Who Pays for the Medical Costs of Smoking? New Evidence from the Employer Mandate

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Abstract

This paper uses the Affordable Care Act's employer mandate to examine how employer-sponsored health insurance (ESI) affects smokers' wages. Estimates, based upon Medical Expenditure Panel Survey data, suggest that smokers and non-smokers who will receive ESI due to the employer mandate tend to bear the cost of that coverage via relatively lower wages. Compared to non-smokers, the paper's estimates suggest that smokers experience an additional mandate-related wage offset of between 19 and 33 cents per hour. The size of effect aligns well with the actual medical expenditure differences between employed smokers and non-smokers.

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

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

1 Introduction

Levine et al. (1997) use NLSY data to show that smokers earn between 4 and 8% less than non-smokers after controlling for a variety of observable and unobservable differences between the two groups. Viscusi and Hersch (2001) use the 1987 National Medical Expenditure Survey and find that smokers tend to select riskier jobs but still receive lower total wage compensation than non-smokers despite the hazard-related compensating wage differential. Grafova and Stafford (2009) use 1986-2001 PSID data and find a wage gap between smokers and non-smokers of between 4% and 11%.[1]

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[1] The wage penalty for smokers is similar outside of the United States. Van Ours (2004) focuses on Dutch workers in the 2001 CentER survey and finds that male smokers "earn about 10% less than non-smokers do." Auld (2005) finds even larger wage effects for Canadian males who smoke while Lång and Nystedt (2018) show that the negative wage effects of smoking are much larger in the 2000s relative to the 1970s in Sweden.

Smokers might earn lower wages for at least four reasons. One, smoking could directly reduce productivity via diminished health (leading to sickness and absenteeism) and restrictions on physical abilities. Two, smokers may face unequal or unfair treatment (that is, "discrimination").[2] Three, the decision to smoke might be endogenous to wages. That is, only those who (expect to) earn lower wages choose to smoke. Four, and the focus of this paper, wages for smokers (in the United States) should account for any additional cost of providing employer-sponsored health insurance (ESI).

The cost of ESI matters because ESI is experience-rated at the firm level.[3] Essentially, the cost of ESI for a firm and its workers reflects the actual medical expenditures of the covered group. Because employers typically pay a portion of the cost, ESI therefore creates a cost-wedge between workers with varying medical expenditures (such as smokers and non-smokers) unless employers can adjust wages to reflect the cost of ESI (Summers, 1989).[4] This paper examines how employers respond to these incentives using data on smokers and non-smokers who work at firms affected by the Affordable Care Act's (ACA) employer mandate. The mandate, announced in 2010, requires firms with more than 50 full-time equivalent employees (FTEs) to provide ESI to full-time workers from 2014 onward.[5] The mandate provides a source of identifying variation because it makes some workers relatively more expensive to employ, such as smokers.

To estimate if smokers' wages are lower due to ESI (and therefore if smokers pay for their own medical expenditures), the paper uses data on income, ESI coverage, smoking status, and firm size from the 2006 to 2014 waves of the Medical Expenditure Panel Survey (MEPS) in a difference-in-difference framework. Specifically, the paper examines how the relationship between smoking and hourly wages changes - for MEPS respondents working at firms who are required to provide ESI by the employer mandate - after the employer mandate is announced. If ESI is the

[2] Grafova and Stafford (2009) note that social acceptance of smoking appears to have diminished steadily since the 1980s but that the wage gap has not increased, providing little support for discrimination as a factor.

[3] Gruber (1994) explains that "[g]iven the prevalence of experience rating in insurance markets ... different individuals may cost the employer different amounts" (footnote, p.622). The only alternative to paying experience-rated premiums, while providing ESI to workers, is self-insurance where the firm employs a third-party to negotiate prices with providers and administrate the plan but pays all medical bills directly.

[4] As Cowan and Schwab note, employers could accomplish this by requiring smokers to pay a large contribution towards ESI relative to non-smokers. However, this is legally complicated due to nondiscrimination provisions in the Health Insurance Portability and Accountability Act (HIPAA).

[5] See Even and MacPherson (2018) for a thorough explanation of the details of the mandate. Note that, at the last minute, the mandate was effectively delayed by a Treasury/IRS decision to waive penalties for non-compliance until 2015 for those with 100 FTEs or more and until 2016 for those with 50 to 100 FTEs. See https://www.nytimes.com/2014/02/11/us/politics/health-insurance-enforcement-delayed-again-for-some-employers.html.

cause of some portion of the gap in wages between smokers and non-smokers, the estimates should show that the relationship between hourly wages and smoking is negative after the employer mandate is announced. To support the idea that the mandate caused wages to change, the paper also examines how wages are affected in two "control" groups: (1) workers at firms who are not affected by the mandate because they have fewer than 50 FTEs and (2) workers who are already offered ESI by their employer (without required to do so by the mandate).

The mandate can be used as a source of identification because it affects a well-defined group of firms but does not affect productivity, selection, or discrimination-based explanations for the smoking wage penalty. On the other hand, the way the mandate was implemented means that the paper relies on anticipatory responses. A forward-looking approach is theoretically valid because employment is usually an ongoing arrangement. For that reason, demand for workers (and, therefore, wages) will be affected as soon as employers understand the requirements of the mandate.[6] A complement to this paper's approach would be to also examine what happens once the mandate is binding. However, other ACA provisions, data limitations, and a last-minute policy decision to stagger the implementation date by firm size effectively preclude such an approach (see Section 3 for more on this).

Estimates, presented in Section 4, suggest that the employer mandate is associated with a mild additional decline in wages (between 19 and 33 cents per hour) for smokers relative to non-smokers who work at firms affected by the employer mandate. The estimates are generally not statistically different from zero, potentially due to sample size limitations. In the available data, there are a limited number of smokers who work at firms who do not offer ESI already and must do so because of the employer mandate. On the other hand, the estimated effect of the mandate on smokers' wages is close to regression-based estimates of how smoking affects medical expenditures, particularly if obtaining ESI were to increase medical expenditures for affected workers. The estimates presented in Section 4 have a causal interpretation if nothing else affects smokers' and non-smokers' wages differently in the 2006 to 2014 period. Robustness checks, focusing on how wages change for workers who work at firms that are not affected by the mandate, provide confidence in a causal interpretation. Of course, the Great Recession is a significant concern but it would have to impact

[6] This is not the first paper to use the employer mandate in a forward-looking manner (see Section 3.2).

the difference between the wages of smokers and non-smokers differently at firms that do not offer ESI to undermine identification. There is no immediate reason to suspect this would be the case.

