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# ABSTRACT

## The Relationship between Union Membership and Net Fiscal Impact*

This paper develops the first evidence on how individuals' union membership status affects their net fiscal impact, the difference between taxes they pay and cost of public benefits they receive, enriching our understanding of how labor relations interacts with public economics. Current Population Survey data between 1994 and 2015 in pooled cross-sections and individual first-difference models yield evidence that union membership has a positive net fiscal impact through the worker-level channels studied.

**JEL Classification:** J5, H24, J31

**Keywords:** labor union, taxes, public economic, labor relations, industrial relations, public benefits, collective bargaining, net fiscal impact, social insurance

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This paper offers the first evidence on whether union membership causes workers to use less public benefits and to pay more taxes. Prior work has neither tested nor measured effects on these outcomes, although findings of union wage and benefit premiums gives reason to expect this. The literature's evidence about effects of unionization on wages and benefits is insufficient to understand the effect of unionization on taxes paid or benefits received. First, higher hourly compensation might reduce hours and not increase earnings. Second, tax and public-benefit effects depend on interactions of workers' earnings with household characteristics and with tax and benefit policy. A union-induced 10 percent wage increase will have different tax and benefit implications for a worker earning near the poverty line versus one earning at the median, for a childless worker versus with one with 3 children, and for a worker in California versus Mississippi. For example, additional earnings will increase the amount of Earned Income Tax Credit (EITC) a worker collects if her base level of income and family structure are such that the EITC is phasing in but will decrease the EITC amount if the EITC is phasing out.

Through unionization, many workers raise their labor compensation in earnings and employer-provided fringe benefits. The positive effect of unionization on labor earnings is especially pronounced for workers who would otherwise have very low earnings. Frandsen (2012) follows workers after close union elections and finds that unionization strongly raises post-election earnings for workers who were below the 25$^{th}$ percentile of the pre-election earnings distribution but has no effect for workers who were at higher percentiles. Frandsen's focus on earnings, rather than wages, accounts for any reduction in hours induced by higher hourly compensation. He also follows workers even if they leave the establishment and counts earnings as zero if they do not earn from any employer, so this also accounts for any reductions in employment driven by unionization. Union membership also raises workers' likelihood of having private, employer-provided health insurance and other benefits (Buchmueller, DiNardo, & Valletta, 2002; Freeman & Medoff, 1984; Freeman R. B., 1981). Employer expenditures on fringe benefits are 2.5 times higher per hour worked for unionized jobs than for nonunion jobs and, as with earnings, the effects of unions on benefits appear larger in lower-paying establishments (Budd, 2005).

Political leaders, activists, and media have speculated that unionization may have a positive net fiscal impact on public balance sheets by both (1) reducing public-benefit use and (2) increasing tax payments by workers. Low-wage workers have been pushing for improvements in working conditions and for unionization through the OUR Walmart, Fast Food Forward, and the Fight for 15 campaigns, often criticizing nonunion employers' low pay and meager benefits for making working families reliant on public insurance programs. U.S. Representative-elect Alan Grayson spoke with Walmart workers in his community about their right to join a union, arguing that they are paid so little, "they often seek government programs for help." (Sanders, 2012). McDonald's central human-resources department points out to their employees that they may qualify for food stamps and Medicaid (Eidelson, 2013). This issue is not isolated to retail. For instance, Jacobs, Perla, Perry, & Squire (2016) find that a third of frontline manufacturing production workers are enrolled in at least one public safety net program and that this is primarily a result of low wages.

However, the question has not received direct or systematic attention from economists or other social scientists. Economists have understandably focused most of our attention on the effects of unions on wages, employment and hours, and labor and organizational productivity. While these are the first-order, narrowly-economic questions, we have ignored closely-connected questions of social, policy, and economic import. There is some work on labor earnings, the product of wages and hours, but little attention to other kinds of income or on contributions to and dependence on the public fisc. In a similar vein, Reich & West (2015) provide evidence that a change in employees' hourly compensation, spurred by increases in the minimum wage, reduced one form of public-benefit use, food stamp receipt. There has been extensive study of costs and benefits to the public fisc of numerous, other economic phenomenon. Immigration (Auerbach & Oreopoulos, 1999; Storesletten, 2000; Preston, 2014; National Academies of Sciences, Engineering, and Medicine, 2016; Blau, 1984) and early childhood investments (Council of Economic Advisers, 2015; Elango, Garcia, Heckman, & Hojman, 2015) are two prominent examples.

This paper aims to improve our understanding of how labor-relations processes interact with public finance and public policy broadly. We estimate the average annual net fiscal impact of union members to observably-similar non-member workers using data from the Current Population Survey over 1994 to 2015. We measure individual annual net fiscal impact (NFI), which is taxes paid (T) less the cost of public benefits received (B): NFI = T – B. Theory tells us that the key mechanism by which individual unionization would affect these variables is through raising private income among low earners. To add credibility and context, we estimate effects of union membership on private income as well. The analysis yields evidence strongly consistent with the theory that union membership raises private income, lowers public-benefit use, and increases taxes paid, yielding a positive net fiscal impact, and provides the first estimates of the magnitude of these relationships. Additional analysis explores sensitivity to issues arising from the possibility that union membership affects transitions out of employment and handling of covered nonmembers, imputed values, and weights. Looking beyond this paper's main focus on worker-level analysis, the conclusion offers interpretation given evidence on other channels by which union membership might affect NFI, such as by affecting labor productivity, profit, and public policy.

**Research Design**

We would ideally have an experiment among a representative sample of workers where some were randomly assigned to be union members and others to be nonunion. In that case, we could credibly interpret any observed union-nonunion differences in outcomes as causal effects of union membership. Unfortunately, randomization is not feasible. A regression-discontinuity design would also offer credible identification (DiNardo & Lee, 2004; Frandsen, 2012; Sojourner, Frandsen, Town, Grabowski, & Chen, 2013). However, this would require the ability to connect the population of individually-identified workers between the establishment where they worked during a NLRB unionization election and later, individually-identified measures of taxes paid and benefits received. This is also not feasible.

We generate evidence based on multiple regression with aggressive controls and first-difference models, which study within-worker changes in outcomes associated with changes in unionization status. These are not ideal but are the first and the best-available evidence. Freeman (1984) describes many relevant issues in the study of union effects using CPS data arising from measurement error in the observed union-status variable. In particular, he discusses plausible conditions under which the true effect of union membership is bounded above by the cross-sectional estimator and below by the individual first-difference estimator. We will interpret our results in this framework.

To get a nationally-representative sample, we use the Current Population Survey (CPS), which includes detailed data on all key variables (Flood, et al., 2015). The study period is 1994 to 2015, the longest over which the necessary variables are all available. Careful linking is required to maximize sample size conditional on the necessary variables. Specifically, we focus on the subsample who were given both the Annual Social and Economic Supplement (ASEC) and the Outgoing Rotation Group (ORG) survey. The ASEC contains income, tax, and benefit data, which are necessary as outcomes. The ORG measures union-membership status, necessary as the treatment of interest. Two sets of individual, longitudinal identifiers recently-produced by the Minnesota Population Center enable both linking of ASEC to ORG responses within year (MARBASECID, Pacas & Flood, 2016) and linking of ORG responses across consecutive years (CPSID, Drew et al. 2014).

We impose standard sample restrictions and show robustness to alternative sample construction choices. First, as is common in the study of union effects on wages, our primary sample screens in only non-student, employed, wage and salary workers age 18 or older.[1] Alternative estimates presented in robustness analysis are based on a sample including all adults, whether employed or not, and yield substantively similar results. Second, the sample includes only individuals present in the CPS surveyed household in both year *t* and *t*+1 to permit use of first-differences. Third, the primary analysis sample excludes all observations with imputed income or union-status values in line with Bollinger & Hirsch (2006) and Hokayem, Bollinger and Ziliak (2014) advice about how to reduce bias in this kind of setting. Robustness analysis shows how this exclusion affects estimates. The Appendix gives details on the linking process, sample construction, and the treatment of imputed values.

[1] Non-workers generally cannot belong to unions and plausibly have different unobserved characteristics than workers. If unionization impacts public balance sheets by reducing employment, our primary analysis will miss this channel.

The primary analysis sample is 120,953 individuals, each observed in two consecutive years. This includes 3,742 individuals (3.1%) moving from union in the first wave to non-union in the second, 3,986 (3.3%) moving from non-union to union, 14,185 (11.7%) who are union in both waves, and 99,040 (81.9%) who are non-union in both waves. We treat covered non-members as nonunion. If covered non-members are really closer to union members than to nonunion workers, this is a conservative assumption, because it diminishes the contrast between union and nonunion categories. The results are robust to alternative classifications of covered nonmembers.[2] All cross-sectional analysis uses each observation's sample weights. Longitudinal analysis gives each individual the average sample weight of its two observations. Dollar amounts are inflated to 2015 dollars.

**Outcomes**

The primary outcome of interest is individual annual *net fiscal impact* (NFI) on public balance sheets, defined as taxes paid less the cost of public benefits received.[3] The sample average (standard deviation) is $8,862 ($14,327) (Table 1), suggesting that workers pay an average of $8,862 more in tax liabilities than the value of  tax credits and public benefits collected. In the cross-section, union members average $11,505 in NFI and non-union workers average $8,399 implying a raw $3,106 or 37 percent difference that workers in unions contribute to the public purse over workers not in unions.

To measure *taxes paid* by each individual, we add up reported annual federal and state income tax liabilities before credits, property tax, Social Security, and federal retirement plan payroll deductions. Income from tax credits – Earned Income, Make Work Pay, Child, Child Care, and Stimulus – are also included in this sum but enter with negative sign. The sample mean (SD) is $10,290 ($13,030), with union members paying $2,757, or 28 percent, more than non-union workers on average. Federal income

[2] Table A.7 presents the results of our analysis when including covered non-members in the union category while Table A.8 presents the results when including covered non-members as their own category. The results show that including covered non-members with union members has a small effect on the results but still in line with the main results of the paper. Moreover, including the covered non-members as their own control has no virtually no effect on the main results.

[3] Taxes paid and benefits paid are winsorized to the 99th percentile to reduce the influence of extreme values, which may be due to measurement error. NFI is their difference. Results using unwinsorized outcomes are qualitatively similar.

tax and Social Security payroll deductions are the largest components. Appendix Table A.1 gives summary statistics on the detailed components with those entering the sum negatively denoted (-).

To measure the public cost of *public benefits received*, we add up the reported value of benefits received through various programs. Following Bitler and Hoynes (2016), we look at the private-market value of three major public benefits.[4] Namely these are Food Stamps (SNAP), welfare in the form of Temporary Assistance for Needy Families (TANF) (Aid to Families with Dependent Children or AFDC prior to welfare reform), and Unemployment Insurance (UI).We further take advantage of the full list of programs for which the Census Bureau collects data following Sherman et al. (2012). These other programs are smaller in magnitude and cover a smaller portion of the population. They include the private-market value of supplemental Social Security Income, Medicaid, and Medicare benefits, and of school-lunch, housing, home heating subsidies, post-secondary educational assistance, Social Security, workers compensation, veteran's benefits, and survivor's benefits.[5] These benefits average $1,427 annually. Union members report $349 or 24 percent less in public benefits than non-union workers.

