Are the Costs of Employer-Sponsored Health Insurance Passed on to Workers at the Individual Level?

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Abstract

This paper uses the Affordable Care Act's employer mandate to examine how the cost of employer-sponsored health insurance (ESI) is shared among workers. The mandate provides identification because ESI is experience rated. Estimates, based upon data from the Medical Expenditure Panel Survey (MEPS), suggest that, for each \$1 difference in medical expenditures, the employer mandate is associated with between a \$0.37 and \$0.52 relative decline in wages compared to before the mandate. Robustness checks show that only workers who work for employers who are affected by the employer mandate experience this wage decline and that the decline cannot be explained by demographic characteristics that correlate with medical expenditures.

Keywords: Wages, Medical Expenditures, Employer-sponsored Health Insurance, ESI

JEL Codes: I13, J23, J24, J31, J32, J33

1 Introduction

In the United States, experience-rating ensures that the cost of employer-sponsored health insurance (ESI) reflects the actual medical expenditures of employees.[1] Experience rating therefore creates a cost-wedge between otherwise-similar workers wherever ESI is offered - and a reason to cherry-pick employees with lower medical expenditures - unless wages are free to adjust for differences in medical expenditures. Given the incentives facing employers, many researchers have examined if medical expenditure cost differences are shifted onto workers via reduced wages and/or diminished employment prospects (including Gruber, 1993; Sheiner, 1999; Jensen and Morrisey, 2001; Levy and Feldman, 2001; Bhattacharya and Bundorf, 2009; Cowan and Schwab, 2011, 2016; Lahey, 2012; Bailey, 2013, 2014; Lennon, 2018, 2019). The literature finds that, all else equal, well-defined groups who have higher expected medical expenditures earn lower wages because of ESI.

What remains unclear from these studies is the level at which cost-shifting occurs. Is it only at the group level? Or - given variation in health status across individuals - can employers shift some or all of the cost of ESI onto those particular workers who have greater medical expenditures? As an example, Gruber (1993) finds that relative wages for "females of child-bearing age" fell by the expected cost of mandated maternity coverage. However, Gruber could not determine if the wage offset was largest for those who subsequently had fertility events compared to those that did not. That is, Gruber's (and others') findings are consistent with both a \$x difference in wages for each of n individuals within a group and a \$0 difference for some fraction p of the group and a $(^{\$x}/_{1-p}) \times n$ difference in wages for the remaining $1 - p$.

The level at which cost-shifting occurs matters because cost-shifting at the individual level would undermine the supposed risk-pooling benefits of ESI. Groups of workers are considered ideal insurance pools because they limit the ability of (1) insurers to screen and (2) individuals to seek or change coverage only when they require care.[2] However, if employers face premiums that are sensitive to their employees' expenditures, ESI simply transfers the incentive to screen from the

[1] This is true whether an employer chooses to self-insure or to cover their employees via traditional insurance plans. Self-insured employers hire a third-party administrator to manage the employer's plan(s) but pay the medical claims of their employees directly. Some of the cost can be passed on to workers in the form of employee contributions. However, under the Health Insurance Portability and Accountability Act those contributions cannot vary across employees even if they have different medical expenditures.

[2] In other words, ESI mitigates the asymmetric information problems in health insurance markets detailed by Arrow (1963).

insurance company to the employer. If employers can shift the cost of ESI onto individual workers via lower wages then ESI ceases to function as insurance. On one hand, this seems trivial: workers simply pay for ESI via lower wages. On the other hand, ESI could create barriers to employment for workers whose total compensation (wages plus benefits such as ESI) exceeds the value of their marginal product.[3]

However, employers can only perfectly shift costs onto workers (at the individual level) if they can accurately predict future medical expenditures for each worker. The existence of insurance suggests that is not possible - for employers, insurers, or individuals.[4] Moreover, laws regarding the protection of health information prevent employers from observing the expenses incurred by specific workers.[5] However, many indicators of health and potential medical expenditures are easily observed. At a job interview, physical injuries or impairments, obesity status, the odor or other characteristics of tobacco use, and perhaps conditions such as asthma or emphysema, among others, would be observable to a potential employer. Given employment is generally an ongoing arrangement, other medical issues could become apparent with repeated interaction between the employer and employee (including information gleaned from changes in daily appearance, time absent due to illness, and so on). If employers are profit-maximizers, and can identify at least some of the workers who are consistently adding to the cost of ESI for the employer, then, all else equal, there should be a negative correlation between individual medical expenditures and wages in labor market data, even after accounting for differences in expenditures among groups.

Using this logic, Levy and Feldman (2001) ask if ESI causes individual workers with higher medical expenditures to earn lower wages. They note that the main threat to identification is that workers who use less medical care might be systematically more productive, either innately or via reduced absenteeism. To get around this, Levy and Feldman focus only on those who change jobs in an individual fixed-effects framework. Despite what theory would predict, they find little evidence of individual-specific cost-shifting. However, they note that the decision to switch to or

[3] Illustrating such a trade-off, Marks (2011) found that minimum wage increases lead to fewer workers being offered ESI.

[4] Perfect foresight would render insurance infeasible.

[5] Insurance companies do produce reports (for their customer, the employer) about the dates and times of various medical procedures and the associated costs. These reports do not contain identifying information but there is evidence to suggest employers can connect the dots, even in very large employers. As one example, AOL CEO Tim Armstrong caused a firestorm on social media in 2014 when he blamed changes in employee compensation on medical costs incurred by just two employees (out of about 5,000 employees). More on this here.

from an employer that offers ESI is likely to be endogenous: “[w]e attribute our failure to find useful results to the absence of exogenous variation in health insurance status; those who gain or lose health insurance are almost certainly experiencing other productivity-related changes that render our fixed-effects identification strategy invalid.” Levy and Feldman note that clean identification would require exogenous variation in insurance coverage.

The Affordable Care Act (ACA) provides such exogenous variation via the employer mandate. Enacted in March of 2010, the employer mandate requires employers with more than 50 full-time equivalent (FTE) employees to provide ESI to employees who work more than 29 hours per week from 2014 onward. Using that variation, this paper seeks to address whether individuals with higher medical expenditures experience relatively lower wages after the ACA's employer mandate is announced. The mandate provides identification because it forces affected employers (defined as those required to provide ESI because of the mandate) to consider the cost of employee medical expenditures when determining compensation, for the first time.[6]

Specifically, to examine if employers shift the cost of health insurance onto workers at the individual level, the paper pairs the employer mandate as a source of exogenous variation with data from the 2006-2014 waves of the Medical Expenditure Panel Survey (MEPS). The paper's main estimates focus on how wages change for workers at affected employers - those that (1) do not offer ESI prior to the employer mandate's implementation and (2) have more than 50 FTEs - and rely on difference-in-difference (comparing wages before and after the employer mandate's implementation for workers with varying medical expenditures) and triple-difference (adding an indicator for ESI/no-ESI prior to the mandate's implementation) approaches. As a baseline, and to establish that the mandate had bite, the paper's estimates first show that - after the employer mandate is announced - wages for all workers at affected employers decline. This baseline finding is interesting in its own right but adds little to the existing literature on how ESI affects wages (see Kolstad and Kowalski, 2016, for example). Instead, this paper's main contribution is to highlight that the relative decline in wages is greater for individuals with larger medical expenditures. In particular, the paper's estimates suggest that between 37 and 52 cents of every $1 difference in current medical expenditures is shifted onto workers at the individual level, depending on specification.

[6]As Levy and Feldman note, workers with lower medical expenditures might be more productive. However, this does not threaten identification because any effect of health on productivity should be incorporated in wages - before the mandate was announced.

The identifying assumption is that nothing else affects the wages of workers with varying medical expenditures differently over this time period. The observed effects are large given current medical expenditures are only an indicator of future medical expenditures (see Section 5 for more on this point), employees would pay some of their own expenses via deductibles and co-insurance, and that employee medical expenditures are a tax deduction for employers.

Several researchers have used similar approaches to identify the effect medical expenditures have on wages for various groups but the employer mandate is unique. First, the mandate is at the federal rather than state level which side-steps data limitations that have precluded studying individual-level cost-shifting in the past. This is not a criticism of existing work on this topic, using state level mandates to identify individual-specific wage offsets would slice MEPS data too thinly. On the other hand, only MEPS gathers the necessary information on individual medical expenditures, ESI status, and wages to tackle the question. Second, the mandate represents a significant extensive margin change requiring ESI to be provided where none was offered before rather than a small change in what is covered where ESI is already in place. Third, neither workers nor employers are choosing to obtain ESI resolving both the confounding endogeneity and productivity issues noted by Levy and Feldman.

A major caveat is that the observed effects are anticipatory. Existing studies show that ESI will be paid for via lower wages. Given employers had more than three years notice about the employer mandate the question becomes: when will the wage offsets occur? If the market for labor were akin to a spot market, then the effect of the mandate should not be observed until the day ESI is offered. However, if the labor market is not a spot market, the announcement of the mandate should immediately alter an employer's willingness to pay for various workers. Based on a similar argument, Garrett and Kaestner (2015), Mathur et al. (2016), and Even and MacPherson (2018) consider how the employer mandate affected part-time employment before 2014. Like this paper, they focus on that period because employers should be forward-looking and data from 2014 onward could be difficult to interpret (due to other potentially-confounding ACA provisions). Wherever employment is an ongoing arrangement, if employers can infer who will be costly to cover (and are aware of the mandate) a negative relationship between medical expenditures and wages should emerge after the announcement (March of 2010) rather than the implementation (January of 2014) of the mandate. Importantly, this negative relationship between wages and medical expenditures emerges without ESI being in place. Further increasing the incentive to be forward-looking, the cost (to employers) of ESI for 2014 was to be based on the expected costs of each firm's employee pool in 2013.[7] As a result, only changes made prior to the end of 2012 would help to reduce the cost of ESI in 2014.