The estimates in Section 4 first establish that the mandate had bite.[7] In particular, for workers at affected firms, relative wages decline by over $3,000 per year ($1.53 per hour in a specification with a complete set of controls and time trends) in the years after the employer mandate is announced. Importantly, there is no similar effect on wages for workers at firms who are not required to provide coverage by the mandate. The timing and size of the effect is important because $3,000 is quite close to the after-tax cost of ESI for a firm required to provide coverage due to the mandate.[8]

Indeed, one of the main contributions of this paper is that the empirical estimates are plausible, in stark contrast to those of Cowan and Schwab (2011). Cowan and Schwab examine how smokers's wages are affected by ESI using NLSY79 and MEPS (Medical Expenditure Panel Survey) data. Their approach compares wages for smokers and non-smokers at firms that offer and do not offer ESI. In the MEPS data (2000 to 2005), they find that smoking is associated with a $1.36 per hour wage offset for smokers who have ESI while "smokers who are not insured through their employer endure no such wage penalty."[9] A $1.36 per hour ($2,720 per year) effect on wages due to ESI is not plausible and suggests that Cowan and Schwab may be picking up something other than the effect of ESI on smokers' wage.

To further illustrate the value of this paper's approach to identification, Appendix A examines how the employer mandate affects wages for a variety of groups with differing medical expenditures. These estimates highlight that the employer mandate has predictable effects on wages for more than just smokers. The appendix also presents estimates of the effect of ESI on wages for the same groups using the empirical approach of Cowan and Schwab (2011). Those estimates show that Cowan and Schwab's approach is not reliable.

[7] Finding that ESI-related mandates affect wages is not a novel contribution. See Kolstad and Kowalski (2016) for example.

[8] The Kaiser Family Foundation reports the total cost of ESI to be $6,690 for an individual in 2017. For the employer, some of that cost is mitigated by employee contributions to coverage and some is mitigated by the fact that the employer's contribution is tax deductible at the corporate tax rate (35% during the sample period). See https://www.kff.org/report-section/ehbs-2017-summary-of-findings/ for more on the cost of ESI and http://www.ncsl.org/research/health/employer-and-individual-tax-incentives-to-offer-he.aspx on the tax deductions available to employers.

[9] This approach was first used by Bhattacharya and Bundorf (2009) to examine the role ESI may play in the determination of obese workers' wages.

The paper proceeds as follows. Section 2 reconsiders Cowan and Schwab's identification strategy and explains why the question of how ESI affects smokers' wages needs to be revisited. Section 2 also examines the empirical regularities of smoking, including demographic characteristics of smokers and their medical expenditures. Section 3 lays out the paper's empirical strategy, including a description of the data and how it is used to estimate the effects of interest. Section 4 reports the paper's main findings and considers the robustness of those estimates.[10] Section 5 concludes.

2 Smoking, ESI, and Medical Expenditures

2.1 Existing Literature on ESI and Smoking

As mentioned, Cowan and Schwab (2011) examine how ESI affects smokers' wages. They use NLSY79 and MEPS (Medical Expenditure Panel Survey) data to compare wages for smokers and non-smokers at firms that offer and do not offer ESI. They suggest that ESI causes smokers to experience a $1.36 per hour wage penalty (using MEPS data from 2000 to 2005) while "smokers who are not insured through their employer endure no such wage penalty."[11] These effects are causal if nothing other than ESI differentially affects the wage difference between smokers and non-smokers at firms that offer and do not offer ESI. The problem is that firms that offer ESI are different from those that do not in ways that may increase productivity-related gaps in wages between workers, invalidating Cowan and Schwab's identifying assumption. One difference between firms that offer and do not offer ESI is firm size (as measured by number of employees). In the Medical Expenditure Panel Survey (MEPS) data used in this paper, the mean number of employees at a firm that does not offer ESI is 54 while the mean for those that offer ESI is 183. This is a problem for Cowan and Schwab's approach to identification because a large literature has studied how firm size affects wages (see Oi and Idson, 1999 for an overview). One takeaway from that literature is that increased firm size allows for greater specialization and can therefore increase the wage gap between workers with varying abilities and/or productivity. That means that simply controlling for firm size (as Cowan and Schwab do) is not sufficient. Instead, firm size should be interacted with indicators of worker productivity - including, but not limited to, smoking status.

[10] Appendix A examines how these estimates contrast to the existing literature.

[11] Cowan and Schwab's approach was first used by Bhattacharya and Bundorf (2009) to examine the role ESI may play in the determination of obese workers' wages.

Of greater concern is the fact that Cowan and Schwab's MEPS-based estimates imply a $2,720 negative effect ($1.36 per hour for 2,000 hours - 50 weeks at 40 hours per week) on annual wages for working smokers aged 18-64. Using NLSY79 data, the same estimate is $3,320. At the same time, Cowan and Schwab report that ever-smokers (smoked more than 100 cigarettes in their life) aged 18-64 have only $481 of additional medical expenditures per year compared to never-smokers.[12] First, it is unclear if smoking is the cause of that $481 difference.[13] Second, the $481 difference in medical spending appears to be reported for all survey respondents (including those not in the labor force) rather than working adults - the population covered by ESI. If selection into the labor force is a function of health and, in turn, smoking status, then the difference in medical expenditures between ever-smokers and never-smokers covered by ESI could be less than $481. Moreover, due to cost-sharing, an employer would pay only a portion of those additional expenses. Essentially, it is unlikely that the observed effects on smokers' wages are caused by the additional cost of ESI for smokers.[14]

On the other hand, a mismatch between the difference in medical spending and observed wage offsets is not proof that Cowan and Schwab's findings are erroneous. Employers could be over-penalizing smokers by assuming they have much larger medical expenditures than they actually do. In that case, Cowan and Schwab's estimates could be valid. Because of the potential for employer-error, this paper does more than simply question Cowan and Schwab's identification strategy and associated estimates. First, the paper shows that working adults who smoke have relatively few additional medical expenditures compared to non-smokers. Then, the paper establishes that the employer mandate is a reliable source of variation. Last, the paper shows that employers do not over-penalize smokers for their medical spending.

2.2 The Effect of Smoking on Medical Expenditures

Cowan and Schwab's estimates suggest something other than ESI could be affecting the wages of smokers relative to non-smokers wherever ESI is offered. One candidate is firm size, as described earlier. Another is the fact that smokers who work at firms that offer ESI tend to have less education

[12] They use MEPS data to examine medical speeding differences. Their NLSY data has no medical expenditure data.

[13] Cowan and Schwab report that they examined conditional differences in medical spending but do not include those in the paper.

[14] Cowan and Schwab go through a series of empirical exercises designed to support their approach. Appendix subsection A.2 explains why each of these exercises is of limited value.

than non-smokers at those firms. Table 1 illustrates that, at firms with ESI, 54% of smokers have no more than a High School education compared to 34% of non-smokers. The same numbers are 70% and 62% for smokers and non-smokers where ESI is not offered. That means that almost 66% of non-smokers at firms with ESI have a college degree or a graduate degree compared to only 46% of smokers. Put together, that means that at firms that offer ESI, there are relatively fewer smokers, overall, and non-smokers have significantly more education. It is possible that, at firms that offer ESI, smokers and non-smokers may be doing very different work in ways that are hard to observe by a researcher.

