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Employer-Sponsored Health Insurance and the Gender Wage Gap: Evidence from the Employer Mandate

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In the United States, female workers tend to have higher medical expenditures than male workers. Due to experience rated premiums, the cost of providing employer-sponsored health insurance (ESI) therefore differs by gender. This article examines if that cost difference contributes to the gender wage gap. Identification comes from the exogenous variation provided by the Affordable Care Act's employer mandate. Estimation uses a difference-in-difference framework with data from the Medical Expenditure Panel Survey. Findings suggest the portion of the gender wage gap attributable to ESI is smaller than existing estimates in the literature and is statistically no different to zero once individual medical expenses are included as a control. In addition, the article's empirical approach highlights that existing work on the role of ESI in the gender wage gap does not separately identify the effect of ESI from plausible alternatives.

JEL Classification: I13, J16, J23, J31, J32, J71

1. Introduction

This article examines if employer-sponsored health insurance (ESI) contributes to the gender wage gap in the United States. ESI could cause some portion of the wage gap because the cost of ESI (for firms) is a function of employees' actual medical expenditures and females tend to have higher medical expenditures.[1] ESI therefore creates a cost wedge between males and females because employee contributions toward ESI cannot legally vary by gender.[2]

The cost wedge is not trivial. Cylus et al. (2011) find that annual health care spending in 2004 was 32% more for females than for males. Cylus et al. explain that females spend more per capita than males across all payers and services. The difference is also not solely related to child-bearing with "significant variations in per person spending by gender across age groups." Bertakis et al. (2000), Woolhandler and Himmelstein (2007), and Bertakis and Azari (2010) also report that female medical expenditures tend to be higher than males. Bertakis and her coauthors find that a large part of the difference is due to frequency of primary care visits and use of diagnostic services. In the 2006 to 2014 Medical Expenditure Panel Survey (MEPS) data used in this article, annual medical expenditures (for respondents aged 18–64 and employed) were $3388 for females and $2272 for males.

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[1] The full cost of ESI is pushed onto firms either via experience rating (premiums reflect the actual expenditures of employees) or self-insurance.

[2] The Health Insurance Portability and Accountability Act (HIPAA) forbids discriminatory distinctions in benefit generosity and employee contributions. See https://www.shrm.org/resourcesandtools/tools-and-samples/hr-qa/pages/offeringdifferentbenefitsfordifferentemployees.aspx.

Given these medical expenditure differences, employers who offer ESI should prefer (at the margin) to hire males unless female wages are free to adjust to account for the cost of ESI.[3] However, Daneshvary and Clauretie (2007), using spousal job characteristics as an instrumental variable, find little evidence to suggest that females receive lower wages than males due to ESI. Cowan and Schwab (2016) reach the opposite conclusion. They use a difference-in-difference empirical strategy that compares wage gaps between males and females at firms that do and do not offer ESI. Their strategy is borrowed from Bhattacharya and Bundorf (2009), who examine how ESI affects obese workers' wages. Cowan and Schwab's findings suggest that hourly wages are \$0.50 to \$1.50 (in 2002 dollars) larger for males when ESI is offered. Even though firms that do and do not offer ESI are surely different, these estimates have a causal interpretation if the identifying assumption—that unobserved effects on wages are the same for males and females—is valid.

This article first establishes that Cowan and Schwab's (and, in turn, Bhattacharya and Bundorf's) identifying assumption does not hold. To do so, the article subjects their identification strategy to a series of falsification tests using wage and medical expenditure differences among other groups. This exercise highlights that there are differences between firms that do and do not offer ESI that will tend to increase wage gaps between workers—for reasons that are unrelated to ESI. These identification issues ensure that the effect of ESI on the gender wage gap remains an open question. To resolve that open question, the article uses the exogenous variation provided by the Affordable Care Act's (ACA) employer mandate as a source of improved identification. Because the employer mandate required all firms with more than 50 employees to offer coverage from 2014 onward, it creates a natural experiment to test if females are paid less due to employers having to offer ESI. Most importantly, using the employer mandate for identification avoids comparisons across firms that do and do not offer ESI.

As a further contribution—because MEPS contains data on individual medical expenditures—this article's empirical approach can test if males and females who have similar medical expenses face similar labor market outcomes. In other words, the article can examine if the effect of ESI on female wages is robust to controls for individual medical expenditures.[4] This is an important extension to this literature because, even if females are paid less when ESI is offered, it is not clear the causal relationship is between gender and wages or between individual medical expenditures and wages. In either case female wages will tend to be lower due to ESI. However, one would reflect a relationship between gender and wage outcomes where the cost of ESI is shared among females at the group level while the other implies a potentially efficient system of wages and benefits tailored to the individual regardless of gender. The difference is subtle but important for understanding policy implications. It could be that females who have low medical expenditures will be statistically discriminated against because of the higher expenditures of other females. In such a situation it would be easy to make a case for a policy response to "level the playing field." On the other hand, if males and females with similar medical expenses face similar reductions in wages, then market forces are at work: males and females with larger medical expenditures are paid relatively lower wages.