Private income is the key mechanism by which unionization is theorized to affect taxes paid and public benefits received.[6] To measure *private income*, we sum income from alimony, farm income, non-farm business income, child support, dividends, interest, rent, retirement, wage and salary income wages, assistance from friends and relatives, and income from other sources. For homeowners, we also include the flow value of housing services so that "income" from both housing and other investments is captured. Focusing on private income, in general, rather than labor income, in particular, makes sense for two reasons. First, nonunion workers may compensate for lower hourly compensation at their primary

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[4] Bitler and Hoynes (2016) look at fourth major program: the Earned Income Tax Credit. We include this in *taxes paid*.

[5] Most of these tax and benefit-income variables are reported by the individual respondent about him or herself individually. However, some of the benefits are supplied at the family-level: public housing, Medicare, Medicaid, food stamps, school lunch, and home heating. To match the individual-level sample-selection criteria and unionization measure, we construct an individual-level measure for each of those benefits. We allocate the total family's cost of the benefit equally to all adults in the family.

[6] Unionization may affect public balance sheets through the political economy as well, by encouraging political support for higher tax rates and more expansive public benefit programs. This channel is largely outside the scope of the current analysis. The concluding discussion explores this more fully.

employer by devoting extra time to other income-generating strategies including self-employment. These could affect outcomes. It does not make sense to ignore the available information on these channels. Second, if union members enjoy a long-term flow of higher income, this might allow them to accumulate greater assets, which would return additional income in interest, dividends, and the value of housing services, all of which would affect outcomes. The sample average (SD) is $51,821 ($36,378) in annual private income, with union members reporting $10,113 or 20 percent more than non-union workers. By far, the largest component is wage and salary income with an overall average of $47,904 and union workers earning $7,817 or 17 percent more that nonunion workers on average.[7]

**Empirical Methodology**

To examine whether the raw mean differences by union status hold up in closer comparisons, we use mean regression analysis. The primary predictor of interest is an indicator of union membership. The excluded category is nonunion workers. Covered non-members, who work under a union contract without joining the union, are conservatively categorized as nonunion workers.

To isolate the relationship between outcomes and union status, we condition on other three types of observable determinants of the outcomes. First, we include a standard set of wage determinants ($\boldsymbol{X}$) following Bollinger & Hirsch (2006): potential experience in quartic form, indicators for educational attainment, marital status, race and ethnicity, sex, foreign-born, part-time work, size of metropolitan area, industry, occupation, employment by federal government, by state government, or by local government (private sector omitted). Second, we include measures of family structure ($\boldsymbol{F}$) because these govern tax liability and benefit eligibility. We condition on the number of adults in family, number of children aged birth to 5 in family, and number of children aged 6 to 18 in family. Third, individuals' tax liabilities and income from public benefits will also depend on states' current economic and policy conditions. These may also be correlated with the likelihood of union membership. To mitigate this possible sources of omitted-variable bias, we include state-year fixed effects ($\boldsymbol{1_s 1_t}$) in all of our models, ensuring that all

[7] Table A.1 contains summary statistics for each component of private income, public benefits, and taxes paid.

comparisons are made between individuals of different union status within the same state-year. Table 1 presents summary statistics of selected variables.

We estimate three specifications. The first specification is a pooled cross-section, regressing outcomes on an indicator for union membership, on individual wage determinant and family structure variables, and state-year fixed effects. This is the Bollinger & Hirsch specification augmented with family structure and state-year fixed effects:

$$y_{it} = \beta\, 1(union)_{it} + \gamma_1 \boldsymbol{F}_{it} + \gamma_2 \boldsymbol{X_{it}} + \mathbf{1_s 1_t} + \varepsilon_{it}. \tag{1}$$

In this specification, the identifying assumption is that, comparing across workers in the same state and year and controlling linearly for observed differences in standard wage determinants and family structure, the unobservable determinants of outcomes are not conditionally associated with union membership. $\beta$ measures the mean difference in outcomes between union workers and otherwise-similar non-union workers.

To tighten the comparison further, we relax the assumption that linear controls are adequate and construct a very large set of indicators for highly-interacted combinations of control variables. The first set of controls fully interacts the variables more-closely related to tax liability and benefit eligibility. Specifically, we fully interact number of kids 0-6, number of kids 6-18, total adults in family, marital status (6 categories: married spouse present, married spouse absent, separated, divorced, Widowed, and never married/single), sex, Hispanic origin, African American, Asian, Foreign-born status, state, and year. That is, we construct 101,249 cells representing all combinations of these variables and with indicators denoted $\boldsymbol{1(F)_{it}1_s1_t}$. In this specification, comparisons are only made between individuals in the same demographic cell-state-year. We also interact the wage-determinant variables with each other and denote this set of cell indicators as $\boldsymbol{1(W)_{it}}$. Specifically, we interact federal public sector, state public sector, local public sector, industry (13 categories), occupation (6 categories), part-time status, metropolitan size (7 categories), potential experience (in 5 year bins for a total of 10 groups), and education (4 categories: less than H.S., H.S or equivalent, some college or Associate's degree, and college degree or more) for a total of 26,545 more cells. Specification 2 is thus:

$$y_{it} = \beta\, 1(union)_{it} + \boldsymbol{\gamma_1} \mathbf{1}(\boldsymbol{F})_{it} \mathbf{1}_{\boldsymbol{s}} \mathbf{1}_{\boldsymbol{t}} + \boldsymbol{\gamma_2} \mathbf{1}(\boldsymbol{W})_{\boldsymbol{it}} + \varepsilon_{it} \tag{2}$$

The third specification recognizes that union and non-union workers may differ in unobservable ways correlated with unionization status and with NFI. In that case, these unobservables may not be credibly controlled for by cross-sectional comparisons, even with very flexible controls. To address this, we exploit the longitudinal nature of the data to estimate a specification with individual fixed effects. Ideally, this identifies the effect of unionization as the average change in NFI experienced by the workers who switch between union and nonunion status, conditional on other changes in observables such as educational attainment, family structure, and state-year. More importantly, it allows us to control for stable unobserved aspects of individuals by largely ignoring people who are always union or never union and focusing on changes in outcomes coincident with changes in unionization status *holding the worker fixed*. The identifying condition here is that changes in unionization status are not correlated with changes in outcomes conditional on changes in observables.

The nature of the outcomes studied here warrant a modification in the specification usually used to study union effects in longitudinal data. Hourly wages and weekly hours, the outcomes usually studied, adjust quickly when a person changes a job and, hence, union status. The timing of the switch within the year separating the two observations does not matter. However, the outcomes studied here are stocks across a year (annual taxes due or benefits received) and, so, the timing of change within the year matters. Though union status and conditioning variables are defined at two points in time twelve months apart, all outcomes are not defined at a point in time but with respect to the 12 months prior to that point in time. Consider two cases of a person who switches from non-union to union across the year but at different times during that year. If the person switches immediately after the first survey and stays union for the whole intervening year, then the estimated effect would be accurate. Outcomes from the first wave refer to the fully-nonunion year prior to the first observation and outcomes from the second wave refer to the fully-union year prior to the second observation. The difference in outcomes matches the difference in union status. However, if the person switched union status only immediately prior to the second survey, the person would really be non-union during both years reported in the outcomes. We would see the same measured change in union status in both cases but, in the second case, the measured effect would be zero because, for the purposes of outcomes, the person was nonunion in both waves. Assuming that the timing of switches is distributed uniformly across the year, switches occur halfway between the first and second survey on average. For this reason, a change in union status across a 12-month period represents an expected change for half the year. So, the estimated effect is half of the true effect.[8] Including a 0.5 constant in the specification corrects for this, effectively doubling the estimate that would otherwise be obtained and letting $\beta$ express the implicit effect of union status on *annual* outcomes. This issue does not arise with estimating wage effects because, like union status, wage is defined at a point in time.

Specification 3 gives the individual fixed-effect estimate:

$$\Delta_i(y_{it}) = \ (0.5)\beta\ \Delta_i(union_{it}) + \ \gamma_1\Delta_i(\boldsymbol{F}_{it}) + \gamma_2\Delta_i(\boldsymbol{X}_{it}) + \ \mathbf{1_s 1_t} + \ \Delta_i(\varepsilon_{it}). \tag{3}$$

**Results**

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[8] Ideally, we would measure the share of each year spent in each union status. Ignoring covered non-member status, suppose $s_t$ measures the share of year $t=1,2$ a person spends working union in year-$t$ and $Y_u$ is the instantaneous flow of an outcome for each moment spent in union status $u$. The union effect is $\beta \equiv Y_1 - Y_0$. An observed outcome is $Y_t = Y_0(1-s_t) + Y_1(s_t)$. Let $u_t$ measure union status at the end of year-$t$. Our fixed effects analysis relates $\Delta Y \equiv (Y_2 - Y_1)$ to the observable $\Delta u \equiv (u_2 - u_1) \in \{-1,0,1\}$ but $\Delta Y$ really depends on latent $\Delta s \equiv (s_2 - s_1) \in [-1,1]$. Given persistence in jobs, $\Delta u$ and $\Delta s$ should be positively correlated. To take a simple case, if there is no change in the year prior to the first observation ($s_1 = u_1 \in \{0,1\}$) and there is no more than a single change in $u$ over the intervening year, then the sign of $\Delta s$ equals the sign of $\Delta u$ but the magnitude of the change in treatment is overstated: $\Delta s \in [-1,0) \Leftrightarrow \Delta u = -1$, $\Delta s = 0 = \Delta u$, and $\Delta s \in (0,1] \Leftrightarrow \Delta u = 1$. An observed $\Delta Y$ generated by a given true change in treatment $\Delta s$ but is attributed to a change in measured treatment $\Delta u$ with larger magnitude. The estimated effect will be attenuated to zero. Suppose that the switch occurs at a random, uniformly-distributed time during the intervening year independent of $(Y_0, Y_1)$, $s \sim U[0,1]$. Conditional on a change, the average magnitude of change is $E|s_2 - s_1| = 0.5$, although $E|u_2 - u_1| = 1$. Then, $\hat{\beta} = E[\Delta Y/\Delta u] = E[\Delta Y/2\Delta s] = 0.5(E[\Delta Y/\Delta s]) = 0.5\ \beta$. Are the assumptions of this case plausible? Uniform $s$ is natural. The realism of the assumption that people make no more than one switch in status annually is difficult to evaluate. Just over 90% of individuals in the sample have the same status at the start and end of a year, consistent with a high degree of stability in status. Acknowledging that $s_1$ = 0.9 if $u_1$ = 1 and $s_1$ = 0.1 if $u_1$ = 0 would suggest amplifying the cross-sectional and longitudinal estimates by another 25 percent, as 1/(0.9-0.1).

