The Trade-off Between Wages and Health Insurance: Evidence from the Individual Mandate

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Abstract

Compensating wage differentials pertaining to health insurance have received a large amount of attention within the health and labor economics literature. Understanding how health insurance policies can influence the trade-offs workers place between wages and health insurance is arguably as import as understanding the general trade-off itself. In this paper, I analyze the impact that the Affordable Care Act's Individual Mandate has had on compensating wage differentials. Using a sample of workers from the Current Population Survey's Displaced Worker Supplement, I estimate that workers who gain health insurance through a job switch tend to have larger wages than other workers. My results are consistent with the positive-signed trade-offs examined in numerous empirical studies of wage differentials, and suggest the need for better data pertaining to additional fringe benefits.

1   Introduction

One of the oldest, least ambiguous predictions of economic theory is that there will be a compensating wage differential between two otherwise equal jobs if one offers fringe benefits while the other does not. The analysis of this differential is particularly important when the distinguishing benefit is health insurance. Understanding the trade-off that workers place between wages and health benefits can inform numerous questions related to wages and health policy. Are workers more willing to substitute wages for health insurance benefits after a health scare? Is the observation that wages taper off towards the ends of careers influenced by an association between the value of health insurance and age? While economic theory would project that such trade-offs would exist, there has been a well-documented struggle in the empirical literature to identify wage differentials influenced by health insurance.

Unfortunately, numerous attempts to analyze this trade-off has resulted in wrong-signed estimates. This is likely due to the inability to control for unobserved changes in worker productivity over time, or for the potential impact that working at a firm offering health insurance can have on productivity. In this paper, I attempt isolate wage differentials from both unobserved productivity measures and unobserved influences on productivity from receiving employer-provided health insurance (EPHI), in an attempt to investigate both the existence of this trade-off and *whether insurance policies like the Individual Mandate have an affect on wage-health benefits trade-offs.* Following Simon (2001), I exploit data on workers displaced due to massive layoffs and plant closings, as job losses of this sort are arguably orthogonal to worker productivity. Unlike previous studies, I am able to exploit a recent policy change, the Individual Mandate of the Affordable Care Act, to isolate differential estimates from potential increases in productivity brought about by EPHI, which is likely uncorrelated with the policy.

Numerous prior studies have failed to produce robust estimates of wage health insurance differentials consistent with theory. In response, many theories of model mispecification have been developed, as well as methods to curtail model and data shortcomings. DeVaro and

Maxwell (2014) argue that previous studies have failed to adequately account for firm and establishment size in estimating differentials. Despite implementing detailed controls for firm and establishment size, they estimate different signed magnitudes depending on slight variations of their model specifications. A possible cause for their study's lack of robustness is endogeneity in job-switching motives, which I attempt to control for in my analysis. While Simon (2001) and Lehrer and Pereira (2007) control for this issue by examining wages for workers displaced by plant closings, Simon estimates wrong-signed trade-offs while Lehrer and Pereira are not able to achieve consistent estimates.

A number of recent studies have developed both empirical and theoretical strategies to circumvent the issues described above. In my analysis, I adapt strategies used in previous papers and attempt to overcome the issues that some of these studies faced. Examining public employee wages and benefits, Clemens and Cutler (2014) instrumented for aggregate benefit spending with a measure of expected inflation of health costs, finding no significant trade-off between wages and benefits. While benefits other than health insurance play a major role in determining differentials, their results suggest the need for either a stronger instrument or an identification strategy that is better able to isolate out the effects from additional benefits. While examining displaced workers, Lehrer and Pereira (2007) also use a Generalized Method of Moments (GMM) approach to derive point estimates for wage differentials, arguing that typical OLS approaches are more prone to possessing biased wage differential coefficients. While they do not find evidence of a compensating wage differential, they find evidence that the provision of health insurance has influenced wage inequality across sectors – this suggests the need for adequate industry controls in estimating differentials.

There do exist a small number of papers that have successfully identified wage differentials consistent with theory, though the identification strategies of these papers result in limited external validity. Examining women with spouses, Olson (2002) uses husband health benefit and employer information as an instrument for women's health benefits, finding that women with their own employer provided health insurance tend to accept wages that are 20%

lower than they otherwise would. In fact, spousal benefit information appears to be the only instrument that consistently achieves wage differential estimates that coincide with theory, though this leaves the question of whether the same effect would be observed among non-married workers. Kolstad and Kowalski (2010) find that Massachusetts' health care reform led to insurance-offering firms paying roughly $6,000 less annually per worker. While this potentially suggests a trade-off, it does not directly examine an employee's willingness to accept a wage. I attempt to examine this feature in my analysis.

Numerous theories have been developed to explain the literature's inability to consistently produce wage differential estimates consistent with theory. Most commonly, the issue is described as a data problem: that estimates are biased in the positive direction due to insufficient data (and hence unobserved in estimations) on key variables on measures of ability, firm performance and other fringe benefits. On the other hand, some argue that economic theory of wage differentials may need reexamining. O'Brien (2003) argues that the environment of firms offering health insurance may be inherently different than non-offering firms, and that workers' marginal products of labor (and hence their wages) are inherently tied to the firm that they work at. This argument is supported by Yamada and Vu (2016), who find that Vietnamese firms that begin to offer health insurance tend to become more profitable both overall and per worker. This is a significant finding, as it suggests that heterogeneity in firm environments can significantly impact worker productivity. While I use industry controls in my study, like numerous studies I am unable to account for other sources of firm-level heterogeneity.

My paper contributes to the previous literature by analyzing the impact of a health policy, the Individual Mandate, on wage differentials. This approach allows for insight into both the efficacy of health policies in influencing health behaviors, as well the trade-off workers place between wages and health benefits, the same relationship the numerous studies are devoted to, through the channel of policy effects. I present a theoretical argument for why the Individual Mandate should increase societal value of health insurance and thus influence

wage differentials in the same manner as previous theory has suggested, and I adopt previous approaches developed in the literature to address factors that could bias the estimated policy effect.

Consistent with many findings in the literature, I estimate that in response to the Individual Mandate, the wage responses of workers who obtained insurance through a job switch are higher relative to other workers. This wrong-signed estimate is likely influenced by sources of bias I am unable to control for. While my identification strategy allows for me to control for unobserved firm-specific effects on worker productivity, I am unable to control for unobserved variation in additional benefits, which are likely correlated with the enactment of the Individual Mandate. Nonetheless, my findings highlight the need for a stronger understanding of the prominence of additional employee benefits, and I argue that this will require an effort to make richer data publicly available.

1.1   Background on the Individual Mandate

Embedded within the Affordable Care Act (ACA) of 2010, the Individual Mandate for United States citizens became effective in 2014. Uninsured individuals are forced to pay the maximum amount between a set fine and a percentage of income. For 2014, the fine was $95 per adult and $47.50 per child, while the percent of family income was set at 1 percent. There was a maximum penalty of $285 per family. The policy was gradually phased in so that penalties grew considerably each year. By 2016, the fine was $695 per adult and $347.50 per child, while the percent of family income was 2.5 percent. The maximum penalty per family was set at $2,085, notably higher than the amount set in 2014.