A limitation of this article's approach is that relying on the employer mandate for identification ensures that the article's estimates reflect anticipatory effects. Employers were informed of the

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[3] ESI is just one of many potential causes of the observed gender wage gap; for more on other causes see Waldfogel (1998), Altonji and Blank (1999), Blau and Kahn (2000), Mulligan and Rubinstein (2008), Manning and Saidi (2010), Bertrand et al. (2010), or Goldin (2014)

[4] Note that the goal here is only to examine if the group offset is robust to the inclusion of controls for individual medical expenditures. In contrast, Lennon (2018a) focuses on the level and robustness of individual medical expenditure pass-through.

mandate in early 2010 and, because they cost more to cover, theory predicts that forward-looking employers would reduce their demand for females in the lead up to implementation—employing fewer females, paying females a lower wage, or both. A forward-looking approach is appropriate because employment is an ongoing relationship. Moreover, while the mandate did not bite until 2014, the cost of coverage was to be based on employee characteristics during 2013.[5] For this reason, firms seeking to minimize the cost of compliance with the mandate would have had to react prior to 2013. Garrett and Kaestner (2015), Mathur et al. (2016), and Even and MacPherson (2018) also search for anticipatory responses to the employer mandate. They examine if the employer mandate caused an increase in part-time employment because it applied only to employees who work more than 29 hours per week. Garrett and Kaestner and Mathur et al. find little effect on part-time employment levels. On the other hand, Even and MacPherson's work suggests that about 700,000 workers are in involuntary part-time employment due to the ACA's passage.[6]

An advantage of focusing on anticipatory effects is that it avoids having to consider other ACA provisions which might affect labor market outcomes after 2014. The most obvious of these would be the ACA's health insurance exchanges. These 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 differently for male and female workers (see Lennon 2018a for more on this).

As a preview of the article's findings, for employees who work at firms that are affected by the mandate, estimates suggest the gap in wages between males and females increases by $1.59 per hour (all estimates are in 2014 dollars) after the mandate is announced. That is, after employers are informed that they must provide ESI to workers, the wage gap between males and females increases significantly. The effect is caused by the employer mandate if nothing else affected the wage gap between males and females after 2010, such as general labor market trends. Supporting a causal interpretation, robustness checks show that the gender wage gap is not increasing during this time period for employees who work at firms that are unaffected by the mandate. These findings cannot be explained by changes in productivity or reduced absenteeism due to improved health because the mandate's announcement does not alter the current productivity or health of MEPS respondents.

Estimates also suggest that the effect of ESI on wages is sensitive to controls for individual medical expenditures. Specifically, the estimated effect of ESI on the gender wage gap decreases to $1.18 per hour and is no longer statistically different from zero when controls for medical expenditures at the individual level are included.[7] The goal of this exercise is only to show that it is not clear that employers pay groups that tend to have higher medical expenditures lower wages due to ESI. It is not the case that employers are able to determine the expected medical expenditures of every individual precisely. An insurance company could not do that for individual customers, either. Instead, the claim is that employers appear to be able to infer enough about workers to broadly

[5] In February 2014, penalties for non-compliance were postponed until 2015 for firms with more than 100 employees and to 2016 for firms with fewer than 100 employees. The estimates presented in the article include data from 2014 because the announcement of the delay was made two months after the supposed implementation date, muting its potential impact on employment decisions. Estimates differ only mildly when excluding 2014. In any case, delays and uncertainty surrounding the employer mandate would tend to work against finding significant effects.

[6] Even and MacPherson's work also extends beyond the 2014 implementation date.

[7] The effect of individual medical expenses is estimated to be a $0.16 reduction in hourly wages for each unit difference in log medical expenditures and is significant at the 1% level. If a full-time worker works about 2000 hours per year, a $0.16 per hour effect amounts to just a $320 difference in annual wages for a log unit difference in medical expenditures (roughly a doubling of medical expenditures, for example: $2000 versus $1000). See Lennon (2018a) for more detail on how individual medical expenditures and wages are related.

determine which of them have higher medical expenditures even after considering the modifying effects of gender, race, age, and other observable characteristics on expected medical expenditures.

The article proceeds with a brief review of the relevant literature and how this article contributes in section 2. Section 3 explains the data used in this article and the estimation strategy used to produce the estimates in section 4. Section 5 examines the robustness of those estimates. Section 6 concludes.

2. Background and Literature

Passed in 2010, the Affordable Care Act contained an "employer mandate" requiring employers with more than 50 full-time equivalent employees to provide ESI to full time workers (those who work over 29 hours in a usual week) from January 1, 2014.[8] While over 80% of MEPS respondents who work at a firm with more than 50 employees were already offered ESI, many employees could have expected to gain ESI coverage due to the mandate. However, economic theory predicts that those workers, rather than their employers, will bear the costs of that coverage.[9]

More formally, following Bhattacharya and Bundorf (2009) and Lennon (2018b), 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 the worker's 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. Suppose a worker with medical expenditures e_i adds premium p_{ik} to firm k's ESI costs. For simplicity, assume premiums are actuarially fair so that $p_{ik} = e_i$. An employer could choose to pool these costs across their N employees so that wages for worker i at firm k are

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

Under such a system, wages would be equal to the value of marginal product minus the firm-level average cost of providing coverage $\overline{p}_k$, where $\overline{p}_k = \frac{1}{N}\Sigma_{i=1}^{N} e_i = \frac{1}{N}\Sigma_{i=1}^{N} p_{ik}$. However, this leaves 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. Let each of the subgroups be denoted as n_j. For $i \in n_j$, then equilibrium wages (excusing the abuse of notation) would be

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

With this group-specific offset, the wages of each member of each group would be adjusted by the average medical expenditures of the group ($\overline{p}_{jk}$). This is potentially an equilibrium if the costs of searching for profitable deviations exceed the benefits.[10]

Given these theoretical predictions, Summers (1989) called for research into the empirical regularities of employment benefits such as ESI. Summers was particularly concerned that wage

[8] The enforcement of this provision was later delayed to 2015 for firms with 100 or more employees and to 2016 for firms with between 50 and 100 employees. Firms were informed of the delay in February of 2014.

[9] Employers who did not comply would have to pay an "Employer Shared Responsibility Payment" of $2000 per employee (employers could exclude 30 full-time employees from the penalty calculation). Given wages can be adjusted (workers value the coverage) and the tax treatment of employee medical expenditures, it would make little sense not to comply with the mandate.