In addition, smokers and non-smokers differ from one another in several other ways that could affect both productivity and medical expenditures. Summary statistics for wages, medical expenditures per year, age, job tenure, education, race, gender, education, marital status, firm size, and self-reported health are presented in Table 1. The summary statistics are stratified by ESI and smoking status.[15] For workers aged 27-59, 27,275 out of a total of 37,989 (72% or so) report being offered ESI. The data also shows that 6,220 workers aged 27 to 59 in the MEPS data from 2006-2014 report smoking at their first year-end interview, just over 16% of the analysis sub-sample.

Perhaps most surprisingly, the data presented in Table 1 suggests that smokers with ESI tend to have slightly lower medical spending than non-smokers (the large standard deviations indicate that the difference is not statistically significant). However, these are unconditional means and workers with and without ESI and smokers and non-smokers are different along many dimensions. For example, in Table 1 it is clear that smokers have a shorter average job tenure, are more likely to be male, have poorer self-reported health, and are less likely to be married. Because smokers and non-smokers are different in these various ways, the unconditional mean difference in medical expenditures cannot be relied upon as the measure of expenditures that an employer will respond to (unless they can observe smoking status but not other demographic characteristics).

[15] To avoid double-counting, the table presents data only from the first year-end interview complete by each MEPS respondent.

Table 1: **Summary Statistics**

	Offered ESI		Not Offered ESI	
	Non-Smoker	Smoker	Non-Smoker	Smoker
	Mean/(Std. Dev.)	Mean/(Std. Dev.)	Mean/(Std. Dev.)	Mean/(Std. Dev.)
Hourly Wage ($)	24.32	19.78	13.58	12.10
	(13.54)	(10.71)	(8.49)	(6.17)
Annual Medical Expenditure ($)	3,251.27	3,010.55	1,884.50	2,006.28
	(8650.61)	(7030.95)	(6354.48)	(6620.90)
Age (Yrs)	42.56	42.62	40.47	40.65
	(9.36)	(9.35)	(9.13)	(9.32)
Job Tenure (Yrs)	8.42	7.16	3.54	3.04
	(8.33)	(7.84)	(5.23)	(5.38)
# of Employees	188.41	163.41	54.77	47.43
	(191.88)	(181.97)	(110.84)	(97.35)
	Percent	Percent	Percent	Percent
Education				
High school or less	34.17	54.35	61.85	70.02
College	50.80	41.30	33.21	28.34
Graduate	15.03	4.35	4.94	1.64
Race				
White	68.02	70.98	75.92	72.64
Black	20.05	20.72	14.90	20.02
Other	11.93	8.30	9.18	7.34
Marital Status				
Single	37.96	51.18	42.42	57.67
Married	62.04	48.82	57.58	42.33
Gender				
Female	49.66	43.43	53.51	43.04
Male	50.34	56.57	46.49	56.96
Firm Size				
Less than 50 Employees	10.32	12.01	42.94	44.57
50 to 300 Employees	57.54	61.16	40.48	43.04
More than 300 Employees	32.14	26.83	16.57	12.39
Self-Reported Health				
Excellent/Good	64.08	55.12	55.23	46.90
Fair/Poor	35.92	44.88	44.77	53.10
Observations	23,153	4,122	8,616	2,098

Source: Medical Expenditure Panel Survey, 2006-2014, workers aged 27-59. All dollar values have been adjusted to 2014 values using the CPI (www.bls.gov).

For that reason, Table 2 examines regression estimates of the relationship between smoking and medical expenditures while controlling for observable differences. The estimating equation used to produce the estimates in Table 2 is of the form

$$Annual\ Medical\ Expenditures_{it} = \beta_0 + \beta_1 Smokes_{it} + \Pi X_{it} + \epsilon_{it}.$$

In the estimating equation, $Smokes_{it}$ represents an indicator variable that equals one if person i smokes at time t, X_{it} represents demographic controls (age, race, gender, and so on) along with location (census region), industry, and occupation fixed effects, and ϵ_{it} is an idiosyncratic error term. Table 2 provides estimates from four specifications of the above estimating equation. The estimates are based on MEPS data for workers aged 27-59. The first specification in the table includes no controls and is therefore an estimate of the unconditional mean difference in medical spending between smokers and non-smokers. Note that there is no claim of causation here. The estimate is statistically significant at the 5% level and suggests that smokers have medical expenditures that are \$220 lower than non-smokers. The estimates in the second column include demographic controls (age, gender, race, education level, marital status, and an indicator for ESI) and suggest that smoking is associated with \$129 of additional medical spending per year (although the estimate is not significantly different from zero at conventional measures). The third column presents estimates that include census region, industry, and occupation fixed effects. The final column adds hourly wages to the demographic controls and fixed effects (to control for any income effects on medical spending). In the third and fourth column, the effect of smoking on medical expenditures remains positive but not significantly different from zero.

In sum, these estimates suggest that smokers and non-smokers are different in ways that would affect medical expenditures regardless of smoking status. In particular, the unconditional effect of smoking on medical spending is negative but adding controls changes the sign of the effect. Regardless, the effect of smoking on medical expenditures for working adults appears to be quite small no matter how it is measured.

It is worth noting here that Cowan and Schwab explain that the contemporaneous medical expenditures of smokers and non-smokers is perhaps not the relevant metric because it is possible for workers to switch between the two groups over time. Fundamentally, Cowan and Schwab's

Table 2: **Smoking and Medical Expenditures - Regression Estimates**

	(1) Annual Medical Exp.	(2) Annual Medical Exp.	(3) Annual Medical Exp.	(4) Annual Medical Exp.
Smokes	-220.0312**	129.1397	80.0650	103.9382
	(89.947)	(99.466)	(100.288)	(100.046)
Observations	43,485	37,605	37,604	37,604
Individual Controls	N	Y	Y	Y
Region, Industry, and Occupation Fixed Effects	N	N	Y	Y
Income (Hourly Wage)	N	N	N	Y

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. All dollar values have been adjusted to 2014 values using the CPI (www.bls.gov). Standard errors are clustered at the individual level. Demographic controls: age (quadratic), gender, race, education, marital status.

argument is valid: employers should be responding to the expected medical expenditures of all past and present smokers rather than only the observed medical expenditures of current smokers.

However, Cowan and Schwab make the strong claim that the ability to switch between groups will diminish the medical spending difference between the two groups.[16] For that reason, despite their regression estimates being a comparison of current smokers relative to current non-smokers, Cowan and Schwab report the medical expenditures of ever-smokers as the measure employers would care about. However, employers do not have enough information to accurately determine the difference Cowan and Schwab report. Consider that there are at least eight "types" of workers defined by past, current, and future smoking behavior: there are people who currently smoke and will never quit, people who currently smoke and will quit in the future, people who used to smoke and will never smoke again, people who used to smoke and currently do not but will smoke again, people who have never smoked but will smoke in the future, people that do not currently smoke and will not smoke in the future, and so on.