A subset of the population is exempt from the policy. Incarcerated individuals, Native Americans and individuals citing religious reasons are not required to possess health insurance. Additionally, individuals with income sufficiently low that purchasing insurance would consume more than 8 percent of their income are exempt from the law. Individuals affected by the policy must possess a plan that meets the federal government's definition of "essential care."

While the Individual Mandate is currently in effect, the Tax Cuts and Jobs act of 2017 repealed this policy, with a plan to completely phase it out by 2019.

2 The Theory of Compensating Wage Differentials

In this section, I outline the theory of compensating wage differentials, and show how this problem is influenced by the Individual Mandate through its impact on an individual's decision to purchase health insurance. The theory of compensating wage differentials originated from Smith (1776). Smith posited that along with wages, there are numerous other objects that influence labor market equilibrium. Compensating wage differentials encompass the idea that, all else equal, the wage a worker would be willing to accept at two firms is relatively higher at the firm offering lower levels of extra benefits (such as health insurance, vacation time, pension plans, etc.). In turn, the difference between the two wages the worker would be willing to accept is the *compensating wage differential* needed for a worker to be indifferent between the job with less benefits and the job with more benefits.

Formally, a worker's marginal willingness to accept a wage is a function of his/her education (E_i) ability (A_i) and the benefits (B_j) offered by the firm:

$$w_{ij} = f(E_i, A_i, B_j)$$

where $\frac{\partial w_{ij}}{\partial E_i} > 0, \frac{\partial w_{ij}}{\partial A_i} > 0$ and $\frac{\partial w_{ij}}{\partial B_j} < 0$.

Consider now an individual's decision of whether to purchase health insurance. Nyman (2003) developed a conceptual framework in which a consumer buys health insurance only if their expected gain in utility from becoming sick with insurance exceeds their expected loss in utility from remaining healthy but paying the insurance premium. Let M_i and M_u denote medical expenditures, conditional on becoming sick, for when an individual is insured and uninsured, respectively. Similarly, let C_i and C_u denote consumption (conditional on becoming sick) for insured and uninsured individuals respectively. Suppose an individual

gets utility from the benefits of medical procedures and consumption: $U = U(M, C)$. Assume that there are utility functions U_s and U_h associated with being sick and healthy, respectively. Then, if R denotes cost of the insurance premium and π is the individual's probability of falling ill, an individual will purchase insurance only if

$$\mathbb{E}U_i - \mathbb{E}U_u = \pi\left[U_s(M_i, C_i) - U_s(M_u, C_u)\right] + (1 - \pi)\left[U_h(0, Y - R) - U_h(0, Y)\right] > 0$$

where Y is an individual's income[1], while U_i and U_u denote utility while insured and uninsured, respectively.. If an individual mandate is in place, however, there is some fixed fine τ and individual must pay[2] so that the individual's income becomes $Y - \tau$. This generates a negative income effect so that M_u and C_u decrease. Denote their new values by M_u^m and C_u^m. In this case, the individual will purchase insurance only if

$$\mathbb{E}U_i - \mathbb{E}U_u = \pi\left[U_s(M_i, C_i) - U_s(M_u^m, C_u^m)\right] + (1 - \pi)\left[U_h(0, Y - R) - U_h(0, Y - \tau)\right] > 0 \quad (1)$$

As $M_u^m < M_u$ and $C_u^m < C_u$, the first term in (1) increases. Clearly the second term increases as $Y - \tau < Y$, meaning the overall expression is more likely to be positive. Thus, the introduction of the mandate implies that individuals are more likely to purchase insurance at given premium values. This suggests that there is a higher level of average societal value placed upon health insurance,[3] which is encompassed by B_j from earlier. Thus, theory projects that the Individual Mandate should decrease wage differential indirectly by magnifying the consumer's value placed on benefits B_j. Even if documented positive estimates of the wage differential are driven from increases in productivity (A_i) brought about by obtaining insurance, examining the impact of the Individual Mandate alone should isolate this effect, as the phenomenon of increased productivity through EPHI is not influenced by the policy.

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[1]Note that $C_i = Y - M_i$. The relationship between C_u and M_u is analogous.

[2]It is reasonable to assume $\tau < R$, as otherwise all rational individual would purchase insurance trivially.

[3]There is a possibility that the cost of insurance premiums, R, would increase in response to the policy. This theory assumes that this increase would be sufficiently small so that it is effectively outweighed by the introduction of the find τ.

This projection from the theory is tested empirically in the following sections.

3   Data

In order to examine the effect of EPHI on wage differentials in a way that minimizes skill and individual-level bias, my study requires data from workers who faced exogenous shocks causing them to switch jobs. One such common shock is a plant closing – in such a case the force driving workers to switch jobs is likely unrelated to their individual skill level or preferences. One appropriate source of data for such an analysis is the Displaced Workers Supplement (DWS) of the Current Population Survey (CPS). Conducted every other year since 1984, the DWS provides reasons for workers becoming unemployed, their current employment status, as well as wage and benefit information from their current and previous jobs. Most importantly for this analysis, the DWS includes information on whether each worker's current and previous jobs offered health insurance. As a means to control for time trends in income, I also obtain information on U.S. inflation rates from each year in the sample – data for these rates come from the St. Louis FRED.

While the DWS functions as a solid source of data for my analysis, it does not come without limitations. For example, while the DWS includes information on health insurance for each job a worker holds, it does not include information on the amount of money workers put towards group plan premiums. Further, the DWS does not include information on other import fringe benefits. Finally, the DWS asks workers whether their previous employer offered insurance, but only if the worker possesses insurance at the time of the survey, regardless of how it was obtained. This means that the DWS source of information for current insurance status serves only as a proxy for whether a worker's employer offers insurance.[4] As the unit of observation within this dataset is a worker displaced from their previous job, the results of my analysis only capture a particular class of workers. Subsequently, one should exercise

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[4]One way to circumvent this issue is to match DWS respondent's with the ASEC survey, another CPS supplement, conducted in March. Unfortunately, this matching process is time intensive and, given the time scope of this analysis, I choose to forgo matching responses.

caution in generalizing the results of my analysis to broader populations.

Although the DWS has notable limitations, it still functions as a strong source of information for my analysis. My sample consists of respondents from each even year since 2002[5]. Following Simon (2001), I only include displaced workers who held a full time job at their previous job and found a new full-time job at the time of the survey. I prune observations whose reason for displacement was not related to a mass-layoff or a plant closing. To avoid discrepancies in the manner by which health insurance is obtained, I omit workers younger than 20 and older than 65 from my sample. After completing this filtering process and pruning observations with missing wage information, I have 8,788 observations across 8 survey years.[6] Summary statistics for this sample are displayed in Table 1. I also provide summary statistics for treatment and control groups (as described in Section 4) in Tables 2 and 3.