[10] 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 beyond the scope of the article.

adjustments may not be feasible and that mandated benefits might therefore exclude certain workers from employment. In response, authors such as Gruber (1993), Sheiner (1999), Jensen and Morrisey (2001), Lahey (2012), and Bailey (2014) have found evidence that groups with higher medical expenditures experience lower wages, reduced employment levels, or both.

Daneshvary and Clauretie (2007) contribute to this literature by examining how researchers should attempt to value ESI. They focus on the general challenges faced when estimating the trade off between wages and ESI. Their empirical strategy uses 2001 MEPS data along with spouse's firm size and the presence of family insurance coverage as instrumental variables. In contrast to theoretical predictions, their estimates suggest that "[h]ealth insurance does not contribute to the unexplained portion of the gender pay gap." However, estimating how ESI affects the gender wage gap was not the main goal of the article. Indeed, Cowan and Schwab (2016) note that Daneshvary and Clauretie's approach is not ideal: "instruments for health insurance of the type used in Daneshvary and Clauretie (2007) are likely to be endogenous." To try to resolve these endogeneity problems they take a difference-in-difference approach: comparing male–female wage gaps at firms with and without ESI. Based upon NLSY79 and MEPS data from 2002 to 2008 their estimates suggest that the gender wage gap is larger by between \$0.50 and \$1.50 per hour when ESI is offered.

However, violating Cowan and Schwab's identifying assumption, there are differences between firms that do and do not offer ESI which could be expected to magnify existing wage differences between groups. One example is firm size (as measured by number of employees).[11] This confounds identification because firm size has been shown to increase the earnings of similarly productive workers (see Oi and Idson, 1999). If females have unobservable differences in productivity then the gender wage gap will be magnified at larger firms for reasons that are unrelated to ESI.[12] The problem is that ESI is much more common in larger firms ensuring that wage gaps at ESI and non-ESI firms likely differ due to both the cost of ESI and differences in firm size.[13]

Because ESI is correlated with other firm characteristics, the ideal source of identification is an exogenous change in the provision of ESI while holding firm characteristics constant. This would allow the researcher to isolate the effect of ESI from other confounding differences on the firm's demand for labor (and, in turn, on wages). The Affordable Care Act's employer mandate provides such a source of identification. Conveniently for this article, the mandate also creates two "control" groups that either (i) already receive coverage from their employer or (ii) are not covered by the Act's provisions. The next section describes the data and estimating equation used to estimate the effects of interest.

3. Empirical Framework

Data and Sample Selection

The empirical analysis in this article relies on data from the 2006–2014 Medical Expenditure Panel Survey. The Agency for Healthcare Research and Quality describes the MEPS as "a set of

[11] In their estimates, Cowan and Schwab control for firm size but do not do so differentially by gender. See footnotes to Table 3 in their article (pp. 108).

[12] Note that this claim does not require that females are less able.

[13] In the 2006 to 2014 MEPS data used in this article, 23% of men who do not have ESI work at firms with more than 50 employees. In contrast, 55% of men who have ESI work at firms with more than 50 employees. In addition, the average hourly wage at firms with 50 or fewer employees is \$17.18 but \$23.24 at firms with more than 50 employees.

large-scale surveys of families and individuals, their medical providers, and employers across the United States."[14] The household component of the survey uses a revolving cohort design. A new cohort joins the survey each calendar year and stays in the sample for two years. Each survey respondent completes five interviews across that time which collect data on health care usage, out of pocket costs, and insurance coverage, along with demographic and employment information. As many variables are reported only as an annual figure, the article focuses on the end-of-year interviews. Relevant summary statistics for males and females working at firms that do and do not offer coverage are presented in Table 1.[15]

Conveniently, MEPS can be used to construct a subsample of respondents who must receive coverage due to the employer mandate: respondents who work at firms with more than 50 employees and are not already offered ESI by their employer. The analysis in this article focuses on what happens to the relative wages of males and females in that subsample given they must be provided ESI in the near future. Note that it is not possible to create a sub-sample which identifies every MEPS respondent who will receive coverage due to the mandate. This is because MEPS does not directly ask about total employee numbers at a respondent's work. Instead, the survey asks (i) how many employees work at your work location and (ii) how many locations does your employer have? This creates ambiguity for respondents who report fewer than 50 employees at their location but also that their employer has more than one location. To avoid any potential bias, the estimation sample includes only respondents who work at firms that are certainly affected by the mandate.[16]

In addition, those under 27 are excluded from the estimates because the ACA's dependent coverage mandate affected younger workers' labor supply (see Antwi et al. 2013, Depew 2015, Hahn and Yang 2016, and Goda et al. 2016). Workers aged 60 and over are also excluded because they could be expected to retire prior to or very shortly after the mandate's implementation.

Lastly, the article's main estimates focus on the first end-of-year interviews for Panels 11 through 19 of the MEPS covering from the end of 2006 to the end of 2014. While the estimates are not qualitatively different, pooling both year-end interviews is indicated to be problematic by a Breusch-Pagan Lagrange multiplier test. However, when treating the data as a panel, a Hausman test indicates that a fixed rather than random effects estimation would be appropriate. However, a fixed effects approach cannot be used to study the effect of gender as it is invariant in the data. For this reason, the data are treated as a repeated cross-section by dropping the second interview with each respondent. As wages and employment do not change markedly between each year for most respondents, repeating the analysis with only data from the second year-end MEPS interview produces almost identical findings.

Estimation

The article uses the MEPS data described above in a difference-in-difference framework to estimate the effect of ESI on the wages of male and female MEPS respondents who are employed

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[14] See http://meps.ahrq.gov/mepsweb/.

[15] The data description in this section borrows liberally from Lennon (2018a).

