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Are the costs of employer-sponsored health insurance passed on to workers at the individual level?

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ABSTRACT

Because employer-sponsored health insurance (ESI) is experience rated, employers have an incentive to try to offset its cost by paying lower wages to employees who have greater medical expenditures. The existing evidence on this topic, however, illustrates only that ESI is associated with lower wages for groups of workers who are costlier to cover. In contrast, I use the variation provided by the Affordable Care Act's employer mandate to examine if differences in medical expenditures are passed on to workers at the individual level. My estimates rely on Medical Expenditure Panel Survey data in a dose response difference-in-difference framework that examines how wages change for workers with varying medical expenditures when they must soon be offered ESI. I find that each \$1 difference in medical expenditures is associated with a \$0.35 to \$0.51 wage offset after the employer mandate's announcement wherever ESI must soon be offered to workers. Placebo analyses, focusing on workers whose employers are not affected by the mandate, provide support for a causal interpretation. I also show that my findings are not sensitive to sample selection or data reliability issues and that they cannot be explained by the effects of the Great Recession, demographic characteristics that correlate with medical expenditures, or location- or industry-specific idiosyncratic shocks.

1. Introduction

Research shows that health and health behaviors can impact labor market outcomes. Examples include the effects of obesity status (Baum and Ford, 2004; Greve, 2008; Han et al., 2009, 2011; Bhattacharya and Bundorf, 2009; Lindeboom et al., 2010; Mosca, 2013; Caliendo and Gershitz, 2016; Kinge, 2016; Chu and Ohinmaa, 2016), substance use including smoking and drinking behaviors (Van Ours, 2004; Auld, 2005; Grafova and Stafford, 2009; Cowan and Schwab, 2011; Lång and Nystedt, 2018), and particular conditions including diabetes (Kahn, 1998; Zhang et al., 2009), cancer (Bradley et al., 2005; Moran et al., 2011; Heinesen and Kolodziejczyk, 2013; Jeon, 2017), and mental health issues (Ettner et al., 1997; Baldwin and Marcus, 2007; Frijters et al., 2014; Peng et al., 2016). These studies consider how health can impact labor market outcomes via productivity differences, employer discrimination, or selection into certain health behaviors.

In the United States, however, health can also affect labor market outcomes via employer-sponsored health insurance (ESI). ESI can affect outcomes because it is experience rated, which ensures that the cost of ESI for an employer depends on the actual medical expenditures of their employees.[2] For employers, ESI 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 expenditures. Given how ESI can affect overall compensation, it is unsurprising that studies show that groups of workers with greater medical expenditures (such as females, older workers, and obese workers) tend to earn lower wages wherever ESI is available (Gruber, 1994a; Sheiner, 1999; Levy and Feldman,

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[2] The cost of ESI reflects expenditures whether an employer chooses to self-insure or opts for a traditional insurance plan. 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.

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2001; Bhattacharya and Bundorf, 2009; Cowan and Schwab, 2011, 2016; Lahey, 2012; Bailey, 2013, 2014; Lennon, 2018, 2019).

What remains unclear from these studies is the level at which cost-shifting occurs.[3] Is it only at the group level? Or, given variation in health status across individuals within such groups, can employers shift some or all of the cost of ESI onto those particular workers who have greater medical expenditures? The level at which cost-shifting occurs is important because cost-shifting at the individual level could undermine the risk-pooling benefits of ESI. In theory, employment-based groups pool risk effectively because they limit the ability of insurers to screen group members while also precluding individuals from seeking coverage only when they require care. ESI can therefore mitigate asymmetric information problems inherent in health insurance markets (Arrow, 1963). If, however, employers face premiums that are sensitive to their employees' expenditures, ESI transfers the incentive to screen from the insurance company to the employer. To the extent that employers can shift the cost of ESI onto individual workers, ESI provides only a limited form of insurance. On one hand, such an outcome seems efficient: workers pay for a valuable workplace benefit (ESI) via lower wages. On the other hand, ESI could create barriers to employment for workers whose total compensation (wages plus the cost of ESI) exceeds the value of their marginal product.[4]

Recognizing that employers have an incentive to shift costs onto individual workers, Levy and Feldman (2001) use 1996 Medical Expenditure Panel Survey data to determine whether wage offsets occur at the individual level. Because workers who use less medical care might be systematically more productive, they rely on a fixed-effects strategy that exploits changes in ESI status over time for identification. They find little evidence, however, of individual-specific cost-shifting noting that "[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."

The Affordable Care Act's (ACA) employer mandate provides the necessary exogenous variation. Specifically, the mandate requires employers with more than 50 full-time equivalent (FTE) employees to provide ESI to those who work more than 29 hours per week from 2014 onward.[5] I exploit the employer mandate's requirements to estimate how ESI affects wages at the individual level in a "dose response" difference-in-difference framework. In particular, using 2006–2014 Medical Expenditure Panel Survey (MEPS) data, I examine whether workers with greater medical expenditures, who work where ESI must soon be offered, earn relatively lower wages after the mandate's announcement. My main estimates show that, among workers who work wherever ESI must soon be offered, annual wages are lower by between 35 and 51 cents for each $1 difference in annual medical expenditures after the 2010 announcement of the mandate (relative to the same relationship in the years before the mandate). These effects are about as large as could be expected given employees would pay some of their own expenses via deductibles and co-insurance, current medical expenditures are only an indicator of future medical expenditures, and that employee medical expenditures are tax deductible for employers.

To support a causal interpretation for my findings, I then show that there is no change in the relationship between wages and individual medical expenditures during the same time period among workers in two placebo groups: workers who are already offered ESI and workers who work for employers who are too small (i.e., fewer than 50 FTEs) to be covered by the employer mandate's requirements. More generally, my estimates can be considered as causal if there are no idiosyncratic shocks that affect workers who have varying medical expenditures differently, and who work wherever ESI must be soon offered, over this time period. I further support my identification strategy by showing that, wherever the mandate requires ESI to soon be offered, workers with average medical expenditures experience a decline in wages similar to the average after-tax cost of providing ESI. While helpful for establishing that the mandate can be a valid source of identification, such a finding adds little to the existing literature on how ESI affects wages (see Kolstad and Kowalski, 2016, for example). Instead, my main contribution is to show that ESI-related wage offsets are largest for those individuals with greater medical expenditures, even after controlling for differences in medical spending across groups.[6] Put together, my findings suggest that (1) workers who work wherever ESI must soon be offered experience relatively lower wages after the employer mandate is announced, and (2) the size of the effect on wages varies across individuals based on their current medical expenditures.

Note that my findings, because I focus on what happens in the years between the announcement and implementation of the employer mandate, represent anticipatory effects. An anticipatory approach helps to avoid confounding ACA provisions that come into effect from 2014 onward, such as the individual mandate, the ACA's insurance exchanges, and medicaid expansions.[7] Moreover, employment is generally an ongoing arrangement, which matters because existing studies consistently show that ESI will be paid for via lower wages. At issue is only the timing of ESI-related wage effects. If labor is demanded in a spot market, lower wages will be observed only on the day ESI must be offered. On the other hand, because the labor market is not a spot market, theory suggests the mandate will cause anticipatory changes to labor demand (and, in turn, wages). Relying on a similar argument, and because the employer mandate only required that ESI be offered to full-time employees, Garrett and Kaestner (2015), Mathur et al. (2016), and Even and Macpherson (2019) study how the employer mandate affected part-time employment with mixed findings.

An additional challenge for identification is that I observe only current medical expenditures while employers should predominantly care about future expenditures. For that reason, I show that current medical expenditures are a strong predictor of future expenditures, at least in my sample (see discussion in Section 5 and estimates in Appendix A). Note, however, that my approach does not depend on employers observing workers' exact medical expenditures. Indeed, laws regarding the protection of health information are supposed to prevent employers from observing the history of expenses incurred by specific workers.[8] Despite such efforts, employers may be able to determine health status and potential medical expenditures. At a job interview, for example,

[3] That is, the findings in the literature 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 $1-p$.

[4] Illustrating such a trade-off, Marks (2011) finds higher minimum wages lead to fewer workers being offered ESI.

[5] The mandate was later delayed, see Section 2.

[6] Notably, we might be concerned that workers strategically delay care. However, I find no statistically significant change in medical expenditures in response to the mandate. See Section 3 and Table A14 in Appendix A for more on this concern.

[7] Data limitations also favor an anticipatory approach because only a small fraction of MEPS respondents provide data around the mandate's implementation date and work for an employer who must offer ESI. Even if there were a sufficient sample, such an approach is valid only if there were no anticipatory effects.

[8] Insurance companies produce reports for employers about the dates and costs of employees' medical spending. These reports are anonymous but there is evidence to suggest employers can connect the dots. As one example, in 2014 AOL CEO Tim Armstrong blamed changes in employee compensation on medical costs incurred by just two employees (out of about 5000 employees). More here, last accessed 8/24/2020.

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. Other medical issues could become apparent with repeated interaction between employers and employees including information gleaned from changes in daily appearance, time absent due to illness, and so on. To the degree that employers can identify at least some individuals with substantial ESI costs, theory predicts there will be a negative correlation between individual medical expenditures and wages in labor market data, all else being equal.

In Section 2, I highlight how my work contributes to the existing literature on mandated employment benefits. I also explain the employer mandate's implementation timeline and requirements. In Section 3, I provide a conceptual framework to think about ESI and wages and then describe my approach to estimation. I explain my MEPS data in Section 4. In Section 5, I present my main estimates while Section 6 examines their robustness. I offer concluding remarks in Section 7.

2. Background and existing literature

Summers (1989) provides a succinct overview of the economics of mandated benefits, highlighting how they are similar to payroll taxes, how they differ, and why that makes them politically popular. Summers was particularly concerned that mandated benefits could lead to exclusionary hiring practices if wages were not free to adjust for the cost of benefits employers must provide. If a mandated benefit resulted in such behavior Summers saw value in public provision of the benefit because “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 employer mandate appears to be the type of mandated benefit that Summers’ was concerned about. Enacted in March of 2010, the mandate requires employers who have more than 50 FTEs to offer affordable health coverage to workers who work more than 29 hours in a usual work week. Even and Macpherson (2019) explain that a plan “is deemed affordable if the employee's cost of coverage does not exceed 9.5% of the employee's household income.” If a firm does not offer compliant coverage (i.e., a plan that covers 60% of medical expenditures), or if workers obtained federally-subsidized health coverage in private markets (ACA “exchanges”), then the employer would be subject to financial penalties (of at least $2000 for each FTE after the first 30 FTEs).[9] These requirements, collectively referred to as the “employer mandate,” were scheduled to go into effect on January 1, 2014. In July of 2013, however, the penalties for non-compliance were postponed to 2015 and, in February of 2014, to 2016 for employers with 50 to 100 FTEs.[10] To the degree that delays reduced employers’ incentives to react, my estimates will understate the mandate's impact on workers. Note that the employer mandate remains in place for 2020.[11]

Ideally, to provide clean identification, the mandate would have become effective on the day the ACA passed. However, if employment is an ongoing arrangement, and if employers can infer who will be more costly to cover, there should be anticipatory effects (Garrett and Kaestner, 2015, Mathur et al., 2016, and Even and Macpherson, 2019 make a similar argument). Further emphasizing the potential for anticipatory responses by employers, the cost (to employers) of ESI for 2014 was to be based on the expected costs of a firm's employee pool in 2013. It is also worth noting that while many struggled to decipher the ACA's various requirements, there is ample evidence that insurers and industry groups had developed comprehensive reports by mid-2011 advising employers of the upcoming changes and how to prepare for them.[12]

In addition to Summers’ theoretical analysis, it is possible to predict how the employer mandate will affect labor market outcomes because numerous studies document the empirical regularities of mandated employment conditions and benefits. Examples include Gruber and Krueger (1991), Gruber (1994a,b), Acemoglu and Angrist (2001), Baicker and Chandra (2006), Baicker and Levy (2008), Lahey (2012), Bailey (2013, 2014), and Cowan and Schwab (2011, 2016). Gruber (1994a), who studies state-level maternity benefit mandates, is the canonical example of this type of work. He finds that wages fall for groups likely to benefit from mandated maternity coverage, including young females and married men, relative to the same groups in states that did not mandate such coverage as part of ESI. However, Gruber's data does not allow him to examine if the incidence of the cost of these benefits is similar across workers or if individuals who have multiple or complicated births face larger wage reductions. Other authors encounter a similar data limitation when studying state-level mandates (including Lahey, 2012 and Bailey, 2014). They could examine outcomes at the individual level by using data on medical expenditures and health conditions from the Medical Expenditure Panel Survey (MEPS). However, any empirical approach that relies on variation in a handful of states at different times would slice MEPS data very thinly.[13] In addition, MEPS data is only available from 1996 onward, whereas state mandates that significantly increased the cost of providing ESI were mostly implemented in the 1970s and 1980s (see Gruber, 1994a).