With that said, timing issues remain. Specifically, the paper's estimates suggest that employers reduce their demand for, and wages paid to, workers with greater current medical expenditures shortly before having to offer ESI. Timing concerns are mitigated in two ways: (1) current medical expenditures are a strong predictor of future expenditures (see Section 5) and (2) errors in judgment made by an employer amount to a classical measurement error problem and bias estimates towards zero.[8] Robustness checks provide further confidence that the observed effects are causally-related to the employer mandate. First, these checks show that the observed effects on wages are largest at smaller employers (among those affected by the mandate). Intuitively, it is these smaller employers who should be able to closely monitor their employees and determine likely differences in medical expenditures. Second, there is no observed change in the relationship between wages and individual medical expenditures for workers who are employed at similar employers that already provide ESI. Third, there is no observed change in wages for workers at employers who do not have enough employees (less than 50 FTEs) to be covered by the employer mandate. Last, the observed effects persist when controlling for differences in medical expenditures between groups (such as groups defined by age, race, level of education, gender, and so on) after the mandate is announced.

It is worth emphasizing that the paper does not rely on employers observing actual medical expenditures or inferring them correctly. Instead, the empirical question is: to what degree can employers infer and respond to individual medical expenditures, given an incentive to do so? To answer that question, this paper illustrates a clear relationship between individual medical expenditures and relative wages in labor market survey data, that emerges only for workers at employers required to offer ESI because of the employer mandate and only after those employers are informed of a new ESI requirement. These relative wage differences persist even after controlling for differences in expected medical expenditures that are associated with characteristics observable to

[7] See Section 2 and 4 for more on this.

[8] That is, while employers are setting their willingness to pay for labor based on an expectation of future medical expenditures that is different from the current medical expenditures observed in the MEPS data, so long as they are right on average, then OLS estimates are biased towards zero.

the researcher and the employer, such as gender, age, and so on. The paper's findings suggests that employers can infer enough information to know who is adding to the cost of ESI for the employer at or close to the individual level. If the cost of ESI continues to outpace inflation, the incentives to monitor and respond to medical expenditure differences among workers will become stronger, among all employers.

Section 2 highlights how this paper contributes to the existing literature on mandated employment benefits. Section 3 explains the implementation of the employer mandate and details the identification strategy it allows. The data used to produce empirical estimates is described and summarized in Section 4. Section 5 presents the main estimates. Section 6 examines the robustness of these estimates. Section 7 concludes.

2 Contribution to Existing Work on Wages and ESI

Summers (1989) provides a succinct analysis of the economics of mandated benefits, highlighting the ways in which they are similar to payroll taxes, where they differ, and why that makes them politically popular. Summers was concerned that mandated benefits could lead to exclusionary hiring practices if wages were not free to adjust for the cost of the benefit employers were forced to provide. If a mandated benefit resulted in such behavior Summers saw value in public provision of the benefit: "publicly provided benefits do not drive a wedge between the marginal costs of hiring different workers and so do not give rise to a distortion of this kind." The paper sparked a wave of research into the empirical regularities of employment conditions and benefits including Gruber and Krueger (1991), Gruber (1993, 1994), Acemoglu and Angrist (2001), Baicker and Chandra (2006), Baicker and Levy (2008), Lahey (2012), Bailey (2013, 2014), and Cowan and Schwab (2011, 2016).

The canonical paper in this area is Gruber (1993). Gruber studies state-level maternity benefit mandates that require employers in those states to provide maternity coverage if they offer ESI to their employees. Gruber focused on those likely to benefit from these mandates and found that wages fell for these groups relative to the same groups in states that did not mandate maternity coverage. Gruber's analysis of the consequences of maternity benefits is the type of work Summers suggested would be valuable. However, Gruber's data does not allow him to examine if those who have multiple or complicated births face larger wage reductions. Other authors, such as Bailey and

Lahey, encounter the same kind of limitation when studying diabetes, prostate-cancer screening, and infertility mandates. They could circumvent these issues by using Medical Expenditure Panel Survey (MEPS) data. The MEPS collects data on individual medical usage and expenditures along with labor market and health insurance information. However, any paper that relies on variation in only a handful of states at different times would slice the MEPS data very thinly. Additionally, MEPS in its current form only stretches back to 1996, several years after the mandates studied by Gruber and others.

Note that the literature tends to focus on how ESI affects wages. Intuitively, wage effects can capture the various ways in which employers could react in order to minimize their exposure to the costs of employee benefits. For example, for employees who leave or are terminated, new hires who replace them may be paid the same wage but have lower medical expenditures, those who are not "healthier" might be hired at a lower wage, and wage increases and promotions for existing employees may be biased towards employees the employer would like to remain at the employer, absent any quality or productivity differences. Wage effects could also be caused by extensive margin (hiring/firing) changes even if there are no observable effects on unemployment rates or duration: workers with higher medical expenditures might be fired and then hired elsewhere at a slightly lower wage. In such a case, employment rates for workers with higher medical expenditures might be unchanged but wages are lower.[9]

Most of the work in this area focuses on changes in the generosity of existing (and optional) coverage. However, Thurston (1997) and Buchmueller et al. (2011) examine the case of Hawaii after the state mandated ESI for many workers in 1974. Both find relative wages fall for workers who obtained ESI. Kolstad and Kowalski (2016) examine the effects of the 2006 health care reform in Massachusetts and find that wages at employers who were required to provide ESI fall by approximately the cost of coverage. In each case, the data precludes an analysis of the distribution of wage reductions as a function of individual medical expenditures. Only Levy and Feldman (2001) attempt to address the issue of individual-specific cost-shifting by estimating wage change

[9]Examining non-wage outcomes is challenging using MEPS as it was not intended to be a comprehensive survey of labor market dynamics. As a result, the sample size, while perfectly adequate for providing an overview of medical expenditures and health in the population, becomes a problem when focused on only those aged 27-55 who work at employers with more than 50 employees and are not already offered ESI. In particular, examining non-wage outcomes such as job switching or unemployment duration for this subset of the sample would rely on just a few dozen observations per year.

regressions using data from the 1996 Medical Expenditure Panel Survey. However, as mentioned earlier, their identification strategy ensures endogeneity problems.[10]

Note that Levy (1998) examines an additional important avenue for cost-shifting, employee contributions towards the cost of coverage. Levy finds that worker contributions play an important role in employee sorting and provide employer flexibility to tailor benefit packages to match workers' preferences. Unfortunately, the role of employee contributions cannot be examined in this paper as the focus is on workers at employers where insurance is newly-mandated but not yet in place. In any case, employee contributions cannot vary across individuals for the same coverage due to anti-discrimination laws. For that reason, the incentive to shift the cost of medical expenditures onto workers with greater medical expenditures persists.

Overall, prior studies of the incidence of mandated benefits have been limited by data availability and suitability. The provisions of the Affordable Care Act, in conjunction with the data provided by the Medical Expenditure Panel Survey, solves identification issues and provides a clearer method to analyze how mandated health coverage affects individual labor market outcomes. The next section details the paper's empirical strategy.

3 Empirical Strategy

Economic theory predicts that workers, rather than employers, will bear the costs of employment benefits such as ESI. Conceptually, following Bhattacharya and Bundorf (2009), in a competitive labor market where wages are the only form of compensation, the wage of worker i, w_i, will equal the value of her marginal product (MRP). In such a world, if ESI is mandated, wages would have to be modified by the cost of coverage. Suppose that premiums are actuarially fair and health expenditures vary across workers. A worker with medical expenditures e_i will add premium $p_{ik} = e_i$ to the cost of ESI at firm k. Employers could pool all costs across their N employees so that wages for worker i at firm k are

$$w_{ik} = MRP_{ik} - \frac{1}{N}\Sigma_{i=1}^{N} e_i.$$

[10] Pauly and Herring (1999), using the 1987 National Medical Expenditure Survey, claim to address the question of whether there is individual-specific cost-shifting. However, their terminology is loose. Their finding is still a group offset, not an individual-specific offset.

However, this cannot be an equilibrium. In a competitive labor market, it leaves significant arbitrage opportunities open for workers and employers. On the other hand, authors have generally assumed the incidence of premiums cannot be individual specific. If they were, wages for each worker would be

$$w_{ik} = MRP_{ik} - p_{ik} = MRP_i - e_i. \tag{1}$$

Most strikingly, a system resembling Equation 1 cannot be called insurance. Instead, authors have hypothesized that employers are capable of adjusting wages at the group level. That is, suppose N employees can be partitioned into m subgroups, $m \leq N$. For $i \in n_j \in N$, $j = 1, 2, ..., m$, wages (excusing the abuse of notation) for worker i would be

$$w_{ijk} = MRP_{ijk} - \frac{1}{n_j}\Sigma_{i=1}^{n_j} e_{ijk}. \tag{2}$$

This is potentially an equilibrium if workers (and employers) do not find it profitable to incur the costs of exploiting any remaining opportunities for arbitrage. It is straightforward to make the same predictions in a general equilibrium labor market search model built upon the work of Mortensen (1990) and Bowlus and Eckstein (2002). A general equilibrium model would see the requirement to provide ESI work like a variable tax that affects some workers (high medical expenditures) but not others (low medical expenditures). This is akin to a leftward demand curve shift for workers with higher medical expenditure and causes reduced equilibrium wages, a lower level of employment, and/or longer periods of job search for those workers across the economy. While the model would predict reduced wages for workers with higher medical expenditures across all types of employers the effects are concentrated at employers who did not already offer coverage.[11]

Given these theoretical predictions, the paper's empirical analysis examines if wages are responsive to differences in individual medical expenditures, as opposed to only group level differences, after ESI is mandated as one of the provisions of the ACA. Employers could ignore the mandate but would incur a penalty for not providing ESI of $2,000 per employee excluding the first 30 employees. Given empirical evidence shows employers can and do pass on the cost of ESI to their employees (at least as a group), it would make little sense to pay a $2,000 penalty per employee when employers

[11]Intuitively, the reduction in demand for workers at affected employers diminishes the outside options of workers at non-affected employers, thereby reducing their reservation wage. The details of such a model are excluded for the sake of space in this version of the paper but are available from the author.

could offer ESI but pay lower wages to cover its cost. As a result, an employer who did not provide coverage before the ACA's announcement can be expected to choose to provide ESI or at least to prepare for that possibility even if they never end up doing so.