We begin the regression analysis with NFI as the outcome. Specification 1 estimates that union membership is associated with a $1,290 increase in NFI (Table 2: Top panel: Column 1). The controls account for 42 percent of the $3,106 raw difference in union versus non-union sample means but 58 percent of the difference remains. In specification 2, which includes a much more flexible control set, the estimated association falls by less than 2 percent to $1,264. Though the standard error increases, from $92 in specification 1 to $138 in specification 2 due to the large fall in degrees of freedom from the flexible controls, the association remain significant at the 1 percent level. Specification 3 gives the individual fixed-effect estimate. The estimated effect of union membership on NFI here is $540, significant at the 5 percent level.

Next, the NFI result is decomposed between taxes paid and benefits received, as reported in the lower panels of Table 2. The logic of the analysis and the specifications used are the same. Only the outcomes differ. Union members pay about $1,200 more (approximate average for specifications 1 and 2) in taxes each year, according to the cross-sectional regressions. This result is stable and highly significant statistically across both cross-sectional specifications. The individual fixed effect analysis yields an estimated union-membership effect of $216 on annual taxes paid, though this is not statistically significant. Union members collect $102 less (average for specifications (1) and (2)) in public benefits than observably-similar nonunion workers though the results for Specification 2 is not statistically significant. In the panel, the estimated effect is larger: union membership reduces benefit received by $324 annually and this is significant at the 5-percent level. Whereas cross-sectional analysis suggests NFI effects are driven by more taxes paid, longitudinal analysis suggests a stronger role for reductions in benefits received.

The propensity to remain employed may differ by union-membership status, which could bias the primary analysis towards the results we found. Suppose union companies were more likely to go out of business than other companies and, so, throw a higher share of employees out of employment, onto public benefits, and into lower tax liabilities. These kinds of workers would fall out of our primary sample due to the sample-inclusion requirement that workers be employed in both periods. Unionization would, by this channel, have a negative impact on taxes paid, positive impact on benefits received, and negative impact on NFI but this channel would be hidden from our primary analysis. Here, we present relevant evidence to assess how this concern might affect results.

First, the premise of the concern is false. The premise of the concern is that union workers are more likely to transition of employment than nonunion workers. Contrary to the concern, transition out of employment – into unemployment, school, or idleness – is more likely for non-union workers than for union workers. Among nonunion workers in year *t*, 93.7 percent are employed in year *t+1*. Among union workers in *t*, 96.1 percent are employed in year *t+1*. Table A.2 gives transition probabilities of the full sample across all states.

Second, the estimated results strengthen, rather than weaken, when the employed-only restriction on the sample is dropped. We expand the sample so it includes all people older than 18 and estimate models that add indicators for unemployed, in school, and idle in addition to employed union, leaving employed nonunion as the omitted category (summary statistics for all outcomes and predictors by status are presented in Table A.3). The regression results, presented in Table 3, corroborate our main findings and are, in most cases, stronger. The estimated coefficient of union membership on NFI is about $1,534 in specification 2 and $976 in the individual fixed effect model, higher than the original sample (~$1,300 and $540, respectively). Estimated union effects on taxes paid are higher in this sample as well and here all are statistically significant at the 5 percent level. In the primary analysis sample, the individual fixed effect estimate was about $200 and not statistically significant. In this extended sample, the fixed effect estimate is nearly double (~$400). Estimated effects on benefits received are also nearly twice as much in this extended sample. The individual fixed effect estimate was about -$325 in the main sample but is -$565 in the extended sample. Finally, for private income, we see similar union premiums in our main and extended samples.

The effects for those not working follow expected patterns. As compared to non-union workers, those who are unemployed, idle, or in school have negative NFI, pay less in taxes, receive more public benefits and earn less in private income. These results are robust to all 3 specifications.

**Heterogeneous effects: sector and education**

Union membership may have different effects for public-sector workers than private-sector workers for various reasons. Union membership rates differ dramatically between the sectors. Union members now comprise about 7 percent of private-sector workers but about 38 percent of public-sector workers (Hirsch & MacPherson 2003).

To examine whether the relationships between union membership and outcomes are stronger in certain subgroups, we return to our primary sample (non-student, employed, wage and salary workers age 18 or older) and generalize specification 1 by interacting all of its coefficients with an indicator for public-sector. The effect of union membership is statistically different between public and private sector workers for all outcomes (Table 4: top panel). Union members are estimated to earn $1,769 more in private income than similar nonunion workers in the public sector. Among workers in the private sector, union members enjoy a much larger advantage, earning $6,192 more than similar non-union workers. The estimated difference in the union coefficient between sectors is a practically and statistically significant $4,223. Consistent with this, union membership has a much larger estimated effect on taxes paid in the private sector than the public sector. Somewhat surprisingly, the reduction in public-benefits received associated with union membership is larger in the public sector than the private sector. It is negative and statistically significant in each sector. Following the tax result, the positive estimated effect of union membership with NFI is larger in the private sector than the public sector.

We also look at different effects among workers with different education levels, in particular workers with at least a baccalaureate degree (BA) versus those without one. Theoretically, if union membership reduces public benefit use anywhere, it will be among those with lower wages. This is what we find. Rather than looking for heterogeneity by wage directly, which is endogenous to union membership, we proxy for propensity to earn low wages with lack of a BA. Among college graduates, union members have $2,724 more in private income than similar non-union workers. The union difference is $5,541 among those with no college degree. Union membership does not relate to public-benefits received among college grads but it does among those without a degree. The effects are also larger on taxes paid and NFI among those without a college degree.

**Displaced Worker Survey**

The Displaced Worker Survey (DWS) gives us another look at the issue where change is union status is credibly more exogenous than in the primary sample. This sample focuses only on individuals who report being displaced from a prior job as a result of a plant or firm closure in the prior three years. These individuals were recently forced out of a job through no fault or choice of their own. Their current outcomes, current employment and current union status, and union status at the job from which they were displaced, are observable. Prior outcomes are not observed. With only current outcomes measured, only a cross-sectional model can be estimated. Further, this sample is much smaller, containing only 2,823 workers. Despite these limitations, it offers a different cut at the problem.

$$y_{it} = \beta_1 1(union)_{it} + \beta_2 1(union\ in\ prior\ job)_{it} + \gamma_1 \boldsymbol{F}_{it} + \boldsymbol{\gamma_2} X_{it} + \mathbf{1_s 1_t} + \varepsilon_{it}. \tag{4}$$

This is similar to specification 1, except it adds a control for union status in a prior job, and the sample is limited to individuals who found a job in the prior 3 years after being laid off for reasons outside their control. The identifying assumption that unobservable influences are conditionally mean independent of current union status may be more credible here than in the main analysis because there is a control for past union status and the reason for leaving the past union status was outside of the worker's control. As Table 5 shows, the coefficients all have the same sign as in specification 1 of the main analysis and are all larger in magnitude. The sample size is almost 100 times smaller and the standard errors are much larger. Estimates on NFI, taxes paid, and private income are all still statistically significant but that on benefits received is not.

Additional robustness analysis is discussed in the appendix. We explore robustness to different ways of handing cases with imputed income and union membership, different ways of handing covered nonmembers, and using an unweighted sample.

**Conclusion**

The analysis provides the first and best-available evidence that union membership has a large, positive net fiscal impact at the individual-worker level. Union members appear to pay more every year in federal, state, and local taxes than do similar non-union workers, which is connected to the fact that they earn thousands more dollars in annual private income on average. Furthermore, union members appear to receive less in public benefits on average. Aggregating across NFI components and measuring NFI at the individual level, we observe that union members contributed on average \$1,300 more per year to the public balance sheet than similar non-union workers. The fixed-effect estimate is smaller in magnitude but points to the same substantive conclusion, union membership is estimated to cause an additional \$540 more per year in NFI. If one accepts the conditions laid out in Freeman (1984), an unbiased estimate lies between these two figures. This is the first analysis focusing on or quantifying this effect of unions. Though the prevalence of unionization is declining, this evidence suggests that nearly 15 million American union members are contributing an average of between \$540 and \$1,300 more annually to the public balance sheet than they would otherwise be. If the U.S. union membership rate stayed at its 1994 level of 17.4 percent, 8.4 million nonunion workers in 2015 would have been union members.

This worker-level analysis ignores other channels by which union membership might affect NFI. Nailing down the exact magnitude of the effects through these other channels is beyond the scope of this paper. However, available evidence allows discussion and approximation of some other, potentially-important channels. To achieve a full accounting of the net fiscal impact of unionization, one must understand from where the higher, private compensation of union members derives. Lee & Mas (2012) estimate that unionization reduces firm equity by 10 percent, implying a 10 percent reduction in the stream of future profits or stream of payments to equity owners. As Lee & Mas discuss, this 10 percent reduction is composed of two parts: a change in the overall size of the pie and a change in the way the pie is split. The former is the reduction in organizational productivity ($p$). The latter is the change in labor's share of surplus ($s$). A 10 percent reduction in profits is consistent with any combination such that $-p-s = -10$. Lee & Mas assume that unionization triggers an 8 percent wage premium for labor ($s$=8) and a negative 2 percent impact on productivity ($p$=2). However, their data is consistent with other ($p$,$s$) combinations. Consider the implications of these two channels separately.

For any given level of $p$, consider an increase in labor's share ($s$). Organizations are assemblages of workers and capital aimed at producing value. After consumer surplus is deducted and suppliers are paid, the enterprise's surplus must be divided among labor and capital. For a given level of productivity, unions shift the distribution of an organization's surplus towards workers and away from investors.[9] So, the overall net fiscal impact should account for the fact that each extra dollar in union members' earnings coming through this channel implies a dollar less in shareholder earnings. The question becomes what is the difference between the NFI of the marginal dollar in workers' pockets compared to the NFI of the marginal dollar in investors' and managers' pockets.

First, the effects of unionization on worker taxes paid and benefits received should be offset by changes in associated impacts among firm owners. For a back-of-the-envelope estimate, we turn to estimates of marginal effective tax rates and compliance rates. In this period, the marginal federal tax rate on capital income from large C-Corporations businesses was 35 percent. It seems reasonable to assume that the cost of public benefits used by shareholders will not be affected, as ownership of companies is concentrated among those unlikely to be on social safety programs. The average effective marginal federal tax rates on low- and moderate-income workers' income was 31 percent, including changes in both taxes paid and benefits received (U.S. Congressional Budget Office, 2016).[10] These effective marginal tax rates should be adjusted for differential noncompliance. Only 1 percent of labor income is lost to noncompliance, while approximately 10 percent of business and corporate income goes untaxed due to noncompliance (U.S. Internal Revenue Service, 2012). Our estimates derived from the microdata over the study period are very consistent with this estimate of 31 percent from external sources. In the fixed effect estimates (Table 2: specification 3), unionization caused a $1,614 increase in private income

[9] Our analysis accounts for effects of unionization via differences in the distribution of wage and salary income among employees.