In many mandated benefit studies, identification relies on intensive margin changes in the generosity of existing coverage, such as adding maternity, infertility, or diabetes coverage benefits to ESI. On the extensive margin, 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 obtain ESI. Kolstad and Kowalski (2016) focus on the effects of the 2006 health care reform in Massachusetts. They find that wages at employers who were required to provide ESI fall by approximately the cost of coverage. In each of these studies, the available data precludes an analysis of how wages change 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 regressions using data from the 1996 Medical Expenditure Panel Survey. As I mention earlier, however, their identification strategy ensures endogeneity problems.

Note that the existing work in this area tends to focus on how mandated benefits affect wages because differences in wages can capture the various ways employers can react to the costs of providing benefits. In the case of ESI, new hires may be offered lower wages if they appear “unhealthy,” those who are “healthier” might be offered higher wages, and wage increases and promotions for existing employees may be biased towards employees who add less to the cost of ESI. Changes on the extensive margin (hiring/firing) can also affect wages even if there are no observable effects on unemployment rates or duration. For

[9] See the complete description of compliant coverage here. Last accessed 8/24/2020.
[10] For more information on the delay see here. Last accessed 8/24/2020.
[11] The IRS explains the mandate's requirements, including increased penalties for non-compliance, for 2020 here. Last accessed 8/24/2020.
[12] One example is the Hudson Institute report for franchise owners dated September 2011 – see here. Last accessed 8/24/2020.
[13] To obtain state identifiers, these researchers would also have to obtain access to unrestricted MEPS files at a Census Research Data Center.

example, workers with higher medical expenditures who lose their job might then be hired elsewhere at a slightly lower wage. In such a case, employment rates for workers with greater medical expenditures are stable but their wages are lower.[14] 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 employers flexibility to tailor benefit packages to match workers' preferences. Unfortunately, I cannot examine the role of employee contributions in response to the employer mandate as my focus is on the period before ESI is required. In any case, employee contributions towards ESI cannot vary across workers for the same coverage.[15] For that reason, the incentive to shift the cost of medical expenditures onto workers with greater medical expenditures persists. In the case of the employer mandate, employers cannot reduce their responsibility by providing low-quality coverage because the mandate requires generous coverage with no cost sharing for essential benefits, that has at least a 60% actuarial value, includes limits on out of pocket maximums, and also restricts employee contributions to coverage to 9.5% of income. Adjusting wages to account for generous ESI coverage may be the only feasible option.

Given the limitations in existing studies, the employer mandate is valuable for three reasons. First, the mandate is at the federal rather than state level, which side-steps the data limitations that have precluded studying individual-level cost-shifting in the past. Second, the mandate represents an 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. I provide a conceptual framework for ESI and wages and describe my approach to estimation in the next section.

3. Conceptual framework and empirical strategy

3.1. Conceptual framework

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_i). In such a world, if ESI is mandated, wages would then have to be modified by the cost of ESI 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. In theory, employers could pool all costs across their N employees so that wages for worker i at firm k are

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

Eq. (1), however, cannot be an equilibrium in a competitive labor market because it leaves arbitrage opportunities open for workers and employers. At the other extreme, the literature generally assumes that the incidence of ESI cannot be individual specific. If incidence were individual specific, wages for each worker would be

$$w_{ik} = MRP_{ik} - p_{ik} = MRP_i - e_i. \tag{2}$$

Instead, the literature seems to have settled on the idea 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 $i \in n_j$, $j = 1, 2, \ldots, m$, wages (excusing the abuse of notation) for worker i would be

$$w_{ijk} = MRP_{ijk} - \frac{1}{n_j}\Sigma_{i=1}^{n_j} e_{ijk}. \tag{3}$$

In (3), wages are equal to each worker's marginal product minus the average medical spending within their group, j. Such a situation is potentially an equilibrium if workers (and employers) do not find it profitable to incur the costs of exploiting any remaining opportunities for arbitrage.[16]

The problem for those who have studied ESI's effect on wages is that it is not easy to distinguish empirically between Eqs. (2) and (3) – wages for individual workers in group j could differ because productivity varies across workers in group j, workers in group j have different medical expenditures, or both. Despite these challenges for identification, the literature has generally assumed that Eq. (2) does not describe wages wherever ESI is part of compensation, but whether that is true or not is an empirical question. To determine whether wages are best described by Eq. (2) or Eq. (3), I rely on the variation provided by the employer mandate. If the employer mandate does not affect productivity, then changes in wages that are correlated with individual differences in medical expenditures (after the mandate's announcement, wherever workers are not already offered ESI) suggest that employers can pass along the cost of ESI at the individual level.

3.2. Approach to estimation

Because the employer mandate requires only those employers who have 50 FTEs or more to offer ESI, I use a difference-in-difference framework to estimate the mandate's effect on the earnings of workers with varying medical expenditures. In particular, my main findings estimate whether the relationship between wages and medical expenditures changes after the mandate's announcement, wherever ESI must soon be offered, relative to the period before the employer mandate's announcement. Because my unit-specific characteristic (medical expenditures) is continuous, my approach should be considered as a "dose response" difference-in-difference (Argys et al., 2020). In placebo analyses, to support a causal interpretation for my main findings, I examine the same relationship between wages and medical expenditures after 2010 for workers who work for employers that are not required to offer ESI by the employer mandate. In both

[14] As a practical matter, examining non-wage outcomes is challenging using MEPS as, for relevant subsets of the sample, there might only be a few dozen observations per year that can inform us about non-wage outcomes such as job switching or unemployment duration.

[15] The Health Insurance Portability and Accountability Act of 1996 (HIPAA) ensures that "[g]roup health plans cannot charge an individual more for coverage than other similarly situated individuals based on any health factor." See the Department of Labor's instructions for employers here. Last accessed 8/31/2020.

[16] Note that it is straightforward to make similar predictions in a general equilibrium labor market search model (see Mortensen, 1990 and Bowlus and Eckstein, 2002). A general equilibrium model would position the requirement to provide ESI as 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 expenditures and will cause a decline in equilibrium wages, a lower level of employment, and/or longer periods of job search for those workers across the economy. While such a 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. 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.

my main findings and placebo analyses, the basic estimating equation is;

$$\begin{aligned} \textit{Labor Market Outcome}_{it} = \quad & \beta_0 + \beta_1 \textit{Medical Expenditures}_{it} + \beta_2 \textit{After EM}_{it} \\ & + \beta_3 \textit{Medical Expenditures} \times \textit{After EM}_{it} + \Pi X_{it} + \varepsilon_{it}. \end{aligned}$$

In the estimating equation, $\textit{Labor Market Outcome}_{it}$ represents an outcome of interest for person *i* at time *t*. In my main estimates, I focus on annual wages but $\textit{Labor Market Outcome}_{it}$ can be any variable that responds to a change in labor demand including hours worked, hourly wages, or unemployment duration. The right hand side of the estimating equation controls for the relationship between wages and health expenditures across the entire sample period using a continuous measure of health spending ($\textit{Medical Expenditures}_{it}$). Next, the estimating equation controls for the main effect of the employer mandate ($\textit{After EM}_{it}$), ensuring that $\hat{\beta}_2$ captures changes that affect all workers equally in the period after the employer mandate is announced, regardless of medical expenditures. I consider 2006 to 2010 as "before" the employer mandate and 2011 to 2014 as "after" meaning that the $\textit{After EM}_{it}$ indicator variable equals 1 from 2011 to 2014, and 0 otherwise. Designating 2010 as "before" the employer mandate is appropriate because the mandate passed in late March of 2010 limiting any potential effect on annual wages in 2010; I provide estimates where I define the treatment period differently as a robustness check (see Table 3). In turn, the $\hat{\beta}_3$ coefficient estimates changes in the relationship between medical expenditures and wages in the period after the employer mandate is announced. The estimate represents a causal effect only under an identifying assumption that there are no idiosyncratic shocks that affect workers who have varying medical expenditures differently and who work at firms that must offer ESI because of the employer mandate. In my preferred specification, I include demographic controls and fixed effects (X_{it}) including age, sex, education, marital status, race, census region, occupation, and industry. In all specifications, ε_{it} represents an idiosyncratic error.

Intuitively, my dose response difference-in-difference approach first estimates the slope of the relationship between wages and medical expenditures across the entire sample period. I then control for changes that affect all workers equally after 2010. Finally, the interaction term examines whether the relationship between wages and medical expenditures is *different* after the employer mandate is announced. Theory predicts the relationship will change only for workers who must be offered ESI because of the mandate (i.e., $\hat{\beta}_3 < 0$). On the other hand, for workers who are already offered ESI the relationship between wages and medical expenditures should remain unchanged because their wages already incorporate the cost of ESI (i.e., $\hat{\beta}_3 \approx 0$ when using a placebo group). In Section 4, I explain the MEPS data I use to determine whether such predictions are accurate.

Before doing so, it is worth noting here that one potential source of bias is that some workers could delay medical care in anticipation of receiving mandated ESI coverage. On the other hand, there are several reasons why delaying medical care would be either unnecessary or infeasible. The first is that a majority of workers without ESI from their own employer have medical coverage from some other source (Medicaid, non-group coverage, or group coverage via a partner's employer).[17] In particular, looking at the years 2011 to 2014, 51.6% of MEPS respondents who work at firms with more than 50 workers and are not offered ESI report that they have some other form of health insurance. These respondents have no insurance-related reason to delay care.[18]

The second reason is that, among workers without coverage, only some medical care can be effectively postponed and, even then, only for a limited period of time. Third, an informed decision to delay care would only be possible if a worker intended to remain employed full-time at the same employer, the worker knew their employer would have at least 50 FTEs after 2014, they were sure that the ACA would not be wholly or partially repealed, and they were confident that the mandate would come into effect on time. They would also need to know that the firm would comply with the employer mandate rather than pay the mandate's shared-responsibility penalties and be sure that their employer would offer coverage that meaningfully reduces the out-of-pocket cost of the delayed care. Finally, a strategic decision-maker would have to consider the possibility that access to care may be limited if many individuals delayed care until after the ACA came into effect.

Given so many unknowns, other sources of coverage, the non-pecuniary costs of delaying required medical care (pain, risk, and so on), the fact that urgent and emergency care cannot be delayed, and that health insurance plans tend to have significant cost-sharing – especially for the first dollar of spending – it is unlikely that workers would delay a meaningful amount of care. Confirming such a prediction, I show as an appendix item (see Table A14) that there is a small and statistically insignificant decline in the conditional mean of medical expenditures for workers at firms required to offer coverage due to the employer mandate. I present these estimates only as an appendix item because medical expenditures could change (1) because workers have incentives to delay (consistent with endogenous delays in care that would bias my estimates) or (2) because workers who are getting and retaining jobs at affected employers are exactly those who have lower medical expenditures (consistent with my argument that firms have an incentive to employ workers with lower medical expenditures). While I cannot distinguish between these competing explanations, the decline is small and statistically no different from zero, easing concerns that my estimates are biased by delays in medical spending among workers who expect to get ESI soon.

4. Data

My estimates rely on Medical Expenditure Panel Survey (MEPS) data from 2006 to 2014. The Agency for Healthcare Research and Quality (AHRQ) describes MEPS as "a set of large-scale surveys of families and individuals, their medical providers, and employers across the United States" and explains that "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.[19] MEPS respondents participate in five interviews across a two-year period where they provide detailed data on health care usage, medical expenditures, and health insurance coverage, along with demographic and employment information. Accurately capturing annual medical expenditures for each MEPS respondent is a priority for AHRQ. For example, MEPS has a medical provider component where they sample respondents' care providers to determine exact medical

[17] Note that I do not attempt to account for other coverage because (1) the employer mandate requires that the worker must be offered ESI if they work full-time at a firm with 50+ FTEs regardless of other sources of coverage and (2) an employer will not know which of their workers has coverage from some other source.

[18] Also, many workers without insurance may have some insurance available via a partner/spouse that they could activate if necessary. However, I do not have any data on such coverage options.