In the MEPS data used in this paper, around 15 percent of survey respondents in each year work at employers that do not offer ESI but also have more than 50 employees. For the employers that employ these workers, the employer mandate provides an incentive to consider employee health coverage costs for the first time. In contrast, at employers that already provided ESI there should be little or no change for workers with higher medical expenditures. The way the mandate impacts the labor market therefore lends itself to a difference-in-difference approach to estimation. The relationship of interest is the effect medical expenditures have on wages that is due to the incentives created by having to provide ESI to employees in the near future. The basic estimating equation is;

$$\begin{aligned} Labor\ Market\ Outcome_{it} = \beta_0 + \beta_1 Medical\ Expenditures_{it} + \beta_2 After\ EM_{it} \\ + \beta_3 Medical\ Expenditures \times After\ EM_{it} + \Pi X_{it} + \epsilon_{it} \end{aligned}$$

In the equation, $Labor\ Market\ Outcome_{it}$ stands for some labor market outcome of interest for person i at time t. This can be any variable that responds to a change in labor demand such as hours worked, wages, or unemployment duration. The right hand side of the estimating equation controls for the pre-existing relationship between wages and health expenditures using a continuous measure of health expenses ($Medical\ Expenditures_{it}$). Then, the estimating equation controls for the main effect of the employer mandate ($After\ EM_{it}$), a binary variable taking on the value of 1 after the employer mandate is announced. This captures any changes which affect all workers equally in the period after the employer mandate was announced. The coefficient on the interaction term gives a measure of the effect of the mandate on labor market outcomes as a function of medical expenditures. The estimating equation is completed by allowing for a set of demographic controls X_{it} including age, sex, education, marital status, race, census location, and industry.

Intuitively, this difference-in-difference approach first estimates the slope of the relationship between wages and medical expenditures over time. It then asks whether or not that slope is different after the employer mandate is announced (the coefficient on the interaction term is a measure of that difference). Theory suggests it should change for workers at employers who are required to be offered ESI from 2014 onward due to the mandate. On the other hand, for workers who are already offered ESI the relationship between wages and medical expenditures should remain stable because their wages already account for the cost of ESI.

4 Data

The empirical analysis uses data from the Medical Expenditure Panel Survey (MEPS) from 2006 to 2014. The Agency for Healthcare Research and Quality describes the MEPS as "a set of large-scale surveys of families and individuals, their medical providers, and employers across the United States. MEPS is the most complete source of data on the cost and use of health care and health insurance coverage." Each year a sub-sample of households participating in the previous year's National Health Interview Survey (NHIS) are selected to participate.[12] Respondents participate in five interviews across a two-year period and provide data on health care usage, out of pocket costs, and insurance coverage, along with demographic and employment information. The data is useful for examining labor market outcomes as a function of medical expenditure because MEPS provides data on the actual medical expenditures of individuals.

The paper focuses on Panels 11 through 19 of the MEPS covering from the end of 2006 to the end of 2014. Medical expenditures are provided as an annual figure at the year-ending third and fifth interviews. This means that responses from just these two interviews are helpful for the paper's analysis. However, the estimates reported in the paper use only the first year-end interview with each respondent.[13] That is, the data is treated as a repeated cross-section. This choice is made for several reasons. The first is that data on the second year-end interview for the most recent MEPS panel is never available, by design. To exploit the panel nature of the data, estimates would therefore have to ignore those who joined the survey in 2014. As the paper's estimates focus on what happens after 2010, excluding new respondents in 2014 is a trade-off. Second, only one MEPS panel straddles the before and after 2010 period limiting the usefulness of fixed effects strategies. Given employers had three years to prepare and the time it might take to make adjustments, focusing on data one year right before and after the mandate's announcement is not a valid strategy in any case.

[12] Policy relevant subgroups (such as low income households) are over-sampled by the NHIS and subsequently MEPS. See http://meps.ahrq.gov/mepsweb/.

[13] Additionally, some respondents only appear in what should have been their "second" year in MEPS due to joining a dwelling unit while it was already part of MEPS.

Third, MEPS completes two year-end interviews only with those respondents who remain in scope (typically this means they remain in the same house or "dwelling unit"). In the data, less than 70% of respondents who meet the sample selection criteria (age 27-55, working at an employer with 50 or more employees) at their first year-end interview are subsequently interviewed by MEPS at the end of year two and still meet the sample selection criteria. Attrition is likely not random, particularly with respect to medical expenditures and wages. In addition, respondents move between having ESI and not, between employers who are and are not covered by the mandate (more or less than 50 employees), or both. This means respondents could switch between control and treatment groups over time or might fail to meet the sample selection criteria (working at an employer with more than 50 employees) in one of their two years.[14]

Individuals aged 26 and under are excluded because their labor supply could have been affected by the ACA's dependent coverage mandate. The dependent coverage mandate allowed workers aged 26 and under to remain on their parents' insurance if they were not offered insurance coverage elsewhere.[15] Individuals older than 55 are excluded because labor force attachment rates begin to drop off at that age, often due to health issues. If the employer mandate leads to lower wages for those with higher medical expenditures, retirement decisions may be affected. Focusing on those aged 55 or younger helps to minimize the role that may play.

Summary statistics for the restricted sample, split by workers at employers who do and do not offer coverage, are presented in Table 1. Overall, there seems to be some variation across years but no patterns of changes that would lead to concerns that cannot be easily controlled for. For example, if affected employers hire more males they might be trying to lower the cost of ESI because males tend to use less health care services than females. The literature shows men tend to be less diligent about making and keeping doctor appointments, filling prescriptions, have little to no fertility-related expenses, and live shorter lives (see Mustard et al., 1998 for more on this topic). However, flexibly controlling for the returns to gender at affected employers before and after

[14]Estimates reported in the paper are robust to pooling the sample (using both year end interviews, when available) and clustering standard errors at the individual level. However, those estimates are not presented here because a pooled OLS approach cannot address how medical expenditures and wages might be sequentially correlated in ways that are difficult to understand with just two year-end observations per respondent.

[15]Antwi et al. (2013) found that the dependent mandate was associated with a 3% reduction in hours worked and that those aged 26 and under were 5.8% more likely to be working part-time. Depew (2015) and Hahn and Yang (2016) studied state level dependent mandates prior to the ACA and found similar intensive margin effects. See Goda et al. (2016) for a review of how health insurance affects the labor supply decisions of younger adults.

Table 1: Summary Statistics by Year - Ages 27-55, Employed

	2006	2007	2008	2009	2010	2011	2012	2013	2014
Offered ESI									
White	72.7%	70.7%	63.6%	65.4%	65.1%	66.5%	64.7%	64.1%	62.6%
Black	18.5%	17.9%	22.7%	22.6%	22.9%	22.0%	22.0%	21.9%	23.3%
Other	8.8%	11.5%	13.7%	12.0%	12.0%	11.6%	13.3%	14.0%	14.2%
High school or less	39.4%	37.6%	36.7%	34.4%	35.6%	34.2%	30.4%	28.7%	29.1%
College	46.2%	46.5%	46.4%	49.2%	46.7%	50.4%	54.0%	55.9%	54.7%
Graduate	14.5%	15.9%	16.9%	16.4%	17.6%	15.4%	15.6%	15.4%	16.2%
Male	53.4%	54.1%	49.3%	51.9%	51.9%	51.8%	54.1%	51.4%	54.2%
Married	65.5%	66.2%	63.2%	61.1%	60.3%	59.4%	61.5%	59.0%	57.7%
Age in Years	41.65	41.83	41.23	41.56	41.32	41.37	41.21	41.20	40.76
(Std. Dev.)	(8.12)	(8.13)	(8.3)	(8.13)	(8.27)	(8.36)	(8.34)	(8.41)	(8.43)
Annual Wage ($2014)	55,175	56,128	54,229	53,287	53,685	52,804	55,257	54,063	54,302
(Std. Dev.)	(37,483)	(40,148)	(37,199)	(36,348)	(37,615)	(37,639)	(37,992)	(37,334)	(39,747)
Weekly Hours	42.5	42.5	42.5	42.0	42.1	41.7	42.2	42.3	42.2
(Std. Dev.)	(8.7)	(8.3)	(8.9)	(8.6)	(8.8)	(8.1)	(8.6)	(9.0)	(9.0)
Medical Expenditures ($2014)	3,127	3,010	2,825	2,915	2,768	2,626	2,410	2,544	2,513
(Std. Dev.)	(5,725)	(5,258)	(5,112)	(5,594)	(5,560)	(5,362)	(5,064)	(4,975)	(5,045)
Total Observations	1,894	1,461	2,022	1,756	1,556	1,928	1,783	1,705	1,927
Not Offered ESI									
White	78.4%	77.3%	69.8%	73.2%	72.4%	69.3%	68.3%	74.0%	61.8%
Black	15.7%	18.1%	21.1%	18.8%	19.0%	18.5%	21.7%	20.9%	28.2%
Other	5.9%	4.6%	9.1%	7.9%	8.6%	12.2%	10.0%	5.1%	9.9%
High school or less	57.5%	63.8%	64.7%	62.7%	61.8%	62.4%	60.1%	59.7%	53.7%
College	36.2%	25.7%	26.4%	29.7%	33.3%	29.6%	36.5%	36.4%	41.2%
Graduate	6.3%	10.5%	8.9%	7.6%	4.8%	8.1%	3.4%	3.9%	5.1%
Male	42.8%	45.8%	51.0%	49.0%	49.1%	46.9%	49.7%	46.6%	47.3%
Married	58.8%	61.1%	51.9%	59.8%	51.3%	52.5%	51.4%	53.4%	44.3%
Age in Years	39.66	40.10	39.28	40.26	39.84	39.39	39.37	39.41	39.12
(Std. Dev.)	(7.75)	(8.23)	(8.2)	(8.42)	(8.36)	(7.86)	(8.36)	(8.32)	(8.07)
Annual Wage ($2014)	26,225	26,338	26,465	23,135	22,336	23,039	24,907	19,970	21,723
(Std. Dev.)	(29,163)	(24,132)	(31,183)	(21,070)	(19,615)	(23,370)	(23,347)	(16,807)	(19,590)
Weekly Hours	34.6	34.7	34.3	33.1	32.3	33.0	34.8	33.1	35.3
(Std. Dev.)	(13.4)	(12.1)	(14.1)	(12.5)	(11.8)	(12.3)	(11.9)	(11.1)	(17.9)
Medical Expenditures ($2014)	1,981	2,115	1,821	1,630	1,912	1,443	1,300	1,695	1,794
(Std. Dev.)	(4,715)	(4,862)	(4,421)	(3,736)	(5,540)	(3,074)	(4,218)	(4,767)	(5,005)
Total Observations	343	256	337	285	263	379	420	355	367

Source: Medical Expenditure Panel Survey 2006-2014. Medical expenditures and wages adjusted to 2014 dollars using the CPI (www.bls.gov).

the mandate ensures that remaining effects are due to medical expenditures. What is of note are persistent differences between employers who do and do not provide ESI, particularly in the annual wages paid to workers. As can be seen from the hours worked in a typical week, this is partially due to working fewer hours, however those who work where ESI is not offered are also more likely to be younger, have only a high school education or less, are less likely to be married, and are more likely to be female. Empirically, each of these factors is associated with lower wages. This highlights the importance of controlling for these variables in the estimates presented in the next section.