[10] Frandsen (2012) finds little effect of unionization on earnings above the 20th percentile of earnings.

and a $540 increase in NFI, suggesting a 33 percent effective marginal rate. The cross-sectional estimate in specification 2 suggests a 28 percent rate. In the period studies, marginal tax federal revenue was approximately equal from both sources: marginal revenue from labor income = $0.99*0.31 = 0.307 \approx 0.90*0.350 = 0.315$ = marginal revenue from capital income. From these calculations, to the extent that unionization affected only income distribution within the firm, the net fiscal impact on workers of unionization appears approximately fully offset by reduced taxes paid by firm owners. The pie-splitting channel appears to have been a wash. The recent reduction in corporate tax rates change the interpretation going forward. Now, if union membership shifts resources from capital to labor, it will also shift income to a higher rate, raising taxes paid, and increasing the positive net fiscal impact.

By this pie-splitting channel, unions also appear to reduce Americans' reliance on the social safety net by shifting resources earned in the private economy from owners to workers. Unions help make work pay by raising lower-paid workers' private income, reducing their use of public benefits and increasing their contribution of taxes to the public fisc. While this may come at the expense of income to firm owners and investors, their self-sufficiency is likely much less impacted.

Now, hold labor share fixed and consider the case where unionization changes productivity. Unionization may cause some *ceteris paribus* boost to labor productivity (Freeman & Medoff, 1984; Sojourner, Frandsen, Town, Grabowski, & Chen, 2013). On the other hand, if it lowered productivity, this would generate real economic cost with negative fiscal impact through many channels. Changes in on-the-job productivity are only partly reflected in the analysis. Changes in productivity that affect workers' earnings holding employer fixed are reflected.

Our comparisons between similar individuals in the same state-year considers only channels involving labor-management bargaining that changes the creation and distribution of value within organizations. However, unions have fiscal impacts through policy channels as well. For instance, organized labor often advocates for larger public budgets, higher tax rates on higher-income individuals and corporations, and more generous social safety nets. In addition to influence exerted through political action, working-class legislators have different policy preferences than other legislators (Carnes, 2012;

Carnes, 2013) and unionization increases the likelihood of working people holding elected office (Sojourner, 2013). Brady, Baker, & Finnigan (2013) provide that states with higher levels of unionization have more generous public-benefit programs for the working poor and lower rates of working poor.

Decreased unionization over recent decades has shifted resources away from workers, especially workers with lower earning power. This has likely decreased their tax payments and increased their reliance on public benefits. Put another way, as unionization erodes, working families' ability to stay clear of the public safety net erodes. Going beyond the effects of unionization in the firm or the labor market, these results enrich our broader view of how labor-relations processes interact with public economics and public policy.

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## Tables

*Table 1 – Summary statistics for longitudinally-linked sample*

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| *Outcomes* | | | | |
| Net fiscal impact | $8,862.42 | $14,326.50 | -$46,140.16 | $71,915.36 |
| Taxes paid | $10,289.70 | $13,030.14 | -$9,295.38 | $71,915.36 |
| Benefits received | $1,427.28 | $5,477.84 | $0 | $38,212.23 |
| Private income | $51,821.23 | $36,378.42 | -$17,434.94 | $206,800.70 |
| *Treatment* | | | | |
| 1(union member) | 0.15 | | 0.00 | 1.00 |
| *Selected demographics* | | | | |
| Number adults in family | 2.08 | 0.91 | 1.00 | 12.00 |
| Number of children 0-5 | 0.26 | 0.58 | 0.00 | 5.00 |
| Number of children 6-18 | 0.60 | 0.93 | 0.00 | 11.00 |
| Potential experience, years | 22.81 | 11.97 | 0.00 | 76.50 |
| Percent Married | 63.6% | | 0.00 | 1.00 |
| Percent H.S. Degree or Equiv. | 32.5% | | 0.00 | 1.00 |
| Percent College Degree or More | 29.1% | | 0.00 | 1.00 |
| Part-Time Worker | 12.3% | | 0.00 | 1.00 |
| Public Sector - Federal | 3.1% | | 0.00 | 1.00 |
| Public Sector - Local | 10.8% | | 0.00 | 1.00 |
| Public Sector - State | 5.5% | | 0.00 | 1.00 |

Source: CPS-ASEC 1994-2015. Notes: a) set of demographic controls also includes indicators of gender (2), race-ethnicity (4), foreign-born, metropolitan size (7), industry (13), occupation (7). Sample includes 241,906 observations of 120,953 individuals employed over 2 consecutive years each without missing variables or imputed union status. b) Controls for marital status include 6 groups and educational status include 4 groups. c) All means are weighted using sample weights and all dollar amounts are inflated to 2015 dollars. Taxes paid, benefits received, and private income are winsorized to their 99th percentile. NFI is the difference between winsorized taxes paid and benefits received.

*Table 2 – Estimates of conditional association of union-membership on four outcomes using longitudinally-matched observations and various sets of conditioning variables*

| Specification: | 1 | 2 | 3 |
|---|---|---|---|
| | Outcome: net fiscal impact | | |
| 1(union member) | 1289.8*** | 1264.3*** | 540.0** |
| | (91.6) | (138.1) | (254.4) |
| | Outcome: taxes paid | | |
| 1(union member) | 1108.6*** | 1240.2*** | 216.3 |
| | (85.6) | (129.7) | (208.5) |
| | Outcome: public benefits received | | |
| 1(union member) | -181.3*** | -24.2 | -323.7** |
| | (35.0) | (47.1) | (144.9) |
| | Outcome: private income earned | | |
| 1(union member) | 4661.6 *** | 4588*** | 1614.0*** |
| | (205.3) | (302.4) | (575.1) |
| Demographics | Yes | Yes | Yes |
| State-year FE | Yes | Yes | Yes |
| Individual FE | | | Yes |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 241,906 observations of 120.953 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.

*Table 3 – Results including Idle, Unemployed and In School*

| Specification: | 1 | 2 | 3 |
|---|---|---|---|
| | | Outcome: net fiscal impact | |
| 1(union member) | 2006.6*** | 1534.1*** | 976.4*** |
| | (108.8) | (134.5) | (292.4) |
| 1(unemployed) | -5533.2*** | -5935.9*** | -745.8** |
| | (189.8) | (245.5) | (333.6) |
| 1(idle) | -6373.6*** | -4338.5*** | -1155.2*** |
| | (192.8) | (237.1) | (397.8) |
| 1(in high school or college) | -469.2** | -3831.6*** | -928.2* |
| | (230.1) | (307.9) | (524.8) |
| | | Outcome: taxes paid | |
| 1(union member) | 1513.9*** | 1326.1*** | 411.8** |
| | (83.5) | (108.3) | (187.8) |
| 1(unemployed) | -3282.7*** | -3586.4*** | -413.6** |
| | (106.3) | (156.1) | (196.8) |
| 1(idle) | -2473.3*** | -1876.7*** | -214.6 |
| | (110.8) | (174.8) | (219.8) |
| 1(in high school or college) | -1026.6*** | -2409.7*** | -998.0*** |
| | (122.9) | (180.9) | (219.6) |
| | | Outcome: public benefits received | |
| 1(union member) | -492.7*** | -208.0*** | -564.6** |
| | (62.9) | (70.9) | (220.2) |
| 1(unemployed) | 2250.5*** | 2349.5*** | 332.2 |
| | (131.8) | (169.7) | (261.4) |
| 1(idle) | 3900.3*** | 2461.9*** | 940.6*** |
| | (145.6) | (164.0) | (335.0) |

| | | | |
|---|---|---|---|
| 1(in high school or college) | -557.4*** | 1421.9*** | -69.8 |
| | (179.1) | (249.9) | (480.8) |
| | | Outcome: private income earned | |
| 1(union member) | 6541.9*** | 5571.0*** | 2094.4*** |
| | (198.1) | (244.6) | (534.8) |
| 1(unemployed) | -17644.6*** | -18258.9*** | -2638.0*** |
| | (403.3) | (540.5) | (625.2) |
| 1(idle) | -14093.6*** | -10858.0*** | -2130.4*** |
| | (338.8) | (474.8) | (711.6) |
| 1(in high school or college) | -7514.4*** | -14207.1*** | -7112.6*** |
| | (385.3) | (488.0) | (762.4) |
| Demographics | Yes | Yes | Yes |
| State-year FE | Yes | Yes | Yes |
| Individual FE | | | Yes |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 486,964 observations of 243,482 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.

*Table 4 – Estimated coefficients by selected subsamples for Specification 1*

| Outcome: | Net Fiscal Impact | | Taxes Paid | | Benefits Received | | Private Income | |
|---|---|---|---|---|---|---|---|---|
| | Public | Private | Public | Private | Public | Private | Public | Private |
| 1(union member) | 510.7*** | 1717.1*** | 210.7 | 1619.6*** | -300.0*** | -97.5** | 1769.4*** | 6192.0*** |
| | (157.9) | (114.1) | (148.9) | (104.2) | (56.4) | (43.4) | (323.2) | (292.1) |
| Difference | -1206.5*** | | -1408.9*** | | -202.5*** | | -4422.5*** | |
| N | 49,403 | 192,503 | 49,403 | 192,503 | 49,403 | 192,503 | 49,403 | 192,503 |
| | BA+ | No BA | BA+ | No BA | BA+ | No BA | BA+ | No BA |
| 1(union member) | 741.4*** | 1575.5*** | 713.8*** | 1358.7*** | -27.7 | -216.8*** | 2723.6*** | 5541.1*** |
| | (257.6) | (98.2) | (244.2) | (90.8) | (90.1) | (38.6) | (479.9) | (238.0) |
| Difference | -834.1*** | | -645.0** | | 189.1* | | -2817.5*** | |
| N | 47,283 | 194,623 | 47,283 | 194,623 | 47,283 | 194,623 | 47,283 | 194,623 |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 486,964 observations of 243,482 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{subsample} regression model using specification 1 from Table 2 with all predictors interacted a subsample indicator. d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.

*Table 5 – Estimates from the Displaced Worker Survey*

| Specification: | Net Fiscal Impact | Taxes Paid | Benefits Received | Private Income |
|---|---|---|---|---|
| Current Union Member | 2939.4** (1343.1) | 2646.5** (1208.2) | -293.0 (575.2) | 8028.6** (3119.2) |
| Prior Union Member | 701.5 (1194.4) | 448.4 (1056.7) | -253.1 (541.7) | 1744.1 (2637.7) |
| N | | 2,823 | | |

Source: CPS-ASEC 1994-2015 DWS. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 2,823 individuals in 1 period. c) Specification as in specification 1 of Table 2 with addition of 1(prior union member). d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.

## Appendix

### Additional Robustness Analysis

Several robustness checks were conducted for this analysis, particularly focused on the intricacies of the sample construction. As Appendix Table A.4 shows, the final sample used in this paper is a result of various sample restrictions which are detailed below.

Understanding the sampling methodology of the CPS is key to understanding our sample. The CPS is a monthly survey designed to collect data primarily on employment; the Basic Monthly CPS's are the source of the official unemployment statistics. The Basic Monthly CPS consists of about 60,000 dwellings. Each dwelling is selected to be in the CPS for 4 consecutive months, then out of the CPS rotation for 8 months, and then back again for 4 more months. Each of these months is referred to as a Month-In-Sample (MIS) for a total of 8 MIS's for any given dwelling. The ORG questions refer to a survey that is given only to dwellings from MIS groups 4 or 8 (i.e. these are the months after which these dwellings will either be out of sample for 8 months or out of the CPS entirely). The questions encompassed in the ORGs focus on more specific labor questions, most important to our study is the union membership question. The union membership question is thus asked only of one-fourth of any given Basic Monthly CPS.