[19] Policy relevant subgroups (such as low income households) are over-sampled by the NHIS and subsequently MEPS. See http://meps.ahrq.gov/mepsweb/. I use MEPS-provided survey weights in my regression estimates to account for such over-sampling.

Table 1
Summary statistics by year – ages 27–55, employed.

	2006	2007	2008	2009	2010	2011	2012	2013	2014
Not offered ESI by employer									
White	76.9%	74.8%	68.7%	70.6%	71.1%	67.8%	66.7%	70.3%	61.9%
Black	18.0%	20.1%	22.3%	20.9%	20.6%	21.5%	23.1%	23.6%	29.2%
Other	5.0%	5.1%	8.9%	8.5%	8.4%	10.6%	10.1%	6.1%	8.9%
High school or less	63.6%	63.7%	64.3%	62.1%	63.2%	61.5%	59.5%	59.5%	56.3%
College	31.3%	26.5%	28.6%	31.6%	30.9%	31.5%	37.1%	37.5%	38.6%
Graduate	5.1%	9.8%	7.1%	6.3%	5.9%	7.0%	3.4%	3.0%	5.2%
Male	44.6%	43.9%	49.2%	47.1%	49.8%	46.8%	49.0%	43.6%	46.0%
Married	57.6%	59.6%	52.5%	57.9%	50.5%	51.5%	50.6%	49.9%	42.5%
Age in years	39.72	39.78	39.29	39.82	39.62	39	39.1	39.16	39.05
Std. Dev	7.96	8.3	8.25	8.27	8.44	7.89	8.29	8.41	8.16
Annual earnings ($2014)	$23,763.16	$21,878.36	$23,034.84	$20,244.05	$20,127.14	$20,104.25	$21,540.09	$17,169.85	$21,477.03
Std. Dev	24,647.88	22,100.45	23,878.76	19,540.85	20,943.10	22,148.60	22,877.69	14,294.76	21,565.52
Medical expenditures ($2014)	$1761.91	$1843.45	$1725.63	$1962.82	$2134.38	$1132.07	$1203.07	$1695.36	$1722.92
Std. Dev	5008.72	4209.60	4176.47	6034.65	7116.46	3090.61	3776.38	4816.94	4945.92
Observations	616	314	358	340	311	423	484	411	496
Offered ESI by employer									
White	71.7%	70.6%	63.8%	64.4%	65.4%	66.0%	64.4%	63.4%	63.2%
Black	20.1%	18.8%	23.1%	23.4%	22.8%	22.3%	22.4%	22.4%	22.9%
Other	8.2%	10.6%	13.1%	12.2%	11.8%	11.8%	13.2%	14.2%	13.8%
High school or less	41.4%	38.6%	38.0%	35.4%	36.3%	34.9%	31.7%	30.1%	32.2%
College	45.9%	46.7%	46.5%	49.2%	46.9%	51.0%	53.9%	56.1%	52.8%
Graduate	12.7%	14.7%	15.5%	15.4%	16.8%	14.1%	14.4%	13.8%	15.0%
Male	53.4%	53.3%	47.9%	51.0%	51.9%	51.1%	53.2%	51.2%	52.1%
Married	63.2%	65.2%	61.8%	60.0%	60.0%	59.0%	60.0%	57.3%	58.3%
Age in years	41.46	41.56	40.99	41.29	41.09	41.26	40.98	41.01	40.87
Std. Dev	8.11	8.28	8.33	8.22	8.36	8.4	8.42	8.57	8.42
Annual earnings ($2014)	$52,797.42	$52,898.28	$51,588.11	$50,520.29	$51,509.49	$50,584.33	$52,687.24	$50,674.46	$50,250.82
Std. Dev	35,616.90	39,950.47	35,394.96	34,107.62	36,390.19	36,841.66	36,202.01	35,415.37	37,663.58
Medical expenditures ($2014)	$3256.25	$3280.42	$2959.98	$3188.35	$2995.25	$2682.81	$2566.99	$2567.92	$2697.14
Std. Dev	7341.36	7114.29	6419.57	7348.15	7185.31	6354.86	6253.42	5451.50	6328.51
Observations	3422	1533	2030	1799	1582	1945	1800	1673	2418

Source: Medical Expenditure Panel Survey 2006–2014, employed respondents age 27–55. Medical expenditures and wages adjusted to 2014 dollars using CPI values (www.bls.gov).

expenditures. MEPS also corrects for insurance-negotiated discounts that may not be apparent to respondents. I provide more details about MEPS and its various components in Appendix B.

My main estimation sample consists of employed MEPS respondents age 27–55 who appear in MEPS Panels 10 through 19 (2006 to 2014). I exclude individuals age 26 and under because their labor supply after 2010 could be affected by the ACA's dependent coverage mandate.[20] I exclude individuals older than 55 because if the employer mandate leads to lower wages for those with higher medical expenditures, that could affect retirement decisions for those workers and bias my estimates. Indeed, Ayyagari (2019) finds that individuals plan to retire about 4 to 7 months earlier because of the ACA's provisions. I present estimates including these older workers as a robustness check (see Table 3).

Because the employer mandate exempts firms with fewer than 50 full-time equivalent employees, my main estimates conservatively exclude respondents who report working at firms with fewer than 50 employees.[21] I rely on information on the number of employees at a respondents' work location and whether or not the employer has other locations to assign MEPS respondents into "more than 50" or "fewer than 50" categories. Any worker who reports more than 50 employees at their work location can be assigned to the "more than 50" bucket, regardless of whether there are more business locations. Any worker who reports fewer than 50 employees at their location and that their employer has no other locations is assigned to the "fewer than 50" size bucket. Because of the ambiguous number of employees, I exclude respondents who report fewer than 50 workers at their location but also that their employer has more than one location. I provide estimates where I restore such respondents to my sample in Appendix A (Table A8). In addition, because some respondents may not be able to report accurately, I examine how sensitive my main estimates are to the 50 employee cut-off by using 75 and 100 worker cut-offs in Table 3.

[20] The dependent coverage mandate allowed workers aged 26 and under to remain on their parents' insurance (if they were not offered insurance coverage elsewhere). Antwi et al. (2013) finds 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 effects. See Goda et al. (2016) for a review of how health insurance affects the labor supply decisions of younger adults.

[21] MEPS respondents likely count part-time workers, which means their employer may not have 50 full-time equivalent employees.

Table 2
Main estimates of effect of employer mandate.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: Age 27–55, not offered ESI by employer, 50+ employees				
After EM	0.011	0.001	0.015	229.91
	(0.046)	(0.046)	(0.045)	(1213.44)
Log annual medical expenditure	0.013*	0.003	0.003	651.64***
	(0.007)	(0.007)	(0.007)	(238.07)
After EM × log medical expenditure	−0.024**	−0.022**	−0.024***	−869.16***
	(0.010)	(0.009)	(0.009)	(286.32)
Observations	5158	5065	5061	5061
N	3901	3832	3828	3828
Panel B: Age 27–55, offered ESI by employer, 50+ employees				
After EM	−0.014	−0.038*	−0.033	−1103.85
	(0.024)	(0.022)	(0.021)	(1128.70)
Log annual medical expenditure	0.023***	0.016***	0.013***	618.76***
	(0.002)	(0.002)	(0.002)	(117.37)
After EM × log medical expenditure	0.003	0.002	0.002	84.17
	(0.004)	(0.003)	(0.003)	(175.07)
Observations	28,986	28,800	28,797	28,797
N	18,200	18,074	18,071	18,071
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. The effect size and significance remain stable across specifications. In the final column, I present estimates using level wages as the independent variable for context.

I present summary statistics for my estimation sample, split into groups of respondents who work for employers who do and do not offer ESI, in Table 1.[22] In my data, medical expenditures are reported as an annual figure at the year-ending third and fifth interviews, ensuring only these two interviews are helpful for my analysis. In my summary statistics, I present information only from each respondents' first year in the survey (even if they entered in the second year, which can happen if, for example, an individual joins a MEPS household). In my regression estimates I use both year-end responses whenever available, weight my estimates using MEPS-provided respondent weights, and cluster standard errors at the individual level.

In Table 1 there are several differences between those who are and are not offered ESI, including large differences in annual earnings. The summary statistics suggest that those who are not offered ESI work fewer hours across the sample period. In addition, those who work where ESI is not offered are younger, have less education, are less likely to be married, and are more likely to be female. Such patterns highlight the importance of controlling for individual characteristics in regression estimates. Note that I focus on annual rather than hourly wages for two reasons. One, annual wages are a summary measure that can capture how a shift in labor demand affects workers via either lower wages or lower quantities of employment/hours. Two, MEPS allows respondents to report earnings flexibly and most report an annual figure. Taking 2014 as an example, 64% of my sample reported their earnings as an annual figure while only 17% of respondents reported hourly wages.

I use the data summarized in Table 1 to examine how the relationship between annual wages and medical expenditures changes after 2010 wherever ESI is not already offered to workers. If employers are forward-looking, and if they view ESI as being more expensive to provide to individual workers who have greater medical expenditures, then I should find a significant negative relationship between wages and medical expenditures after 2010 only for workers who are not already offered ESI. As I mention earlier, I use respondents who work at firms that already offer ESI as a placebo group to help establish causation. I present my main findings in the following section.

5. Main estimates

In Table 2, I present OLS estimates of how the relationship between medical expenditures and annual wages changes in the period after the employer mandate is announced. In Panel A, because the employer mandate applies only wherever there are 50 FTEs or more, I focus on workers not already offered ESI but who work where there are more than 50 employees. In the specification in the first column of Table 2, I do not include any demographic controls or fixed effects. I add controls and then fixed effects in subsequent specifications. The dependent variable is the log of annual wages (except for the final column, where I present estimates using the level of annual wages for context). The coefficient on “After EM × Medical expenditures” is the estimate of interest in each specification because it measures how the relationship between wages and medical expenditures changes after the announcement of the employer mandate. That (dose response) difference-in-difference coefficient is consistently negative and statistically significant at the 1% level in a specification with a complete set of controls and fixed effects, indicating that those who have greater medical expenditures experience lower wages in the period after the announcement but prior to the implementation of the employer mandate.

Put differently, I find large and statistically significant anticipatory effects associated with the employer mandate. In particular, the coefficients in the first three columns of Table 2 are elasticities because both medical expenditures and annual wages are log-transformed (due to their skewed distribution, see Table 1). The coefficients imply that for a 100% difference in medical

[22] Note that to avoid distorting my summary statistics, I eliminate 39 responses (from among those aged 27 to 55 and employed at a firm with more than 50 workers) who report more than $100,000 in annual medical expenditures. For many of these individuals there are also data quality and missing information concerns. In Table A5 in Appendix A, I restore those individuals with more than $100,000 in annual medical expenditures to my sample. The estimates show that these individuals have little effect on my main findings.

Table 3
Effect of employer mandate – sensitivity.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: Age 27–59				
After EM × log medical expenditure	−0.021**	−0.021**	−0.022**	−807.31***
	(0.009)	(0.009)	(0.009)	(276.86)
Observations	5638	5532	5528	5528
N	4248	4169	4165	4165
Panel B: Age 27–64				
After EM × log medical expenditure	−0.015	−0.015*	−0.016*	−446.32
	(0.009)	(0.009)	(0.009)	(272.72)
Observations	6026	5908	5904	5904
N	4530	4444	4440	4440
Panel C: First MEPS interview only				
After EM × log medical expenditure	−0.027**	−0.026**	−0.026**	−852.15**
	(0.012)	(0.012)	(0.012)	(348.58)
Observations	3199	3144	3143	3143
N	3199	3144	3143	3143
Panel D: 75+ employees				
After EM × log medical expenditure	−0.033***	−0.029***	−0.030***	−1111.20***
	(0.011)	(0.010)	(0.010)	(316.79)
Observations	4370	4288	4284	4284
N	3335	3275	3271	3271
Panel E: 100+ employees				
After EM × log medical expenditure	−0.028**	−0.023**	−0.025**	−871.60***
	(0.011)	(0.011)	(0.010)	(323.54)
Observations	4083	4002	3998	3998
N	3100	3041	3037	3037
Panel F: After EM = 2010 to 2014				
After EM × log medical expenditure	−0.019*	−0.017*	−0.017*	−608.33*
	(0.010)	(0.009)	(0.009)	(322.60)
Observations	5158	5065	5061	5061
N	3901	3832	3828	3828
Panel G: 2010 omitted				
After EM × log medical expenditure	−0.024**	−0.022**	−0.023**	−822.58**
	(0.010)	(0.010)	(0.009)	(322.85)
Observations	4677	4590	4586	4586
N	3690	3625	3621	3621
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable.

expenditures, there would, approximately, be a $100 \times \hat{\beta}$ difference in annual wages. Specifically, the estimates in column three, a specification with a complete set of demographic controls along with occupation, location, and industry fixed effects, suggest that a 100% difference in medical expenditures is associated with a 2.4% relative decline in wages after the employer mandate is announced. Among those in my estimation sample, respondents who work for employers that would be affected by the employer mandate earned an average of $24,896 per year and medical expenditures were $1722 per person on average. Back of the envelope calculations therefore suggest that an individual with medical expenditures of about $3400 relative to one with about $1700 in medical expenditures would earn $598 less per year. Such a wage offset amounts to $0.35 for each dollar of additional medical expenditures.[23] The estimates in column four use the level of annual wages as the outcome variable to aid interpretation. Those estimates suggest that for a 100% difference in medical expenditures, annual wages would be $869 lower after 2010, all else equal. For a similar $1700 difference in medical expenditures, the estimates therefore imply a pass-through of $0.51 for each $1 of medical expenditures.