5 Estimates

Table 2 presents estimates of how the relationship between medical expenditures and individual wages (annual) changes after the employer mandate is announced. The estimates are based on MEPS data from survey respondents who report that they work at employers with more than 50 employees (again, the employer mandate does not apply to employers with fewer than 50 employees). In all specifications, the years from 2006 to 2010 are considered as "before" the employer mandate and the years 2011 to 2014 as "after." Treating 2010 data as "before" the employer mandate makes the most sense because the ACA passed in late March of 2010 limiting the ability of the Act to affect annual wages in 2010. However, excluding 2010 or treating 2010 as part of the post-treatment period has little impact on the estimates presented below.

In Panel A of Table 2, estimates are based on employees who work for employers where ESI is not already an employment benefit. The dependent variable is the log of annual wages (except for the final column). The coefficient on "After EM $\times$ Medical expenditures" is the estimate of interest in each specification in Panel A. This coefficient reflects how the relationship between wages and medical expenditures changes after the announcement of the employer mandate. Each column adds additional control variables that could be expected to effect wages or income. The difference-in-difference coefficient is consistently negative, indicating that wages are negatively related to medical expenditures after the announcement of the mandate.

The coefficients reported are elasticities because both medical expenditures and annual wages are log-transformed due to their skewed distribution (see Table 1). For the coefficient on the "Log Medical Expenditures" term, the log transformation means that for a 100% difference in medical

Table 2: Main Estimates of Effect of Employer Mandate

	(1) Log Wages	(2) Log Wages	(3) Log Wages	(4) Log Wages	(5) Log Wages	(6) Log Wages	(7) Log Wages	(8) Log Wages	(9) Annual Wages
Panel A - Not Offered ESI									
After Employer Mandate	0.0626	0.0664	0.0662	0.0645	0.0611	0.0650	0.0755*	0.0722	2,087**
	(0.0448)	(0.0449)	(0.0453)	(0.0447)	(0.0448)	(0.0442)	(0.0443)	(0.0444)	(1,059)
Log Medical Expenditures	0.00434	0.00277	-0.00661	0.00306	0.00377	0.00657	0.00583	0.00587	609.1***
	(0.00657)	(0.00657)	(0.00650)	(0.00663)	(0.00669)	(0.00667)	(0.00664)	(0.00662)	(173.8)
After EM × Med. Expenses	-0.0260***	-0.0259***	-0.0270***	-0.0260***	-0.0256***	-0.0260***	-0.0264***	-0.0256***	-735.59***
	(0.00940)	(0.00939)	(0.00932)	(0.00926)	(0.00927)	(0.00919)	(0.00913)	(0.00914)	(242.9)
Observations	2,834	2,834	2,772	2,772	2,772	2,772	2,772	2,772	2,772
Panel B - Offered ESI									
After Employer Mandate	-0.00712	-0.00512	-0.0391*	-0.0389*	-0.0379*	-0.0406*	-0.0390*	-0.0399*	-1,931**
	(0.0243)	(0.0242)	(0.0228)	(0.0222)	(0.0221)	(0.0219)	(0.0219)	(0.0218)	(1,041)
Log Medical Expenditures	0.0295***	0.0273***	0.0109***	0.0190***	0.0195***	0.0179***	0.0174***	0.0168***	692.9***
	(0.00250)	(0.00252)	(0.00239)	(0.00236)	(0.00236)	(0.00234)	(0.00234)	(0.00234)	(112.2)
After EM × Med. Expenses	-0.000788	-0.000741	0.000376	0.000542	0.000133	0.000903	0.000877	0.00116	115.9
	(0.00366)	(0.00364)	(0.00341)	(0.00333)	(0.00331)	(0.00328)	(0.00328)	(0.00326)	(161.2)
Observations	15,930	15,930	15,800	15,800	15,800	15,800	15,797	15,797	15,797
Panel C - Triple Difference									
After EM	-0.00712	-0.00547	-0.0377*	-0.0375*	-0.0368*	-0.0393*	-0.0369*	-0.0378*	-1,808.2*
	(0.0244)	(0.0242)	(0.0228)	(0.0222)	(0.0221)	(0.0219)	(0.0219)	(0.0217)	(1,038)
Log Medical Expenditures	0.0295***	0.0274***	0.0119***	0.0199***	0.0204***	0.0189***	0.0183***	0.0177***	748.2***
	(0.00250)	(0.00252)	(0.00238)	(0.00236)	(0.00236)	(0.00234)	(0.00234)	(0.00233)	(111.1)
Not Offered ESI	-0.759***	-0.756***	-0.670***	-0.653***	-0.656***	-0.660***	-0.658***	-0.661***	-18,992***
	(0.0361)	(0.0361)	(0.0360)	(0.0355)	(0.0355)	(0.0358)	(0.0358)	(0.0357)	(1,089)
After EM × No ESI	0.0697	0.0720	0.106**	0.104**	0.100**	0.110**	0.112**	0.110**	4,320***
	(0.0510)	(0.0510)	(0.0511)	(0.0503)	(0.0501)	(0.0497)	(0.0496)	(0.0496)	(1,565)
No ESI × Med. Expenses	-0.0252***	-0.0246***	-0.0230***	-0.0207***	-0.0213***	-0.0169**	-0.0169**	-0.0164**	-193.8
	(0.00702)	(0.00703)	(0.00688)	(0.00686)	(0.00686)	(0.00685)	(0.00682)	(0.00680)	(213.8)
After EM × Med. Expenses	-0.000788	-0.000718	0.000319	0.000478	0.000110	0.000923	0.000859	0.00113	110.7
	(0.00366)	(0.00364)	(0.00341)	(0.00333)	(0.00331)	(0.00328)	(0.00327)	(0.00326)	(161.0)
EM × Med. Expenses × No ESI	-0.0252**	-0.0251**	-0.0278***	-0.0269***	-0.0258***	-0.0282***	-0.0283***	-0.0276***	-919.3***
	(0.0101)	(0.0101)	(0.00994)	(0.00984)	(0.00983)	(0.00978)	(0.00975)	(0.00973)	(303.7)
Observations	18,764	18,764	18,572	18,572	18,572	18,572	18,569	18,569	18,569
Age		Y	Y	Y	Y	Y	Y	Y	Y
Education			Y	Y	Y	Y	Y	Y	Y
Gender				Y	Y	Y	Y	Y	Y
Region FE					Y	Y	Y	Y	Y
Industry FE						Y	Y	Y	Y
Marital Status							Y	Y	Y
Race								Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The first column of the table presents a specification with no demographic controls. Each column then adds controls as indicated. The effect size and significance remains almost constant across specifications. The final column presents estimates using level wages as the independent variable in order to provide context for the dollar amounts represented by the elasticity values in the log specifications presented in the other columns.

expenditures, there would, approximately, be a $100\times\beta$ difference in annual wages.

In Table 2, first note that the coefficient on the main "Employer Mandate" term in Panel A reflects wage changes after 2010 for a worker with zero medical expenditures. In the MEPS data used here, average annual medical expenditures were $1,743 for workers without ESI. Therefore, using the estimates from a specification with a complete set of controls (column 8), the estimated change in wages for a worker with average medical expenditures, at an affected firm, after the employer mandate was $7.22 - 2.56 \times ln(1743) = -1.11\%$.

The estimates in column eight further suggest a 2.56% difference, after 2010, in the annual wages of otherwise-similar workers who have a 100% difference in medical expenditures. In the MEPS sample, full time workers at employers that would be affected by the employer mandate earned $24,674 in annual wages. As mentioned above, average medical expenditures were typically $1,743 per year. Some back of the envelope calculations will show that an individual with medical expenditures of about $3,400 compared to one with about $1,700 in medical expenditures could expect to receive $632 less in annual wages. This amounts to $0.37 of each dollar of medical expenditures being passed through to an employee.[16] The estimates in column nine repeat the same estimation as in column eight using the level of annual wages to aid interpretation. They show that for a 100% difference in medical expenditures, annual wages would be $735 lower after 2010, all else equal. For a $1,700 difference in medical expenditures, this implies a pass-through of $0.43 per $1.

A pass through in the range of $0.37 to $0.43 for each $1 difference in medical expenditures is quite large for several reasons. One, the cost of employee medical care is tax deductible for employers. The U.S. tax code heavily subsidizes employee medical coverage: each dollar of medical expenditures reduces the employer's tax liability by $\$1 \times \tau$ if τ is the marginal rate of tax the employer faces. Two, the effects observed are anticipatory and based upon current rather than unknown future medical expenditures. Employers must be both forward-looking and believe there is a non-zero probability that the mandate will be enforced for it to have any effect in the years prior to implementation. While current medical expenditures are not a perfect predictor of tomorrow's expenditures, differences between individuals appear to convey information about future medical expenditure differences. For example, repeating the estimation in column eight of Table 2 but with next year's medical expenditures as the dependent variable suggests that an additional $1

[16] $24,674 $\times$ 2.56% = $631.65 and $631.65/$1,700=$0.37.

of medical expenditures in year t is associated with \$0.63 greater medical expenditures in year $t+1$.[17] In addition, research across a variety of academic fields has shown that current medical expenditure is a good predictor of future expenditure (as mentioned earlier, see Bertsimas et al., 2008 for a review). Three, employers who experience relatively high employee turnover would have a diminished incentive to respond to the mandate. With that said, in the MEPS data used here over 60% of workers at employers without ESI have employment tenure of two years or more.[18] Four, as an alternative to shifting costs onto workers via lower wages, employers could have planned to shift cost differences onto workers by having large co-pays and deductibles.[19] The mandate's provisions limit the effectiveness of such a strategy.[20]

The estimates in Table 2 are little different if those aged up to 59 are included. However, the estimated effect becomes smaller and is measured with less precision when the sample includes those up to age 65. This is not surprising because the paper is focused on anticipatory responses and such workers are close to retirement age.