[16] In the eight years of data used in the article, 40,418 respondents report working at a firm that has fewer than 50 employees. Of these, 7562 report that there are fewer than 50 employees at their work location, the employer has more than one business location, and that they are not already offered health coverage by their employer. It is not possible to know which of those 7562 workers belong in the sample and which do not and therefore they are all excluded from the analysis.

Table 1. Selected Summary Statistics by Gender, ESI, and Time Period from MEPS 2006–2014

	All	N = 57,353	Males (no ESI)	N = 13,609	Males (ESI)	N = 17,655	Females (no ESI)	N = 14,368	Females (ESI)	N = 15,673
2006–2010	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Hourly wages										
All	20.72	14.36	16.46	12.31	26.93	16.25	14.57	10.47	22.25	13.24
White	20.89	14.48	16.48	12.14	27.26	16.30	14.66	10.63	22.62	13.38
Black	18.02	11.66	14.77	10.10	22.36	13.08	13.08	8.68	19.80	11.40
High school (or less)	14.72	8.28	13.46	7.92	19.45	9.35	10.97	5.57	15.50	7.34
College (or more)	26.26	16.41	22.45	16.59	32.19	17.90	19.03	13.07	25.98	14.25
Female	0.5									
Holds ESI from employer	0.55									
Female × holds coverage	0.53									
Offered ESI from employer	0.68		0.1				0.15			
Female × offered coverage	0.68									
White	0.75		0.79		0.76		0.75		0.7	
Black	0.16		0.13		0.14		0.17		0.21	
Married	0.59		0.6		0.67		0.58		0.52	
Age	41.24	11.78	39.57	12.28	42.66	11.17	40.16	12.06	43.01	11.26
High school or less	0.47		0.61		0.41		0.53		0.36	
More than high school	0.42		0.32		0.45		0.39		0.89	
More than college	0.11		0.07		0.14		0.08		0.15	
Employer size 0–49	0.54		0.77		0.45		0.69		0.42	
Employer size 50–99	0.12		0.08		0.12		0.11		0.13	
Employer size 100–199	0.59		0.04		0.07		0.04		0.07	
Employer size 200–299	0.1		0.05		0.12		0.07		0.12	
Employer size 300–399	0.02		0.01		0.03		0.01		0.03	
Employer size 400+	0.17		0.06		0.22		0.08		0.24	
Annual medical exp.										
All	2926.83	8536.95	1735.19	6835.16	2756.57	9723.37	2939.13	7657.41	4062.97	9063.03

White	3026.75	8611.22	1772.07	6650.01	2936.24	10,016.05	3069.84	7786.23	4234.61	9121.75
Black	2769.99	8662.61	1651.71	8438.52	2280.13	8374.98	2508.82	7369.32	3831.00	9455.90
High school (or less)	2329.59	7884.18	1313.97	6657.08	2402.59	8409.10	2397.55	7599.17	3555.67	9452.42
College (or more)	3476.00	9067.81	2415.58	7036.15	3019.41	10,582.37	3579.12	7712.59	4359.62	8837.22

	All	N = 49,529	Males (no ESI)	N = 12,792	Males (ESI)	N = 14,385	Females (no ESI)	N = 13,111	Females (ESI)	N = 12,836
2011–2014	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Hourly wage										
All	19.75	13.74	15.47	11.18	25.95	15.52	13.79	9.92	22.46	13.22
White	19.72	13.64	15.31	10.65	25.95	15.40	13.85	9.91	22.73	13.29
Black	17.38	11.54	14.15	10.14	22.15	13.06	12.26	7.55	20.23	11.70
High school (or less)	13.82	7.60	12.60	6.25	18.48	9.08	10.32	4.67	15.38	7.84
College (or more)	24.18	15.50	19.90	15.01	30.20	16.76	17.05	12.17	25.38	13.87
Female	0.49									
Holds ESI from employer	0.51									
Female × holds coverage	0.49									
Offered ESI from employer	0.65									
Female × offered coverage	0.65									
White	0.7		0.75		0.72		0.7		0.66	
Black	0.18		0.16		0.16		0.19		0.22	
Married	0.54		0.52		0.62		0.5		0.49	
Age	40.98	12.03	39.38	12.38	42.87	11.4	39.27	12.22	43.34	11.47
High school or less	0.42		0.56		0.37		0.47		0.3	
More than high school	0.48		0.38		0.5		0.47		0.55	
More than college	0.1		0.06		0.13		0.07		0.15	
Employer size 0–49	0.56		0.76		0.46		0.7		0.43	
Employer size 50–99	0.12		0.08		0.12		0.11		0.13	
Employer size 100–199	0.06		0.04		0.07		0.04		0.07	

(Continues)

Table 1. (Continued)

	All	N = 49,529	Males (no ESI)	N = 12,792	Males (ESI)	N = 14,385	Females (no ESI)	N = 13,111	Females (ESI)	N = 12,836
2011–2014	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Employer size 200–299	0.1		0.05		0.11		0.07		0.12	
Employer size 300–399	0.02		0.01		0.03		0.01		0.03	
Employer size 400+	0.15		0.05		0.2		0.07		0.21	
Annual medical exp.										
All	2802.57	9602.28	1648.62	7037.78	2731.69	10,587.73	2725.64	9234.81	3986.53	10,255.21
White	2940.09	9872.77	1700.40	7276.17	2895.40	10,381.15	2917.31	10,277.68	4303.35	10,544.41
Black	2626.91	9916.06	1441.58	6162.28	2531.95	13,732.09	2330.54	5948.46	3576.42	10,447.68
High school (or less)	2060.11	8184.70	1225.80	5765.44	2494.48	11,355.77	2003.04	7309.38	2943.66	7177.52
College (or more)	3359.82	10,508.63	2225.17	8435.81	2881.37	10,137.74	3383.39	10,643.84	4430.92	11,280.67