To lend support to a causal interpretation for my findings, in Panel B of Table 2, I focus on how wages change for workers at employers who already offer ESI. If the employer mandate is the cause of the estimates I present 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 estimates in Panel B consistently show no significant effect across each specification. Note that such a finding does not mean there is no relationship between wages and medical expenditures at these employers. It shows only that the relationship does not change in the years after the employer mandate is announced. Notably, in all specifications, I find that wages and medical expenditures are positively correlated, regardless of ESI status. This 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 unobserved

[23] $24,896 × 2.4% = $598 and $598/$1700= $0.35.

relationship between experience, age, wages, and medical expenditures. As long as that relationship is stable during my sample period, it causes no issues for identification.

A relative decline in wages of $0.35 to $0.51 for each $1 difference in medical expenditures is large for several reasons. One, 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. Two, the U.S. tax code heavily subsidizes employee medical coverage: while such spending is not taxed as income for workers, it is also the case that 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. Three, my estimates are based only upon current rather than future medical expenditures. That being said, research shows that current medical expenditures are a good predictor of future expenditures (see Bertsimas et al., 2008 for an overview of such work). In my data, repeating the estimation in column three of Table 2 but with next year's medical expenditures as the dependent variable suggests that an additional $1 of medical expenditures in year t is associated with $0.50 to $0.67 in greater medical expenditures in year $t+1$ for that individual. I provide those estimates in Appendix C. Four, employers who experience relatively high employee turnover would have a diminished incentive to respond to the mandate. In my MEPS data, however, over 60% of workers at employers without ESI have employment tenure of two years or more. Last, 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. The mandate's affordability requirements (see Section 2 for the details), however, tend to limit the effectiveness of such a strategy, particularly given the average wage (see Table 1) at firms that did not already offer ESI.

Note that, in Table 2, the coefficient on the main “Employer Mandate” term in Panel A reflects wage changes after 2010 for a worker with zero medical expenditures. As I mention earlier, average annual medical expenditures were $1722 for workers without ESI in my sample. Therefore, using the estimates from column four, the change in wages for a worker with average medical expenditures after the employer mandate would be roughly $229 - 869 \times \ln(1722) = -\$2583$, which is a reasonable estimate of the cost of providing ESI to the average worker after accounting for employee cost-sharing and ESI's favorable tax treatment.[24] That is, the estimates in Table 2 show that the mandate *mattered*: workers with average medical expenditures experience relatively lower wages in the period after the mandate is announced. My findings also show that the effect on annual wages increases for workers with greater medical expenditures.

As I mention earlier, I focus on annual wages because that is how MEPS respondents tend to report their earnings. However, using annual wages means that the effects I observe could be caused by a decline in hourly wages or a decline in hours worked. For that reason, Table A2 in Appendix A uses hourly wages (in logs and levels) and then an indicator for part-time employment as dependent variables rather than annual wages. Those estimates suggest that changes in hourly wages are mainly driving my main findings. In the next section, I consider how sensitive my estimates are to my sample selection and variable definition decisions. I also examine whether my estimates are robust to alternate placebo groups, whether the assumption of parallel trends is valid, and how my estimates vary when also allowing for group-level ESI-related wage offsets.[25]

6. Sensitivity and robustness

My main estimates focus on a panel of respondents age 27–55 who report that their employer has 50 or more employees. In those estimates I consider the treatment period (“After EM”) to be the years 2011 to 2014. Further, my estimates use two years of data for respondents (whenever possible) with standard errors clustered at the respondent level. To illustrate that those sample restrictions and estimation choices are not driving my findings, in Section 6.1, I show that my estimates are not sensitive to reasonable alternate sample selections or treatment period definitions. I also show that my estimates are not driven by non-random attrition from the sample and that my estimates are very unlikely to be affected by endogenous changes in firm size or by respondents' inability to report firm size accurately.[26]

In Section 6.2, I consider if my main estimates could be driven by a poorly chosen placebo group. In Section 6.3, I examine outcomes for an alternate placebo group: workers at firms with fewer than 50 employees. The employer mandate does not apply to this group and therefore we should not find negative effects on wages, as a function of medical expenditures, after 2010. In Section 6.4, I examine the validity of the parallel trends assumption by examining the relationship between wages and medical expenditures for workers that are and are not offered ESI by their employer in the years before the mandate is announced. In Section 6.5, I examine whether my estimates could be caused by changes in wages at the group level.

6.1. Sensitivity to sample restrictions and variable definitions

I present estimates in Panels A and B of Table 3 where I include those aged up to 59 and then up to 64 in my sample. The point estimates become smaller and are measured with considerably less precision when the sample includes those up to age 64. This is not surprising, however, because my approach focuses on anticipatory responses from forward-looking employers. Such employers would be less concerned about the costs of ESI for workers who are very close to retirement or becoming eligible for medicare coverage.

In my sample, fewer 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 from the sample is perhaps not random, particularly with respect to medical expenditures, ESI status, and wages. To determine whether attrition biases my estimates, I repeat my analysis using only the first end of year response for each respondent and present those estimates in Panel C of Table 3. Reassuringly, the estimates are quite similar to my main estimates.

A further potential threat to identification is that an employer could try to avoid the mandate by reducing the number of workers they employ. Such a strategy is only an option for employers who are close to the mandate's 50 worker cut-off. However, if these

[24] The Kaiser Family Foundation reports that the before-tax cost of ESI to an employer was $5179 in 2015 for a single adult worker. See here, last accessed 8/31/2020.

[25] It is worth noting here that I would like to be able to employ a fixed effects strategy as a robustness check. For example, focusing on respondents who provide data either side of the implementation date, I could estimate changes in earnings for workers who obtain ESI (without changing jobs). Leaving aside the small number of respondents when focusing on a single MEPS wave, my main findings suggest there are anticipatory responses, which invalidates such an approach.

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

Table 4
Effect of employer mandate for workers at small employers (50 to 300 employees, single location).

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: Age 27–55, not offered ESI by employer, 50 to 300 employees				
After EM	−0.027	−0.002	0.010	1244.59
	(0.069)	(0.069)	(0.067)	(1957.07)
Log annual medical expenditure	0.015	0.010	0.009	845.39**
	(0.011)	(0.011)	(0.011)	(374.89)
After EM × log medical expenditure	−0.015	−0.018	−0.021*	−919.79**
	(0.014)	(0.014)	(0.013)	(449.71)
Observations	2374	2354	2354	2354
N	1834	1818	1818	1818
Panel B: Age 27–55, offered ESI by employer, 50 to 300 employees				
After EM	0.092	0.088	0.039	2335.37
	(0.085)	(0.077)	(0.069)	(3802.01)
Log annual medical expenditure	0.028***	0.019***	0.012**	612.82**
	(0.006)	(0.006)	(0.006)	(266.37)
After EM × log medical expenditure	−0.004	−0.005	0.004	−34.44
	(0.010)	(0.009)	(0.009)	(488.04)
Observations	3257	3238	3238	3238
N	2121	2109	2109	2109
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls or fixed effects limited to respondents who work at a firm with between 50 and 300 workers at their job location. I then add control variables for education, race, gender, marital status, age, and age squared. In the final two columns I add fixed effects for census region along with industry and occupation codes.

firms could reduce their number of employees below 50, the respondents who work at those firms would then be excluded from my sample, potentially introducing bias. Easing any such concerns, my estimates are similar when I restrict the sample to only those MEPS respondents who report working at employers with more than 75 employees and then 100 employees in Panels D and E in Table 3. Those estimates also suggest that my findings are likely not driven by respondents being unable to accurately report firm-size around the 50 worker cutoff. Note that my sample always excludes MEPS respondents who report that there are fewer than 50 employees at the respondents' work location but also that their employer has other work locations. For these respondents, it is unclear if their employer will have to offer ESI because of the employer mandate. In Appendix A (Table A8), I find that restoring respondents who work where they are not offered ESI, there are 25 to 50 workers, and there is at least one more work location to my sample has only mild effects on my main estimates. Adding those who report 1 to 25 employees (and no ESI and more than one location) considerably attenuates my estimates. To the extent that treatment status (being required to offer ESI by the employer mandate) among the excluded respondents is correlated with their employer being closer to the 50 employee cut-off, the pattern of estimates supports the idea that relative wages decline for workers with greater medical expenditures after 2010, only if those workers are employed where they will have to be offered ESI because of the employer mandate.

In Panels F and G, I present estimates where I define the treatment period (= "After EM") to include 2010 and then where I exclude 2010 from the analysis completely because it is fair to argue that treatment status was ambiguous (i.e., the mandate was announced in March of 2010). Those estimates also show a statistically significant effect of the employer mandate on the wage-expenditure relationship.

Note that, in every sensitivity analysis in Table 3, I present only the interaction terms from each specification. I present the full set of estimates and further discussion for each sensitivity analysis in Appendix A. In Appendix A, I also show that my findings are robust to several other checks including when I use the level of wages and medical expenditures (rather than logs), restore respondents who report extreme medical expenditures to my sample, restrict my sample to only full-time workers, restrict my sample to those who report that their employer has only a single business location, and where I exclude 2014 MEPS responses (because the ACA's other provisions come into effect that year).

6.2. Placebo group validity

The most striking difference between employers that do and do not offer ESI is firm size, with larger firms being much more likely to offer ESI. That means that my main estimates inherently focus on workers at small employers while my placebo analysis (Panel B of Table 2) consists of respondents who work at relatively larger employers. As a result, it may be invalid to compare outcomes across these samples. To ensure this is not a relevant source of bias, in the estimates in Table 4, I restrict my sample to respondents who work for employers who have 50 to 300 workers at their location. Such respondents, regardless of ESI status, are perhaps more comparable to one another.[27]

In Table 4, the first column presents a specification with no demographic or other controls. I then add demographic controls and fixed effects in subsequent columns. In specifications with a complete set of controls and fixed effects, the coefficients on the interaction terms are quite similar to the corresponding estimates in Panel A and Panel B of Table 2 despite smaller sample sizes. The similarity of the estimates eases concerns about the validity of the comparison between firms with and without ESI when using a broader sample in Table 2. Indeed, while not statistically different

[27] I present estimates where I restrict my sample to those who report that their employer has only a single business location in Appendix A. There, I lose many respondents and some of my specifications suffer from a lack of precision.

Table 5
Estimates of effect of employer mandate for workers at unaffected employers (<50 employees).

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.012	0.005	0.006	208.77
	(0.042)	(0.040)	(0.038)	(1383.68)
Log annual medical expenditure	0.036***	0.026***	0.017***	670.62***
	(0.004)	(0.004)	(0.004)	(122.70)
After EM × log medical expenditure	−0.008	−0.007	−0.006	−164.26
	(0.006)	(0.005)	(0.005)	(177.81)
Observations	13,580	13,448	13,448	13,448
N	8992	8905	8905	8905
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls or fixed effects, limited to respondents who work at firms with fewer than 50 employees. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable.

from my main findings, the estimate using level wages suggests that the impact of the employer mandate on workers with greater medical expenditures could be larger in these "small" firms. Such a finding hints at the mechanism driving the relationship between medical expenditures and wages: at smaller employers, relative to large firms, it may be easier to determine which workers are increasing ESI costs.