Panel B of Table 2 focuses on how wages change for workers at employers who are already offered ESI. If the employer mandate (rather than broad labor market trends) is the cause of the effects seen in Panel A, then the coefficient on the difference-in-difference term in Panel B should be no different from zero. Confirming such a prediction, the estimate consistently shows no significant effect across each specification.[21] This does not mean there is no relationship between wages and medical expenditures at these employers. It shows only that the relationship does not change because of the employer mandate. In fact, in all estimates presented, wages and medical expenditures are positively correlated. This is a common finding and can be viewed as an income effect. That is, workers who earn higher wages spend more on many goods and services, including health care. It could also be indicative of selection into jobs that offer ESI or some relationship between experience, age, wages, and medical expenditures that may be poorly accounted for in cross-sectional regression estimates. As long as that relationship is stable over the time period studied in this paper, it causes

[17] Available from the author upon request.

[18] Tenure of two years or more increases to about 80% for employees who are offered ESI.

[19] Anand (2016) finds that net hourly compensation decreases by 52 cents for each one-dollar increase in health insurance (at the group level) and that much of the cost is borne by employees in the form of higher employee premium contributions.

[20] See the IRS's descriptions of compliant coverage here.

[21] Note that the coefficient on the "After Employer Mandate" term is negative in each specification. Such a finding is only an issue for identification if wages at these same employers were also inversely related to individual medical expenditures. The estimate on the interaction term show that they are not.

no issues for identification.

In Panel C of the table, all observations are pooled into a triple-difference specification. This specification interacts the terms of interest from the difference-in-difference estimations with a dummy for whether or not the worker is offered ESI by their employer already. The triple-difference coefficient therefore represents how medical expenditures (the first difference) and wages are related after the employer mandate is announced (the second difference) at employers who do and do not already offer ESI (the third difference). The estimate corresponding to "After EM $\times$ Med. Expenses $\times$ No ESI" suggests that for a 100% increase in medical expenditures, annual wages will be lower by 2.76% (in the eighth column) at employers who do not already offer ESI after 2010 relative to those that do offer coverage. That is, the data consistently shows that workers who have higher medical expenditures face lower wages after the employer mandate is announced if they work at employers who must now provide ESI.

One major threat to identification is that firm-size (in terms of number of employees) might non-randomly change in response to the mandate. However, the estimates in Table 2 are almost identical when the sample is further restricted to only MEPS respondents who report working at employers with more than 75 employees. This ameliorates two concerns. One, that the findings are driven by employers reducing size (in terms of employees) to avoid the mandate. Two, that the findings are driven by employees being unable to accurately report firm-size.

The estimates presented in Table 2 focus on changes in annual wages. What is not clear is whether the changes are due to a reduction in hourly wages, hours worked, or more (or longer) spells of unemployment. Any or all of these could also be affected by a reduction in the demand for labor. Table A1 in Appendix A repeats the triple-difference estimation from Panel C of Table 2 but uses hourly wages and an indicator for part-time employment as dependent variables rather than annual wages. Those estimates show that changes in hourly wages closely match the changes in annual wages observed in Table 2. The next section will consider the robustness of the main estimates and attempt to understand the mechanism driving the findings.

6 Robustness and Mechanisms

This section considers if the main estimates in Section 5 are driven by changes over time in wages by employer size, industry, or location instead of the employer mandate. Additionally, the section examines the validity of assuming parallel trends using placebo tests on the announcement date of the mandate and by examining pre-trends in the relationship between wages and medical expenditures for workers that are and are not offered ESI by their employer. The section then examines if individual medical expenditures may be predictable via group characteristics.[22] That is, are the observed effects just the product of employers shifting costs based on easily-observed demographic characteristics?

6.1 Employer Size, Industry, and Location

The most striking difference between employers that do and do not offer ESI is firm size. Almost all employees in the MEPS data who work at employers with 300 employees or more are offered ESI. This means that the estimates in Table 2 compare employees at small employers who are not offered insurance to those at small *and* large employers who are offered coverage. As a result, the "control" group in the natural experiment set-up of this paper is potentially invalid: there may be reasons why employees at small employers are treated differently after 2010. To ensure this is not a source of bias, Table 3 restricts the sample to employees who work for employers who have fewer than 300 workers.[23] The estimates in the table are produced using the same triple-difference estimation approach used in Panel C of Table 2.

The first column presents a specification with no demographic or other controls. Each column then adds controls as indicated. The final column provides estimates using the dollar amount of annual wages to provide context for the magnitude of the estimates. What is striking is that the effects increase in both economic size and statistical significance relative to Table 2 despite a smaller sample size. The negative wage effect of a 100% difference in medical expenditures is now estimated at 3.64% in a specification with a full set of demographic controls. A back of the envelope

[22] Appendix B reports estimates from a matching exercise designed to ease concerns that the results are driven by sample selection issues.

[23] Similar effects are observed for restrictions to 250 and even 200 or fewer employees but the sample size becomes small and limits statistical power.

Table 3: Triple-Difference Estimates of Effect of Employer Mandate for Workers at Smaller employers (< 300 Employees)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	$ Annual Wages
Affordable Care Act	0.00307	0.00153	-0.0357	-0.0345	-0.0363	-0.0454	-0.0420	-0.0399	-2,127
	(0.0351)	(0.0349)	(0.0332)	(0.0323)	(0.0322)	(0.0318)	(0.0317)	(0.0315)	(1,433)
Log Medical Expenditures	0.0281***	0.0252***	0.00918***	0.0178***	0.0178***	0.0158***	0.0154***	0.0145***	574.0***
	(0.00359)	(0.00361)	(0.00346)	(0.00344)	(0.00343)	(0.00339)	(0.00339)	(0.00337)	(148.4)
Not Offered ESI	-0.781***	-0.783***	-0.723***	-0.696***	-0.703***	-0.710***	-0.708***	-0.714***	-21,074***
	(0.0543)	(0.0543)	(0.0555)	(0.0552)	(0.0552)	(0.0560)	(0.0561)	(0.0559)	(1,506)
After EM × No ESI	0.0569	0.0652	0.101	0.0970	0.0966	0.122	0.125	0.124	6,579***
	(0.0797)	(0.0799)	(0.0818)	(0.0806)	(0.0803)	(0.0796)	(0.0796)	(0.0796)	(2,342)
No ESI × Expenses	-0.0120	-0.0105	-0.00640	-0.00344	-0.00367	0.000792	0.000601	0.00132	372.1
	(0.0102)	(0.0102)	(0.0101)	(0.0102)	(0.0102)	(0.0102)	(0.0102)	(0.0102)	(294.5)
EM × Expenses	-0.00337	-0.00265	0.000224	0.000746	0.000461	0.00199	0.00190	0.00205	201.6
	(0.00527)	(0.00524)	(0.00495)	(0.00483)	(0.00481)	(0.00476)	(0.00475)	(0.00472)	(219.7)
EM × Expenses × No ESI	-0.0290*	-0.0298**	-0.0314**	-0.0327**	-0.0319**	-0.0371**	-0.0369**	-0.0364**	-1,368***
	(0.0148)	(0.0148)	(0.0148)	(0.0147)	(0.0147)	(0.0146)	(0.0146)	(0.0145)	(435.4)
Observations	8,792	8,792	8,744	8,744	8,744	8,744	8,743	8,743	8,743
Age		Y	Y	Y	Y	Y	Y	Y	Y
Education			Y	Y	Y	Y	Y	Y	Y
Gender				Y	Y	Y	Y	Y	Y
Region FE					Y	Y	Y	Y	Y
Industry FE						Y	Y	Y	Y
Marital Status							Y	Y	Y
Race								Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls. Each column adds control variables as indicated. The final column shows the estimates using the level of annual wages to help provide context for the magnitude of the log-log specification estimates.

calculation would imply a pass through of $0.52 for each dollar of medical expenditures.[24] This approach eases concerns about the validity of the comparison or "control" group used in earlier estimates. It also hints at the mechanism driving the relationship between medical expenditures and wages. At larger employers, it may be too costly or difficult to determine who is increasing costs via medical expenditures.[25]

Table 4 re-estimates the triple-difference specification presented in Panel C of Table 2 using only workers at employers with fewer than 50 employees before and after the employer mandate is announced. As the mandate does not cover these employers the estimated coefficient on the

[24] $24,674 × 3.64% = $898.13 and $898.13/$1700=$0.52.

[25] As in Table A1, estimations using hourly wages as the dependent variable find similar effects. Within the 50 to 300-employee sub-sample of the data, there is no statistically significant effect on hours worked or part-time employment but the probability of employees with higher medical expenditures working at an employer who is affected by the mandate is lower and that estimate is statistically significant at the 5% level. These are available from the author upon request.