Interestingly, we see that weekly earnings decrease on average for workers between the prior and current jobs at the time of their survey response. This is seen both in Table 1 and Figure 1. This observation suggests some form of wage scarring brought about by displacement, but is not explored further here. Partitioning workers into a group of those that obtained health insurance after switching jobs and a group containing all other workers, we find that workers obtaining health insurance had slightly better wage outcomes on average (see Figure 2). Considering only years in which the Individual Mandate was in effect, we see the opposite (see Figure 3). While this suggests that the Individual Mandate may influence wage-benefit trade-offs, a formal analysis is required. The appropriate framework is developed in the following section.

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[5]The DWS reclassified its definition of a displaced worker in 2002. To avoid issues with this discontinuity, I do not include prior survey years in my analysis.

[6]For a robustness exercise, I examine a subsample of workers that excludes workers with relatively high wages. This subsample consists of 6,502 workers.

4   Modeling Approach

In order to isolate the affect of the Individual Mandate on wage differentials, I employ a differences-in-differences (DID) framework where the unit of analysis is a worker recently displaced due to a plant closing or mass layoff. I use the set of workers who switch from a non-EPHI job to an EPHI job as the treatment group, and assign all other workers to the control group. Let W_{its} denote the wage of worker i during year t and state s. Then, my econometric model is given by

$$W_{its} - W_{i(t-1)s} = \beta_0 + \beta_1 M_t + \beta_2 TREAT_i + \beta_3(M_t \times TREAT_i) + \beta_4 \boldsymbol{X}_{its} + \gamma_s + \epsilon_{its} \qquad (1)$$

Here, M_t is an indicator variable for whether the Individual Mandate was in place for year t, $TREAT_i$ is an indicator variable for whether the worker is in the treatment group, $\boldsymbol{X}_{its}$ is a vector of control variables for factors including age, sex, and marital status., and γ_s denotes state fixed effects. Given the form of the model, β_3 is the DID estimator, whose estimated value is of the most interest. I choose to use two separate definitions of the control group. One is the simple complement of the treatment group: all workers who do not switch from a non-EPHI to an EPHI job. For my other definition, I restrict the control group to workers who switch *away* from a job offering health insurance to one that does not. The second definition serves as a more pure reference group in estimating trade-offs, while the first definition allows for comparison to a broader class of workers.

While theory would project a negative sign for β_3, there are potential confounding effects that could still bias the DID estimator, meaning its interpretation should not be causal. First, although my strategy of using displaced workers allows for relatively cleaner identification relative to previous wage-benefit studies, there is still potential endogeneity influencing workers' decisions to switch to particular types of firms – workers able to secure jobs offering health insurance after a job loss may have significantly different characteristics than other workers. Further, I am unable to control for other forms of benefits offered by firms (vacation,

thrift plans, transportation subsidies, etc.) that may have adjusted in response to the Individual Mandate. Since the phenomenon of EPHI firms potentially making workers more productive should exist before and after the Individual Mandate, the two variables are likely uncorrelated with each other. The presence of other unobserved benefits, however, could potentially be correlated with the presence of the Individual Mandate, and are likely to produce an upward bias on coefficient estimates.

Along with my main specification outlined in equation (1), I estimate a similar model in which the dependent variable is the weekly wages earned by a worker at their current job, rather than the weekly wages earned at their current job minus those earned at their prior job[7]. Notice coefficient estimates do not make predictions about the sign of $W_{its} - W_{i(t-1)s}$ – for example, it is possible for a large portion of workers to earn less at their current job after the Individual Mandate was enacted, but for there to be a positive estimate of β_3. This would simply imply that the change for these workers was less negative, which could be driven by lower prior wages or higher current wages. Similarly, a negative estimate of β_3 could imply lower current-job wages or higher prior-job wages – there are two conflicting channels. Estimating the model presented in equation (1) with current wages as on the left hand side should provide a greater sense of which effects are dominating, given the estimates found with the wage change on the left hand side.

As I employ a DID approach, I check that the parallel trends assumption is satisfied for both outcome variables. Figures 4 and 5 display mean survey-time weekly wages and mean wage changes, respectively, by year and treatment group status.[8] Besides a single outlier point in the wage change trends, the parallel trends assumption appears to be satisfied. Note that the Individual Mandate was enacted in 2014, corresponding to when the trends for control vs. treatment groups in both the wage change and current wage graphs begin to diverge from each other.

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[7]Throughout the remainder of the paper I commonly refer to current minus prior job weekly wages as a "wage change" to avoid verbosity.

[8]As described in Simon (2001), the higher level of variability in wage changes may be a cause of recall bias.

5 Results

In this section, I provide an overview and interpretations of the results of my model estimations. I discuss the implications of these findings in the Discussion section. OLS estimation results for my main model specification are included in Tables 5 - 8. Each model differs in whether it includes industry and/or state fixed effects. Whether these fixed effects are included is denoted within each table. As described earlier, I use two definitions for the control group. Tables 5 and 6 contain estimation results when the control group is all workers who did not switch from a non-EPHI job to a job with EPHI. Tables 9 and 10 contain estimation results where the control group is workers who switched from an EPHI to a non-EPHI job.

I first consider the results laid out in Table 5. Across all three estimations, I estimate that, all else in the model equal, the enactment of the Individual Mandate is associated with a weekly wage change that is roughly $65 greater for workers switching from a non-EPHI to an EPHI job relative to other workers.[9] Across each estimation, this result is significant at the 1 percent level of significance. In my model containing both industry and state fixed effects, I estimate that ceteris paribus, weekly wage changes are $36.64 higher for workers switching to EPHI jobs relative to other workers. While I estimate that the enactment of the Employer Mandate is associated with a ceteris paribus $25.68 increase in weekly wage changes for all workers, this effect is not precisely estimated.

When restricting the control group to all workers who switched from an EPHI to non-EPHI job (Table 1), I find estimates of the policy effect and treatment effect that are larger in magnitude. Using my model that includes state and industry fixed effects, I estimate that the enactment of the Employer Mandate is associated with a weekly wage change that is $223.84 larger for workers switching to EPHI jobs relative to workers switching away from EPHI jobs – this is nearly 4 times the estimated relative effect when the control group is the simple

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[9]Note that this change does not necessarily suggest that for these workers, current weekly wages were $65 higher than they were at their prior job. Rather, the results suggest that the change between the two wages is either less negative or more positive in magnitude relative to other workers.

complement of the control group. All else in the model equal, being in the treatment group is associated with a $107.53 increase in the wage change relative to workers switching away from EPHI jobs. Both this estimate and the DID estimate are significant at the 0.1 percent level of significance. Finally, the enactment of the Employer Mandate is associated with a $91.66 reduction in each workers' wage change – this effect is significant at the 5 percent level of significance.