6.3. Alternate placebo group

In Table 5, I present estimates where I limit my sample to MEPS respondents who work for employers with fewer than 50 employees. These respondents form a second placebo group because the employer mandate does not require these employers to offer ESI. If my main findings are causally-related to the employer mandate, then wages should not respond to medical expenditures differently after 2010 for these respondents. In line with that prediction, I find small and statistically insignificant effects across each specification. Taken together with Panel B of Table 2, the estimates in Table 5 highlight that the only group of workers who see a change in the wage-expenditure relationship after 2010 are those workers who work for employers who have more than 50 employees and do not offer ESI to their workers, precisely the employers who are impacted by the mandate's requirements.

It is worth noting here 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. I do not remove respondents who work where there are 50–100 employees from my main estimates because any employer who can maintain an employee pool with below average expenditures would be able to obtain experience-rated coverage that is cheaper than community-rated coverage. Therefore, a preference for workers with lower medical expenditures persists. Of course, if employers who have workers with lower medical expenditures strategically opt out of SHOP marketplaces, then those markets will tend to feature adversely selected groups. The potential for adverse selection is exacerbated by the fact that employers can enter or leave the SHOP marketplaces whenever they wish.[28] In any case, the estimates in Panel E of Table 3 limit my sample to respondents who work where there are more than 100 employees, and those estimates are similar to my main findings.

Note that the pattern of estimates in Tables 2 and 5 rule out macro-economic explanations for my findings. For example, perhaps my findings are explained by the cost of medical care rising faster than wages after 2010. However, if my estimates were caused by such changes, then the same effect should also be apparent at smaller employers (Table 5) and regardless of ESI status (Panel B of Table 2). Alternatively, perhaps location- or industry-specific changes are driving my findings. As one example, perhaps firms that do not offer ESI are disproportionately located in geographic areas where wages and and the cost of medical care change for unrelated reasons after 2010. Similarly, if certain industries are disproportionately represented among MEPS respondents who are not offered ESI, and those industries fare poorly after 2010, then wages might fall for workers in those industries and the decline might be related to individual productivity, which could be lower for those with greater medical expenditures. I can test for such potential confounding effects by allowing location, industry, and occupation to have time-specific effects (essentially a two-period "trend"). I present the estimates from such an exercise in Table 6 as part of a broader consideration about whether my estimates reflect changes in wages at the individual or group level.

Similarly, comparing the period before 2010 to after 2010 raises concerns with how the economic events of 2007–2009 and subsequent economic recovery impact my analysis. Siemer (2014) finds small employers experienced between 4.8 and 10.5% slower employment growth from 2007 to 2009. This matters because employers that do not offer ESI tend to be smaller, which could bias my estimates if that reduced growth happened to vary with respect to medical expenditures. If the Great Recession's impact on smaller employers was driving my findings, however, I should find that MEPS respondents who work for employers with fewer than 50 employees experience similar wage changes. Given that I only find an effect on wages at precisely those firms that are affected by the employer mandate, economy-wide events, including the Great Recession, cannot be driving my findings.

6.4. Parallel trends

In Fig. 1, I examine whether we can assume parallel trends in the absence of the employer mandate. In particular, I present the post-estimation marginal effect of medical expenditures on annual wages by year for workers offered ESI (solid line) and those not

[28] More information on SHOP is available at https://www.healthcare.gov/small-businesses/provide-shop-coverage/shop-marketplace-overview/. Last accessed 9/1/2020.

Table 6
Estimates of effect of employer mandate with flexible demographic controls and fixed effects.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.115	−0.022	0.781**	20,046.08*
	(0.079)	(0.085)	(0.314)	(10,464.88)
Log annual medical expenditure	0.013*	0.003	0.003	588.99**
	(0.007)	(0.008)	(0.008)	(245.63)
After EM × log medical expenditure	−0.024**	−0.021**	−0.026***	−775.51***
	(0.010)	(0.010)	(0.010)	(291.08)
Observations	5149	5056	5052	5052
N	3899	3830	3826	3826
Demographic controls × After EM		Y	Y	Y
Fixed effects × After EM			Y	Y

Standard errors clustered at the respondent level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls. I then add control variables and fixed effects as indicated. Note that in these estimates, I further interact each control and fixed effect with the indicator for the period after the employer mandate's announcement. This has the effect of allowing the effect of characteristics such as gender, age, education, and so on, to vary after 2010. The estimates also control for the case where industries or occupations might experience idiosyncratic shocks after 2010, which could bias my estimates. Again, the final column presents estimates using the level of annual wages for context. Note that the "After EM" coefficient is inflated because it is interacted with a number of demographic characteristics plus region, industry, and occupation fixed effects. The coefficient therefore corresponds to the estimate for the omitted category for all of these interaction terms simultaneously.

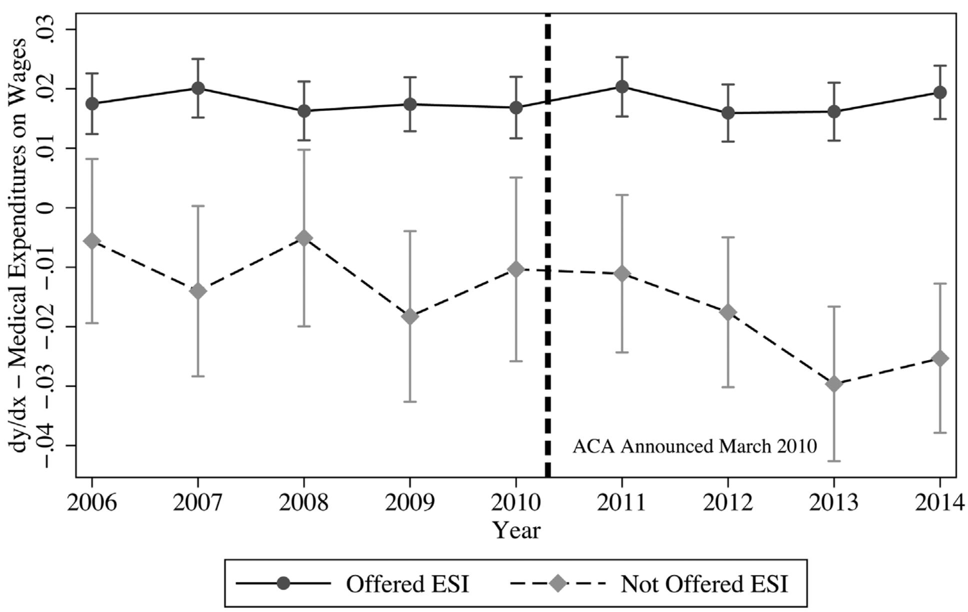

Fig. 1. Marginal effect of medical expenditures on wages by year and ESI status.

offered ESI (dashed line). Compared to my main estimates, to produce Fig. 1, I replace the "After EM" indicator in my estimating equation with a series of indicators, [1(year=t)], for each year of my sample.[29] The modified estimating equation is

$$\begin{aligned} Log\,Annual\,Wages_{it} = \; & \beta_0 + \beta_1 Log\,Medical\,Expenditures_{it} \\ & + \sum_{t=2006}^{2014} \beta_t [1(year = t)] \\ & + \sum_{t=2006}^{2014} \gamma_t [1(year = t)] \times Log\,Medical\,Expenditures_{it} \\ & + \Pi X_{it} + \varepsilon_{it}. \end{aligned}$$

I estimate the coefficients from such an equation using data on MEPS respondents who are and are not offered ESI (and who work where there are more than 50 employees) separately. I then plot the marginal effect of medical expenditures on wages ($\hat{\beta}_1 + \hat{\gamma}_t$) for each t in Fig. 1.[30] The figure illustrates that the relationship between medical expenditures and wages becomes increasingly negative in the years after the mandate is announced only for workers not offered ESI. For workers who are already offered ESI, the relationship is stable over time. The pattern of estimates eases concerns about parallel trends in the absence of the employer mandate.

6.5. Group- or individual-specific effect?

In Table 6, I present estimates from specifications that allow the effects of demographic controls and fixed effects to also vary over

[29] This is essentially an event study modified for my dose-response approach.

[30] The marginal effect in 2006 is equal to $\hat{\beta}_1$ because $\hat{\gamma}_{2006}$ is zero by construction (i.e., 2006 is the omitted period).

time. In these specifications, all of the control variables are further interacted with the indicator for the time period after the employer mandate was announced. Such an approach can examine if my findings are driven by employers treating groups who have greater medical expenditures (such as females, older workers, different races, 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 a combination of demographic characteristics then the individual-specific effects should "disappear" when I introduce such controls. Instead, I find that the coefficients on the difference-in-difference terms in each specification are essentially the same as my main findings.[31] Moreover, because I interact location (census region), occupation, and industry fixed effects with the After EM indicator, these estimates show that location-, occupation-, or industry-specific events cannot be driving my findings.

7. Discussion and conclusion

I use the Affordable Care Act's employer mandate to determine whether employers can shift the cost of providing ESI onto individual workers with greater medical expenditures by paying lower wages. The period between the mandate's announcement and actual implementation provides a unique opportunity to study this issue because the employer mandate changes the cost of employing workers with varying medical expenditures but does not change how medical expenditures might be related to productivity. My main findings show that wages and medical expenditures become increasingly negatively related after the employer mandate is announced wherever the employer mandate requires ESI to soon be offered. The effect on wages amounts to a pass-through of at least 35 cents of each dollar of individual medical expenditures. When I examine the same wage-expenditure relationship for workers at firms who already offer ESI or who are not covered by the employer mandate, I find no similar effect. I then show that my findings cannot be explained by appealing to economy-wide events such as the Great Recession or subsequent recovery and cannot be related to idiosyncratic shocks to certain industries or locations, because such events would be expected to affect firms on both sides of the employer mandate's 50 worker cut-off. Last, there is no evidence of pre-trends in wages that would threaten a causal interpretation.

If workers pay for the cost of ESI via lower wages, then the supposed risk-pooling benefits of employment-based health coverage are undermined. Such a claim is true even if employers are unable to determine the precise medical expenditures of every worker. An insurance company could not do that, either. Instead, my findings are consistent with the idea 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) has higher medical expenditures. If the cost of ESI continues to outpace inflation, the incentives to monitor and respond to medical expenditure differences among workers will become stronger, for all employers who offer ESI.

Because the distinction between an individual and a group blurs when groups are defined narrowly enough, a weaker summary of my findings would be that existing research on the effect of ESI on the wages of various groups of workers has defined groups too broadly. My findings, at a minimum, show 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). My estimates show that these sub-groups could be as small as an individual. However, it could also be the case that my estimates do not control for the exact sub-group 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 can use to inform them about differences in future medical expenditures among employees. Indeed, only some of those variables are available in my 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 my approach, that I emphasize a number of times, is its focus on anticipatory effects. However, my estimates would be biased toward zero if employers were not convinced the law would ever come into effect or were unaware of their responsibilities. Regardless, using the pre-implementation time period to study the mandate's effects is the only feasible approach because, once all of the elements of the ACA are implemented, credibly identifying the effect of the mandate separately from the rest of those provisions would be not be possible.[32] In addition, assuming employers are forward-looking is valid because employment is an ongoing arrangement (rather than determined in a spot market). A naive approach using data from the years before and after the mandate's implementation date may find no effect of the employer mandate on wages. Such a finding would be erroneous because my estimates show that wage adjustments have already occurred (this is true even if my supposed mechanism is not the *cause* of those adjustments) before the eventual implementation of the mandate.

Indeed, my focus on the period between the mandate's announcement and its implementation could be viewed as a strength relative to existing work on the effect of insurance mandates. That work typically studies mandates where there is a short time period between announcement and implementation. Therefore, there is no way to determine whether the effects are caused by changes in the demand for or supply of labor. My findings are likely related to changes in demand because the employer mandate's effects on labor supply, prior to its actual implementation, are likely to be small. Moreover, the direction of any anticipatory labor supply response is ambiguous because of the ACA's broader changes and information asymmetries between workers and employers. Only an employer is likely to know if they will have 50 FTEs or not after 2014, whether they can they move some workers to < 30 hours per week to avoid providing ESI, whether they can change their capital/labor mix to reduce their exposure, what the cost of ESI will be, how much will they charge employees, and so on. Furthermore, Table 1 suggests that the wages of many individuals who work for employers who do not offer ESI might allow them to qualify for heavily-subsidized insurance coverage via the Act's insurance exchanges.[33] Therefore, because individuals were required by law to have coverage from

[31] While it is not possible to present all the interaction terms here, the estimates show consistent negative effects on the wages of females, older workers, and those who have a college education in each specification (see Lennon, 2018, 2019 for more on these group-level differences). Each of these groups tends to have higher medical expenditures and such estimates provide further support for the idea that the mandate causes workers with larger medical expenditures to experience lower wages.

