondent and Nonrespondent Stayers and Switchers

Year 1	Year 2	Sample size	Sample percent	All mean ΔlnWage	Men mean ΔlnWage	Women mean ΔlnWage
CPS ORG:		438,343				
Respond	Respond	258,015	58.86%	0.028	0.028	0.028
Respond	Impute	55,338	12.62%	0.002	0.004	-0.002
Impute	Respond	56,294	12.84%	0.040	0.037	0.046
Impute	Impute	68,696	15.67%	0.012	0.011	0.013
CPS ASES:		110,776				
Respond	Respond	80,327	72.51%	0.032	0.032	0.032
Respond	Impute	11,689	10.55%	0.006	0.022	-0.014
Impute	Respond	10,417	9.40%	0.043	0.026	0.064
Impute	Impute	8,343	7.53%	0.032	0.034	0.031

Shown is average one-year real wage growth for four sets of workers based on response status. Sample includes those who are full-time non-students ages 18-65 in both years.

Table 8: Selection Wage Equations in Differences and Levels Using Panel Sample: Selection Estimates and the Effect of Proxy Reports

	CPS ORG			CPS ASES		
	Δ equation	Δ equation + level instruments	levels regression	Δ equation	Δ equation + level instruments	levels regression
Men:						
Inverse Mills ratio	-0.033	0.006	-0.074**	0.116	-0.006	-0.018
Δ Non-spouse proxy	-0.026**	-0.026**	n.a.	-0.026**	-0.026**	n.a.
Δ Spouse proxy	-0.024**	-0.024**	n.a.	-0.022**	-0.021**	n.a.
Non-spouse proxy	n.a.	n.a.	-0.068**	n.a.	n.a.	-0.084*
Spouse proxy	n.a.	n.a.	-0.014**	n.a.	n.a.	0.004
N	218,553	218,553	218,553	55,442	55,442	55,442
Women:						
Inverse Mills ratio	-0.031	-0.008	0.014	0.100	-0.034*	0.049
Δ Non-spouse proxy	-0.016**	-0.016**	n.a.	-0.014	-0.011	n.a.
Δ Spouse proxy	-0.020**	-0.020**	n.a.	-0.009	-0.008	n.a.
Non-spouse proxy	n.a.	n.a.	-0.086**	n.a.	n.a.	-0.071**
Spouse proxy	n.a.	n.a.	-0.008	n.a.	n.a.	0.010
N	172,554	172,554	172,554	42,141	42,141	42,141

The equations identify selection based on the indicator variables for interview timing: February and March in the ORG and month in sample 1/5 in ASES. Response is coded 1 if there is response in both years and 0 otherwise. The “Δ equation” results are from a difference specification including changes in the two proxy variables; education; square, cubic and quartic experience; marital status; citizenship for foreign born; and industry and occupation changes; plus year dummies. Because industry and occupation definitions change in 2003, we omit 2002-2003 panel observation. The “Δ equation + level instruments” results add the variables in levels to the selection but not wage equation. The “levels regression” column provides results from our principal model (column 3 in Tables 4 and 5) using the first year of the panel sample.

Table 9: Composition of Cross Section and Panel Samples

	Cross Section		Panel	
Relationship	Percent	Impute	Percent	Impute
CPS ORG:				
Head/Co-Head	83.2%	0.308	87.7%	0.269
Child	7.0%	0.458	5.4%	0.453
Partner/Roommate	5.1%	0.349	3.5%	0.305
Other	4.8%	0.459	3.4%	0.424
CPS ASES:				
Head/Co-Head	84.8%	0.169	88.9%	0.157
Child	5.6%	0.326	4.6%	0.319
Partner/Roommate	4.3%	0.221	3.1%	0.190
Other	5.3%	0.298	3.4%	0.286

Measured is each worker’s relationship to the reference person for the household. We collapse the BLS classifications of relationship into four groups: head/co-head of household (primary individuals, heads with relatives, husbands and wives), children of heads, partners or roommates, and all other relationships.

Table 10: Selection Model Results from Head/Co-Head versus Full Samples

	Men		Women	
	Head/co-head	Full sample	Head/co-head	Full sample
CPS ORG:				
Inverse Mills ratio	-.095**	-0.166**	-.004	-0.034
Non-spouse proxy	0.005	-0.002	-.038**	-0.037**
Spouse proxy	0.003	0.008**	.006**	0.010**
CPS ASES:				
Inverse Mills ratio	-0.089	-0.276**	-0.086	-0.062
Non-spouse proxy	-0.008	0.00008	0.002	-0.024
Spouse proxy	0.003	0.012**	0.024**	0.023**

Full sample results were shown previously in Tables 4 and 5.
* significant at 5%
** significant at 1%