Tables 7 and 8 display analogous estimations to Tables 5 and 6, only with the dependent variable being a worker's current weekly wage. First considering the state and fixed effects model in Table 7 (where the control variable is again the complement of workers switching from a non-EPHI job to an EPHI job), I do not estimate a significant treatment effect associated with the Individual Mandate (I estimate an effect near zero in magnitude). Interestingly, I estimate that all else in the model equal, membership in the treatment group is associated with an $89.33 *decrease* in a worker's current weekly wage relative to workers not in the treatment group. This result is significant at the 0.1 percent significance level. I discuss the implications of this finding, along with my others findings, in the Discussion section.

When restricting the control group to workers switching away from EPHI jobs (Table 8) (and considering the state and industry fixed effects model), I estimate that the enactment of the Individual Mandate is associated with a weekly wages that are $157.63 higher for workers in the treatment group relative to the control group (significant at the 0.1 percent level). Restricting the control group reverses the sign of the estimated effect of treatment group membership relative to results found in Table 7, though this result is not statistically significant.

5.1   Robustness

Note that the distribution of weekly wages of the original sample of workers is considerably skewed right by workers earning relatively high wages. As the focus of policy interventions like the Individual Mandate is on workers earning median and lower-level wages, I consider a

robustness exercise in which I re-estimate the model described by Tables 5 through 8, only now restricting my sample to workers earning $1,200 or less per week at both their current and prior jobs. This cutoff leaves a sample of workers all earning median or lower levels of weekly income. The purpose of this robustness exercise is to examine whether my original results withstand under this particular subset of the original sample population.

Across Tables 9 through 12, my results are qualitatively similar to the ones found in Tables 5 through 8, though of slightly different magnitudes. These results are robust to the inclusion of state and industry fixed effects, as were my previous results. Table 9 estimates the effect of the Individual Mandate on wage changes, excluding large earners and defining the control group as the simple complement of the treatment group. Again considering my model with state and industry fixed effects, I estimate that the Individual Mandate is associated with an increase in wage changes that is $64.28 larger for treatment group workers (significant at the 1 percent level of significance). Similar to when including larger earners in the sample, I find that this association rises considerably when defining the control group as workers switching away from EPHI jobs: in this case, I estimate that the increase in wage changes is $95.73 larger for treatment group workers (Table 10 column 3) and significant at the 0.1 percent level. Note that this increase is smaller in magnitude than the estimated increase earlier, suggesting that higher-earning workers were driving a large portion of the earlier results.

When examining current weekly earnings (excluding large earners) and defining the control group as the complement of the treatment group, I again estimate a negative association between treatment group membership and current weekly wage. In Table 11 column 3, I estimate that all else in the model equal, membership in the treatment group is associated with weekly wages that are $30.36 lower (significant at the 0.1 percent level). Similar to my previous results, I see that this effect switches sign when restricting the control group to workers who switched away from health insurance.

5.2 Discussion

For the majority of my specifications, I produce positive-signed estimates for the DID estimator regardless of the dependent variable, implying that there is a positive association between the Individual Mandate and wage differentials. This estimated association is particularly large in magnitude when the control group is workers switching *from* EPHI jobs to non-EPHI jobs – when including state and industry fixed effects I estimate that in response to the Individual Mandate (all else in the model equal) the mean change in treatment group workers' current weekly wages is $157.63 (or roughly $8,200 in yearly terms) *higher* than changes in workers switching away from EPHI jobs. I find similar signed estimates for my wage changes estimations – for my main specification presented in Table 5, I estimate that ceteris paribus, the mean wage change is $65.12 greater (or $3,400 in yearly terms) for workers switching to EPHI jobs relative to all other workers. I find this association to be robust when eliminating higher earners from my sample, though the smaller magnitude of estimated coefficients suggests that some of the original effect was tied to variation in higher-earning workers' salaries.

One peculiar finding is the negative estimate of the treatment effect found in Table 11. While this finding is consistent with the theory on wage differentials, it is directly opposed by the finding within the same specification that ceteris paribus, the mean change in wage changes in response to the Individual Mandate for workers switching to EPHI jobs was $58.56 higher relative to all other workers in the sample. This finding suggests the need for greater data on worker ability and additional benefits offered by employers, as the two effects are likely biased by these factors.

The consistently positive DID estimates are in opposition to the theory presented in section 2. Similar to previous findings in the literature, this estimate is likely upward biased by unobserved factors that are correlated with the Individual Mandate, such as the provision of other fringe benefits and the forces that drive workers to seek particular types of jobs among job displacement. Given that the presence of additional fringe benefits likely have a

significant effect on my DID estimates, these omitted benefits are also likely to contribute a considerable amount to the bias on worker wage differentials.

6 Conclusion

Omitted measures of individual productivity, as well as firm-level productivity, are likely to bias traditional estimates of wage differentials. These factors are less likely to be correlated with the enactment of policies like the Individual Mandate, though the Individual Mandate is likely to be correlated with the magnitude of additional fringe benefits offered to employees, as well as the motives that encourage workers to pursue jobs with EPHI benefits. In this paper, I consistently produce positive estimates on the association between the Individual Mandate and wage responses of workers switching to EPHI jobs relative to other workers. These results are robust across numerous specifications and sample groups. My findings are likely biased by unobserved variation in additional benefits offered to employees – given the prominence of this bias, unobserved benefits likely have a large confounding effect on estimates of wage differentials that do not rely on policy changes.

Given the prominent role employers have in providing health insurance in health systems across the world, it is essential to understand the extent to which workers are willing to substitute between wages and health insurance, and how policies can influence these substitution preferences. A strong understanding of the efficacy of health policies in encouraging the uptake of health insurance is needed to ensure that effective public health policies can be enacted when required. Our understanding of wage-insurance trade-offs can be enhanced by empirical research – unfortunately, deriving empirical measurements of these trade-offs will remain difficult without more detailed data regarding additional employee benefits available, and researchers' efforts may be better placed on other questions until such data becomes available. The emergence of experimental economics provides a means to circumvent this shortcoming – well designed experiments could elucidate the trade-offs workers place between

insurance and wages, though achieving external validity would be difficult without a large effort to produce a representative sample. A stronger understanding of how benefits vary in response to job changes will pave the way for future research better able to capture the true values of these trade-offs, though this will only be possible through a large effort to make employee benefit data across numerous industries publicly available.