Notes: Summary statistics are split into two time periods, 2006–2010 and 2011–2014, because of the use of the ACA's mandate on coverage passed in 2010 for identification later in the article. The statistics are based on the MEPS sample aged 18–64 from 2006 to 2014 as noted. The number of observations refers to the number of observations for which hourly wages was reported. As such, the sample only considers those who report that they worked for a wage during the survey period.

at firms affected by the employer mandate (EM). The estimating equation is of the following form:

$$HourlyWage_{it} = \beta_0 + \beta_1 Gender_{it} + \beta_2 AfterEM_{it} + \beta_3 AfterEM_{it} \times Gender_{it} + \Pi X_{it} + \epsilon_{it}$$

In the equation, $HourlyWage_{it}$ is the hourly wage of person *i* at time *t.* The gender dummy ($Gender_{it}$) controls for the general relationship between wages and gender across the sample period. In all estimates $Gender_{it}$ = 1 for males; therefore, a positive coefficient indicates male wages are greater than female wages. The $AfterEM_{it}$ term controls for changes in wages that affect males and females similarly in the years after the employer mandate was announced. In the estimates presented, *AfterEM* = 1 for 2011 and subsequent years because the mandate was enacted in March of 2010. If firms were able to react immediately, then the estimates presented can be considered as a lower bound. The coefficient on the interaction term gives a measure of how wages change differently by gender after the mandate is announced. If nothing else affects the wages of males and females differently after the employer mandate was announced, then the estimated coefficient has a causal interpretation. The estimating equation is completed by allowing for a set of typical demographic controls X_{it} such as age, sex, education, marital status, race, location, and industry.

This approach avoids comparing wage differences across firms that differ in a myriad of ways in addition to ESI. To illustrate the importance of this difference, the article also replicates and updates Cowan and Schwab's analysis. Cowan and Schwab's estimating equation is:

$$HourlyWage_{it} = \beta_0 + \beta_1 Gender_{it} + \beta_2 ESI_{it} + \beta_3 ESI_{it} \times Gender_{it} + \Pi X_{it} + \epsilon_{it}$$

The only substantive difference between this estimating equation and the one presented earlier is an ESI dummy replacing the time (*AfterEM*) dummy. The ESI term captures any changes which affect all workers equally at firms that offer ESI. The coefficient on the interaction of the gender and ESI terms in the estimating equation gives a measure of the effect of ESI on wages as a function of gender, if the identifying assumption holds. However, if whatever is driving the gender wage gap (discrimination, unobservable differences in productivity, and so on) is also a function of firm characteristics that are correlated with ESI then identification becomes clouded. In such a case, a difference-in-difference estimation of the effect of ESI and gender on wages will reflect a mechanical association that is not *caused* by ESI.

4. Empirical Findings

Estimates Using Firms With and Without ESI

The first two columns in Table 2 are a replication of Cowan and Schwab's main findings with and without demographic, location, and industry controls. Throughout the article, the coefficients of interest are presented both with and without controls to add context and provide confidence that the observed effects are robust and not due to careful choice of specification. The estimates use 2006–2014 MEPS data in combination with the difference-in-difference estimating equation laid out in section 3. The dependent variable is hourly wages in each specification. The interaction term "Offered ESI × Male" suggests that males experience relatively higher wages when employed at firms that offer ESI. That is, as Cowan and Schwab found, the gender wage gap tends to be larger when ESI is offered. Strictly speaking, Cowan and Schwab use "holds coverage." MEPS asks respondents if they are offered ESI and whether or not they take that coverage but estimates change little using one or the other as take-up

Table 2. Replication of Cowan and Schwab's Main Findings, MEPS 2006–2014 Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage
Offered ESI	9.643***	4.638***			8.065***	3.608***	5.776***	3.595***
	(0.159)	(0.158)			(0.258)	(0.248)	(0.118)	(0.133)
Male	1.807***	2.330***	3.084***	3.601***		3.428***		3.403***
	(0.168)	(0.174)	(0.168)	(0.156)		(0.136)		(0.136)
Offered ESI × male	2.363***	1.776***						
	(0.248)	(0.225)						
Employees (100's)			1.843***	1.096***				
			(0.0559)	(0.0499)				
Male × employees			0.404***	0.151**				
			(0.0874)	(0.0746)				
Caucasian					0.999***	−0.111		
					(0.211)	(0.214)		
Offered ESI × caucasian					3.127***	2.287***		
					(0.297)	(0.272)		
College educated							4.574***	4.477***
							(0.196)	(0.189)
Offered ESI × college							6.386***	5.537***
							(0.250)	(0.238)
Observations	38,438	38,243	38,438	38,243	34,357	34,195	38,438	38,438
Demographic controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Firm size	No	Yes	No	Yes	No	Yes	No	Yes
Region fixed effect	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). Controls include gender, marital status, age (cubic), race, location, industry, and education as appropriate given the coefficient of interest in the specification. ESI: employer-sponsored health insurance.
***$p < 0.01$.
**$p < 0.05$.
*$p < 0.1$.

is 85–90% (see Table 1). The estimates in this article use "offered coverage" for two reasons. One, the employer mandate requires firms to *offer* coverage, workers do not have to take the offer. Two, employees who do not accept the offer of coverage at time t are usually free to do so at time $s > t$.

In the second pair of estimates (columns 3 and 4), the estimating equation is altered slightly. It substitutes a continuous measure of firm size (in hundreds of employees) for the dummy for ESI. The third column represents estimates without demographic, location, and industry controls, while the fourth column includes a complete set of controls. In that fourth column, the term "Male × Employees" shows that for every 100 employees hourly wages increase $1.10 per hour for all workers and by an additional $0.15 for men. It is hard to explain these findings without suggesting firm size and productivity are related.[17] As ESI is typically offered at all larger firms it is not clear that ESI is the *cause* of Cowan and Schwab's findings. Note that this article is not attempting to explain away Cowan and Schwab's findings via firm size. The article is agnostic about the source of firm size-related wage differences. The value of the exercise is that it shows that ESI is only one of potentially many *different* determinants of wages for workers at firms with ESI compared to those without ESI.