[16]This claim is made with little supporting evidence. The effect of switching between the two groups on the expected medical expenditures of current and future smokers depends on many factors, including who switches, when they switch, the rate of smoking "take-up", rates of "relapse" into smoking from non-smokers, and so on. It is not hard to imagine that the ability to move between the groups makes the medical expenditure difference between the two groups larger rather than smaller in cases where those who select out of smoking are those who care about their health and would tend to have lower medical expenditures regardless of smoking status.

An employer does not observe each of these types. At best, employers can observe current smoking status and an incomplete history of past behavior. For that reason, this paper focuses on the medical expenditures of current smokers relative to current non-smokers. It is possible that current spending differences among workers is a poor measure of expected spending but any bias introduced is mitigated by discounting due to employee turnover and the time value of money. That is, even if the future medical expenditures of a 25-year-old smoker will be much larger than what is predicted by the current spending differences among older workers who smoke, it is unlikely that the current employer will be the one who faces those costs.

3 Empirical Framework

3.1 The Theoretical Effect of ESI on Wages

Economic theory predicts that workers, rather than employers, will bear the costs of ESI.[17] Following Bhattacharya and Bundorf (2009), in a competitive labor market where wages are the only form of compensation, the equilibrium wage of worker i, w_i, should equal the value of her marginal product (MRP_i). If health insurance is mandated as an employment benefit, a competitive labor market would require wages to be modified to account for the new cost of coverage. If premiums are actuarially fair a worker with medical expenditures e_i adds premium p_{ik} to firm k's costs. In such a case, an employer could pool all medical costs across their N employees so that wages for worker i at firm k are

$$w_{ik} = MRP_{ik} - \bar{p}_k.$$

In this case, wages would be equal to the value of a workers' marginal product minus the firm-level average cost of providing coverage $\bar{p}_k$ where $\bar{p}_k = \frac{1}{N}\Sigma_{i=1}^{N} e_i = \frac{1}{N}\Sigma_{i=1}^{N} p_{ik}$. However, in a competitive labor market, this would leave arbitrage opportunities open for workers and firms. For that reason, the literature has supposed that a firm's N employees can be partitioned into $j \leq N$ subgroups.[18] Let each of the subgroups be denoted as n_j. For $i \in n_j$, then equilibrium wages

[17] This section borrow heavily from Lennon (2018).

[18] If $j = N$ then subgroups are individual workers. Generally, authors who study how health coverage affects wages have dismissed this possibility without evidence. For more details on the ability of employers to pass along health care costs at the individual level, see Lennon (2019a).

(excusing the abuse of notation) for worker i would be

$$w_{ijk} = MRP_{ijk} - \frac{1}{n_j}\Sigma_{i=1}^{n_j} p_{ijk} = MRP_{ijk} - \bar{p}_{jk}.$$

In such a case, the wages of each member of each group will be adjusted by the average medical expenditures of the group ($\bar{p}_{jk}$). This is potentially an equilibrium if the costs of searching for profitable deviations exceed the benefits.[19]

Many authors have found evidence of this kind of group-specific wage offset, including Gruber (1993), Sheiner (1999), Jensen and Morrisey (2001), Lahey (2012), Bailey (2014) and Lennon (2018). This paper complements their work by examining how the relationship between smoking and wages changes for working adults aged 27-59 at firms affected by the employer mandate in the years after the mandate was announced.

3.2 Sample Selection

The paper uses MEPS data from 2006 to 2014. MEPS is a nationally-representative rotating panel of U.S. individuals. Each respondent is part of MEPS for two years and MEPS reports information on each respondent's health and employment status. Importantly, it also contains the number of employees where the individual works and whether health coverage is offered or not. This allows the researcher to identify which respondents are working at firms who must provide coverage due to the employer mandate.

Those under 27 are excluded from the empirical analysis in this paper because the ACA affected them via the dependent coverage mandate.[20] Workers aged 60 and over are excluded because they might retire prior to or very shortly after the mandate's implementation. The data used in the paper includes several years either side of when the employer mandate was passed in 2010. In all estimates, the 2011 to 2014 period is considered to be "After EM" where EM stands for employer mandate. Data from 2014 is included in the estimates presented in the paper because the mandate

[19]Of course, examining static equilibrium outcomes cannot capture the variety of dynamic adjustments required to achieve them. Indeed, there is no obvious reason for "firms" to exist in the framework presented here. A more general model including labor market frictions, heterogeneous workers, firm characteristics and size as choice variables, and so on, is well beyond the scope of the paper.

[20]See Antwi et al. (2013), Depew (2015), Hahn and Yang (2016), and Goda et al. (2016) for how the dependent mandate affected younger workers' labor supply.

was delayed to 2015 in the summer of 2013 and further delayed to 2016 for firms with 50 to 100 FTEs by a Treasury/IRS decision in February 2014. The estimates presented in the paper are similar when 2014 is excluded. However, extending beyond 2014 moves into a period where identification becomes increasingly clouded by the myriad provisions of the ACA and because it is not possible to determine who gained coverage because of the mandate in the 2015 MEPS data.

In the remaining sample, more than 85 percent of respondents who work for employers with more than 50 workers were offered health coverage by their employer in every year.[21] The paper focuses on the remaining 10 to 15 percent of respondents in each year who work for employers that do not offer coverage but must do so because of the ACA's mandate. The mandate requires these MEPS respondents' employers to consider the costs of employee health coverage for the first time, providing a causal estimate of the effect of the medical costs of smoking on wages under the identifying assumption that nothing else affects the relative wage gap between smokers and non-smokers over the same time period.

3.3 Anticipatory Effects

As mentioned earlier, the paper relies on anticipatory responses to the employer mandate. Using the employer mandate in this forward-looking manner is not unique to this paper: Garrett and Kaestner (2015), Mathur et al. (2016), and Even and MacPherson (2018) consider how the ACA's announcement affected part-time employment because only workers who work more than 30 hours per week would have to be offered ESI. Those authors focus on anticipatory effects because employers could preemptively shift workers to part-time employment in order to avoid the cost of providing ESI. Even and Macpherson suggest that "700,000 additional workers without a college degree are in [involuntary part-time] employment as a result of the ACA employer mandate." On the other hand, Garret and Kaestner and Mathur et al. report a null result.[22]

A forward-looking approach is appropriate because the employer mandate (announced in 2010) required ESI to be offered to full time workers by January 1, 2014. Given employment is an ongoing relationship, theory suggests employers should have immediately reduced their demand

[21] The number falls to just over 72% when firms with fewer than 50 workers are included.