Table 4: Triple-Difference Estimates of Effect of Employer Mandate for Workers at Unaffected employers (< 50 Employees)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	Log Wages	$ Annual Wages
Affordable Care Act	0.0435	0.0456	0.0495	0.0457	0.0402	0.0358	0.0384	0.0407	1,632
	(0.0530)	(0.0529)	(0.0527)	(0.0520)	(0.0516)	(0.0515)	(0.0512)	(0.0510)	(1,808)
Log Medical Expenditures	0.0350***	0.0341***	0.0238***	0.0309***	0.0307***	0.0297***	0.0279***	0.0264***	1,243***
	(0.00654)	(0.00655)	(0.00653)	(0.00647)	(0.00643)	(0.00643)	(0.00642)	(0.00640)	(225.5)
Not Offered ESI	-0.501***	-0.499***	-0.435***	-0.423***	-0.422***	-0.406***	-0.408***	-0.418***	-10,508***
	(0.0488)	(0.0487)	(0.0490)	(0.0484)	(0.0480)	(0.0485)	(0.0482)	(0.0481)	(1,458)
After EM × No ESI	-0.0318	-0.0329	-0.0602	-0.0592	-0.0568	-0.0441	-0.0434	-0.0453	-1,349
	(0.0628)	(0.0627)	(0.0625)	(0.0617)	(0.0613)	(0.0612)	(0.0609)	(0.0606)	(1,943)
No ESI × Expenses	-0.0300***	-0.0302***	-0.0311***	-0.0282***	-0.0285***	-0.0278***	-0.0277***	-0.0268***	-1,003***
	(0.00846)	(0.00845)	(0.00839)	(0.00831)	(0.00826)	(0.00825)	(0.00821)	(0.00818)	(255.9)
EM × Expenses	-0.00582	-0.00563	-0.00758	-0.00824	-0.00783	-0.00749	-0.00695	-0.00737	-291.9
	(0.00856)	(0.00854)	(0.00842)	(0.00832)	(0.00827)	(0.00821)	(0.00819)	(0.00814)	(312.7)
EM × Expenses × No ESI	-0.00274	-0.00288	0.00132	0.00173	0.00170	0.000756	0.000604	0.00119	3.474
	(0.0112)	(0.0112)	(0.0111)	(0.0109)	(0.0109)	(0.0108)	(0.0108)	(0.0107)	(353.6)
Observations	6,359	6,359	6,290	6,290	6,290	6,289	6,289	6,289	6,289
Age		Y	Y	Y	Y	Y	Y	Y	Y
Education			Y	Y	Y	Y	Y	Y	Y
Gender				Y	Y	Y	Y	Y	Y
Region FE					Y	Y	Y	Y	Y
Industry FE						Y	Y	Y	Y
Marital Status							Y	Y	Y
Race								Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls. Each column adds control variables as indicated. The final column shows the estimates using the level of annual wages to help provide context for the magnitude of the log-log specification estimates.

triple-difference term (EM × Expenses × No ESI) should be no different from zero. As can be seen, it is statistically insignificant in all specifications. In addition, the coefficient on EM × Expenses is also insignificantly different from zero. This suggests that there was no change in the relationship between wages and medical expenditures for workers at employers who offer or do not offer ESI to their employees after the employer mandate was announced.

In Table 2 the appropriate control group was assumed to be workers at employers with 50 employees or more but who already have ESI. That is, the comparison was between employers covered by the mandate who do and do not already offer coverage. An alternative approach would compare workers at employers with do not have ESI but are above and below the 50 employee cut-off for the employer mandate. It is easy to see what the estimates from such an analysis would show by re-examining Table 4 and Panel A of Table 2. Table 4 shows there is no change in the wage-expenditures relationship at employers with 50 or fewer employees after 2010. On the other hand, Table 2 shows the wage-expenditures relationship for workers at employers with more than 50 employees changes noticeably after 2010.

Taken together with Table 2, the estimates in Table 4 show that the only group of workers who see a change in the expenditure-wage relationship after 2010 are those workers who work at employers with more than 50 employees and are not already offered ESI by their employer. This is precisely the group affected by the employer mandate.

It is worth noting that other ACA provisions allowed (but did not require) employers with fewer than 50 employees to obtain ESI via the Small Business Health Options Program (SHOP). The SHOP marketplaces feature community- rather than experience-rated insurance plans. Designed to reduce the problems of small risk pools, employers with 50-100 employees were to gain access to the SHOP marketplace in 2017. However, even if all employers with more than 50 workers could access the SHOP marketplace, a preference for workers with lower medical expenditures persists as these employers have to provide ESI but do not *have* to use the SHOP marketplace. If an employer who is required to offer ESI can maintain an employee pool with below average expenditures they would be able to obtain experience-rated coverage that is cheaper than community-rated coverage. Of course, if employers strategically opt out, then those markets will begin to unravel. The potential for adverse selection is exacerbated by the fact that employers can duck in and out of SHOP marketplaces as they please.[26]

An additional threat to identification is that MEPS data only provides the census region respondents live in (Northeast, Midwest, South, or West). Controlling for region may not adequately capture spatial variation in the costs of receiving care. The type of variation that would confound the findings of the paper would require health care costs to rise faster than wages in some areas within a region and not others over time. Then, results might be picking up a mechanical association between health expenses and relatively lower wages. However, a mechanical effect of rising relative health care costs should be observed regardless of whether or not an employer offers coverage. This source of confounding variation can only be an issue if the MEPS *by chance* sampled relatively more workers at employers who did not provide ESI who also happen to live in areas with a rising health care cost to wage ratio *after* the mandate was announced. Related to this point, if certain

[26] See https://www.healthcare.gov/small-businesses/provide-shop-coverage/shop-marketplace-overview/.

industries tend to attract high expenditure workers (or cause workers to have high medical costs), the industry is disproportionately represented in the workers studied in this paper, and the industry experiences some negative industry-specific shock post-2010, the effects observed may again be some mechanical association that looks like its due to the employer mandate.

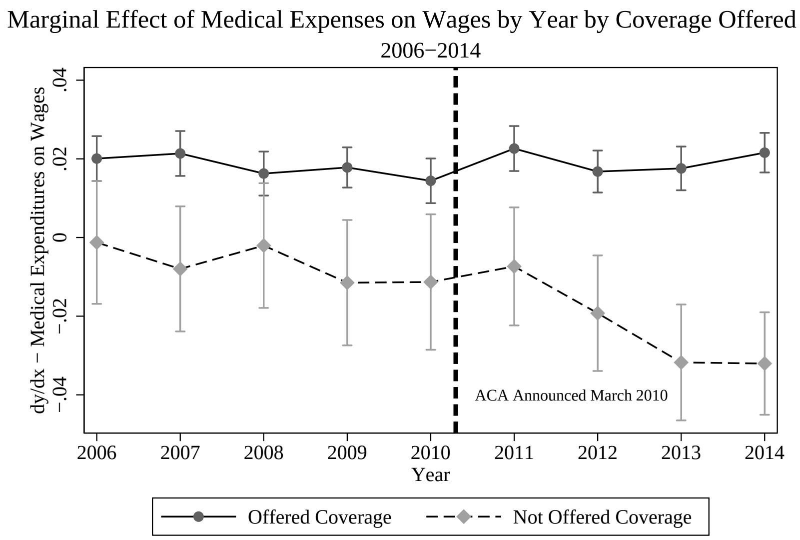
However, Table 4 highlights that there is no effect on wages at employers with fewer than 50 employees. If the estimates in Table 2 were due to location or industry specific changes, they should be observed at employers of all sizes, but they are not. Another way to test for these potential effects would be to allow region or industry to have time-specific effects (essentially a two-period, pre- and post-EM, linear trend). The estimates from such an exercise are presented in Table 5 as part of a broader consideration whether or not employers are using heuristics that are associated with medical expenditures rather than considering individual medical expenditures even after allowing for group differences.

6.2 Parallel Trends

Figure 1 presents the post-estimation marginal effect of medical expenditures on log annual wages by year for workers offered ESI (solid line) and those not offered ESI (dotted line). Each point on the line is the slope of the estimated elasticity between expenditures and wages in a given year. This means this is essentially an event study of the employer mandate's effect on wages for workers at employers who do and do not offer coverage. It illustrates how the relationship between medical expenditures and wages was trending prior to 2010 and how it changes in the years after 2010. The estimates corresponding to workers who are already offered ESI is stable over time. For those at employers affected by the employer mandate, the relationship changes noticeably post-2010.

Comparing the period before 2010 to after 2010 raises concerns with how the events of 2007-2009 and subsequent economic recovery impact the analysis. Difference-in-difference and triple-difference strategies tend to ease these kinds of concerns as the focus is on differences between the labor market outcomes of individuals who work at employers who do and do not provide ESI before and after 2010. If the 2007-2009 recession affected all employers similarly then there are no concerns. However, Siemer (2014) finds reduced employment growth in smaller employers. Siemer's findings are relevant because employers that don't offer ESI tend to be smaller. Siemer's estimates suggest small employers have between 4.8 and 10.5% slower employment growth from 2007 to 2009. This

Figure 1: Post-estimation Marginal Effect of Medical Expenditures on Wages by Year and by Offered Coverage/ESI



would bias this paper's estimates away from zero if the reduced growth happened to be biased against workers with relatively low medical expenditures. If so, the findings in Section 5 could be a product of the recession and not the employer mandate but there is no clear reason small employers should hire fewer healthier (potentially more productive) workers due to a recession. If that were happening, it should also be apparent at employers with fewer than 50 employees, but it is not.

Regardless, this potential source of bias can be directly addressed using MEPS data from before the mandate's announcement. This involves repeating the analysis of Table 2 using data only from 2006-2011 and imposing a placebo treatment date of 2008 or 2009 to re-estimate the effect of medical expenditure on wages for workers at employers who do and do not offer coverage. If it is the case that economic recovery in the years after the employer mandate's announcement lead to the effects seen, then the economic downturn in the years after 2006 and 2007 should show the opposite effect. The coefficients of interest from this kind of exercise are statistically zero in specifications using either 2008 or 2009 as a "placebo" treatment date suggesting the recession before 2010 did not affect workers with different medical expenditures in different ways.