Every March, CPS administers the Annual Social and Economic Supplement (ASEC) to all dwellings in the March Basic CPS. Among the questions asked are detailed breakdowns of annual income sources and social program benefit receipt. In order to conduct our analysis, it is necessary for us to link the March Basic CPS to the ASEC which is a more tedious ordeal in practice. We use the newly created identifiers of the Minnesota Population Center (MARBASECID) for this purpose; the exact algorithm and more detailed explanation of the CPS sampling methodology is included in Flood and Pacas (2016).

Nonresponse for various CPS variables are a potential source of bias for our analysis, as evidenced by the growing literature on imputations in CPS. Our general approach for dealing with nonresponse is to drop cases with nonresponse and then conduct robustness checks. More specifically, when a respondent refuses to respond to a particular survey question, rather than leaving the field blank, the Census Bureau allocates a value from a donor set comprised of respondents from that same sample. The process by which the allocation is conducted is known as the hot-deck imputation procedure and, in essence, takes a nonrespondent and matches based on a set of measured attributes. For earnings items, this set broadly consists of age, sex, race, employment status, and industry/occupation. As early as 1986, Lillard, Smith and Welch investigated the Census Bureau's approach to dealing with missing data and pointed out that the hot-deck procedure for imputing income likely affects results regarding income and earnings. More recently, Hirsch and Schumacher (2004, 2006) have shown that "coefficient bias resulting from imputation of a dependent variable (earnings) can be of first-order importance."

In our analysis, item nonresponse on earnings, union-status, and all sources of income raise familiar issues. The Census imputes values for missing data. However, relying on these imputed values has been shown to introduce bias in analysis like ours. Bollinger and Hirsch (2004) showed that using imputed earnings as an outcome, "if the attribute under study is not used as a census match criterion in selecting a donor, wage differential estimates (with or without controls) are biased towards zero." More importantly, "this bias is large and exists independent of any from the nonrandom determination of missing earnings" (p. 691). Bollinger and Hirsch (2004) estimated attenuation bias from missing union-status data to be about 5 percentage points for estimates between 1999 and 2001. The prevalence of imputations has only increased since 2001. We drop observations with imputed union status to reduce the attenuation bias introduced by the imputation itself. About 6 percent of the full sample have imputed union status. Secondly, the hot-deck procedure used for imputing earnings leads to attenuation that is roughly the size of the imputation rate (Bollinger & Hirsch, 2006). As they suggest, "the simplest approach to account for match bias is to omit imputed earners from wage equation (and other) analyses" (p. 517). Following their recommendation, we also drop individuals with any imputed earnings, who are about 45 percent of the sample.[11] Third, for respondents who answer the March Basic CPS but refuse to

[11] The analyses of Bollinger and Hirsch (2004, 2006) focuses primarily on a single earning variable from the ORG files while we focus on a larger set of variables from the CPS-ASEC (listed in Table A.9). But the imputation

answer the longer ASEC questions, the Census Bureau performs a “full-line” impute for these cases, imputing answers to every income question. In other words, there are respondents in the March Basic for whom there is not enough income data collected. Rather than leaving these cases as non-responses, the Census Bureau uses a hot-deck procedure to impute the values of the missing income data (Stewart, 2002). Hokayem, Bollinger and Ziliak (2014) analyzes the role of nonresponse for the CPS-ASEC including the full-line impute and find evidence of bias from non-response. More importantly, in a longitudinal framework, dropping respondents with full-line imputes is preferred. In effect, comparing a full-line impute in one time period to an actual response in the second time period introduces unnecessary measurement error. We drop respondents with full-line imputes, who are about 14 percent of the sample. These three categories are not mutually exclusive. Dropping all observations with any type of imputation means dropping just over half (57 percent) of our otherwise-eligible sample. Table A.4 breaks down the resulting sample sizes from each imputation restriction imposed here.[12]

A different form of measurement error arises from any inaccurate reports of union status. While we present specifications for a balanced, pooled cross-sectional model, we also use an individual fixed-effect model. Doing so allows us to make a better causal claim of the effect of unionization. Indeed, Freeman (1984) points out two relevant facts: (1) there is substantial measurement error in reported union status and (2) this can bias down estimates based on individual fixed effects. This measurement error comes from inaccurate responses, rather than the nonresponse discussed above. Freeman further argues that cross-sectional estimates can be interpreted as an upper bound on the causal effect of unionization, due to likely positive omitted-variable bias. That is, given high union wages, it is typically assumed that firms selected workers with higher unobserved ability which cannot be controlled for in a standard cross-sectional setup. Furthermore, the fixed effect estimate can be interpreted as a lower bound due to attenuation caused by the union-status measurement error.

method is nearly the same for both surveys and, more importantly, imputation is more pronounced in the CPS-ASEC. These facts further warrant dropping the observations with imputed earnings and union status.

[12] We also look at the effect of dropping free/reduced price lunch and housing subsidies from our analysis. We find that there is no significant effect of doing so our final model includes these benefits. Results not presented.

Table A.5 displays estimated coefficients on each of 4 outcomes under each of the three specifications after including only one kind of imputation at a time, then, in the final row, including all of them. The cross-sectional results of specifications 1 and 2 are quite stable. Fixed effect estimates are qualitatively stable except for inclusion of full-line imputes and inclusion of all imputes. These findings echo Bollinger & Hirsch (2004, 2006). Imputations attenuate the union coefficient towards zero.

**Taxes**

All tax variables are imputed in the CPS-ASEC using a Census Bureau created tax model. These variables include federal and state taxes, local property taxes, payments to social security and federal retirement. Also included are different credits such as the earned income tax credit, the child and additional child tax credit, and the Making Work Pay stimulus of 2009-2010 and the federal stimulus payments of 2008. The general approach the Census Bureau uses for imputing taxes is to statistically match CPS tax units to a Statistics of Income (SOI) public use file from the IRS (CPS tax documentation). State and local taxes follow a similar procedure but includes different parameters as is relevant to specific state tax laws.

Wheaton and Stevens (2016) review different methods for calculating taxes in the CPS-ASEC and find that, on average, the Census Bureau's method produces roughly the same results as those using other tax models. However, no research has looked at whether the choice of tax model results in different results across union membership status. Future research would benefit from looking at the potential bias of tax models across different subgroups.

**Weights**

We check the effect of weights on our results. Table A.6 presents estimates that do not use weights. Results are largely stable.

**Is higher private income the channel?**

Presumably, union members pay more taxes and collect less public benefits because they have higher incomes from private sources. Do we see evidence of this hypothesized channel in the data? In the cross-sectional analysis, union members earn about $4,625 (average for specifications (1) and (2)) more than nonunion workers. In the longitudinal analysis, the estimated union effect on income is $1,614. For this outcome, the fixed effect estimate is statistically significant at 1 percent, despite attenuation issues. Full estimates for all these models are not reported.

Because increased private income is the primary channel through which union membership increases tax payments and reduces public benefit receipt, our specifications for taxes, public benefits and NFI exclude income as a control. Including it would overcontrol (Wooldridge, 2005). This theorized channel provides a testable implication. If private income is indeed the channel by which taxes are increased and benefits reduced for union members, then the coefficient on union membership should decrease substantially when private income is included in the regression.

This is largely the case for NFI and income, but much less so for public-benefit receipt. In all specifications and for all outcomes, adding private income linearly reduces the magnitude of the estimated union effect. For all specifications with NFI and taxes paid, the union coefficient with linear income is not significant. This can be seen by comparing the first two rows of estimates in the first six columns of Table A.10. Because taxes are not linear functions of income, we go on to add higher-order polynomial terms of private income to allow more flexibility in the relationship. These are reported in the table's lower panels. Although the cross-sectional specifications yield statistically-significant estimates, estimates remain much smaller than in the specifications excluding income. For instance, adding a quartic function of private income to specification 1 makes the estimated union effect coefficient on NFI fall from $1289.80 to $241.90, a reduction of two-thirds in magnitude. In all fixed effect models of NFI and taxes paid, union membership is not significant once any function of private income is included. The NFI results follow the tax results and largely confirm the theory. Differences in private income associated with union membership largely explain the association between union membership and NFI observed when excluding private income.

The results for benefits are different. The specification-1 estimates drop in magnitude and significance, consistent with the theory. However, estimates in specifications 2 and 3 do not change substantively with the addition of private income terms, providing some evidence against the theory.

Alternatively, it may be a spurious result driven by a long lag in public-benefit changes as income changes. Certainly, individuals have incentives to reduce tax liabilities immediately but to delay loss of benefits as long as possible.

*Table A.1: Summary statistics for variables and underlying components in primary sample, union subsample, and non-union subsample*

| Sample: | All | Union | Non-union |
|---|---|---|---|
| *Net fiscal impact (winsorized)* | 8862.4 | 11505 | 8398.5 |
| | (14326.5) | (13304.0) | (14448.8) |
| *Net fiscal impact (unwinsorized)* | 7967.5 | 10566.3 | 7511.3 |
| | (27112.6) | (25953.7) | (27285.5) |
| | | | |
| *Taxes paid (winsorized)* | 10289.7 | 12635.2 | 9877.9 |
| | (13030.1) | (12358.8) | (13101.2) |
| *Taxes paid (unwinsorized)* | 10918.9 | 13105.9 | 10534.9 |
| | (18600.8) | (18103.9) | (18660.3) |
| Federal income tax liability before credits | 5540.3 | 6446.4 | 5381.2 |
| | (13378.7) | (13357.5) | (13376.1) |
| State income tax liability before credits | 1605.1 | 2051.3 | 1526.8 |
| | (3918.9) | (3831.6) | (3928.8) |
| Annual property taxes | 1003.6 | 1251.9 | 960.1 |
| | (2138.0) | (2208.8) | (2122.4) |
| Social security retirement payroll deduction | 3155.7 | 3451.6 | 3103.7 |
| | (2353.1) | (2290.4) | (2360.1) |
| Federal retirement payroll deduction | 141.6 | 315.3 | 111.1 |
| | (1036.5) | (1333.0) | (972.0) |
| Earned income tax credit (-) | 235.6 | 115.1 | 256.8 |
| | (888.9) | (603.9) | (928.4) |
| Additional child tax credit (-) | 53.66 | 29.44 | 57.91 |
| | (329.3) | (251.4) | (340.9) |
| Child tax credit (-) | 154.3 | 183 | 149.2 |
| | (549.1) | (608.9) | (537.7) |
| Credit received from making work pay (-) | 45.14 | 43.68 | 45.40 |
| | (170.6) | (170.0) | (170.7) |
| Federal stimulus payment (-) | 38.73 | 39.37 | 38.62 |
| | (221.4) | (230.0) | (219.9) |
| | | | |
| *Income from Public Benefits (winsorized)* | 1427.3 | 1130.2 | 1479.4 |
| | (5477.8) | (4922.3) | (5568.0) |
| *Income from Public Benefits (unwinsorized)* | 2951.4 | 2539.7 | 3023.7 |
| | (19710.7) | (18851.9) | (19856.8) |
| Supplemental Security Income (SSI) | 18.27 | 11.64 | 19.44 |
| | (421.1) | (369.6) | (429.5) |
| Welfare (public assistance) | 14 | 5.99 | 15.40 |
| | (343.3) | (205.4) | (362.2) |
| Person market value of Medicare | 1029.3 | 895.8 | 1052.8 |
| | (9588.6) | (9169.3) | (9660.1) |
| Person market value of Medicaid | 1028.8 | 891.4 | 1053.0 |
| | (9580.6) | (9166.9) | (9651.2) |
| Person value of food stamps | 63.97 | 24.97 | 70.82 |