[22] For more on the effects of the ACA on part-time employment see https://ldi.upenn.edu/brief/how-has-affordable-care-act-affected-work-and-wages. Note that if employers tended to shift smokers into part-time employment, rather than modifying relative wages, then this paper's estimates would be biased towards zero.

for workers who would be more costly to cover.[23] Moreover, the cost of coverage for 2014 was to be based on the expected costs of a firm's employee pool in 2013. If some employers are not forward-looking, then the observed effects will be understated.

Focusing on anticipatory effects has the advantage of avoiding other ACA provisions that might affect labor market outcomes after 2014. One obvious complication would be the ACA's health insurance exchanges. These insurance exchanges provide "affordable" coverage options outside of employment. Examining the period after 2014 could cloud identification if these exchanges or other ACA provisions affected self-employment patterns, job search efforts, or alleviated health coverage-related job lock differentially for obese workers.

More fundamentally, data limitations - and the last minute decision to stagger the implementation of the mandate by firm size - prohibit anything other than an analysis of anticipatory effects. In terms of data, with a two year rotating panel, MEPS is too short and too small to examine how wages are affected for those who actually obtain ESI due to the mandate. To do such an analysis, the researcher would focus on respondents for whom there is data available from before and after the implementation of the mandate. However, in any given wave of MEPS there are only about 400 survey respondents who work for an employer with 50 FTEs or more and who are not already offered ESI. Only a fraction of these are smokers.

The last-minute implementation delays don't help. In the summer of 2013, the mandate was delayed by one year for all employers. In February 2014, the Treasury and the IRS issued a joint statement to the effect that, even though the mandate was nominally in place, no "shared responsibility penalties" would be levied until 2016 for firms with 50 to 100 FTEs.[24] This kind of variation would be welcome (to a researcher) in many settings. However, there are only about 200 respondents in the 2015/2016 wave of MEPS that (1) work for a firm with 100 FTEs or more at the end of 2016 and (2) were not offered ESI already. Among those 200 respondents, not all are working in the same job as the prior year. Job separation and attachment, particularly in the year ESI is mandated, must be viewed as endogenous. Moreover, there are fewer than 40 smokers in that group. In addition, to be able to claim causation, the researcher would have to somehow establish that there were no anticipatory responses to the mandate. One solution to these issues

[23]In the data used in this paper, more than 60% of workers at firms affected by the employer mandate (more than 50 FTEs, no ESI from 2011-2014) had employment tenure of 2 years or more.

[24]See https://www.treasury.gov/press-center/press-releases/Pages/jl2290.aspx.

would be to obtain more or better data. However, suitable large data sets - those that examine respondents' smoking history, firm size, ESI availability, and wages - are uncommon. Any attempt to examine what happens to those who actually get ESI due to the mandate would have to contend with extremely small sample sizes regardless of the data used. Relying on anticipatory responses and pooling the MEPS data before and after the announcement of the mandate - what is done in this paper - is the only feasible approach.

3.4 Estimation

The way the employer mandate impacts the labor market lends itself to a difference-in-difference approach to estimation. The basic estimating equation is as below;

$$Hourly\ Wage_{it} = \beta_0 + \beta_1 Smokes_{it} + \beta_2 After\ EM_{it} + \beta_3 Smokes \times After\ EM_{it} + \Pi X_{it} + \epsilon_{it}.$$

In the equation, $Hourly\ Wage_{it}$ is the hourly wage of person i at time t. The right hand side includes controls for the pre-existing relationship between wages and smoking status using an indicator for smoking ($Smokes_{it}$). The indicator is equal to one for those who smoke and zero otherwise. Then, the estimating equation controls for the main effect of the employer mandate ($After EM_{it}$), which equals one after the employer mandate (EM) is announced (from 2011 to 2014). The co-efficient of interest is on the interaction of these two terms. It provides a measure of the change in smokers' wages in the period after the mandate and has a causal interpretation if nothing else affects the wage difference between smokers and non-smokers at firms without ESI during the sample period. The estimating equation also allows for typical demographic controls X_{it} including age, gender, education, marital status, race, location, occupation, and industry.

The next section uses the above estimating equation and MEPS data to provide the main findings of the paper. Given the costs of smoking (see Tables 1 and 2), the tax treatment of medical expenditures, and cost-sharing at the point of service, ESI-related wage offsets associated with smoking can be expected to be relatively small and hard to detect, in contrast to the existing literature.

4 Main Estimates

Table 3 presents three sets of estimates. In the first panel, the estimates focus on workers at firms required to offer ESI due to the employer mandate. This group consists of MEPS respondents who report working at a firm with more than 50 workers that does not offer ESI to their workers. The first column of estimates is for a specification with no controls. It shows that smokers experience lower wages across the sample period.[25] In addition, the estimate associated with the After EM (employer mandate) and interaction terms are both negative. The second column of estimates adds controls for individual demographic information including age, gender, race, education, and marital status. Because medical expenditures vary with respect to these individual characteristics, they are also interacted with the employer mandate indicator in the reported estimates. The estimates in the third column add region, occupation, and industry fixed effects. The estimates suggest that smokers earn less than non-smokers and that all workers at firms required to offer coverage due to the employer mandate earn less relative to before the mandate's announcement. Lastly, the estimates suggest that smokers face a small additional negative wage offset (of between 19 and 33 cents depending on specification) after the employer mandate is announced. Note that the coefficient associated with the interaction term is not statistically different from zero at conventional measures in any specification.

The second panel focuses on MEPS respondents who work at firms with fewer than 50 employees using the same specifications as in Panel A. Again, smokers generally earn less than non-smokers. On the other hand, and in contrast to what happens in Panel A, wages for all workers appear to increase mildly (between 14 and 74 cents) after the employer mandate is announced. In addition, smokers' wages appear to increase relative to non-smokers by between 5 and 10 cents. Again, the estimates of interest are not statistically different from zero at conventional measures.

The third panel examines what happens to wages for MEPS respondents who work at firms that already offered ESI. Following the same pattern as in Panels A and B, smokers tend to earn less than non-smokers over the sample period. Additionally, wages for all workers appear to fall by between 4 and 44 cents after the mandate and by an additional 9 to 33 cents for smokers. Again, these estimates are not statistically different from zero at conventional measures. However, it

[25] Likely this represents the combination of productivity, selection, and discrimination effects described in the introduction.