7 Tables

Summary statistics

TABLE 1: Summary statistics for key variables

Variable	Mean	(Std. Dev.)	N
Weekly earnings at current job	777.695	(602.598)	8788
Weekly earnings at lost job	889.501	(601.531)	8788
Age	41.286	(10.981)	8788
Hours worked each week at current job	80.341	(29.15)	8788
Female	0.444	(0.497)	8788
High School	0.593	(0.491)	8788
Bachelor's degree	0.247	(0.431)	8788
Advanced degree	0.098	(0.298)	8788
Married	0.586	(0.493)	8788
Any reported disability	0.018	(0.135)	8788
Survey year 2002	0.158	(0.365)	8788
Survey year 2004	0.175	(0.38)	8788
Survey year 2006	0.132	(0.339)	8788
Survey year 2008	0.116	(0.32)	8788
Survey year 2010	0.133	(0.339)	8788
Survey year 2012	0.118	(0.323)	8788
Survey year 2014	0.09	(0.286)	8788
Survey year 2016	0.077	(0.267)	8788

TABLE 2: Summary statistics for key variables, treatment group only

Variable	Mean	(Std. Dev.)	N
Weekly earnings at current job	666.168	(496.183)	1609
Weekly earnings at lost job	723.006	(518.018)	1609
Age	40.306	(11.425)	1609
Hours worked each week at current job	78.727	(30.188)	1609
Female	0.508	(0.5)	1609
High School	0.633	(0.482)	1609
Bachelor's degree	0.246	(0.431)	1609
Advanced degree	0.075	(0.264)	1609
Married	0.666	(0.472)	1609
Any reported disability	0.018	(0.133)	1609
Survey year 2002	0.154	(0.361)	1609
Survey year 2004	0.151	(0.358)	1609
Survey year 2006	0.131	(0.338)	1609
Survey year 2008	0.106	(0.307)	1609
Survey year 2010	0.134	(0.34)	1609
Survey year 2012	0.11	(0.313)	1609
Survey year 2014	0.11	(0.313)	1609
Survey year 2016	0.104	(0.306)	1609

TABLE 3: Summary statistics for key variables, control group only

Variable	Mean	(Std. Dev.)	N
Weekly earnings at current job	802.691	(621.253)	7179
Weekly earnings at lost job	926.817	(612.559)	7179
Age	41.506	(10.868)	7179
Hours worked each week at current job	80.703	(28.902)	7179
Female	0.43	(0.495)	7179
High School	0.584	(0.493)	7179
Bachelor's degree	0.248	(0.432)	7179
Advanced degree	0.103	(0.305)	7179
Married	0.568	(0.495)	7179
Any reported disability	0.019	(0.135)	7179
Survey year 2002	0.159	(0.366)	7179
Survey year 2004	0.18	(0.384)	7179
Survey year 2006	0.133	(0.339)	7179
Survey year 2008	0.119	(0.323)	7179
Survey year 2010	0.132	(0.339)	7179
Survey year 2012	0.12	(0.325)	7179
Survey year 2014	0.086	(0.28)	7179
Survey year 2016	0.071	(0.258)	7179

TABLE 4: Cross-tabulation of insurance status at current and prior jobs

	Have health insurance now		
Had health insurance at lost job	No	Yes	Total
No	1,237	1,609	2,846
Yes	1,146	4,796	5,942
Total	2,383	6,405	8,788

Source: CPS Displaced Workers Supplement, author's calculations

Regressions

	(1)	(2)	(3)
	Current minus prior weekly wage		
Treat times mandate	64.6409*	67.6609*	65.1184*
	(32.4960)	(32.6537)	(32.7419)
Treat	45.4968**	37.1375*	36.6738*
	(14.6920)	(14.8569)	(14.9081)
Mandate	22.4163	24.2651	25.6786
	(18.9192)	(19.1542)	(19.2320)
Female	2.9760	-10.2302	-8.3673
	(10.3234)	(11.0577)	(11.0968)
Age	-5.5598***	-5.5495***	-5.5912***
	(0.4724)	(0.4799)	(0.4836)
High School	-35.8991	-30.6443	-28.9574
	(21.3489)	(21.6882)	(21.8012)
Bachelor's degree	-59.6527**	-53.2495*	-51.2960*
	(22.7042)	(23.6438)	(23.7568)
Advanced degree	-77.7894**	-75.0951**	-73.5851**
	(25.9717)	(27.1425)	(27.2770)
Any reported disability	-29.0752	-30.0779	-32.1107
	(37.8295)	(37.9666)	(38.0691)
In a labor union	40.7100***	39.3103***	39.4995***
	(10.7447)	(10.8020)	(10.8313)
Married	3.5187	6.2759	8.0066
	(10.6524)	(10.7336)	(10.8183)
CPI	-0.5936	-0.7247*	-0.7621*
	(0.3201)	(0.3411)	(0.3426)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	8788	8788	8788
R^2	0.024	0.037	0.043

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 5: OLS estimates for change between current and prior weekly earnings. All workers included.

	(1)	(2)	(3)
	Current minus prior weekly wage		
Treat times mandate	228.8299***	229.7065***	223.8405***
	(48.0529)	(48.6589)	(48.9153)
Treat	122.6010***	112.8059***	107.5354***
	(20.3010)	(20.9187)	(21.0788)
Mandate	-108.3020*	-94.5458*	-91.6496*
	(43.2896)	(44.0265)	(44.1743)
Female	-13.5097	-20.7704	-18.1770
	(17.9364)	(19.5812)	(19.6403)
Age	-4.0990***	-4.2945***	-4.5006***
	(0.8124)	(0.8355)	(0.8474)
High School	-64.5488	-60.7883	-61.2551
	(36.4738)	(37.3998)	(37.6903)
Bachelor's degree	-100.9811*	-101.1825*	-104.5540*
	(39.8504)	(41.6443)	(41.9611)
Advanced degree	-124.5852*	-113.5082*	-127.6141*
	(48.7199)	(51.1863)	(51.6401)
Any reported disability	-171.9025**	-152.7327*	-152.4278*
	(62.6707)	(63.4730)	(63.8814)
In a labor union	52.2045**	46.2365*	46.6267*
	(19.8868)	(20.2160)	(20.3650)
Married	-1.2990	1.9170	8.9051
	(18.9462)	(19.2529)	(19.4430)
CPI	-1.3865*	-1.7288**	-1.8255**
	(0.5626)	(0.6057)	(0.6102)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	2755	2755	2755
R^2	0.058	0.090	0.114

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 6: OLS estimates for difference between current and prior weekly earnings. Here, the control group is for individuals who switched from an EPHI job to a non EPHI job.

| | (1) | (2) | (3) |
	Weekly earnings at current job		
Treat times mandate	-10.9195	3.4999	-0.7390
	(36.2832)	(35.7179)	(35.4386)
Treat	-116.4332***	-92.2679***	-89.3343***
	(16.4043)	(16.2510)	(16.1360)
Mandate	8.8154	2.7609	6.7492
	(21.1241)	(20.9516)	(20.8159)
Female	-207.2419***	-194.9704***	-194.4939***
	(11.5265)	(12.0953)	(12.0108)
Age	3.5079***	3.3404***	3.1835***
	(0.5274)	(0.5249)	(0.5234)
High School	198.6017***	160.1025***	176.9015***
	(23.8370)	(23.7235)	(23.5968)
Bachelor's degree	572.2114***	503.8641***	505.9616***
	(25.3502)	(25.8625)	(25.7134)
Advanced degree	843.0383***	790.1853***	782.2412***
	(28.9986)	(29.6896)	(29.5235)
Any reported disability	-157.7148***	-144.3658***	-149.4722***
	(42.2383)	(41.5294)	(41.2045)
In a labor union	21.6650	22.2119	20.7966
	(11.9969)	(11.8156)	(11.7233)
Married	106.3769***	100.2219***	105.8924***
	(11.8939)	(11.7408)	(11.7093)
CPI	2.9019***	3.3108***	3.1783***
	(0.3574)	(0.3731)	(0.3708)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	8788	8788	8788
R^2	0.237	0.277	0.297