| | | | |
|---|---|---|---|
| | (464.0) | (264.4) | (490.4) |
| Person value of housing subsidy | 2.39 | 1.627 | 2.524 |
| | (22.6) | (17.6) | (23.36) |
| Person value of school-lunch subsidy | 57.08 | 41.22 | 59.87 |
| | (184.8) | (145.6) | (190.7) |
| Person value of energy subsidy | 2.863 | 1.829 | 3.044 |
| | (40.8) | (31.5) | (42.21) |
| Educational assistance (beyond HS) | 80.2 | 69.27 | 82.12 |
| | (1014.2) | (890.5) | (1034.4) |
| Social security | 343.9 | 166.6 | 375.1 |
| | (2389.9) | (1745.2) | (2484.6) |
| Unemployment benefits | 104 | 135.7 | 98.39 |
| | (1071.3) | (1183.5) | (1050.3) |
| Worker's compensation | 35.47 | 92.28 | 25.49 |
| | (788.3) | (1345.0) | (642.0) |
| Veteran's benefits | 63.25 | 69.88 | 62.08 |
| | (1258.6) | (1171.7) | (1273.2) |
| Disability benefits | 23.1 | 35.31 | 20.96 |
| | (931.2) | (1265.4) | (859.2) |
| Survivor's benefits | 84.73 | 96.21 | 82.71 |
| | (2393.2) | (2514.9) | (2371.2) |
| | | | |
| *Private Income (winsorized)* | 51821.2 | 60338.7 | 50325.8 |
| | (36378.4) | (30772.9) | (37074.4) |
| *Private Income (unwinsorized)* | 53593.0 | 61093.9 | 52276.1 |
| | (50134.6) | (38396.3) | (51810.5) |
| Alimony | 18.99 | 12.81 | 20.07 |
| | (666.0) | (484.2) | (693.0) |
| Non-farm business income | 144.8 | 128.2 | 147.7 |
| | (3716.4) | (3146.2) | (3807.8) |
| Child support | 164.7 | 154.8 | 166.4 |
| | (1406.7) | (1356.1) | (1415.3) |
| Dividends | 278.4 | 235.9 | 285.9 |
| | (2555.9) | (2011.9) | (2639.8) |
| Farm | 20.98 | 19.69 | 21.20 |
| | (1296.3) | (1428.0) | (1271.8) |
| Interest | 385.3 | 400.9 | 382.5 |
| | (2714.9) | (2400.2) | (2766.5) |
| Income from other source not specified | 21.23 | 25.3 | 20.52 |
| | (706.7) | (614.5) | (721.7) |
| Rent | 188.2 | 223.1 | 182.1 |
| | (3103.7) | (3499.0) | (3028.9) |
| Retirement | 395.2 | 287.8 | 414.1 |
| | (3873.7) | (3179.9) | (3982.8) |
| Wage and salary income | 47903.7 | 54553.7 | 46736.1 |
| | (47113.7) | (35829.1) | (48733.1) |

| | | | |
|---|---|---|---|
| Assistance from friends/relatives not in HH | 23.69 | 20.96 | 24.17 |
| | (720.8) | (622.0) | (736.8) |
| Implied value of owner-occupied housing | 4047.9 | 5030.9 | 3875.3 |
| | (5386.7) | (5988.4) | (5255.0) |
| *Observations (individual-year)* | 241,906 | 36,098 | 205,808 |

Note: All means are weighted using sample weights and all dollar amounts are inflated to 2015 dollars. Standard deviations in parentheses. Taxes paid, benefits received, and private income are winsorized to their 99th percentile. NFI is the difference between winsorized taxes paid and benefits received.

*Table A.2 - Transitions including Unemployed, Idle, and In School*

| Status in t+1: | Non-Union | Union | Unempl. | Idle | School |
|---|---|---|---|---|---|
| Status in t: | | | | | |
| Non-union | 90.5 | 3.2 | 2.3 | 3.4 | 0.6 |
| Union | 21.2 | 74.9 | 1.5 | 2.4 | 0.1 |
| Unempl. | 44.1 | 3.8 | 29.6 | 20.7 | 1.8 |
| Idle | 4.9 | 0.3 | 1.3 | 92.9 | 0.5 |
| In School | 24.7 | 0.9 | 4.1 | 11.9 | 58.5 |

Source: CPS-ASEC 1994-2015. Note: Row percentages are calculated using sample weights.

*Table A.3: Summary statistics for variables and underlying components in extended sample and various subsamples*

| Sample: | All | Union | Non-union | Unemployed | Idle | In School |
|---|---|---|---|---|---|---|
| *Net fiscal impact (winsorized)* | 1120.7 | 10833.7 | 7670.5 | -686.8 | -11856.7 | -3497.4 |
| | (18087.5) | (14324.0) | (15857.3) | (13264.0) | (15746.9) | (10150.3) |
| *Net fiscal impact (unwinsorized)* | 384.2 | 10231.7 | 7330.0 | -1522.6 | -13233.9 | -5023.3 |
| | (29131.2) | (26657.9) | (28638.5) | (23438.1) | (26167.9) | (23159.5) |
| | | | | | | |
| *Taxes paid (winsorized)* | 7184.0 | 12368.6 | 9721.4 | 4134.3 | 2370.7 | 308.4 |
| | (11877.1) | (12196.8) | (13295.8) | (8856.6) | (6981.0) | (2570.7) |
| *Taxes paid (unwinsorized)* | 7791.9 | 12911.4 | 10628.6 | 4418.0 | 2553.4 | 357.6 |
| | (17526.3) | (18761.4) | (20331.2) | (12756.0) | (9815.6) | (4650.6) |
| Federal income tax liability before credits | 4002.8 | 6366.3 | 5427.1 | 2260.8 | 1431.3 | 203.7 |
| | (12426.1) | (14002.0) | (14436.7) | (8945.3) | (7484.5) | (4197.4) |
| State income tax liability before credits | 1142.9 | 2018.2 | 1534.3 | 713.4 | 381.2 | 47.29 |
| | (3741.8) | (3880.8) | (4425.9) | (2531.7) | (2179.4) | (566.5) |
| Annual property taxes | 921.7 | 1237.7 | 966.8 | 753.2 | 843.9 | 53.35 |
| | (2104.0) | (2176.2) | (2099.2) | (1909.4) | (2157.3) | (532.9) |
| Social security retirement payroll deduction | 2059.2 | 3397.6 | 3145.6 | 1331.5 | 92.32 | 106.5 |
| | (2665.3) | (2305.5) | (2809.6) | (1905.1) | (602.7) | (425.5) |
| Federal retirement payroll deduction | 72.94 | 304.4 | 87.28 | 11.58 | 3.024 | 1.128 |
| | (743.1) | (1310.3) | (859.3) | (279.8) | (124.0) | (70.97) |
| Earned income tax credit (-) | 193.8 | 118.6 | 255.1 | 374.7 | 103.8 | 32.18 |
| | (814.2) | (613.1) | (922.9) | (1090.8) | (620.7) | (324.4) |
| Additional child tax credit (-) | 45.66 | 29.98 | 57.56 | 76.18 | 29.31 | 7.141 |
| | (307.7) | (251.8) | (341.0) | (377.2) | (257.8) | (116.7) |
| Child tax credit (-) | 106.0 | 180.9 | 140.1 | 97.07 | 37.72 | 6.981 |
| | (462.4) | (605.6) | (523.1) | (426.2) | (289.3) | (107.1) |
| Credit received from making work pay (-) | 31.86 | 43.52 | 42.29 | 52.81 | 11.45 | 4.450 |
| | (145.6) | (169.3) | (164.7) | (181.3) | (94.69) | (41.30) |
| Federal stimulus payment (-) | 30.40 | 39.84 | 37.57 | 51.79 | 16.00 | 3.634 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (194.2) | (230.3) | (215.5) | (246.6) | (139.1) | (46.63) |
| *Income from Public Benefits (winsorized)* | 6063.3 | 1534.8 | 2050.9 | 4821.1 | 14227.4 | 3805.9 |
| | (11977.3) | (7618.6) | (8360.3) | (9711.3) | (14028.9) | (9938.5) |
| *Income from Public Benefits (unwinsorized)* | 7407.7 | 2679.6 | 3298.6 | 5940.6 | 15787.3 | 5380.9 |
| | (22306.3) | (19146.1) | (20151.8) | (19787.1) | (24232.4) | (22805.9) |
| Supplemental Security Income (SSI) | 233.2 | 12.13 | 21.27 | 76.94 | 665.1 | 153.0 |
| | (1498.3) | (386.4) | (461.4) | (881.8) | (2473.2) | (1113.4) |
| Welfare (public assistance) | 45.09 | 7.252 | 17.85 | 145.1 | 91.74 | 46.34 |
| | (604.1) | (224.3) | (376.9) | (1036.3) | (869.4) | (570.0) |
| Person market value of Medicare | 1939.6 | 925.1 | 1102.4 | 1016.5 | 3733.0 | 1239.3 |
| | (10195.7) | (9304.1) | (9759.6) | (9120.9) | (10902.6) | (10926.3) |
| Person market value of Medicaid | 1749.7 | 921.6 | 1090.1 | 1349.2 | 3101.3 | 1774.0 |
| | (10238.5) | (9300.3) | (9743.9) | (9230.7) | (11123.8) | (11065.0) |
| Person value of food stamps | 128.3 | 28.75 | 76.10 | 357.3 | 219.2 | 155.2 |
| | (637.3) | (292.4) | (509.8) | (1093.1) | (801.6) | (653.3) |
| Person value of housing subsidy | 7.504 | 1.771 | 2.671 | 14.38 | 16.58 | 6.242 |
| | (44.70) | (18.59) | (24.77) | (64.54) | (66.85) | (36.87) |
| Person value of school-lunch subsidy | 56.91 | 41.75 | 58.45 | 108.9 | 53.23 | 60.63 |
| | (190.4) | (147.7) | (187.9) | (283.0) | (194.8) | (162.7) |
| Person value of energy subsidy | 8.045 | 1.946 | 3.363 | 17.03 | 16.91 | 4.900 |
| | (65.82) | (32.22) | (44.28) | (91.97) | (93.80) | (49.03) |
| Educational assistance (beyond HS) | 138.7 | 87.01 | 140.1 | 108.8 | 49.04 | 1607.7 |
| | (1442.8) | (992.2) | (1383.5) | (1089.5) | (949.3) | (5068.8) |
| Social security | 2433.6 | 177.5 | 469.1 | 491.9 | 6635.8 | 172.1 |
| | (5763.4) | (1791.6) | (2807.4) | (2742.4) | (7856.4) | (1353.1) |
| Unemployment benefits | 158.7 | 174.9 | 122.4 | 2008.8 | 65.16 | 43.77 |
| | (1465.2) | (1412.4) | (1189.9) | (5292.7) | (999.4) | (750.0) |
| Worker's compensation | 62.58 | 96.76 | 26.83 | 51.71 | 120.0 | 3.699 |
| | (1186.4) | (1371.9) | (721.2) | (974.3) | (1711.0) | (176.7) |
| Veteran's benefits | 158.7 | 73.04 | 60.48 | 74.35 | 361.8 | 35.94 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (2141.9) | (1226.3) | (1253.9) | (1200.8) | (3303.1) | (980.2) |
| Disability benefits | 115.6 | 37.08 | 22.23 | 40.72 | 305.1 | 32.28 |
| | (1888.4) | (1280.7) | (863.3) | (1000.6) | (3027.3) | (986.5) |
| Survivor's benefits | 171.5 | 93.05 | 85.28 | 79.01 | 353.4 | 45.76 |
| | (2775.0) | (2455.6) | (2390.8) | (2129.6) | (3472.6) | (1574.2) |
| | | | | | | |
| *Private Income (winsorized)* | 35185.8 | 59204.0 | 48040.5 | 22264.9 | 11010.4 | 1996.9 |
| | (36770.9) | (30709.1) | (37984.6) | (27150.8) | (18588.8) | (7061.0) |
| *Private Income (unwinsorized)* | 36803.3 | 60217.4 | 50765.4 | 23003.6 | 11083.2 | 2005.3 |
| | (49380.3) | (41249.0) | (56399.8) | (35777.1) | (19942.2) | (7297.0) |
| Alimony | 19.07 | 12.25 | 19.46 | 7.36 | 21.97 | 6.196 |
| | (758.9) | (471.5) | (741.9) | (427.4) | (875.8) | (413.5) |
| Non-farm business income | 1727.6 | 135.0 | 3056.6 | 580.9 | 73.73 | 52.06 |
| | (14536.1) | (3240.0) | (19351.2) | (7167.9) | (2083.0) | (2796.9) |
| Child support | 123.1 | 152.4 | 155.9 | 150.3 | 64.51 | 32.67 |
| | (1248.6) | (1361.0) | (1382.9) | (1276.5) | (984.2) | (489.5) |
| Dividends | 365.4 | 229.6 | 326.7 | 85.30 | 511.1 | 31.18 |
| | (3206.1) | (1967.1) | (2965.8) | (1214.7) | (3965.3) | (872.9) |
| Farm | 131.2 | 19.23 | 232.2 | 16.56 | 6.020 | 0.435 |
| | (4176.1) | (1400.1) | (5563.9) | (1175.8) | (900.2) | (66.06) |
| Interest | 547.8 | 391.1 | 436.4 | 153.7 | 844.7 | 30.49 |
| | (3698.1) | (2350.1) | (3182.2) | (1619.9) | (4838.4) | (474.8) |
| Income from other source not specified | 42.91 | 29.05 | 26.43 | 45.32 | 76.65 | 5.187 |
| | (1191.5) | (754.7) | (910.9) | (1004.9) | (1660.1) | (196.7) |
| Rent | 282.1 | 220.2 | 280.6 | 117.2 | 331.1 | 34.19 |
| | (3800.4) | (3459.2) | (3828.0) | (2586.4) | (4031.6) | (1177.6) |
| Retirement | 1382.0 | 296.6 | 471.7 | 479.1 | 3361.8 | 8.916 |
| | (7197.0) | (3251.5) | (4399.5) | (4560.3) | (10827.6) | (496.1) |
| Wage and salary income | 27982.0 | 53706.1 | 41697.3 | 18012.0 | 1216.5 | 1447.8 |
| | (45139.8) | (38910.9) | (51473.8) | (33011.8) | (9732.1) | (5643.9) |
| Assistance from friends/relatives not in HH | 42.72 | 22.17 | 31.52 | 63.84 | 54.28 | 194.4 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (999.2) | (636.6) | (801.7) | (852.2) | (1185.3) | (2485.5) |
| Implied value of owner-occupied housing | 4157.3 | 5003.6 | 4030.7 | 3292.0 | 4520.9 | 161.8 |
| | (5457.7) | (5989.0) | (5377.8) | (5194.5) | (5520.2) | (1338.0) |
| *Observations (individual-year)* | 486,964 | 38,039 | 268,564 | 12,544 | 158,628 | 9,189 |