Table 3: **Main Estimates**

	(1) Hourly Wage	(2) Hourly Wage	(3) Hourly Wage
Panel A - Treated: More than 50 FTEs, No ESI			
Smokes	-1.9531***	-1.1662***	-0.9454***
	(0.360)	(0.346)	(0.326)
After EM	-0.3433	-1.6044**	-1.5370**
	(0.514)	(0.722)	(0.690)
Smokes × After EM	-0.2958	-0.3359	-0.1900
	(0.478)	(0.460)	(0.437)
Observations	6,395	6,250	6,249
Panel B - 1st Control Group: Less than 50 FTEs, No ESI			
Smokes	-0.9525***	-0.6290**	-0.4922*
	(0.299)	(0.300)	(0.283)
After EM	0.1387	0.7379	0.6272
	(0.365)	(0.589)	(0.566)
Smokes × After EM	0.1079	0.0499	0.0915
	(0.407)	(0.403)	(0.379)
Observations	9,486	9,345	9,345
Panel C - 2nd Control Group: Already Offers ESI			
Smokes	-3.5861***	-1.4550***	-1.0203***
	(0.249)	(0.228)	(0.207)
After EM	-0.4439	-0.1219	-0.0376
	(0.309)	(0.472)	(0.440)
Smokes × After EM	-0.3339	-0.2159	-0.0908
	(0.350)	(0.319)	(0.293)
Observations	32,708	32,441	32,439
Demographic Controls	N	Y	Y
Region, Occupation, & Industry Fixed Effects	N	N	Y

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. All dollar values have been adjusted to 2014 values using the CPI (www.bls.gov). Standard errors are clustered at the individual level. Demographic controls: age (quadratic), gender, race, education, and marital status. All of these controls are also interacted with the After EM indicator because the cost of ESI also varies by age, race, gender, and so on. The final column of estimates also includes a year time trend - instead of year fixed effects - to allow an estimate of the "After EM" term in each regression.

makes sense for there to be a mild negative effect on wages where ESI is offered already. Even ignoring healthcare-specific inflation, the Affordable Care Act mandated that ESI must include several ”essential health benefits” and capped employee contributions towards coverage at 9.5% of income. These requirements make this ”control group” less useful than one might hope.

Overall, these estimates show that the employer mandate affected wages for all workers wherever ESI would now be required and had little to no impact on workers that already have ESI or work at small firms that are exempt from the mandate. In addition, and in line with the actual medical spending differences between smokers and non-smokers, the employer mandate did not lead to large additional wage reductions for smokers.

A series of robustness checks and further supporting evidence are presented in Appendix A. In the appendix, Table A1 examines how the employer mandate affected wages for other groups with medical expenditure differences using the same basic approach to estimation as used to produce Table 3. The estimates again show that the mandate reduced wages for all workers. They also suggest that there are additional mandate-related wage offsets for groups with higher medical expenditures after 2010 (such as for females, obese workers, and college-educated workers). The appendix also examines how estimates using the mandate for identification compare to estimates generated using the estimating equation employed by Cowan and Schwab (2011). In each case, the estimated effect of ESI on these workers wages is unrelated to the sign and/or size of the medical expenditure difference among the workers. Lastly, to highlight how the use of the employer mandate provides a reliable estimate of the effect of ESI on smokers’ wages, Table A3 re-estimates the effect of ESI on smokers’ wages using Cowan and Schwab’s approach to identification. In all specifications using Cowan and Schwab’s approach, the estimates are implausibly large when compared to the actual medical spending differences among smokers and non-smokers.

5 Conclusion

Smokers tend to experience lower wages with productivity, discrimination, and negative selection as primary explanations. However, in the U.S., due to experience-rated health coverage, the medical expenditures associated with smoking could cause smokers to suffer additional wage offsets. Fundamentally, it must either be that non-smokers subsidize the medical spending of smokers or that smokers suffer wage offsets that compensate for the additional cost of health coverage.

To examine which is the case, this paper uses the employer mandate as a source of exogenous variation to study how smokers are affected when ESI must be offered. Economic theory suggests that employers should react to the mandate's announcement by reducing their demand for workers with higher medical expenditures - including smokers. In contrast to the existing literature, the paper finds that smokers suffer a very mild (19 to 33 cents) additional penalty relative to non-smokers.

Upon further examination, the mild penalty makes sense because working adult smokers have relatively few additional medical expenditures relative to non-smokers. Oddly, the existing literature on this topic finds wage offsets that amount to several times the medical expenditures associated with smoking, no matter how generously smoking-related medical expenditures are accounted-for. This paper's approach therefore highlights that the identification strategy used by Cowan and Schwab (2011) (comparing wages of smokers and non-smokers at firms with and without ESI) is questionable.

On the other hand, it is possible that employers could be over-penalizing smokers due to extreme risk aversion or error. The value of using the employer mandate as a source of variation is that it avoids many of the concerns associated with Cowan and Schwab's approach and shows that employers are not over-penalizing smokers. If ESI caused smokers' wages to be relatively lower than non-smokers, the wage offset for smokers should be much larger than for non-smokers, but, at least in this case, it is not.

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A Appendix

To illustrate the value of using the employer mandate as a source of identification, Table A1 presents estimates generated using this paper's empirical strategy (and a similar estimating equation where the indicator for smoking status has been replaced) to examine the effect of the employer mandate on wage gaps for other groups.[A1] These groups include obese workers, males, white workers, and college-educated workers. The table also reports the unconditional mean hourly wages and annual medical expenditures for each sub-group (males/females, black/white, and so on) below the corresponding estimates. Non-obese workers, males, whites, and college-educated workers tend to have higher hourly wages. However, obese workers, females, blacks, and college-educated workers tend to have higher medical expenditures. In each column of estimates, the coefficient corresponding to "After EM × Group Offset" is the coefficient of interest. It is the additional change in wages after the employer mandate is announced for the noted group.

If this paper's strategy is valid then, all else equal, changes in wages (for workers who work at firms required to provide coverage after the employer mandate is announced) should depend on the difference in medical expenditures between groups. In the estimates in Table A1, the main effect of "group" shows that non-obese workers, males, whites, and college-educated workers earn more than obese workers, females, blacks, and workers without a college degree. In addition, the estimates in the first, second, and final columns show that workers suffer a wage penalty after the employer mandate is announced. The penalty is similar to the effect of the mandate in Table 3 in the body of the paper.

The coefficient estimate on the difference-in-difference interaction term gives a measure of the change in wages after the employer mandate for the noted group. In the first, second, and fourth columns, the estimates align reasonably well with the medical expenditure differences between the groups. Obese workers have higher medical spending and face a small reduction in wages relative to non-obese workers after the employer mandate. Male workers have lower medical spending and experience slightly higher wages while those with a college degree have larger medical spending

[A1] Controls are education level, marital status, race, gender, and a cubic in age. Fixed effects include region, industry, and occupation fixed effects. All of the controls are also interacted with the "After EM" indicator because the cost of ESI also varies by age, race, gender, and so on. Standard errors are clustered at the individual level. The observation counts vary because there are cases of non-response in the data for obesity status and the estimates in column three include only black and white workers.