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 7: OLS estimates for difference current weekly earnings. All workers included.

| | (1) | (2) | (3) |
	Weekly earnings at current job		
Treat times mandate	151.3361**	155.0858**	157.6325***
	(47.2065)	(47.3418)	(47.2237)
Treat	33.8300	41.8543*	36.1554
	(19.9434)	(20.3525)	(20.3498)
Mandate	-119.3705**	-119.4156**	-116.1420**
	(42.5270)	(42.8349)	(42.6466)
Female	-194.6300***	-169.8538***	-170.1169***
	(17.6204)	(19.0512)	(18.9611)
Age	2.1708**	2.0037*	2.0699*
	(0.7981)	(0.8128)	(0.8181)
High School	99.2351**	67.5305	82.5481*
	(35.8314)	(36.3875)	(36.3868)
Bachelor's degree	352.8561***	317.9864***	321.1292***
	(39.1484)	(40.5171)	(40.5099)
Advanced degree	510.6822***	483.6820***	460.5479***
	(47.8617)	(49.8008)	(49.8542)
Any reported disability	-197.5064**	-194.8214**	-195.8387**
	(61.5667)	(61.7549)	(61.6721)
In a labor union	12.7292	12.8054	18.4258
	(19.5365)	(19.6688)	(19.6607)
Married	84.2691***	83.0259***	89.1719***
	(18.6125)	(18.7317)	(18.7706)
CPI	2.2768***	2.6202***	2.3749***
	(0.5527)	(0.5893)	(0.5891)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	2755	2755	2755
R^2	0.160	0.205	0.238

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 8: OLS estimates for current weekly earnings. Here, the control group is defined as individuals who switched from an EPHI job to a non EPHI job.
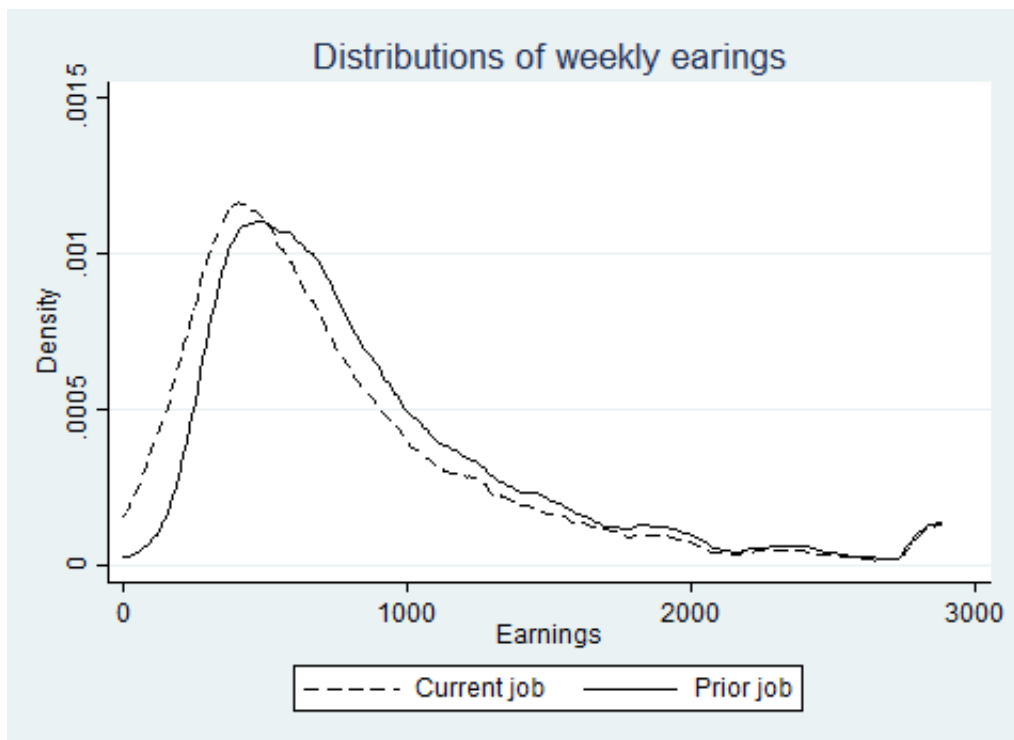
Figures