Source: CPS-ASEC 1994-2015. Note: All means are weighted using sample weights and all dollar amounts are inflated to 2015 dollars. Standard deviations in parentheses. Taxes paid, benefits received, and private income are winsorized to their 99$^{th}$ percentile. NFI is the difference between winsorized taxes paid and benefits received.

*Table A.4 - Sample description*

| Sample: | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| Year: | N | N | N | N | N | N | N |
| 1994 | 150,943 | 133,669 | 121,386 | 86,477 | 11,658 | 5,927 | 2,245 |
| 1995 | 149,642 | 100,490 | 94,223 | 67,658 | 11,658 | 5,927 | 2,245 |
| 1996 | 130,476 | 114,667 | 104,622 | 74,532 | 13,190 | 6,668 | 1,967 |
| 1997 | 131,854 | 115,963 | 105,774 | 75,723 | 26,652 | 13,485 | 4,033 |
| 1998 | 131,617 | 115,369 | 105,640 | 75,736 | 25,672 | 13,059 | 3,934 |
| 1999 | 132,324 | 115,066 | 105,165 | 75,627 | 23,483 | 11,897 | 3,589 |
| 2000 | 133,710 | 115,800 | 105,968 | 76,632 | 22,062 | 11,189 | 3,420 |
| 2001 | 218,269 | 111,062 | 101,365 | 73,573 | 20,726 | 10,636 | 3,238 |
| 2002 | 217,219 | 133,615 | 120,342 | 87,153 | 21,778 | 11,153 | 3,347 |
| 2003 | 216,424 | 135,524 | 122,174 | 88,847 | 23,912 | 12,124 | 3,578 |
| 2004 | 213,241 | 131,818 | 119,186 | 86,624 | 22,802 | 11,562 | 3,480 |
| 2005 | 210,648 | 129,816 | 118,784 | 86,697 | 22,741 | 11,401 | 3,436 |
| 2006 | 208,562 | 128,322 | 117,205 | 85,777 | 24,913 | 12,424 | 3,634 |
| 2007 | 206,639 | 127,990 | 116,991 | 85,685 | 26,498 | 13,479 | 3,992 |
| 2008 | 206,404 | 127,219 | 116,347 | 86,102 | 27,546 | 13,776 | 4,153 |
| 2009 | 207,921 | 128,976 | 117,473 | 86,767 | 28,148 | 13,625 | 4,054 |
| 2010 | 209,802 | 129,156 | 117,896 | 87,378 | 27,916 | 13,434 | 3,893 |
| 2011 | 204,983 | 126,241 | 115,228 | 86,110 | 27,292 | 13,296 | 3,793 |
| 2012 | 201,398 | 125,256 | 114,506 | 85,819 | 27,182 | 13,164 | 3,757 |
| 2013 | 202,634 | 124,254 | 113,802 | 85,412 | 24,583 | 11,498 | 3,292 |
| 2014 | 199,556 | 123,438 | 112,740 | 75,403 | 18,763 | 8,597 | 2,446 |
| 2015 | 199,024 | 122,467 | 111,277 | 83,695 | 7,789 | 3,585 | 1,026 |
| Total | 4,083,290 | 2,716,178 | 2,478,094 | 1,803,427 | 486,964 | 241,906 | 72,552 |

Sample 1 - Full ASEC

Sample 2 - keep mis=4 or 8 and then only the people that link to their ORG. This should include all march people and less april may june

Sample 3 - drop people who don't match on age, sex, race and seem to be at the same job

Sample 4 - drops <18

Sample 5 - Keep NILF and unemployed. drop if person not in both years, drop if union=NIU (civilians 15+ wage/salary workers, excludes self-employed), not matches in sex, hispanic, black, asian, foreign born.

Sample 6 - drop if person not in both years, drop if union=NIU (civilians 15+ wage/salary workers, excludes self-employed), not matches in sex, hispanic, black, asian, foreign born

Sample 7 - keep only March Basic observations from Sample 6

*Table A.5 - Effect of imputations on coefficients*

| Outcome: | Net fiscal impact | | | Taxes paid | | | Benefits Received | | | Private Income | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Specification | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 |
| **Main Sample (Drop all imputations)** | | | | | | | | | | | | |
| 1(union member) | 1289.8*** | 1264.3*** | 540.0** | 1108.6*** | 1240.2*** | 216.3 | -181.3*** | -24.2 | -323.7** | 4661.6*** | 4588.0*** | 1614.0*** |
| | (91.6) | (138.1) | (254.4) | (85.6) | (129.7) | (208.5) | (35.0) | (47.1) | (144.9) | (205.3) | (302.4) | (575.1) |
| N | 241,906 | | | | | | | | | | | |
| Dropped N (%) | 317,200 (56.73%) | | | | | | | | | | | |
| **Drop Imputed Union Status Only** | | | | | | | | | | | | |
| 1(union member) | 949.0*** | 1017.8*** | 547.5 | 722.6*** | 928.2*** | 280.2 | -226.4*** | -89.6 | -267.4 | 2834.6*** | 3517.1*** | 1119.2** |
| | (105.0) | (147.8) | (506.1) | (70.5) | (89.8) | (186.8) | (79.5) | (119.2) | (476.4) | (180.2) | (198.3) | (528.9) |
| N | 526,954 | | | | | | | | | | | |
| Dropped N (%) | 32,152 (5.75%) | | | | | | | | | | | |
| **Drop Imputed Income Only** | | | | | | | | | | | | |
| 1(union member) | 1145.3*** | 1155.9*** | 369.2 | 991.4*** | 1135.3*** | 319.8 | -153.9*** | -20.6 | -49.4 | 3992.1*** | 4111.0*** | 1752.4*** |
| | (82.8) | (117.9) | (252.5) | (75.7) | (107.5) | (205.8) | (34.9) | (52.1) | (148.0) | (188.1) | (262.1) | (580.3) |
| N | 305,750 | | | | | | | | | | | |
| Dropped N (%) | 253,356 (45.31%) | | | | | | | | | | | |
| **Drop Full-Line Impute Only** | | | | | | | | | | | | |
| 1(union member) | 990.8*** | 1058.1*** | 99.4 | 771.6*** | 950.3*** | 18.9 | -219.3*** | -107.8 | -80.5 | 3195.4*** | 3808.9*** | 433.3 |
| | (90.5) | (117.7) | (340.9) | (72.9) | (92.8) | (176.5) | (53.7) | (73.2) | (297.0) | (180.2) | (208.3) | (500.3) |
| N | 478,544 | | | | | | | | | | | |
| Dropped N (%) | 80,652 (14.43%) | | | | | | | | | | | |
| **Include All Imputations** | | | | | | | | | | | | |
| 1(union member) | 919.6*** | 996.5*** | 43.0 | 717.2*** | 898.6*** | 155.3 | -202.4*** | -98.0 | 112.3 | 2786.8*** | 3455.3*** | 805.6 |
| | (99.0) | (136.5) | (447.1) | (67.4) | (83.1) | (174.1) | (74.7) | (108.8) | (417.5) | (170.6) | (186.4) | (499.5) |
| N | 559,106 | | | | | | | | | | | |
| Dropped N (%) | 0 (0%) | | | | | | | | | | | |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 241,906 observations of 120.953 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.