Table A1: Wage Offsets for Various Groups due to the Employer Mandate - MEPS 2006-2014 Data

	(1)	(2)	(3)	(4)
	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage
Group	Obese	Male	White	College
Group	-0.3222	1.8343***	0.8481**	7.2427***
	(0.333)	(0.376)	(0.395)	(1.082)
After EM	-1.4760**	-1.6205**	0.4093	-1.6205**
	(0.722)	(0.690)	(0.743)	(0.690)
Group × After EM	-0.0955	0.5132	-1.8120***	-1.0843
	(0.444)	(0.480)	(0.517)	(1.855)
Observations	6,098	6,394	5,874	6,394
Demographic Controls	Y	Y	Y	Y
Region, Occupation, & Industry Fixed Effects	Y	Y	Y	Y
Group	Obese	Male	White	College
Hourly Wages				
Group=1	$13.26	$14.74	$13.87	$17.59
Group=0	$14.22	$13.07	$12.92	$11.65
Annual Medical Expenditures				
Group=1	$2,484	$1,476	$2,072	$2,699
Group=0	$1,915	$2,564	$2,289	$1,689
Observations				
Group=1	2,051	2,989	4,527	2,529
% of Sample	33.6%	46.8%	77.1%	39.6%

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. Controls are education level, marital status, race, gender, and a cubic in age. Fixed effects include region, industry, and occupation fixed effects. All of the controls are also interacted with the After EM indicator because the cost of ESI also varies by age, race, gender, and so on. Standard errors are clustered at the individual level. Note that observation counts vary due to non-response in the data and, in column three, estimates include only black and white workers.

and experience a relative wage decline after the mandate (although college educated workers experience wages that are $7 per hour greater than those without a college degree across the sample period). Again, the sample size of MEPS is a problem and, in each case, the estimate is not precisely measured: an effect of zero cannot be ruled out.

The only statistically significant interaction estimate is the effect of the mandate on the wages of whites relative to blacks after 2010. The estimate suggests that whites experience a relative wage decline of \$1.81 per hour. The effect on wages is much too large to be due only to medical expenditure differences. However, notice that the main effect of the employer mandate is \$0.41 per hour in that specification. That estimate suggests that black workers experience no wage offset due to the employer mandate. Therefore, the entire effect of the employer mandate, rather than only the difference in the effect across race, is loading onto the interaction term for whites. These estimates would make sense if employers generally do not intend to offer ESI to black workers in the sample. Perhaps black workers in the sample are more likely to be seasonal or are working in jobs with significant employee turnover? Perhaps more of them they are working "off the books" or can be shifted to part-time once the mandate is implemented? This finding is worthy of further examination but is beyond the scope of this paper.[A2]

Overall, Table A1 provides some additional support for the use of the mandate for identification. However, the main purpose of Table A1 is to allow a direct comparison between this paper's empirical strategy and the approach taken by Cowan and Schwab (2011). Their estimating equation takes the form

$$Hourly\ wage_{it} = \beta_0 + \beta_1 ESI_{it} + \beta_2 Group_{it} + \beta_3 ESI_{it} \times Group_{it} + \Pi X_{it} + \epsilon_{it}.$$

In the equation, $Hourly\ wage_{it}$ refers to hourly wages for person i at time t. The right hand side controls for the general labor market relationship between wages and the group of interest ($Group_{it}$).[A3] An indicator for ESI (ESI_{it}) captures differences that affect all workers equally at firms where ESI is offered to workers. The co-efficient on the interaction of these two terms in the estimating equation measures how ESI affects wages for the workers of interest if the identifying assumption (that nothing affects the relative wages of workers differently at firms with and without ESI) is satisfied. The estimating equation is completed by allowing for a set of typical demographic controls X_{it}.

[A2]This discussion borrows heavily from Lennon (2018).

[A3]Bhattacharya and Bundorf (2009) use the same framework to examine how ESI affects the wages of obese workers and Cowan and Schwab (2016) use the framework to examine the effect of ESI on the male-female wage gap. Lennon (2018) and Lennon (2019b) reexamine the validity of the approach to answer those questions.

Table A2: ESI-Related Wage Offsets for Various Groups using MEPS 2006-2014 Data

	(1)	(2)	(3)	(4)
	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage
Group	Obese	Male	White	College
Group	0.3311*	2.3944***	0.0761	10.3650***
	(0.172)	(0.229)	(0.276)	(0.478)
Offered ESI	5.8907***	4.6723***	4.5716***	4.4442***
	(0.159)	(0.172)	(0.263)	(0.146)
Group × ESI	-1.1231***	1.7152***	1.1941***	2.5341***
	(0.238)	(0.239)	(0.291)	(0.254)
Observations	34,707	35,560	32,193	35,560
Demographic Controls	Y	Y	Y	Y
Region, Occupation, & Industry Fixed Effects	Y	Y	Y	Y
Group	Obese	Male	White	College
Hourly Wages				
Group=1	$18.48	$20.53	$19.39	$23.94
Group=0	$19.83	$18.04	$17.67	$14.61
Annual Medical Expenditures				
Group=1	$3,305	$2,129	$2,757	$3,288
Group=0	$2,463	$3,371	$2,919	$2,172
Observations				
Group=1	11,057	18,631	26,602	18,184
% of Sample	31.9%	52.4%	82.6%	51.2%

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. Controls are education level, marital status, race, gender, and a quadratic in age. Standard errors are clustered at the individual level. Note that observation counts vary due to non-response in the data and, in column three, estimates include only black and white workers.

If Cowan and Schwab's strategy is valid then, all else equal, the relative wage gaps for any two groups of workers at firms with and without ESI should depend on the difference in medical expenditures between the two groups. However, Table A2 shows that when their empirical strategy is applied to the wage offsets experienced by other groups who have differences in medical expenditures, the gap in wages between the groups is frequently unrelated to what would be expected based on the cost of ESI for the workers. As just one example, college educated workers have larger medical expenditures than those without a college degree. Given college-educated workers have higher wages overall, Cowan and Schwab's approach would suggest that - whenever ESI is offered - the difference between the wages of workers with and without a college degree should be smaller. Instead, workers with a college degree earn an additional \$2.53 whenever ESI is offered. The estimates in Table A2 suggest that the identifying assumption central to Cowan and Schwab's approach is perhaps invalid. Table A3 in the next subsection examines the effect of ESI on smokers' wages using the same empirical approach.

Note that the estimating equation described above is similar but not identical to the equation used by Cowan and Schwab. For example, in their main estimates they focus on workers who hold ESI from their employer rather than being offered ESI. The main estimates in the body of this paper focus on workers at firms that do not offer ESI to their workers and therefore the offer of ESI is the crucial variable. To maintain consistency, the estimates in this appendix section also use an indicator for a worker being offered ESI rather than taking up that offer. This matters little becuase Cowan and Schwab explain that their estimates are similar using "Held ESI" or "Offered ESI." However, theoretically, the offer of ESI should be the relevant indicator because workers could take up the offer of ESI at some point in the future. That is, employers should still be wary of the potential medical expenditures of workers who do not, yet, take up the offer of ESI.