FIGURE 1: Distributions of current and prior job weekly earnings for all workers in sample.
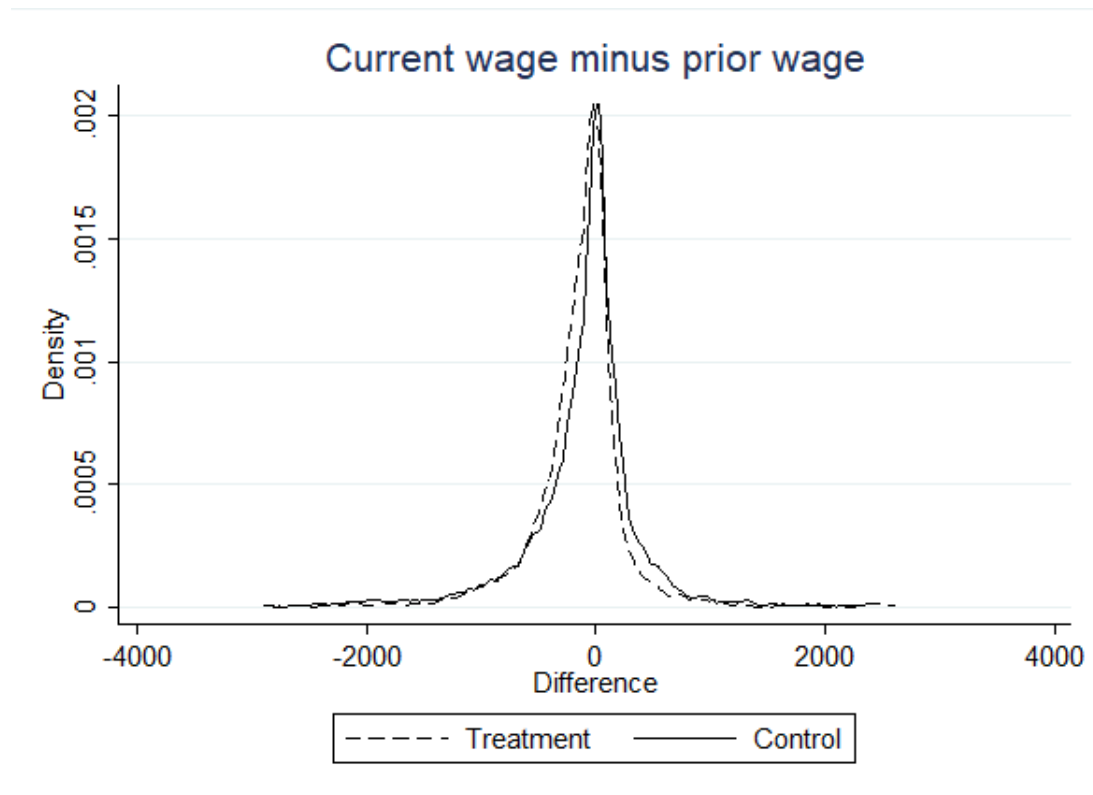
FIGURE 2: Distribution of current minus prior wage for treatment (workers switching from a non-EPHI to an EPHI job) and control groups. All sample years included.
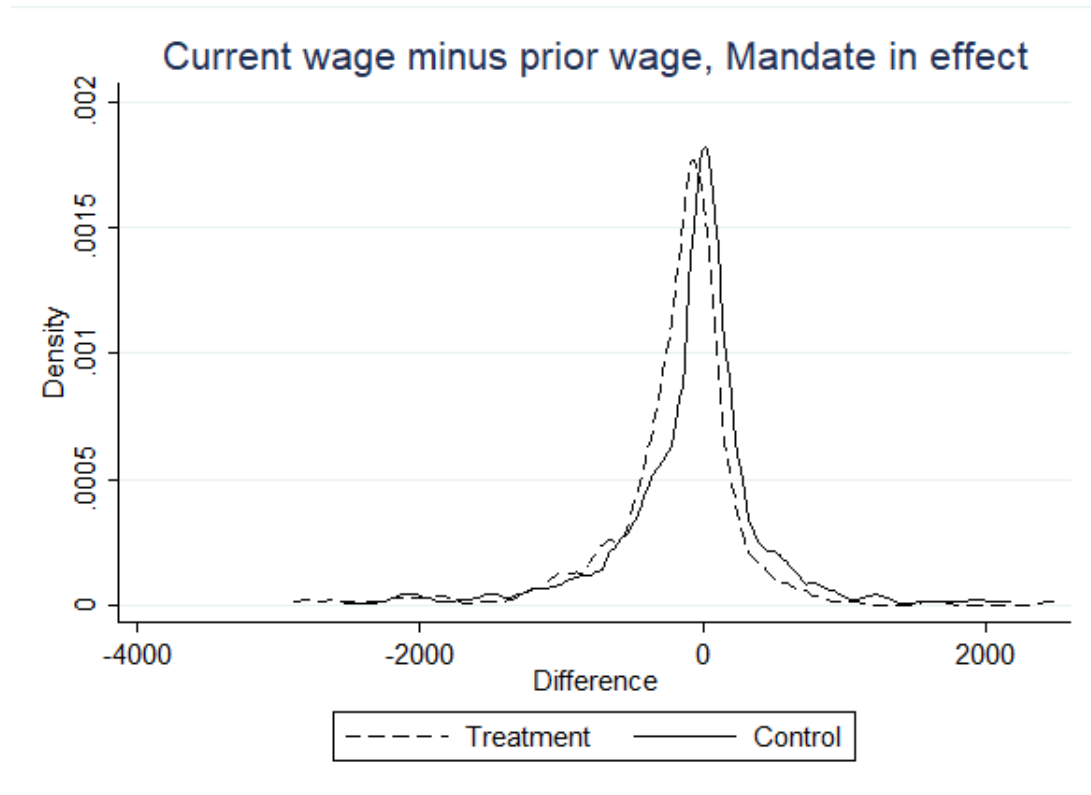
FIGURE 3: Distribution of current minus prior wage for treatment (workers switching from a non-EPHI to an EPHI job) and control groups. Only sample years 2014 and 2016, the years for which the Individual Mandate was in effect, are included.

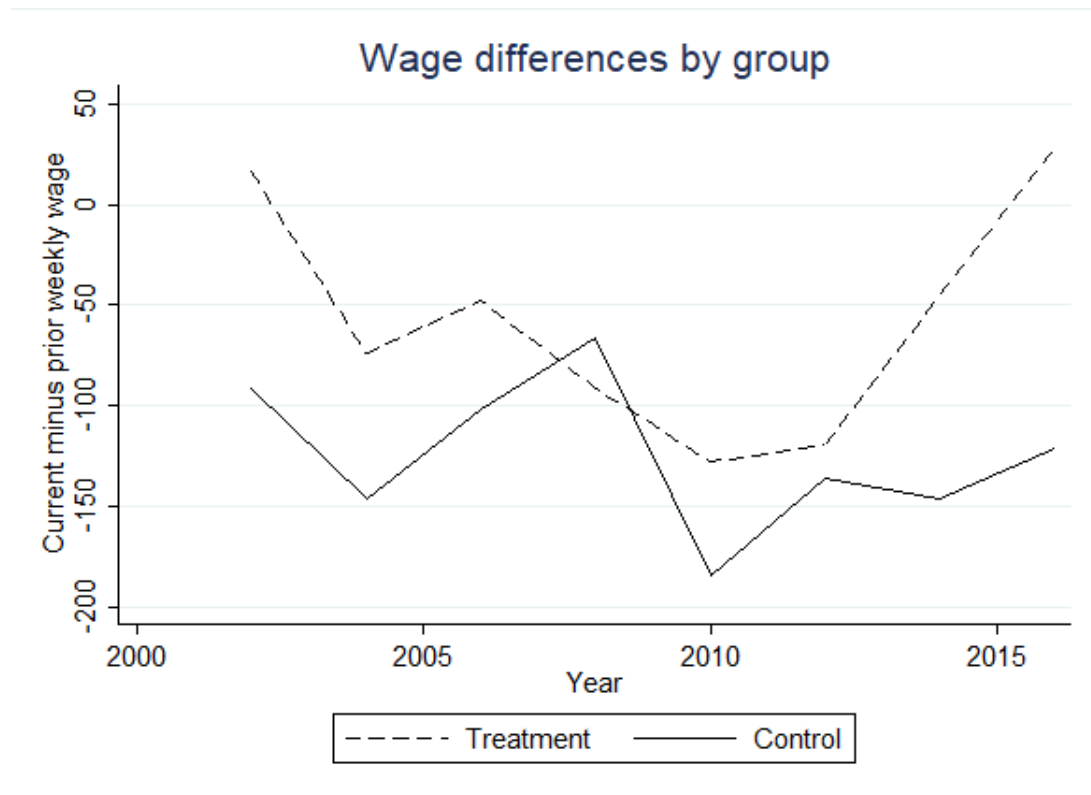FIGURE 4: Checking the parallel trends assumption for worker weekly wages reported at time of survey.