*Table A.6 - Estimates without weights*

| Specification: | 1 | 2 | 3 |
|---|---|---|---|
| | | Outcome: net fiscal impact | |
| 1(union member) | 1200.4 *** | 1208.2*** | 571.8** |
| | (78.4) | (120.5) | (225.4) |
| | | Outcome: taxes paid | |
| 1(union member) | 1013.8*** | 1148.2*** | 281.0 |
| | (72.9) | (111.9) | (187.1) |
| | | Outcome: public benefits received | |
| 1(union member) | -186.6*** | -60.1 | -290.8** |
| | (29.9) | (41.0) | (125.3) |
| | | Outcome: private income earned | |
| 1(union member) | 4268.7*** | 4442.2*** | 1468.4*** |
| | (185.7) | (262.0) | (516.2) |
| Demographics | Yes | Yes | Yes |
| State-year FE | Yes | Yes | Yes |
| Individual FE | | | Yes |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 241,906 observations of 120.953 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. d) Dollar amounts are inflated to 2015 dollars.

*Table A.7: Results including covered non-members with union members*

| Specification: | 1 | 2 | 3 |
|---|---|---|---|
| | | Outcome: net fiscal impact | |
| 1(union member) | 1163.9*** | 1142.1*** | 617.0** |
| | (92.3) | (133.1) | (233.4) |
| | | Outcome: taxes paid | |
| 1(union member) | 997.7*** | 1129.6*** | 337.8* |
| | (83.8) | (122.4) | (189.6) |
| | | Outcome: public benefits received | |
| 1(union member) | -166.1*** | -12.5 | -279.2** |
| | (33.2) | (46.3) | (133.4) |
| | | Outcome: private income earned | |
| 1(union member) | 4335.6 *** | 4225.0*** | 1729.4*** |
| | (206.6) | (294.8) | (529.0) |
| Demographics | Yes | Yes | Yes |
| State-year FE | Yes | Yes | Yes |
| Individual FE | | | Yes |

Note: Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. 241,906 observations of 120.953 individuals over 2 consecutive years each. Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. All means are weighted using sample weights and all dollar amounts are inflated to 2015 dollars.

*Table A.8 – Results including covered non-members as independent control*

| Specification: | 1 | 2 | 3 |
|---|---|---|---|
| | | Outcome: net fiscal impact | |
| 1(union member) | 1289.5*** | 1267.7*** | 610.8** |
| | (93.2) | (138.8) | (258.4) |
| 1(covered non-members) | -8.8 | 70.6 | 639.2 |
| | (235.0) | (344.4) | (398.0) |
| | | Outcome: taxes paid | |
| 1(union member) | 1107.5*** | 1246.5*** | 277.6 |
| | (86.5) | (129.5) | (210.6) |
| 1(covered non-members) | -27.3 | 132.1 | 553.0* |
| | (203.0) | (314.1) | (333.8) |
| | | Outcome: public benefits received | |
| 1(union member) | -182.0*** | -21.2 | -333.2** |
| | (34.8) | (47.5) | (147.6) |
| 1(covered non-members) | -18.4 | 61.5 | -86.2 |
| | (106.6) | (128.1) | (221.6) |
| | | Outcome: private income earned | |
| 1(union member) | 4698.8*** | 4626.4*** | 1784.0*** |
| | (209.2) | (305.3) | (586.8) |
| 1(covered non-members) | 944.6* | 799.7 | 1533.8* |
| | (489.4) | (741.9) | (853.6) |
| Demographics | Yes | Yes | Yes |
| State-year FE | Yes | Yes | Yes |
| Individual FE | | | Yes |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 241,906 observations of 120.953 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.

*Table A.9 - Details of variables*

| Variable | Var Name (IPUMS) | Variable (Census) | Type | Record type | Special construction |
|---|---|---|---|---|---|
| ***Taxes*** | | | | | |
| Federal income tax liability before credits | FEDTAX | FEDTAX_BC | Imputed | Person | None |
| State income tax liability before credits | STATETAX | STATETAX_BC | Imputed | Person | None |
| Annual property taxes | PROPTAX | PROP_TAX | Imputed (asks estimated value of property) | Household | Divided by total adults (age>18) in household |
| Social security retirement payroll deduction | FICA | FICA | Imputed | Person | None |
| Federal retirement payroll deduction | FEDRETIR | FED_RET | Imputed | Person | None |
| Earned income tax credit | EITCRED | EIT_CRED | Imputed | Person | None |
| Additional child tax credit | ACTCCRD | ACTC_CRD | Imputed | Person | None |
| Child tax credit | CTCCRD | CTC_CRD | Imputed | Person | None |
| Credit received from Making Work Pay | MWPVAL | MWP_VAL | Imputed | Person | CPS ASEC 2010-2011 |
| Federal stimulus payment | STIMULUS | STIMULUS | Imputed | Person | CPS ASEC 2009 |
| *Income from Public Benefits* | | | | | |
| Supplemental Security Income | INCSSI | SSI_VAL | Collected | Person | None |
| Welfare (Public assistance) | INCWELFR | PAW_VAL | Collected | Person | None |
| Person market value of Medicare | PMVCARE | P_MVCARE | Receipt not amount | Person | None |
| Person market value of Medicaid | PMVCAID | P_MVCAID | Receipt not amount | Person | None |
| Person value of food stamps | FMFDSTAMP (Not in IPUMS) | F_MV_FS | Collected | Family | Divided by total adults (age>18) in family |
| Person value of housing subsidy | NOT IN IPUMS (FMVHOUSSUB) | FHOUSSUB | Receipt not amount (amount imputed) | Family | Divided by total adults (age>18) in family |

| | | | | | |
|---|---|---|---|---|---|
| Person value of school-lunch subsidy | NOT IN IPUMS (FMVSCHLUNCH) | F_MV_SL | Receipt not amount (amount imputed) | Family | Divided by total adults (age>18) in family |
| Person value of energy subsidy | HEATVAL | HENGVAL | Collected | Household | Divided by total adults (age>18) in household |
| Educational assistance (beyond HS) | INCEDUC | ED_VAL | Collected | Person | None |
| Social security | INCSS | SS_VAL | Collected | Person | None |
| Unemployment benefits | INCUNEMP | UC_VAL | Collected | Person | None |
| Worker's compensation | INCWKCOM | WC_VAL | Collected | Person | None |
| Veteran's benefits | INCVET | VET_VAL | Collected | Person | None |
| Disability benefits | INCDISAB | DSAB_VAL | Collected | Person | None |
| Survivor's benefits | INCSURV | SRVS_VAL | Collected | Person | None |
| *Private Income* | | | | | |
| Wage and salary income | INCWAGE | PEARNVAL | Collected | Person | None |
| Alimony | INCALIM | ALM_VAL | Collected | Person | None |
| Non-farm business income | INCBUS | SEMP_VAL | Collected | Person | None |
| Child support | INCCHILD | CSP_VAL | Collected | Person | None |
| Dividends | INCDIVID | DIV_VAL | Collected | Person | None |
| Farm | INCFARM | FRM_VAL | Collected | Person | None |
| Interest | INCINT | INT_VAL | Collected | Person | None |
| Income from other source not specified | INCOTHER | OI_VAL | Collected | Person | None |
| Rent | INCRENT | RNT_VAL | Collected | Person | None |
| Retirement | INCRETIR | RTM_VAL | Collected | Person | None |
| Assistance from friends not in HH | INCASIST | FIN_VAL | Collected | Person | None |
| Implied value of owner-occupied Housing | HOUSRET | HOUSRET | Estimated value collected of sell + presence of mortage (not amount) | Household | Divided by total adults (age>18) in household |

*Table A.10 - Estimated union-membership coefficients when controlling for various functions of private income*

| | Net fiscal impact | | | Taxes paid | | | Benefits Received | | |
|---|---|---|---|---|---|---|---|---|---|
| Specification: | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 |
| | | | | | **Base Model** | | | | |
| 1(union member) | 1289.8*** | 1264.3*** | 540.0** | 1108.6*** | 1240.2*** | 216.3 | -181.3*** | -24.2 | -323.7** |
| | (91.6) | (138.1) | (254.4) | (85.6) | (129.7) | (208.5) | (35.0) | (47.1) | (144.9) |
| | | | | | **Base Model + Income** | | | | |
| 1(union member) | 71.5 | 91.9 | 143.7 | -73.5 | 97.6 | -163.7 | -145.0*** | 5.7 | -307.4** |
| | (69.8) | (113.0) | (209.0) | (61.5) | (101.5) | (155.6) | (35.0) | (47.2) | (144.8) |
| | | | | | **Base Model + Income + Income$^2$** | | | | |
| 1(union member) | 219.1*** | 184.4* | 171.7 | 166.6*** | 252.7** | -122.8 | -52.5 | 68.3 | -294.5** |
| | (70.3) | (111.7) | (208.8) | (62.0) | (99.9) | (154.8) | (34.9) | (47.4) | (144.6) |
| | | | | | **Base Model + Income + Income$^2$ + Income$^3$** | | | | |
| 1(union member) | 237.8*** | 200.4* | 178.7 | 204.7*** | 280.4*** | -110.4 | -33.1 | 80.0* | -289.0** |
| | (70.4) | (111.7) | (208.8) | (62.0) | (99.4) | (154.6) | (34.9) | (47.1) | (144.4) |
| | | | | | **Base Model + Income + Income$^2$ + Income$^3$ + Income$^4$** | | | | |
| 1(union member) | 241.9*** | 201.4* | 182.4 | 211.3*** | 260.9*** | -105.6 | -30.7 | 59.6 | -288.1** |
| | (70.4) | (111.7) | (208.7) | (62.0) | (99.6) | (154.3) | (34.9) | (46.7) | (144.4) |
| N | | | | | 241,906 | | | | |

Source: CPS-ASEC 1994-2015. Notes: a) Coefficient (within-individual, correlation-corrected SE). Significant at: *10 **5 ***1 percent level. b) 241,906 observations of 120.953 individuals over 2 consecutive years each. c) Coefficient estimates on 1(union member) are presented for each {outcome}x{specification} regression model. d) Weighted using sample weights and dollar amounts are inflated to 2015 dollars.