Cowan and Schwab consider that "it is possible that the provision of ESI is correlated with other job characteristics that lead to a larger female wage gap in those firms that provide it than in firms that do not." They examine how female wages respond to other employment benefits as a check on that theory.[18] They focus on benefits that are not generally costlier by gender and find no association between gender and wages as a function of those benefits. If Cowan and Schwab were to find such a relationship then it would suggest that other differences between firms that offer fringe benefits and those that do not are driving their findings. However, each of their estimates also controls for ESI and its interaction with gender. If ESI is generally offered by firms that offer other employment benefits, it becomes difficult to separate the effects. That is, the estimated effect of other employment benefits on female wages could be larger and statistically significant if the ESI terms were eliminated from the estimates. If so, and because these benefits are not costlier by gender, that would leave two possibilities. One, these other benefits are proxying for the presence of ESI and the effect that ESI has on female wages. Alternatively, these benefits are only offered at firms where differences in wages between males and females are already large for other reasons. In either case, the falsification exercise cannot do what Cowan and Schwab are asking it to do.

Additionally, Cowan and Schwab's empirical strategy can be directly tested by appealing to other groups of workers with differences in wages and medical expenditures. In Table 1, the 2006–2014 MEPS data suggest that white workers are paid more per hour than black workers but medical expenditures are slightly lower for blacks.[19] If ESI causes the gender wage gap to widen, then ESI should reduce or at least not exacerbate the black-white wage gap. The estimates from an exercise to test this claim are presented in the fifth (no controls) and sixth columns (including controls) of Table 2.[20] The prediction would be that firms with ESI should have a slightly smaller black-white wage gap. However, estimates show that the gap in hourly wages is $2.28 larger at firms with ESI.

Similarly, college graduates have both higher medical expenditures (see Table 1) *and* higher wages than non-graduates.[21] This means that ESI should be associated with relatively lower wages

[17] This is a common finding; again, see Oi and Idson (1999).

[18] Other benefits in the NLSY79 data included training programs, profit sharing, retirement plans, childcare, dental insurance, flexible work schedules, life insurance, and parental (not maternity) leave.

[19] White and black workers have a well-established wage gap - see Altonji and Blank (1999) for an overview.

[20] The sample is restricted only to whites and blacks for these estimates.

[21] For more on the college wage premium see Goldin and Katz (2007).

for college-educated employees, if Cowan and Schwab's approach to identification is correct. Instead, in columns 7 (no controls) and 8 (including controls), the estimates show that ESI is associated with larger wages for those with a college degree.

The same basic analysis using any two groups that tend to have different wages (young versus old workers, married versus single workers, and so on) finds that ESI is associated with larger wage differences between the two groups regardless of medical expenditures across the groups. Note that, this does not imply that ESI has no effect on wages. It suggests only that other characteristics of firms that offer ESI have a significant role in wage determination. To obtain clean identification, an exogenous change in ESI status is required that keeps other firm characteristics constant. This is precisely what the ACA's employer mandate does. By requiring employers to provide ESI it forces employers to consider the medical expenditures of their employee pool and to economize along this new dimension as they see fit.

Estimates using Employer Mandate for Identification

Table 3 reports difference-in-difference estimates of the ESI-related gender wage gap using the ACA's employer mandate for identification. Instead of comparing the gender wage gap at firms that do and do not offer ESI, the estimates in Table 3 examine how the wage gap between males and females changes after the employer mandate is announced for workers affected by the mandate. Panel A of Table 3 reports the findings based upon the MEPS sub-sample of respondents who are not already offered ESI who report working at a firm with more than 50 employees. These are the main estimates of interest.[22] If males are less costly to cover, then male wages at firms that must provide ESI due to the mandate should increase relative to females.

The estimates in the first two columns of panel A show the gender wage gap increases at firms that must provide coverage due to the employer mandate in the years after the employer mandate was announced. The \$1.59 effect is statistically significant at the 5% level in the specification with a full set of controls in column 2. The effect is causal if nothing else affects the wages of males and females differently at these firms after 2010. Given the estimates presented are in 2014 dollars, the size of the estimates align reasonably well with the \$.50–\$1.50 range Cowan and Schwab suggest (which were in 2002 dollars). Using the CPI (www.bls.gov) to convert from 2002 to 2014, Cowan and Schwab's estimates would be \$0.66–\$1.97.

In estimates not presented here, the observed effect on the gender wage gap increases to \$2.01 but is no longer statistically significant from zero when the sample is restricted to recent hires (those who report tenure less than two years when responding to MEPS). This suggests that a large portion but not all of the increase in the gender wage gap at affected firms is happening via changes in wages paid to recent hires. However, the sample is too small to be effectively stratified by tenure. Restricting to tenure of no more than two years leaves fewer than 300 observations in total across the four "After EM" years (2011, 2012, 2013, and 2014).

The estimates presented help to resolve concerns with the existing literature on this topic but do not address the important question of group versus individual level effects. In column 3 of

[22] Note that the sub-samples of MEPS data used to estimate the coefficients in panel A and B of Table 3 could be combined into a single triple-difference specification. In that case, the difference-in-difference interaction term (along with the employer mandate and gender dummies) would be further interacted with a dummy for ESI (where ESI = 1 means the respondent was offered health coverage at their job). The estimates are presented as two difference-in-differences for ease of interpretation.