[32] 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 my main findings. See Table A13.

[33] For more information see https://www.healthinsurance.org/obamacare/will-you-receive-an-obamacare-premium-subsidy/ and www.healthcare.gov. Both last accessed 8/24/2020.

2014 onward (although the penalties for not doing so have since been removed), individuals might reduce rather than increase their willingness to work at employers affected by the employer mandate. With that said, isolating whether the effect of coverage mandates on wages is due to labor supply or demand changes is not my primary goal. My findings could be caused entirely by labor supply decisions – that is, 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: workers appear to “pay” for their ESI coverage at the individual rather than only at the group level.

My 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 health insurance 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, I show that because employers ultimately pay for the medical expenditures of their employees, they appear to screen and penalize as an insurer would. As a result, increases in the cost and prevalence of ESI can be expected to create barriers to employment for workers whose total compensation exceeds the value of their marginal product. My 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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Conflict of interest

The authors declare no conflict of interest.

Appendix A. Additional estimates

A.1 Triple difference estimates

My main findings (Panel A in Table 2) are dose response difference-in-difference estimates that examine the relationship between medical expenditures and wages for MEPS respondents who work at firms that do not offer ESI. Specifically, the difference-in-difference interaction term measures how the expenditure-wage relationship changes after the employer mandate is announced. I then examine the same changes for workers who are already offered ESI as a placebo analysis. Combining both analyses into a (dose response) triple-difference analysis is another option. In such a case, the estimating equation would add further interactions for ESI status as below.

$$\begin{aligned} \textit{Labor Market Outcome}_{it} = \; & \beta_0 + \beta_1 \textit{Medical Expenditures}_{it} + \beta_2 \textit{After EM}_{it} \\ & + \beta_3 \textit{No ESI}_{it} + \beta_4 \textit{After EM}_{it} \times \textit{No ESI}_{it} \\ & + \beta_5 \textit{Medical Expenditures}_{it} \times \textit{After EM}_{it} \\ & + \beta_6 \textit{Medical Expenditures}_{it} \times \textit{No ESI}_{it} \\ & + \beta_7 \textit{Medical Expenditures}_{it} \times \textit{After EM}_{it} \times \textit{No ESI}_{it} \\ & + \Pi X_{it} + \varepsilon_{it} \end{aligned}$$

I present estimates from such a specification in Table A1. The triple difference interaction term measures the effect the change in the relationship between medical expenditure and wages at firms that do not offer ESI *relative* to firms that already offer ESI after 2010. Put differently, the triple-difference coefficient represents how differences in medical expenditures and wages are related (the first, dose response, difference) after the employer mandate is announced (second difference) at employers who do and do not already offer ESI (third difference).

The estimates, again, consistently show that workers who have higher medical expenditures face lower wages after the employer mandate is announced if they work for an employer who must soon provide ESI because of the employer mandate. Specifically, the triple-difference interaction term (“After Employer Mandate × Log Medical Expenditures × No ESI”) suggests that for a 100% increase in medical expenditures, annual wages will be lower by

Table A1
Triple difference (dose response) estimates of effect of employer mandate.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Sample: Age 27–55, 50+ employees				
After EM	−0.014	−0.037*	−0.033	−1071.80
	(0.024)	(0.022)	(0.021)	(1127.60)
Log annual medical expenditure	0.023***	0.016***	0.013***	644.84***
	(0.002)	(0.002)	(0.002)	(116.51)
No ESI Offered	−0.792***	−0.657***	−0.616***	−18,154.26***
	(0.039)	(0.038)	(0.038)	(1294.97)
After EM × log medical expenditure	0.003	0.002	0.002	87.00
	(0.004)	(0.003)	(0.003)	(175.03)
After EM × No ESI	0.025	0.036	0.053	1876.27
	(0.052)	(0.051)	(0.050)	(1771.23)
No ESI × Med. Expenditure	−0.010	−0.013*	−0.008	−3.34
	(0.008)	(0.007)	(0.007)	(284.55)
After EM × No ESI × Med. Expenditure	−0.027***	−0.025**	−0.028***	−1103.32***
	(0.010)	(0.010)	(0.010)	(367.28)
Observations	34,144	33,865	33,858	33,858
N	21,550	21,360	21,354	21,354
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. These are triple difference estimates where the triple difference interaction term measures the effect of the employer mandate on the relationship between medical expenditure and wages at firms that do not offer ESI relative to firms that do offer ESI.

Table A2
Estimates of effect of employer mandate on hourly wages and part time status.

	(1) Log hourly wages	(2) $ Hourly wages	(3) Part time status (LPM)
After ACA	0.033	0.38	0.044*
	(0.046)	(0.71)	(0.027)
Log annual medical expenditure	0.034***	1.03***	0.037***
	(0.007)	(0.18)	(0.004)
After EM × log medical expenditure	−0.022**	−0.60***	−0.005
	(0.010)	(0.21)	(0.005)
Observations	4355	4355	4966
N	3294	3294	3748
Demographic controls	Y	Y	Y
Fixed effects	Y	Y	Y

Standard errors clustered at the respondent level are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). I define part-time status as working fewer than 30 hours per week, because that is the cut-off for required benefits under the employer mandate. My sample is restricted to those who are not offered ESI by their employer from 2006 to 2014, who report working between 15 and 60 hours in a usual week. I exclude those who work more than 60 hours per week to avoid bias from excessive overtime. I exclude those who work fewer than 15 hours per week to focus on workers who are strongly attached to the labor force.

2.8% (using the estimate in column three) for those who work at employers who do not offer ESI after 2010 relative to those who work where employers offer ESI.

A.2 Alternate labor market outcomes

Employers can shift the cost of ESI onto workers with larger medical expenditures by reducing wages or by not hiring them at all. Furthermore, in the case of the employer mandate, because the mandate only applied to workers who work 29 or more hours per week, employers could employ workers with greater medical expenditures for fewer hours to avoid providing coverage to those workers. Because this was an option, I present estimates in Table A2 that examine hourly wages (in logs and then levels) and an indicator for part-time employment (fewer than 30 hours per week) as dependent variables.

The estimates in the first and second columns of Table A2 suggest a statistically significant relative decline in hourly wages for employees who work for employers affected by the ACA. In particular, the estimates suggest that for a 100% increase in medical expenditures, hourly wages are lower by 2.2%, relative to the same relationship prior to the mandate. This is similar to the effect on annual wages and suggests my findings are driven by lower hourly wages rather than fewer hours worked. In the second column I provide estimates where the level of hourly wages is the dependent variable. The −0.60 coefficient implies a $0.60 difference in hourly wages for a 100% difference in medical expenditures.

In the third column of Table A2, the dependent variable 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, the estimates are from a linear probability model. The estimates suggest employers did not shift workers with higher medical expenditures into part-time employment. Note that these estimates do not mean that employers did not move *any* workers to part-time employment. Indeed, an interesting finding is that it appears that workers who work for firms affected by the mandate are 4.4 percentage points more likely to report working part time after 2010. The interaction terms only shows that those workers were not more likely to have higher medical expenditures. This finding is perhaps surprising given the costs of the mandate could be minimized by pivoting particularly expensive workers to part-time employment. However, it is also possible that the null finding is because of an insufficient sample size. In each wave-year of my MEPS sample, fewer than 50 work at a firm who is required to offer coverage by the mandate and are part-time.

In addition, a negative relationship between medical expenditures and annual wages could be partly driven by extensive margin changes in employment, including changes in where respondents' work or the length of any periods of unemployment. I do not pursue such analyses because, given MEPS's modest sample size, examining differences in extensive margin employment outcomes (particularly for those age 27–55) would be asking too much of the data.

A.3 Estimates using levels of wage and medical expenditures

The distribution of wages and medical expenditures are heavily right-tailed. In my main estimates, therefore, I log transform both wages and expenditures, which ensures that my point estimates (approximately) represent elasticities. For completeness, I present estimates using the level of wages and medical expenditures in Table A3. The interaction term in these estimates represents the change in wages for each one dollar difference in medical expenditures. The wage offset per dollar of medical expenditure is between 37 and 48 cents, which is quite similar to the estimates when using log wages and medical expenditures.

A.4 Sensitivity to age cut-offs

In my main estimates I limit my sample to those age 27–55. Those age 26 and under are excluded because the dependent coverage mandate affects them during this time. I further exclude those age 55 and over because I am concerned retirement decisions could be affected and bias my estimates away from finding any effect.

My estimates in Table A4 suggest that including those between 56–59 has little impact on my main findings. On the other hand, adding in those over 60 reduces the size and statistical significance of my estimates. This is not surprising because retirement decisions could be endogenous to wages and ESI. Moreover, employers surely have a diminished incentive to react to the medical expenditures of workers who may retire before coverage must be offered to them.

A.5 Sensitivity to extreme outliers

As I explain in the text of the paper, I eliminate 39 responses from my main estimation sample because they have medical expenditures exceeding $100,000 which heavily distorts my summary statistics. Among these high expenditure respondents,

Table A3
Effect of employer mandate for workers – using levels of wage and expenditures.

	(1) Annual wages ($)	(2) Annual wages ($)	(3) Annual wages ($)
After EM	−1125.93	−1042.05	−1185.24
	(2149.65)	(2052.43)	(2033.51)
Medical expenditure (in dollars)	0.34*	0.25	0.26
	(0.18)	(0.17)	(0.16)
After EM × medical expenditure (in dollars)	−0.37*	−0.40**	−0.48***
	(0.20)	(0.19)	(0.18)
Observations	5158	5065	5061
N	3901	3832	3828
Demographic controls		Y	Y
Fixed effects			Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents estimates from a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. In the final column, I further include fixed effects for census region along with industry and occupation codes.

Table A4
Effect of employer mandate – expanding sample to include age 55+.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: Age 27–59, not offered ESI by employer, 50+ employees				
After EM	0.095	0.108	0.106	1571.71
	(0.075)	(0.074)	(0.073)	(2058.17)
Log medical expenditure	0.013*	0.005	0.003	675.79***
	(0.007)	(0.007)	(0.007)	(227.55)
After EM × log medical expenditure	−0.021**	−0.021**	−0.022**	−807.31***
	(0.009)	(0.009)	(0.009)	(276.86)
Observations	5638	5532	5528	5528
N	4248	4169	4165	4165
Panel B: Age 27–64, not offered ESI by employer, 50+ employees				
After EM	0.088	0.115	0.118*	1414.96
	(0.073)	(0.072)	(0.070)	(2029.43)
Log medical expenditure	0.013*	0.004	0.002	538.74**
	(0.007)	(0.007)	(0.007)	(217.41)
After EM × log medical expenditure	−0.015	−0.015*	−0.016*	−446.32
	(0.009)	(0.009)	(0.009)	(272.72)
Observations	6026	5908	5904	5904
N	4530	4444	4440	4440
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable.

there are also often other missing pieces of information leading to additional concerns about data quality and reliability.

Of the 39 that I eliminate, only six report not being offered ESI by their employer at the time of such expenditures (they may have other coverage, of course) while 33 report being offered ESI by their employer. I restore these individuals to my sample in the estimates below. The estimates show that including these responses would have little impact on my findings.

Table A5

A.6 Sensitivity to definition of “Treatment” period

In my main estimates I consider 2011 to 2014 as the “After Employer Mandate” period. Defining the treatment period this way assumes that employers could not immediately react to the employer mandate, which was announced in March of 2010.

To demonstrate that my estimates are not driven by this choice, in Panel A in Table A6, I present estimates where I define the “After Employer Mandate” period as 2010 to 2014. In Panel B, I eliminate 2010 from the analysis completely due to the ambiguity of the treatment date. The estimates suggest that defining the “after” period as including 2010 reduces the size of my estimates. Eliminating 2010 responses, however, has little overall effect compared to my main estimates in Panel A of Table 2.