FIGURE 5: Checking the parallel trends assumption for differences in weekly wage at time of survey and weekly wage at previous job.

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Appendix: Robustness Regressions

	(1)	(2)	(3)
	Current minus prior weekly earnings		
Treat times mandate	64.6115**	65.5189**	64.2833**
	(19.8372)	(19.9587)	(20.0382)
Treat	48.1812***	44.2132***	44.4039***
	(8.7236)	(8.8336)	(8.8758)
Mandate	13.8873	17.2881	18.3447
	(12.4670)	(12.6259)	(12.6883)
Female	-1.7458	-10.4109	-9.3577
	(6.3877)	(6.8940)	(6.9292)
Age	-2.8371***	-2.8270***	-2.8474***
	(0.2873)	(0.2921)	(0.2950)
High School	-19.1465	-15.2104	-14.8979
	(11.8906)	(12.1050)	(12.2119)
Bachelor's degree	-15.5349	-13.7476	-13.6531
	(13.4412)	(14.0076)	(14.1163)
Advanced degree	-27.2932	-28.8646	-30.5441
	(18.0521)	(18.8715)	(19.0338)
Any reported disability	-11.2888	-9.2812	-6.9068
	(23.2882)	(23.3965)	(23.5035)
In a labor union	17.9917**	17.7293**	18.9976**
	(6.6375)	(6.6842)	(6.7234)
Married	-11.0119	-9.8222	-8.4005
	(6.5385)	(6.6024)	(6.6663)
CPI	-0.5124**	-0.7230***	-0.7406***
	(0.1985)	(0.2130)	(0.2144)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	6502	6502	6502
R^2	0.031	0.048	0.055

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 9: OLS estimates for difference between current and prior weekly earnings, excluding individuals earning more than \$1,200 weekly either at current or prior job.

	(1) wage_diff	(2) wage_diff	(3) wage_diff
Treat times mandate	99.2194**	93.0293**	95.7277**
	(30.6013)	(30.9479)	(31.3414)
Treat	136.2013***	139.1078***	137.8518***
	(12.6576)	(13.1098)	(13.3185)
Mandate	10.8092	19.2357	17.1800
	(27.7129)	(28.1847)	(28.4649)
Female	-19.0422	-23.4940	-22.3700
	(11.2224)	(12.1929)	(12.3337)
Age	-2.9010***	-3.0578***	-3.0820***
	(0.5001)	(0.5155)	(0.5273)
High School	-40.4550	-37.1321	-37.2534
	(21.3926)	(21.9236)	(22.2879)
Bachelor's degree	-45.6066	-50.7124*	-53.0428*
	(24.3259)	(25.3445)	(25.7427)
Advanced degree	-58.0230	-62.6448	-66.6193
	(32.9501)	(34.4426)	(34.9950)
Any reported disability	-34.6376	-25.4076	-23.9102
	(40.2899)	(40.7627)	(41.3390)
In a labor union	32.4037**	32.7399**	35.1051**
	(12.2316)	(12.4063)	(12.6004)
Married	-12.7927	-12.7707	-10.8923
	(11.7178)	(11.8940)	(12.0925)
CPI	-1.3234***	-1.5199***	-1.5608***
	(0.3503)	(0.3776)	(0.3841)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	2300	2300	2300
R^2	0.105	0.149	0.165

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 10: OLS estimates for difference between current and prior weekly earnings, excluding workers earning more than $1,200 weekly in either current or prior job. Here, the control group is defined as individuals who switched from an EPHI job to a non EPHI job.

	(1)	(2)	(3)
	Weekly earnings at current job		
Treat times mandate	61.9500**	64.0507**	58.5567**
	(19.8270)	(19.7345)	(19.6915)
Treat	-39.2604***	-31.7841***	-30.3589***
	(8.7191)	(8.7343)	(8.7222)
Mandate	-3.9614	-4.0013	-0.9749
	(12.4606)	(12.4840)	(12.4687)
Female	-76.8241***	-70.6042***	-70.9255***
	(6.3844)	(6.8165)	(6.8093)
Age	0.7760**	0.7269*	0.6775*
	(0.2872)	(0.2888)	(0.2899)
High School	118.1298***	104.1116***	107.5005***
	(11.8845)	(11.9689)	(12.0006)
Bachelor's degree	236.7106***	220.2864***	221.0868***
	(13.4343)	(13.8503)	(13.8720)
Advanced degree	309.1973***	300.4409***	293.1522***
	(18.0429)	(18.6595)	(18.7045)
Any reported disability	-67.0321**	-67.6693**	-68.6917**
	(23.2762)	(23.1336)	(23.0967)
In a labor union	19.5121**	19.9124**	19.8854**
	(6.6341)	(6.6090)	(6.6070)
Married	39.3396***	36.3605***	40.3688***
	(6.5351)	(6.5282)	(6.5509)
CPI	0.8953***	0.9470***	0.9071***
	(0.1984)	(0.2106)	(0.2107)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	6502	6502	6502
R^2	0.105	0.139	0.156

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 11: OLS estimates for difference current weekly earnings, excluding workers earning more than $1,200 weekly in either current or prior job.

	(1)	(2)	(3)
	Weekly earnings at current job		
Treat times mandate	72.8478**	75.2910**	74.4537**
	(27.9172)	(28.0967)	(28.1631)
Treat	43.3137***	47.0111***	47.8035***
	(11.5474)	(11.9020)	(11.9678)
Mandate	-7.8554	-1.9180	-2.0629
	(25.2822)	(25.5880)	(25.5783)
Female	-93.8745***	-79.4493***	-83.3483***
	(10.2381)	(11.0696)	(11.0830)
Age	-0.4117	-0.6690	-0.6966
	(0.4563)	(0.4680)	(0.4738)
High School	54.9129**	41.7844*	49.2503*
	(19.5163)	(19.9038)	(20.0277)
Bachelor's degree	128.7537***	120.4456***	120.1886***
	(22.1922)	(23.0095)	(23.1322)
Advanced degree	210.7867***	209.6054***	193.2649***
	(30.0600)	(31.2693)	(31.4462)
Any reported disability	-90.0464*	-95.1733*	-88.3128*
	(36.7561)	(37.0072)	(37.1469)
In a labor union	28.8545**	29.5906**	33.2866**
	(11.1588)	(11.2633)	(11.3226)
Married	49.4349***	47.1620***	50.5462***
	(10.6900)	(10.7982)	(10.8662)
CPI	0.9314**	0.8363*	0.6787*
	(0.3196)	(0.3428)	(0.3451)
Industry Fixed Effects	No	Yes	Yes
State Fixed Effects	No	No	Yes
Observations	2300	2300	2300
R^2	0.113	0.165	0.197

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 12: OLS estimates for current weekly earnings, excluding workers earning more than \$1,200 weekly in either current or prior job. Here, the control group is defined as individuals who switched from an EPHI job to a non EPHI job.