Table 3. Estimates of ESI-induced Gender Wage Gap, Employer Mandate, MEPS 2006–2014 Data

Panel A MEPS respondents without ESI	(1) Hourly wage	(2) Hourly wage	(3) Hourly wage	(4) Log hourly wage	(5) Log hourly wage	(4) Hourly wage	(5) Hourly wage	(6) Hourly wage	(7) Hourly wage
After EM (After 2010)	−2.667***	10.15	11.63	0.537	0.609	−1.439	12.28	−1.572**	12.69
	(0.876)	(9.643)	(9.682)	(0.533)	(0.533)	(1.022)	(9.793)	(0.787)	(9.803)
Male	0.300	0.803	1.289**	0.0448	0.0680**		1.521***		1.813***
	(0.589)	(0.590)	(0.602)	(0.0290)	(0.0308)		(0.403)		(0.404)
EM × male	1.077	1.593**	1.175	0.0934**	0.0730*				
	(0.777)	(0.776)	(0.791)	(0.0399)	(0.0421)				
Log annual med. exp.			0.191***		0.00915***				
			(0.0469)		(0.00261)				
EM × med. Exp.			−0.162***		−0.00797**				
			(0.0619)		(0.00369)				
Caucasian						1.851***	0.820		
						(0.595)	(0.578)		
EM × caucasian						−0.910	−1.072		
						(0.803)	(0.814)		
College educated								7.217***	6.406***
								(0.629)	(0.614)
EM × college educated								−2.130***	−1.827**
								(0.820)	(0.787)
Observations	2738	2722	2722	2722	2722	2533	2518	2738	2738
Panel B MEPS respondents with ESI	Hourly wage	Hourly wage	Hourly wage	Log hourly wage	Log hourly wage	Hourly wage	Hourly wage	Hourly wage	Hourly wage
After EM (After 2010)	−0.972*	8.149	9.001	0.389	0.429	−1.256*	7.220	−1.647***	4.524
	(0.546)	(8.182)	(8.163)	(0.319)	(0.318)	(0.642)	(8.443)	(0.469)	(8.633)
Male	4.561***	4.019***	4.316***	0.148***	0.162***		3.715***		4.245***
	(0.329)	(0.300)	(0.307)	(0.0103)	(0.0104)		(0.227)		(0.230)
EM × male	−1.011**	−0.521	−0.550	−0.0105	−0.0111				
	(0.476)	(0.434)	(0.442)	(0.0152)	(0.0154)				
Log annual med. exp.			0.197***		0.00892***				

(Continues)

Table 3. (Continued)

Panel B MEPS respondents with ESI	Hourly wage	Hourly wage	Hourly wage	Log hourly wage	Log hourly wage	Hourly wage	Hourly wage	Hourly wage	Hourly wage
			(0.0319)		(0.00118)				
EM × med. exp.			−0.0104		7.81e-06				
			(0.0446)		(0.00168)				
Caucasian						4.642***	1.963***		
						(0.373)	(0.332)		
EM × caucasian						−0.586	−0.283		
						(0.535)	(0.487)		
College educated								12.84***	11.63***
								(0.275)	(0.278)
EM × college educated								−1.534***	−1.400***
								(0.396)	(0.401)
Observations	16,628	16,570	16,570	16,570	16,570	14,657	14,606	16,628	16,628
Demographic controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Industry fixed effect	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Firm size	No	Yes	No	Yes	No	Yes	No	Yes	
Region fixed effect	No	Yes	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A focuses on those who are not already offered ESI. Panel B focuses on those who work at a firm that offers ESI already. Robust standard errors are given in parentheses. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). Controls include gender, marital status, age (cubic), race, location, industry, and education as appropriate given the coefficient of interest in the specification. EM: employer mandate.

***$p < 0.01$.

**$p < 0.05$.

*$p < 0.1$.

panel A of Table 3, the estimation in column 2 is repeated but adds a control for medical expenditures at the individual level and its interaction with the passage of the ACA's mandate.[23] Note that the estimates presented are intended to examine if ESI-related group-level wage offsets are robust to the inclusion of controls for individual medical expenditures—rather than examining the level and robustness of wage offsets caused by individual medical expenditures. In the specification, the "EM × Male" effect decreases in size and is no longer statistically significant when controls for individual medical expenditures are introduced. This suggests that gender is acting as a proxy for individual medical expenditures so that when estimations do not control for both gender and individual expenditures, differences in spending across genders are soaked up by the gender term. The estimates suggest there is a \$0.16 per hour wage offset for each log unit difference in medical expenditures (corresponding to a \$320 difference in annual wages). The relatively small pass-through is perhaps not surprising given medical expenditures (such as insurance premiums) paid by firms are tax deductible, some of the cost would be borne by employees via cost sharing, and some firms might have lots of turnover giving them little incentive to make adjustments in advance of the mandate.[24] In addition, the estimates rely on anticipatory effects. Future medical expenditures are surely more predictable at the group level than at the individual level. For example, by definition, medical expenditures associated with one-off events at the individual level provide no information on future medical expenditures. Individuals who have expenditures that are ongoing and predictable might see a greater pass-through of medical expenditures. Without a longer panel, it is not possible to examine if this is the case.

What is interesting is that research on the effects of ESI on wages has used differences in health status and/or medical expenditure across broad groups to identify wage effects. None of those articles, including Gruber (1993, 1994), Sheiner (1999), Jensen and Morrisey (2001), Bhattacharya and Bundorf (2009), Cowan and Schwab (2011), Lahey (2012), and Bailey (2013, 2014) examine if members of the group they study are affected equally. The estimates in Table 3 suggest employers might form expectations of individual or at least subgroup costs (this could be based on absenteeism, physical characteristics visible at interview, and/or employee behavior). This is arguably easier in smaller firms, precisely the type of firm affected by the ACA's employer mandate given larger firms typically already offered ESI.