Last, if I repeat the main estimates using 2008 or 2009 as a placebo treatment date, it reduces the size and statistical significance of the estimates presented in Table 2. Because Fig. 1 clearly illustrates how the relationship of interest is relatively

Table A5
Effect of employer mandate for workers – restoring extreme Med. Exp.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.119	0.116	0.119	1839.77
	(0.079)	(0.077)	(0.075)	(2104.18)
Log medical expenditure	0.013*	0.003	0.003	638.64***
	(0.007)	(0.007)	(0.007)	(238.77)
After EM × log medical expenditure	−0.025**	−0.022**	−0.024***	−859.19***
	(0.010)	(0.009)	(0.009)	(289.48)
Observations	5164	5071	5067	5067
N	3903	3834	3830	3830
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable.

Table A6
Effect of employer mandate for workers – sensitivity to treatment period.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: After EM = 2010 to 2014				
After EM	0.027	−0.002	−0.008	2535.71
	(0.077)	(0.075)	(0.075)	(2058.79)
Log medical expenditure	0.012	0.003	0.001	611.70**
	(0.008)	(0.007)	(0.007)	(279.76)
After EM × log medical expenditure	−0.019*	−0.017*	−0.017*	−608.33*
	(0.010)	(0.009)	(0.009)	(322.60)
Observations	5158	5065	5061	5061
N	3901	3832	3828	3828
Panel B: After EM = 2011 to 2014, 2010 omitted				
After EM	0.056	0.032	0.026	1337.73
	(0.088)	(0.085)	(0.082)	(2576.23)
Log medical expenditure	0.012	0.004	0.001	618.23**
	(0.008)	(0.007)	(0.007)	(278.51)
After EM × log medical expenditure	−0.024**	−0.022**	−0.023**	−822.58**
	(0.010)	(0.010)	(0.009)	(322.85)
Observations	4677	4590	4586	4586
N	3690	3625	3621	3621
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In Panel A, the "After EM" period is 2010 to 2014, in Panel B, I eliminate 2010 MEPS responses. Therefore, the "After EM" period is 2011 to 2014 and the "before" period is 2006 to 2009. Notice that "*N*" is similar when omitting 2010 responses because most 2010 MEPS respondents also appear in either the 2009 or 2011 MEPS data.

stable before 2010 and changes only after 2010, I do not present these regression estimates.

A.7 Sensitivity to firm size

In my main estimates (Panel A of Table 2), I restrict my sample to those who report that their employer has more than 50 workers. Because respondents may not be able to report firm size accurately, my estimates may not be only capturing firms with 50 full-time employees or more. In particular, the concern is that the employer mandate applies to firms with more than 50 FTEs, whereas MEPS respondents likely report their best estimate of the total number of workers, full- and part-time. If anything, that is likely to bias me away from finding any treatment effect because my sample will include some respondents who work for firms who will not have to comply with the mandate (because they do not have 50 FTEs). To examine whether my 50 worker cut-off introduces bias, in Panel A of Table A7 I limit my sample to those who work at firms with more than 75 employees. The estimates in Panel B limit the sample to 100+ employees. The effect size and significance remain stable across specifications and are similar to my main estimates (Panel A of Table 2). The estimates in Table A7 also ease any concerns about non-random changes in firm size around 50 employees in response to the employer mandate.

Table A7
Effect of employer mandate for workers – sensitivity to firm size cut-off.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: Age 27–55, not offered ESI by employer, 75+ employees				
After EM	0.139	0.132	0.128	2323.90
	(0.087)	(0.084)	(0.082)	(2274.03)
Log medical expenditure	0.018**	0.008	0.008	817.78***
	(0.008)	(0.008)	(0.008)	(271.17)
After EM × log medical expenditure	−0.033***	−0.029***	−0.030***	−1111.20***
	(0.011)	(0.010)	(0.010)	(316.79)
Observations	4370	4288	4284	4284
N	3335	3275	3271	3271
Panel B: Age 27–55, not offered ESI by employer, 100+ employees				
After EM	0.102	0.088	0.087	1435.98
	(0.088)	(0.085)	(0.083)	(2303.22)
Log medical expenditure	0.013	0.002	0.001	657.44**
	(0.008)	(0.008)	(0.008)	(285.06)
After EM × log medical expenditure	−0.028**	−0.023**	−0.025**	−871.60***
	(0.011)	(0.011)	(0.010)	(323.54)
Observations	4083	4002	3998	3998
N	3100	3041	3037	3037
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable. In Panel A, I limit my sample to those who work at firms with more than 75 employees. Panel B further limits the sample to 100+ employees. The effect size and significance remain stable across specifications and are similar to the estimates in Panel A of Table 2 in the paper.

A.8 Ambiguously treated respondents

A limitation in my analysis is that MEPS asks about the number of workers at the respondent's work location and whether the employer has more than one business location. That means that any respondent who reports <50 employees but also that their employer has more than one location are ambiguously treated by the employer mandate. In Table A8, I restore those respondents who report working at a firm that has fewer than 50 workers but also has more than one business location.

Specifically, I present estimates, in Panel A, where I include those who report 25 to 50 workers, and then 1 to 50 workers in Panel B. Because many of these respondents do work where the employer has more than 50 FTEs between their various locations, they are treated by the mandate. However, there are probably a large fraction who do not have 50 FTEs across the various business locations. For that reason, theory would predict that the treatment effect would be smaller when including these ambiguously treated respondents in my sample. Moreover, to the degree that actual treatment status is correlated with the number of employees at the respondents' work location, I would expect that as I include respondents who report smaller and smaller numbers of workers at their work location, the treatment effect will become increasingly difficult to measure accurately.[34] My estimates confirm such predictions, providing further support for a causal interpretation.

[34] Put differently, it is likely that P(treated by mandate | 45 employees at work location, > 1 location) > P(treated by mandate | 5 employees at work location, > 1 location).

A.9 Firms with fewer than 50 workers

In Table 5, I present estimates examining the change in wages as a function of medical expenditures for those respondents who work at a firm with fewer than 50 employees. Those estimates include respondents who are offered ESI and those who are not. For completeness, I present estimates restricting the sample to respondents who work at firms with fewer than 50 employees but who are not offered ESI in Table A9. The estimates are similar to those in Table 5. Note that, as in Table 5, I again exclude any respondent who reports that their employer has more than one location because firm size becomes ambiguous in such cases.

A.10 Using only single location employers

Firm size can be ambiguous in certain cases. Specifically, whether a respondent's employer is affected by the employer mandate is unclear if the respondent reports fewer than 50 workers at their location, but more than one work location. For that reason, I present estimates in Table A10 where I limit my sample to only firms with a single location. The estimates are similar to those in Panel A of Table 2 but suffer from a lack of precision because of the smaller sample size. Reassuringly, the estimate using the level of annual wages remains statistically significant.

A.11 Using one MEPS interview only

In Table A11, I report estimates where I use only the first year end interview with each respondent (even if that was the second year of that particular MEPS wave). I provide these estimates to ease any concerns regarding positive or negative selection among those who respond to MEPS at both year-end interviews rather than only once.

Table A8
Effect of employer mandate for workers – including ambiguous firm sizes.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
Panel A: Include respondents who report 25 to 50 employees, > 1 location				
After EM	0.063	0.064	0.087	1406.76
	(0.069)	(0.067)	(0.065)	(1847.08)
Log medical expenditure	0.011*	0.003	0.001	588.28***
	(0.006)	(0.006)	(0.006)	(204.14)
After EM × log medical expenditure	−0.018**	−0.014*	−0.017**	−608.99**
	(0.009)	(0.008)	(0.008)	(257.18)
Observations	6391	6286	6282	6282
N	4823	4744	4740	4740
Panel B: Include respondents who report 1 to 50 employees, > 1 location				
After EM	0.069	0.076	0.084	2148.12
	(0.056)	(0.055)	(0.052)	(1394.65)
Log medical expenditure	0.009*	−0.000	−0.001	470.99***
	(0.005)	(0.005)	(0.005)	(160.09)
After EM × log medical expenditure	−0.013*	−0.012*	−0.013*	−500.72**
	(0.007)	(0.007)	(0.007)	(207.49)
Observations	9754	9604	9600	9600
N	7267	7152	7148	7148
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. In the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, my estimates use level wages as the independent variable. In these estimates, relative to my main estimates, I restore respondents who report working at a firm with fewer than 50 workers but that also has more than one work location. It is unclear whether these respondents work for a firm who has more than 50 FTEs. I present estimates where I include those who report 25 to 50 workers in Panel A, and 1 to 50 workers in Panel B.

Table A9
Effect of employer mandate for workers at unaffected employers (<50 employees, no ESI).

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	−0.019	−0.026	−0.022	−1022.26
	(0.035)	(0.034)	(0.033)	(843.22)
Log annual medical expenditure	0.011**	0.004	−0.001	117.66
	(0.005)	(0.005)	(0.005)	(132.15)
After EM × log medical expenditure	−0.008	−0.010	−0.009	−186.94
	(0.007)	(0.007)	(0.007)	(188.73)
Observations	8056	7956	7956	7956
N	5539	5471	5471	5471
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls or fixed effects, limited to respondents who work at firms with fewer than 50 employees. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable.

A.12 Using only full time workers

While the mandate requires ESI to be offered only to workers who work more than 29 hours per week, my main estimates do not eliminate respondents based on hours worked. Instead, in Table A2, I examine if some of the effect on annual wages could be because of more part time work. As I mention in Appendix A.2, shifting workers to part time is another way employers could avoid paying the higher costs of those with greater medical expenditures because ESI only has to be offered to those who work 30 hours or more per week.

For completeness, I present estimates restricting my sample only to those who report working ≥ 30 hours per week in Table A12. There, estimates suggest that including part time workers in my main sample has limited effects on my findings.

A.13 Eliminate 2014 responses

My main estimates include 2014 even though some of the ACA's other provisions go into effect at that time and even though the mandate was supposed to come into effect in 2014. I include 2014 data because the mandate was delayed for one year in 2013. For

Table A10
Estimates of effect of employer mandate for workers at single location employers (>50 employees, no ESI).

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.105	0.062	0.091	−1440.08
	(0.122)	(0.120)	(0.120)	(3006.46)
Log annual medical expenditures	0.003	−0.007	−0.006	480.47
	(0.011)	(0.011)	(0.011)	(306.40)
After EM × log medical expenditure	−0.018	−0.012	−0.013	−784.97*
	(0.015)	(0.014)	(0.014)	(450.28)
Observations	2413	2340	2336	2336
N	1784	1732	1728	1728
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls or fixed effects, limited to respondents who work at firms with fewer than 50 employees. I then add control variables for education, race, gender, marital status, age, and age squared. As indicated, in the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, I present estimates using level wages as the independent variable.

Table A11
Estimates of effect of employer mandate – using only first MEPS interview.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.256**	0.238**	0.247**	4765.59*
	(0.102)	(0.101)	(0.100)	(2577.02)
Log medical expenditure	0.012	0.006	0.004	638.02**
	(0.008)	(0.009)	(0.009)	(266.78)
After EM × log medical expenditure	−0.027**	−0.026**	−0.026**	−852.15**
	(0.012)	(0.012)	(0.012)	(348.58)
Observations	3199	3144	3143	3143
N	3199	3144	3143	3143
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents a specification with no controls. I then add control variables as indicated. In the final column, I present estimates using level wages as the independent variable for context.

Table A12
Effect of employer mandate – ≥ 30 h per week.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.018	0.031	0.060	907.84
	(0.086)	(0.082)	(0.079)	(2355.04)
Log annual medical expenditure	0.024***	0.014*	0.010	622.93***
	(0.007)	(0.007)	(0.007)	(225.60)
After EM × log medical expenditure	−0.020*	−0.020**	−0.024**	−745.12**
	(0.011)	(0.010)	(0.010)	(298.90)
Observations	3703	3621	3617	3617
N	2842	2781	2777	2777
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. In the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, the estimates use level wages as the independent variable.

Table A13
Effect of employer mandate – 2006 to 2013 only.