Lastly, the main findings of this paper clearly depend upon something that happens after 2010. Repeating the main estimates using 2008 or 2009 as a placebo treatment date reduces the size and statistical significance of the estimates presented in Table 2. In addition, including 2011 data as pre-treatment increases the size and significance of the effects. This indicates that the effects observed depend upon something that happens in 2010 but that then takes some time to be fully incorporated into the labor market. Those estimates are not presented here because Figure 1 illustrates the timing of the effects.[27]

6.3 An Individual-Specific Effect?

The results presented in Table 2 show wages and medical expenditures are negatively related at employers who do not offer ESI after the mandate is announced. As mentioned earlier, it is still possible that these are group-specific effects that are being picked up by the individual variation that forms those group differences. Focusing on groups who are impacted via a mandate does not rule out individual-specific effects. Similarly, focusing on individual-specific effects does not rule out group-level effects. It is important to account for both using flexible controls for characteristics that may predict individual medical expenditures. Otherwise, the effects being observed for workers at employers that do not offer ESI could be explained by employers using group characteristics to shift the cost of coverage to workers. To put it another way, using individual level data and finding a significant relationship between medical expenditure and wages does not rule out that employers are using age, gender, race, and so on as a heuristic for higher medical expenditures. If it then turns out the members of those groups actually do have larger medical expenditures, this will show up as an individual-specific effect in regression estimates that fail to allow the effect of demographic characteristics to also change due to the employer mandate.

Table 5 provides the estimates corresponding to specifications that allow the various demographic controls' effects to vary over time. The table displays the estimates of interest from a repeat of the regression estimates in Panel C of Table 2. The only difference here is that all of the control variables are also interacted with the "EM" dummy, the "No ESI" dummy, and the interaction between the two. This tests if the individual-specific effects seen in Tables 2 and 3 are driven by employers treating females, older workers, different races, or individuals with more education, or who work in certain industries or regions, differently after the mandate's announcement. If employers affected by the employer mandate were transferring the cost of coverage to employees using heuristics based on demographic characteristics then the effects seen in earlier tables should

[27] Again, available from the author.

Table 5: Triple-Difference Estimates of Effect of Employer Mandate With Flexible Demographic Controls

	(1) Log Wages	(2) Log Wages	(3) Log Wages	(4) Log Wages	(5) Log Wages	(6) Log Wages	(7) Log Wages	(8) Log Wages	(9) $ Annual Wages
EM × Expenditures	0.00101	-0.000307	0.000419	0.00191	0.00187	0.00230	0.00198	0.00241	164.9
	(0.00335)	(0.00340)	(0.00333)	(0.00333)	(0.00333)	(0.00330)	(0.00331)	(0.00331)	(115.6)
EM × Expenditures × No ESI	-0.0262**	-0.0241**	-0.0266**	-0.0246**	-0.0258**	-0.0243**	-0.0251**	-0.0233**	-621.0***
	(0.0106)	(0.0106)	(0.0107)	(0.0108)	(0.0108)	(0.0107)	(0.0107)	(0.0106)	(230.5)
Observations	18,764	18,764	18,572	18,572	18,572	18,572	18,569	18,569	18,569
Age		Y	Y	Y	Y	Y	Y	Y	Y
Education			Y	Y	Y	Y	Y	Y	Y
Gender				Y	Y	Y	Y	Y	Y
Region FE					Y	Y	Y	Y	Y
Industry FE						Y	Y	Y	Y
Marital Status							Y	Y	Y
Race								Y	Y

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls. Each column adds control variables as indicated. Note that here, the controls are also interacted with the dummies for the employer mandate's announcement, ESI, and the interaction between the two. The final column shows the estimates using the level of annual wages to help provide context for the magnitude of the log-log specification estimates.

have disappeared. Instead, they are largely unchanged. The estimates on the various interaction terms are not reported but there are significant negative effects on the wages of females, older workers, and those who have a college education in most specifications. Each of these groups tends to have higher medical expenditures.

This section has attempted to assure the reader that the effects seen in Section 5 are causally related to the employer mandate. There are several potential threats to identification that are ruled out by examining pre-trends in the data and repeating the analysis for employers who are not covered by the mandate. These robustness checks also show that the effects observed are not caused by changes that are affecting industries or geographic areas that have low wages and workers with high medical expenditures.[28] Lastly, Table 5 examined if the estimates were simply picking up changes that can be attributed to broad group differences in medical expenditures after 2010. However, the effect of individual medical expenditures on wages after 2010 remains negative and significant at affected employers even after allowing the effect of demographic controls to change at employers affected by the employer mandate after 2010.

[28] A propensity score matching exercise in Appendix B provides additional reassurance that the MEPS data is not biased towards groups who have low wages and high expenditures after the employer mandate's announcement.

7 Discussion and Conclusion

This paper examines if employers shift the cost of providing ESI onto individual workers with higher medical expenditures via lower wages using the Affordable Care Act's employer mandate. The period between the mandate's announcement and actual implementation provides a unique opportunity to study this issue because the employer mandate changes the expected cost of employing workers with various medical expenditures but does not change how medical expenditures might affect productivity.

Estimates, using MEPS data in a difference-in-difference framework, suggest that workers at employers affected by the employer mandate earn lower wages if they have higher current medical expenditures. The effect on wages amounts to a pass-through of at least 37 cents of each dollar of individual medical expenditures. If workers pay for the cost of ESI via lower wages, then the supposed risk-pooling benefits of employment-based health coverage are undermined. Note that the claim is not that employers are able to determine the medical expenditures of every individual precisely. An insurance company could not do that for individual customers, either. Instead, the claims underlying the paper are that (1) current medical expenditures are an important determinant of future medical expenditures and (2) employers can infer enough to determine which of two workers who are similar in terms of other observable characteristics (such as age, education, race, gender, and so on) have higher medical expenditures.

Of course, the distinction between an individual and a group blurs when groups are defined narrowly enough. A weaker version of the findings of this paper would be that existing research on the effect of ESI on the wages of various groups of workers has defined groups too broadly. This paper, at a minimum, shows employers can shift the costs of ESI onto sub-groups of workers within the groups focused on in the literature (typically defined by some combination of age and gender). The sub-groups could be as small as an individual. However, it could also be the case that this paper's estimates do not control for the exact sub-group as defined by some multi-dimensional combination of location, time, occupation, tenure, absenteeism, productivity, physical appearance, diet, race, gender, education, age, marital status, and so on that employers are using to determine current and future differences in medical expenditures among employees. Only some of those variables are observed in available data. In any case, risk-pooling is still undermined if sub-groups are so narrowly defined that the relationship between wages and individual medical expenditures mimics individual-specific cost-shifting.

A significant limit of the paper is its focus on anticipatory effects. However, effects on wages would be biased towards zero if employers were not convinced the law would ever come into effect or were unaware of their responsibilities. This suggests that the estimates might be best-viewed as lower bounds rather than point estimates. Regardless, using the pre-implementation time period to study the mandate's effects is crucial because once all of the elements of the ACA are fully implemented identifying the effect of the employer mandate separately from the rest of the Act's provisions would be infeasible.[29]

Additionally, because employment is an ongoing arrangement (rather than determined in a spot market), profit-maximizing employers could not wait until 2014 to alter their demand for labor. A naive approach using data from the years before and after the mandate's implementation date in 2014 may find no effect of the employer mandate on wages. Such a finding would be erroneous because the data in this paper suggests that adjustments have already occurred (regardless of the cause of those adjustments) before the eventual implementation of the mandate. Instead, the focus on the period between the mandate's announcement and its implementation could be considered a strength of this paper relative to existing work on the effect of insurance mandates. Existing work has studied mandates that have a short implementation period with no way to determine whether the effects observed are due to reduced demand for or an increased supply of labor. However, the immediate effect of the ACA on labor supply towards affected employers is ambiguous due to the ACA's broader changes and an information asymmetry between workers and employers.

In particular, while individuals struggled to wrap their heads around the new health care law, the health insurance industry reacted swiftly. There is ample evidence that insurers had developed comprehensive reports by mid-2011 advising employers of the Act's regulatory changes and how to prepare for them.[30] Also, an employee may not know if the employer they (want to) work for will offer ESI due to the mandate but the employer likely will. For example, will they have 50 FTEs

[29] Chief among these would be the new health insurance exchanges that were to provide affordable ESI options outside of employment and could cloud identification if they affected self-employment patterns, job search efforts, or alleviated ESI-related job lock. Note that all estimates in the paper include data from 2014 - excluding 2014 does little to change the estimates reported in Sections 5 and 6. In some specifications, the estimates increase in size and significance.

[30] A typical example is the Hudson Institute report for franchise owners dated September 2011 here.

or not after 2014, can they move some workers to < 30 hours per week to avoid providing ESI, can they change their capital/labor mix to reduce their exposure, what will the cost of ESI be, how much will they charge employees, and so on.[31] Also, Table 1 suggests that the wages of many individuals who work at employers without ESI might allow them to qualify for heavily-subsidized insurance coverage on new insurance exchanges.[32] Because individuals were required by law to have coverage from 2014 onward, individuals might reduce rather than increase their willingness to work at employers affected by the employer mandate. At the same time, isolating whether the effect of coverage mandates on wages is due to labor supply or demand changes is not a primary goal of this paper. The findings could be completely due to changes in labor supply (workers choosing to work at employers affected by the employer mandate for lower wages in anticipation of obtaining ESI in the future) without undermining the paper's contribution: ESI fails to pool risk because workers appear to "pay" for their coverage at the individual rather than only at the group level.

Other threats to identification include the effects of the recession and recovery period, selection into and out of offering ESI by employers prior to the mandate, and the effect of other ACA provisions on individual behavior. We would expect that, in the years after 2010, labor market changes would have tended to involve increases in employment among individuals likely to receive low wages and unlikely to receive insurance coverage from their employer. If such workers also had greater medical expenditures it could explain the paper's main estimates. However, the estimates in Table 4 rule out such a possibility. Table 4's estimates examine wages for workers at firms with and with without ESI that have fewer than 50 employees. If the paper's findings can be explained by changes in the composition of who is working at employers where ESI is not offered, and not the employer mandate, we would expect the same changes to be observed regardless of firm size (and especially for firms that are all relatively small). The same argument can be applied to changes in individual behavior in response to the ACA mandate. That is, if medicaid expansion, the dependent care mandate, or anticipatory effects related to the ACA's insurance exchanges explain the findings in Table 2, we would expect Table 4 to show the same pattern, but it does not. Finally, selection effects are ambiguous; it is plausible that relatively higher-wage firms switched to offering ESI sooner (essentially complying with the mandate in advance) than low-wage firms over the period

[31] Employers could charge employee contributions of up to 9.5% of a worker's wage and still satisfy the mandate.