A.1 Effect of ESI on Smokers' Wages using Cowan and Schwab's Approach

To show that the concerns with Cowan and Schwab's approach to identification persist in the MEPS data from 2006 to 2014, Table A3 examines estimates for the ESI-related smoking wage penalty

Table A3: **Alternative Estimates of the ESI-related Smoking Wage Penalty**

	(1) Hourly Wage	(2) Hourly Wage	(3) Hourly Wage
Panel A - More than 50 FTEs			
Smokes	-2.0969***	-0.8339***	-0.5280**
	(0.250)	(0.270)	(0.259)
Offered ESI=1	10.7630***	6.8618***	5.5580***
	(0.185)	(0.175)	(0.177)
Smokes × Offered ESI=1	-2.4457***	-0.9708***	-0.6783**
	(0.335)	(0.334)	(0.316)
Observations	40,755	40,308	40,308
Panel B - Less than 50 FTEs			
Smokes	-0.8911***	-0.5039**	-0.3650*
	(0.209)	(0.208)	(0.198)
Offered ESI	7.8868***	6.0216***	5.0080***
	(0.228)	(0.215)	(0.215)
Smokes × Offered ESI	-2.2347***	-1.2143***	-0.9450**
	(0.442)	(0.424)	(0.398)
Observations	15,694	15,506	15,504
Demographic Controls	N	Y	Y
Region, Occupation, & Industry Fixed Effects	N	N	Y

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. All dollar values have been adjusted to 2014 values using the CPI (www.bls.gov). Standard errors are clustered at the individual level. Demographic controls: age (quadratic), gender, race, education, and marital status.

using an estimating equation of the form

$$Hourly\ wage_{it} = \beta_0 + \beta_1 ESI_{it} + \beta_2 Smokes_{it} + \beta_3 ESI_{it} \times Smokes_{it} + \Pi X_{it} + \epsilon_{it}.$$

In the table, Panel A presents three sets of estimates of the ESI-related wage offset for smokers at firms with more than 50 FTEs. The first specification includes neither demographic controls nor fixed effects. The second specification adds demographic controls. The third includes both demographic controls and fixed effects. For each specification in Panel A, the estimates suggest smokers earn less than non-smokers and that workers with ESI earn more than workers without ESI. The addition of controls reduces the size of the effects in each case suggesting that smokers are different in ways that ensure that the unconditional wage differences between smokers and non-smokers captures more than just the effect of smoking.

The coefficient estimate associated with the interaction term is a measure of the effect of ESI on smokers' wages. Again, the addition of controls reduces the size of the effect in each case. In the final column, the interaction term suggests smokers earn an additional 68 cents less per hour than non-smokers when they work at firms where ESI is offered. The estimated effect is causal only if nothing other than ESI affects the wage gap between smokers and non-smokers wherever ESI is offered. As discussed in Section 2 of the body of the paper, this identifying assumption is not likely to be true. Regardless, it is not plausible that the medical expenditures (see Tables 1 and 2) associated with smoking are the cause of a \$1,360 reduction in annual wages.

Panel B presents the same three specifications for MEPS respondents who work at firms with fewer than 50 employees. The specification in the final column suggests that ESI is associated with a \$1,890 increase in the wage gap between smokers and non-smokers. Again, given the medical expenditure differences, this is an implausible estimate of the effect of ESI on smokers' wages. In contrast, and as shown in the body of the paper, the effects associated with the employer mandate are aligned well with the medical expenditures of smokers relative to non-smokers.

A.2 Addressing Cowan and Schwab's Robustness Checks and Additional Evidence

To avoid the body of this paper becoming a post-hoc referee report, this appendix subsection considers the validity of the robustness checks and evidence offered in support of Cowan and Schwab's main estimates. In their paper, they offer three types of evidence to support their conclusions. The first is that the ESI-related effect of smoking on wages is larger for older workers. Cowan and Schwab report that workers aged 40 and over who smoke experience a \$3.39 ESI-related wage offset (a \$6,780 effect on annual wages). For workers under 40, the wage offset is \$1.52 (or

$3,040 in annual terms). They then suggest that the additional wage offset for older workers is due to their higher medical expenditures. In their data, the medical expenditure difference between smokers and non-smokers for workers aged 18-40 is $75 per year and $484 for those aged 41-64. Either employers are responsive to the costs of smoking or they are not. It is inconsistent to claim that employers are very responsive to the the medical spending of older smokers while not being responsive to the fact that the spending difference among younger workers is only $75 per year. Something just doesn't add up here. In each case, the wage offset is an order of magnitude larger than the difference in medical expenditures that it is supposedly caused by. It is possible that employers are making an error but that error would have to be larger for older workers. It is unclear why that would be the case.

The second piece of evidence is a fixed effects approach where identification comes from changes in smoking and ESI status (see Table 2 in their paper, p. 1097). Using changes in smoking and ESI status is a questionable strategy because these are endogenous choices. Levy and Feldman (2001) use a similar strategy to examine how individual medical expenditures affect wages. They note that "those who gain or lose health insurance are almost certainly experiencing other productivity-related changes that render our fixed-effects identification strategy invalid." Cowan and Schwab's fixed effects estimates are equally consistent with the idea that smokers who experience a negative health shock would seek out a job with ESI. The same health shock might reduce their productivity (and therefore, their wages). The desire to obtain ESI would also affect labor supply decisions and the worker's reservation wage absent any effect on measured productivity. In addition, Cowan and Schwab claim that their fixed effects approach reduces the likelihood that their findings are due to differences in productivity between smokers and non-smokers in jobs that provide ESI compared to jobs that do not. For this to be a valid conclusion, it would have to be the case that underlying productivity differences across workers affect wages completely independently of firm characteristics (size, ESI, industry, and so on). That is unlikely.

The third piece of the puzzle is that they examine how the presence of other fringe benefits affects smokers' wages. The idea is that these other fringe benefits (retirement accounts, child care, dental coverage, and so on) are not more costly to provide to smokers. They find that other fringe benefits are not generally associated with lower wages for smokers (see Table 5, p. 1099). These estimates are supposed to ease concerns that there are differences across firms that offer fringe benefits that explain the paper's main findings. However, in each specification the authors include the main effect of having ESI and its interaction with smoking. To the degree that fringe benefits are correlated with ESI, it is unclear how this exercise helps. If the firms that offer retirement accounts are simply a subset of those that offer ESI, then the coefficient estimate on the "Retirement × Smoking" term could only be expected to be zero. A better approach would have been to eliminate all workers who have ESI from the sample. Of course, the number of workers who are offered a retirement account or dental coverage or maternity leave but are not offered ESI is likely to be extremely small. If so, the exercise cannot do what Cowan and Schwab are asking it to do.