	(1) Log annual wages	(2) Log annual wages	(3) Log annual wages	(4) $ Annual wages
After EM	0.111	0.100	0.100	865.30
	(0.084)	(0.081)	(0.079)	(2238.26)
Log annual medical expenditure	0.013*	0.003	0.002	616.11***
	(0.007)	(0.007)	(0.007)	(238.33)
After EM × log medical expenditure	−0.021*	−0.017	−0.018*	−685.79**
	(0.011)	(0.010)	(0.010)	(306.83)
Observations	4494	4414	4410	4410
N	3422	3360	3356	3356
Demographic controls		Y	Y	Y
Fixed effects			Y	Y

Standard errors clustered at the respondent level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each panel, the first column presents a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. In the final two columns I add fixed effects for census region along with industry and occupation codes. In the final column, the estimates use level wages as the independent variable. These estimates exclude 2014 MEPS responses.

completeness, I present estimates where I eliminate 2014 MEPS responses in Table A13. There, estimates suggest that including 2014 responses in my main sample has limited effects on my findings.

A.14 Did the employer mandate affect medical expenditures?

In Table A14, I examine what happens to medical expenditures after the employer mandate is announced. The goal here is to ease concerns that those who work for firms affected by the mandate might choose to delay care in anticipation of imminent health insurance coverage. My estimation sample consists of MEPS respondents without ESI who work for a firm covered by the employer mandate (i.e., it is the same as my main estimates in Table 2 in the main body of the paper). Specifically, with terms defined as in my estimating equation in Section 3 of the main body of the paper, I examine an estimating equation of the form;

$$\textit{Medical Expenditures}_{it} = \beta_0 + \beta_1 \textit{After EM}_{it} + \Pi X_{it} + \varepsilon_{it}.$$

In the first column in Table A14, I use the log of medical expenditures as my outcome of interest. In the second column, I use the level of medical expenditures as the outcome variable. I include education, race, gender, marital status, age, and age squared as controls. I also include fixed effects for census region along with industry and occupation. Note that, *After EM* is a indicator variable that equals one after 2010 and zero otherwise. Therefore, the coefficient on the *After EM* indicator can tell us whether the conditional mean of medical expenditures changes after 2010 for workers at affected firms.

The estimates in the table suggest there is a small and statistically insignificant decline in medical expenditures among such workers in the years 2011 to 2014. Note, however, that medical expenditures could be changing because workers have incentives to delay (consistent with endogenous delays in care that would bias my estimates) or because workers who are getting and retaining jobs at affected employers are exactly those who have lower medical expenditures (consistent with my argument that firms have an incentive to employ workers with lower medical expenditures). While I cannot distinguish between these competing explanations, the decline is small and statistically no different from zero, easing concerns that my estimates are meaningfully biased by delays in medical spending among workers who expect to get ESI in the near future.

Table A14
Effect of employer mandate on medical expenditures.

	(1) Log medical expenditures	(2) $ Medical expenditures
After EM	−0.099	−301.94
	(0.122)	(264.96)
Observations	5061	5061
N	3828	3828
Demographic controls	Y	Y
Fixed effects	Y	Y

Estimation sample consists of MEPS respondents without ESI who work for a firm covered by the employer mandate. Standard errors clustered at the respondent level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). In each specification, I include education, race, gender, marital status, age, and age squared as controls. I also include fixed effects for census region along with industry and occupation. In the first column, I use the log of medical expenditures as my outcome. In the second column, the estimates use the level of medical expenditures as the outcome variable. Note that, *After EM* is a indicator variable that equals one after 2010 and zero otherwise.

Appendix B. MEPS data and sample construction

AHRQ describes their MEPS survey design and data collection procedures in annual survey documentation provided with each year's data file.[35] Taking the 2008 survey documentation as a representative example, AHRQ explains that (page c-98) that the "MEPS Household Component (HC) collects data in each round on use and expenditures for office- and hospital-based care, home health care, dental services, vision aids, and prescribed medicines." AHRQ specifically does not include insurance premiums as a medical expenditure. Instead, they explain (page c-99) that "[e]xpenditures on this file refer to what is paid for health care services" and that "expenditures in MEPS are defined as the sum of direct payments for care provided during the year, including out-of-pocket payments and payments by private insurance, Medicaid, Medicare, and other sources." They also note that "[p]ayments for over-the-counter drugs are not included in MEPS total expenditures," which is helpful because such expenditures are typically not covered by ESI.

[35] See https://meps.ahrq.gov/mepsweb/data_stats/download_data_files.jsp.

Table B1
Construction of key variables.

Variable	Question wording (2008)	Notes
Total medical expenditures	Calculated variable	Total medical expenditures are calculated from event level information that is cross-checked and edited by contacting a sample of health care providers. The care provider information is presented in MEPS-MPC (medical provider component).
ESI status	Was health insurance offered to any employees at this (job/business)?	I focus on the answer to whether any employee was offered ESI because the employer mandate most affects firms that do not offer ESI to any worker. Note that AHRQ logically edits this variable whenever someone holds ESI through their current main job but did not report being offered ESI at their main job. MEPS also reports whether a respondent was offered ESI by their employer, whether they take up that offer, and checks on reasons for any lack of eligibility.
Annual earnings	Calculated variable	MEPS allows respondents to report employment earnings flexibly. A small fraction report per hour worked, while others report bi-weekly, monthly, or annual salaries. MEPS combines the various pieces of information into hourly and annual wage figures, but imputes these values, by incorporating information on weeks and hours worked, in cases where the respondent reports some other piece of information. I choose to use annual earnings for my main estimates because a large majority of respondents report an annual salary when asked about earnings. For example, in 2014 more than 63.7% of respondents report their annual wage whereas only 16.7% report an hourly wage when asked about earnings. This implies most workers in my sample (which comprises MEPS respondents age 27–55) are salaried rather than earning some amount per hour worked.
Number of employees	How many persons are employed by (EMPLOYER) in a usual week at the location (PERSON) (work/works)/ worked?	Top coded at 500. Those who answer “don’t know” are asked “About how many persons are employed there?” and are then provided with a range such as “10 to 25” or “101 to 500” until they agree with an answer. AHRQ then imputes an exact number employees using an imputation procedure that takes into account job and location characteristics.
Other locations	Does (EMPLOYER) have facilities in more than one location?	Note that respondents are not asked about the number of employees at other locations. It is therefore not possible to determine the exact number of employees whenever a respondent reports more than one employer location. Because the employer mandate applies only to employers with more than 50 employees, a small number of respondents who report fewer than 50 workers at their job location, but also that their employer has other locations have to be eliminated from the sample because it is unclear whether they have 50 or more employees. Note that eliminating these respondents presents no problem for identification unless there are idiosyncratic shocks that are correlated with earnings and medical expenditures for such respondents.

Source: MEPS Survey Documentation available at https://meps.ahrq.gov/mepsweb/survey_comp/survey_questionnaires.jsp. I use 2008 documentation, other years are similar.

In addition, MEPS has a Medical Provider Component (MPC), which is a follow-back survey that collects data from a sample of medical providers and pharmacies that were used by MEPS respondents. AHRQ explains that “[e]xpenditure data collected in the MPC... were used to improve the overall quality of MEPS expenditure data.” Specifically, “logical edits were applied to both the HC and MPC data to correct for several problems including, but not limited to, outliers, copayments or charges reported as total payments, and reimbursed amounts that were reported as out-of-pocket payments.”

AHRQ explains their edit/imputation process in great detail, including how they use data from health care events with complete information to impute expenditures for events with missing information but with similar characteristics, how they deal with capitation, how they assign cost to public clinics and Veterans’ Hospitals, and adjustments for insurance-negotiated discounts (relative to the stated cost of care). The takeaway is that accurately capturing annual medical expenditures for each MEPS respondent is a key priority for AHRQ. Of course, this is unsurprising when MEPS stands for *Medical Expenditure* Panel Survey. I summarize how MEPS creates my key variables of interest in Table B1.

Because of AHRQ's diligence, I can rely on their measure for “total medical expenditures” as the best available information on the medical expenditures of MEPS respondents, and the most reliable representative (after weighting appropriately) information on American workers who work where ESI must be offered because of the employer mandate. Of course, there is surely some remaining error and it would be ideal if the annual total excluded expenditures relating to dental and vision issues, not typically covered by ESI. At the same time, to the extent that dental and vision expenditures over-inflate the annual expenditures of these workers, it biases me away from finding any treatment effects.

Last, it is true that current medical expenditures do not necessarily predict future medical expenditures. That being said, if we assume employers are basing their assessment of the cost of providing ESI to various workers on the best information they have, MEPS data on current medical expenditures is the best available proxy for studying how medical expenditures affect wages. In Appendix C, I examine the relationship between medical expenditures in subsequent years among MEPS respondents in my estimation sample.

Appendix C. Medical expenditures over time

My main findings depend heavily on an argument that current medical expenditures (which are, in turn, determined by individual characteristics and lifestyle choices – things that an employer observes over time) are a good proxy for future medical expenditures, at least among MEPS respondents. If current medical expenditures (or behaviors and lifestyle choices) reveal nothing about differences in medical expenditures among workers in the future, then employers could not meaningfully adjust wages to account for differences in expected medical expenditures.

In Table C1 I show that medical expenditures are highly correlated over time. In particular, the estimates in Table C1 are derived from the following estimating equation;

$$Log\,Medical\,Expenditures_{i,t+1} = \beta_0 + \beta_1 Log\,Medical\,Expenditures_{i,t} + \Pi X_{i,t} + \varepsilon_{i,t}.$$

The estimates in Panel A are elasticities where 0.495 implies that for a 100% difference in medical expenditures at time *t*, medical expenditures are 49.5% larger in time t+1. For Panel A, the sample includes MEPS respondents age 27–55 who work at firms with more than 50 workers regardless of ESI status. In Panel B, I restrict the sample only to those who are not offered ESI, and find a larger relationship between medical expenditures over time.

Table C1
Relationship between medical expenditures at time T and T+1.

	(1) Log Med. Exp. (t+1)	(2) Log Med. Exp. (t+1)	(3) Log Med. Exp. (t+1)
Panel A: Age 27–55, 50+ employees			
Log annual medical expenditure (t)	0.554***	0.503***	0.495***
	(0.011)	(0.012)	(0.012)
N	11,823	11,735	11,734
Panel B: Age 27–55, not offered ESI, 50+ employees			
Log annual medical expenditure (t)	0.667***	0.566***	0.550***
	(0.024)	(0.029)	(0.030)
N	1255	1231	1231
Demographic controls		Y	Y
Fixed effects			Y

Standard errors clustered at the respondent level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. I adjust dollar amounts to 2014 dollars using the CPI (www.bls.gov). The first column presents estimates from a specification with no controls or fixed effects. I then add control variables for education, race, gender, marital status, age, and age squared. In the final column, I further include fixed effects for census region along with industry and occupation codes. The sample here includes MEPS respondents age 27–55 who work at firms with more than 50 workers. In Panel B, I restrict the estimation sample to those not offered ESI. Note that *N* refers to the number of individuals for whom I have annual medical expenditures in two different years.

Note that because I have to have two observations for medical expenditures to perform this exercise, the sample size is much smaller compared to my main estimates (Panel A of Table 2). For example, I lose all those who were in their second year of their MEPS panel in 2006, any who provide their first responses to MEPS in 2014, plus any respondents who do not respond to MEPS in their second year in the survey or who respond only in the second year after joining a MEPS household in the intervening time period.

Appendix D. Composition bias and matching

MEPS is a two-year panel. Such a short time period leads to concerns that changes in the composition of the sample could bias my estimates. That is, the estimates for the difference-in-difference coefficients I present are not the change in labor market outcomes for employees at employers required to provide coverage by the mandate. Instead, they reflect labor market outcomes for employees who happen to be in MEPS and work at affected employers after the employer mandate is announced. MEPS could, by chance, survey systematically different individuals such as those with high medical expenditure and/or lower-wages after 2010. On the other hand, composition effects could work in the opposite direction: if the mandate lowers demand for those workers who are most heavily “treated” by the mandate, then the mandate would reduce the likelihood of a worker with large medical expenditures being observed working at a firm that has to provide coverage because of the employer mandate. If so, my estimates would understate the treatment effect.

To help ease concerns about composition changes after 2010, in Table D1, I present 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 (by year and ESI status). The procedure then matches workers in each period based on observable characteristics (race, education, marital status, age, region, gender, and so on) to compare “apples to apples.” The estimates in Table D1 use 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.

The first row in the table suggests that workers with above median medical expenditures earned $2367.45 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 $3280.57. 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

Table D1
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	2376.45	(2.64)	4989	4707
(2006–2010)	Not offered ESI	899.07	(0.57)	732	734
Post-EM	Offered ESI	3280.57	(7.28)	6616	6321
(2011–2014)	Not offered ESI	−1127.94	(−1.13)	1244	1227

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.

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.

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