[32] For more information see https://www.healthinsurance.org/obamacare/will-you-receive-an-obamacare-premium-subsidy/ and www.healthcare.gov.

studied here. However, estimates suggest individuals with higher wages also have higher medical spending, providing a competing incentive to not offer ESI. In any case, only a handful of MEPS respondents aged 27-55 - who work at a firm with 50 or more employees for two years - experience a within-job change in ESI coverage. It is therefore not possible to say anything concrete about the characteristics of those who gain or lose coverage in the period of time between the employer mandate's announcement and implementation.

The paper's findings should not be viewed as an indictment of the Affordable Care Act, but instead viewed as a basic consequence of ESI. Theory suggests an individual market would suffer from information asymmetries: workers would seek coverage only when they need it and insurers would have incentives to screen out individuals who would require costly care. However, this paper shows that because employers ultimately pay for the medical expenditures of their employees, they screen and penalize as an insurer would. As a result, simultaneous increases in the cost and prevalence of ESI can increase barriers to employment for workers whose total compensation exceeds the value of their marginal product.

These findings - when combined with the existing literature on mandated benefits - make the employer mandate in the Affordable Care Act a curious artifact. If individual workers essentially pay for their care one way or another then, at best, the mandate arbitrarily restricts workers to a benefits package chosen for them by their employer while charging them for the privilege via lower wages. At worst, it could leave many workers unemployed.

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A Alternate Labor Market Outcomes

While the literature (and this paper) focuses mainly on how wages are affected by ESI, employers can shift the cost of ESI onto workers with larger medical expenditures in several ways. In the case of the ACA's employer mandate, because the mandate only applied to workers who work 29 or more hours per week, employers might employ certain workers for fewer hours, for example. To examine if hours are affected for workers with large medical expenditures, Table A1 repeats the triple-difference estimation from Panel C of Table 2 but uses hourly wages (in logs then levels) and an indicator for part-time employment as dependent variables rather than wages. Part-time employment is defined here as working fewer than 30 hours per week because the mandate applies only to those working more than than 30 hours. The estimates in the first and second columns of Table A1 suggest a statistically significant relative fall in hourly wages for employees who work for employers affected by the ACA. In particular, the estimate corresponding to "After EM $\times$ Med. Expenses $\times$ No ESI" suggests that for a 100% increase in medical expenditures, hourly wages are lower by 2.17%. This corresponds well to the estimated effect on annual wages and suggests that the observed effects on annual wages in the main body of the paper are coming from lower hourly wages rather than fewer hours worked. In the second column the effects on the level of hourly wages are provided to ease interpretation. The -0.443 coefficient implies a \$0.44 difference in hourly wages for a 100% difference in medical expenditures.

In the third column of Table A1, the specification uses a dependent variable that equals one if the respondent is working fewer than 30 hours in a regular week and zero otherwise. As the dependent variable is a binary outcome, this is a probit estimation. As the effect on hourly wages lines up well with the effects on annual wages in Section 5, it is not surprising that the estimates suggest employers did not move workers with higher medical expenditures into part-time employment.[A1] Note that the estimates do not mean that employers did not move any workers to part-time employment, only that workers who are employed part-time were not more likely to be workers who have higher medical expenditures.

This finding is surprising given the costs of the mandate could be minimized by pivoting some

[A1]The estimates shown are raw estimates from a probit estimation rather than marginal effects due to the challenges with calculating marginal effects when the specification considers many categorical control variables. In any case, as the estimate is not significantly different from zero, calculating marginal effects would be of no value.

Table A1: Triple-Difference Estimates of Effect of Employer Mandate on Hourly Wages and Hours Worked

	(1) Log Hourly Wages	(2) $ Hourly Wage	(3) Part-Time ("<30 Hours")
After Affordable Care Act	-0.0859***	-1.825***	0.147
	(0.0156)	(0.375)	(0.0942)
Log Annual Medical Expenditures	0.0121***	0.253***	-0.00121
	(0.00171)	(0.0420)	(0.00977)
No ESI	-0.371***	-6.316***	1.093***
	(0.0221)	(0.428)	(0.0967)
After EM × No ESI	0.0993***	2.520***	-0.00808
	(0.0301)	(0.605)	(0.132)
No ESI × Annual Medical Expenditures	0.00574**	0.112*	-0.0106
	(0.00235)	(0.0578)	(0.0139)
After EM × Annual Medical Expenditures	-0.0106**	-0.188*	-0.0177
	(0.00507)	(0.100)	(0.0191)
EM × Annual Medical Expenditures × No ESI	-0.0217***	-0.443***	-0.0194
	(0.00540)	(0.109)	(0.0210)
Observations	18,836	18,836	18,523

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). Each column presents a specification with all demographic controls as seen in the final columns of Table 2. The estimates are little affected by alternate specifications. The first two columns focus on hourly wages. The final column presents estimates using an indicator for part-time employment defined as working fewer than 30 hours per week as the mandate does not mandate coverage for part-time employees.

workers to part-time employment. However, it is also possible that the null finding is due to an insufficient sample size. In each year of the MEPS sample used here, on average, about 350 workers do not already have ESI and work at employers with more than 50 employees. Focusing on the subset of those workers who work part-time relies on fewer than 50 observations per year. Essentially, there are not enough part-time workers who meet the sample restrictions to meaningfully examine the ability of employers to move higher expenditure workers to part-time employment.

In addition, a negative relationship between medical expenditures and annual wages could be partly driven by extensive margin changes in employment. For example, workers with higher medical expenditure might find it relatively harder to secure employment at the employers most affected by the mandate or suffer longer spells of unemployment. To test this hypothesis, a difference-in-difference specification was estimated that considers the likelihood of employment at an employer that offers coverage versus one that does not as a function of medical expenditures before and after the mandate's announcement. The estimates from that exercise suggest higher-expenditure individuals are less likely to obtain employment at affected employers after the mandate was announced but the estimates fall short of traditional measures of statistical significance. They are omitted to economize on space.[A2]

Lastly, MEPS data also allows for the construction of a measure of how many months an individual was unemployed during the survey year. The estimates suggest that individuals with higher medical expenditure experience longer periods of unemployment after the mandate but the coefficients are also not statistically different from zero. These estimates also suffer from the relatively small size of the sample.

[A2] Available from the author.

B Composition Bias and Matching

MEPS forms a panel data set but is used as a repeated cross-section in this paper by discarding all but one of the five interviews with survey respondents. Treating the data as a repeated cross-section (using only one set of responses per survey participant) does not invalidate the difference-in-difference estimation strategy employed. It does, however, introduce composition bias concerns.[B1] That is, the estimates for the difference-in-difference coefficients presented in the earlier tables are not the average change in labor market outcomes for employees at affected employers but instead reflect labor market outcomes for employees who happen to work at affected employers after the employer mandate is announced. While economic theory would suggest that any composition effect caused by the Act would bias estimates towards zero, the MEPS could also have observed more high cost, low-wage workers *by chance* after 2010.[B2]

A propensity-score matching exercise can help ease both causal and random composition bias concerns. Table B1 presents estimates from a propensity score matching exercise. The matching procedure first divides the sample into high and low health cost medical expenditure employees based on the median of health care expenses (the 50th percentile is specific to year and offer of coverage). The procedure then matches workers in each period based on observable characteristics (race, education, marital status, age, region) in order to compare "apples to apples." The estimates in Table B1 are based upon Kernel matching and matches are allowed to be many to one with replacement. The "treatment" effect is the difference in wages between matched workers with medical expenditures above and below the median in the time period of interest. The t-statistics reflect the statistical significance with respect to the null that there is no difference. The t-statistics are calculated using bootstrapped standard errors and the match procedure forces the use of a region of common support for the propensity scores. Alternative matching methods provide qualitatively similar estimates.[B3]

The first row in the table suggests that workers with above median medical expenditures earned

[B1]The composition bias and econometric issues caused by using repeated cross-section data in a difference-in-difference estimation framework are described in detail by Lee and Kang (2006).

[B2]If it lowers demand for such workers, the mandate likely reduces the likelihood of a high cost worker being observed working at an affected firm. For the bias this introduces to impact estimates of wages away from zero the unobserved workers (due to attrition caused by the Act) would have to have been positively selected. That is, they would have to be a set of very high-wage individuals to counteract the relative reduction in wages seen in the data.

[B3]Including caliper and nearest-neighbor methods. Available upon request.

Table B1: Average Treatment Effect - Propensity Score Matching using Kernel Method

Period	ESI?	Average "Treatment" Effect	(t-statistic)	High-Cost Matched	Low-Cost Matched
Pre-EM	Offered ESI	2,376.45	(2.64)	4,989	4,707
(2006 - 2010)	Not Offered ESI	899.07	(0.57)	732	734
Post-EM	Offered ESI	3,280.57	(7.28)	6,616	6,321
(2011 - 2014)	Not Offered ESI	-1127.94	(-1.13)	1,244	1,227

The "Treatment Effect" in the table is the difference in wages between matched workers with medical expenditures above and below the median in the time period of interest (pre- or post-EM) at employers who do and do not provide coverage. While not statistically significant, the effects on workers at employers who do not offer coverage are qualitatively similar to those seen in the OLS difference-in-difference estimates in earlier tables and show higher cost workers face relatively lower wages after 2010 only at employers who do not already offer coverage.

\$2,367.45 more per year than matched workers with below-median expenditures before the mandate's announcement at employers that provide ESI. After the announcement, that gap is \$3,280.57. In contrast, the difference between high and low cost workers at employers that do not offer coverage changes in the opposite direction. That is, workers with more medical expenditures earn less than similar workers with fewer medical expenditures after the employer mandate was announced. However, in both periods, the estimate is not statistically different from zero. This is potentially due to the relatively small number of observations the estimates are based upon. Despite the lack of statistical significance, the change in the sign of the estimate aligns well with the estimates in Table 2 of the main paper. As a result, these estimates reduce concerns that the paper's findings are driven by changes in the characteristics of workers observed after 2010 that are unaccounted for by OLS regression on repeated cross-sections of the MEPS.