Appendix Table A-1: Weighted Means by Response Status, 1998-2008

	ORG Sample		ASES Sample	
Variables	Respondents	Non-respondents	Respondents	Non-respondents
Mean Age	39.50	40.56	40.56	41.03
Under age 25	0.127	0.122	0.074	0.885
Age 25-54	0.835	0.830	0.800	0.767
Age 55 and over	0.038	0.048	0.126	0.145
Elementary School	0.035	0.032	0.032	0.035
H.S. Dropout	0.065	0.065	0.060	0.067
H.S. Graduate	0.270	0.313	0.306	0.340
Some College	0.186	0.185	0.186	0.176
Associates Degree	0.096	0.092	0.099	0.088
Baccalaureate Degree	0.212	0.197	0.212	0.197
Master's Degree	0.079	0.064	0.077	0.067
Professional Degree	0.014	0.015	0.015	0.016
Ph.D.	0.014	0.011	0.014	0.012
Female	0.440	0.433	0.434	0.423
White	0.737	0.664	0.699	0.659
Black	0.101	0.153	0.110	0.148
Asian	0.039	0.049	0.042	0.047
Hispanic	0.137	0.123	0.136	0.134
Non-MSA	0.203	0.171	0.193	0.172
MSA under 250K	0.063	0.051	0.059	0.050
MSA 250K to 500K	0.092	0.085	0.090	0.082
MSA 500K to 1M	0.105	0.098	0.103	0.100
MSA 1M to 2.5M	0.187	0.179	0.186	0.173
MSA 2.5M to 5M	0.131	0.136	0.132	0.130
MSA 5M +	0.220	0.280	0.235	0.293
Northeast	0.046	0.054	0.046	0.062
Mid-Atlantic	0.120	0.166	0.131	0.165
East North Central	0.158	0.164	0.157	0.169
West North Central	0.078	0.055	0.074	0.054
South Atlantic	0.186	0.207	0.192	0.207
East South Central	0.058	0.061	0.059	0.057
West South Central	0.118	0.100	0.116	0.092
Mountain	0.075	0.049	0.070	0.048
Pacific	0.161	0.144	0.156	0.144
Proxy	0.452	0.593	0.471	0.610
February	0.087	0.074	n.a.	n.a.
March	0.089	0.072	n.a.	n.a.
MIS 1or5	n.a.	n.a.	0.258	0.229
Impute Industry	0.006	0.078	0.002	0.011
Impute Occupation	0.007	0.086	0.002	0.011
Impute Union Member	0.005	0.166	n.a.	n.a.
Hourly earnings (2008$)	21.59	n.a.	22.13	n.a.
Sample Size	1,008,718	490,912	459,378	105,344

Primary sample includes full-time wage and salary workers, age 18 to 65, not enrolled in school. Means are weighted. ORG response status based on combined use of weekly and hourly earnings.

Appendix Table A-2: Estimated Marginal Effects in Probit Response Model

	CPS ORG		CPS ASES	
	Male	Female	Male	Female
Elementary school	0.0456**	0.0548**	0.0140**	0.0170**
Grade 9	0.0518**	0.0455**	0.0180**	0.0193**
Grade 10	0.0376**	0.0261**	0.00668	0.00863
Grade 11	0.0319**	0.0269**	0.00221	0.0185**
Grade 11+ (no HS diploma)	0.00135	0.0017	-0.00792	-0.000116
Some college, no degree	0.0145**	0.0126**	0.00709**	0.0120**
Associates degree	0.0146**	0.0151**	0.00522*	0.0116**
Bachelor's degree	0.0121**	0.0165**	-0.00527*	0.00874**
Masters degree	0.0323**	0.0397**	-0.00971**	0.0117**
Professional degree	-0.0313**	0.0132*	-0.0539**	-0.00449
Ph.D.	0.0300**	0.0410**	-0.0213**	0.00509
Potential experience (age-schooling-6)	-0.00828**	-0.0101**	0.00571**	0.00272*
Experience-squared/100	0.0167*	0.0270**	-0.0545**	-0.0440**
Experience-cubed/10,000	-0.0112	0.0226	0.1430	0.1450
Experience-quartic/1,000,000	-0.0154	0.0000	-0.1280	-0.1570
Married	0.00504**	-0.00781**	-0.000565	-0.0212**
Previously married	0.0227**	0.0190**	0.0149**	0.0123**
Black	-0.0833**	-0.0952**	-0.0403**	-0.0487**
Asian	-0.0569**	-0.0637**	-0.0364**	-0.0451**
Other	0.0154**	-0.00674	-0.00656	-0.00124
Hispanic	0.0190**	0.0109**	0.00406	-0.00224
Foreign born, not citizen	0.0227**	0.0311**	0.00774*	0.0180**
Foreign born, citizen	-0.00932**	0.00222	0.00961**	0.0227**
Metro under 250M	0.00389	0.00906**	0.0144**	0.0147**
Metro 250K-500K	-0.0192**	-0.0192**	-0.00104	0.00212
Metro 500K-1M	-0.0283**	-0.0288**	-0.0175**	-0.0183**
Metro 1M-2.5M	-0.0265**	-0.0279**	-0.00605**	0.000582
Metro 2.5M - 5M	-0.0469**	-0.0460**	-0.0128**	-0.00882**
Metro 5M +	-0.0711**	-0.0725**	-0.0338**	-0.0310**
Mid Atlantic	-0.0391**	-0.0406**	-0.00657*	-0.00636
East North Central	-0.0031	-0.00395	-0.000848	-0.00348
West North Central	0.0729**	0.0734**	0.0448**	0.0416**
South Atlantic	-0.00628**	-0.00702**	0.00160	-0.000850
East South Central	-0.0194**	-0.0124**	0.00499	0.0112**
West South Central	0.0369**	0.0352**	0.0419**	0.0396**
Mountain	0.0902**	0.0857**	0.0617**	0.0607**
Pacific	0.0420**	0.0366**	0.0297**	0.0268**
Federal government	-0.0360**	-0.0320**	0.0234**	0.0327**
State government	-0.000436	-0.00976**	0.0246**	0.0118**
Local government	-0.00682*	0.00227	0.0212**	0.0220**
Union member	0.00814**	0.0346**	n.a.	n.a.
Sample Size	827,531	672,099	318,119	246,603

* significant at 5%; ** significant at 1%. Dependent variable = 1 if respondent. Industry, occupation, year dummies included. Included and shown in Table 3 are non-spouse proxy, spouse proxy, February and March (ORG), and month in sample 1 or 5 (ASES). Estimates unweighted; weighted results available from authors.