The estimates in columns 4 – 7 provide additional confidence in the article's identification strategy. These columns present a repeat of the falsification tests using race and college education from Table 2. In Table 2, the effect of ESI on racial and college-related wage gaps was the opposite of what Cowan and Schwab's approach, given the observed medical expenditure differences, would predict. The estimates in the fourth (no controls) and fifth (including controls) columns of Table 3 focus on the black-white wage gap. Given blacks tend to have lower medical expenditures in the data, the employer mandate should reduce the black-white wage gap at firms that must offer ESI in the near future. While not significantly different from zero, the direction of the "EM × Caucasian" term is as expected. The gap between medical expenditures for these groups is small so it is not surprising that its effect is not easily detected in a sample of 2500 or so. In contrast, the gap between medical expenditures for college and noncollege graduates is quite large and the direction and size

[23] Again, observations from 2011 and onward are considered after the employer mandate as many employment and salary decisions were already in place for 2010 before the mandate was announced.

[24] If employers placed any positive probability on ACA repeal and/or delays, it further dampens their incentive to prepare for the mandate's implementation.

of the “EM × College Educated” effect in columns 6 (no controls) and 7 (including controls) is reassuring.

Panel B repeats each of the estimates in panel A using the data of MEPS respondents who work at firms with more than 50 workers that already offer ESI. These estimates are presented to examine if the effects seen in panel A are caused by the employer mandate or simply reflect patterns of wages in the labor market as a whole. For firms with more than 50 workers who already offered ESI, the employer mandate does not change their incentives and therefore estimates should be no different to zero. In column 2, a specification with a full set of typical controls, the effect of “EM × Male” is negative but not statistically different from zero. That is, the male–female wage gap, if anything, is declining rather than increasing. In column 3, the interaction term suggests there are no changes in the effect of individual medical expenditures after the employer mandate is announced at firms that already offer coverage, as would be expected. Interestingly, the estimates seen in columns 4 – 7 suggest that wages of college educated and Caucasian workers decreased at firms that already offered ESI. This suggests that some (but not all) of the effects seen in columns 4 – 7 in panel A reflect changes in the labor market that are potentially unrelated to the employer mandate.

Overall, the estimates in Table 2 suggest that Cowan and Schwab’s approach likely picked up more than just the effect of ESI on wages for groups with different medical expenditures. The estimates presented in Table 3, using the ACA’s employer mandate for identification, concord with a theory that ESI affects wages for groups with different medical expenditures. The fact that the effect on wages is responsive to the group differences in medical expenditures appropriately for each group highlights the value of using the mandate for identification. The estimates also suggest that firms can and do respond to more than just group differences as the effect of gender on wages is not robust to controls for individual medical expenditures. The next section provides further falsification tests of both the identifying power of the mandate and the importance of individual medical expenditures when asking how ESI affects wages for different groups.

5. Robustness

This section focuses on ensuring the announcement of the employer mandate, rather than labor market trends or other events, is responsible for the effects observed.

Parallel Trends

The estimates seen in Table 3 appear to be causally related to the effects of the ACA’s employer mandate. While this is plausible, especially given the corresponding effects seen for other groups with different medical expenditures, they may just represent unrelated trends in the labor market. Figures 1 and 2 present postestimation plots of the gender wage gap and the relationship between medical expenditures and wages by year and by offer of ESI over the 2006–2014 time period. This is essentially an event-study version of the difference-in-difference estimating equation used to produce the estimates in section 4.

In the figures, outcomes of interest are plotted by year. Figure 1 plots the postestimation wages of men and women who work at firms that do and do not offer ESI over time. In Figure 2, the effect of medical expenditures on wages is plotted by year for workers at firms with and without ESI.

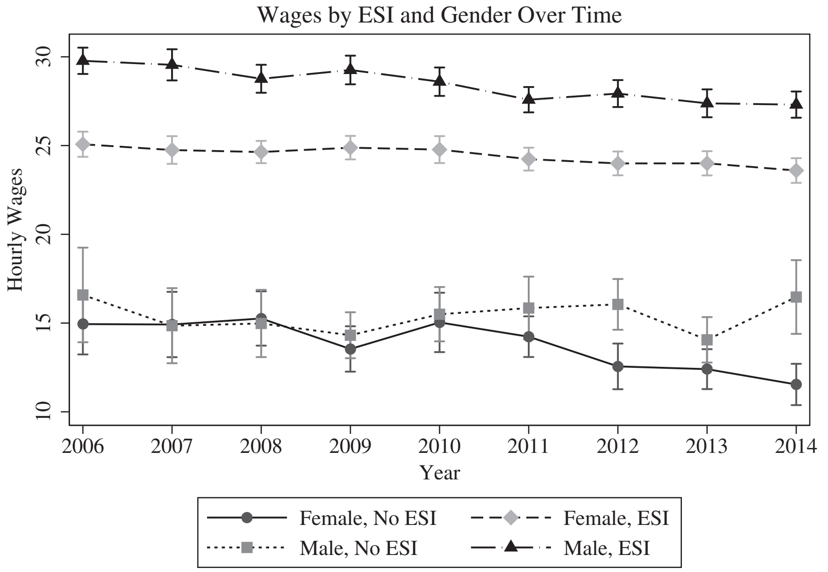

Figure 1. Postestimation Plot of Wages by Gender and ESI Status by Year.

Both figures suggest workers at firms that did not offer ESI experience changes after 2010. The same changes are not easily observed for workers who are already offered ESI by their employer. This pattern eases concerns that the enactment of the employer mandate is correlated with trends in the labor market which explain the article's findings.

Specifically, Figure 1 shows that the gap in wages between males and females does not change much over the sample period at firms that already offer ESI. This suggests that the employer mandate had little to no effect on the employees at these firms, as would be expected. Given over 80% of the sample worked at firms with ESI during the time period, this also suggests that there were also no other (unaccounted for) trends affecting the labor market that affected the gender wage gap.

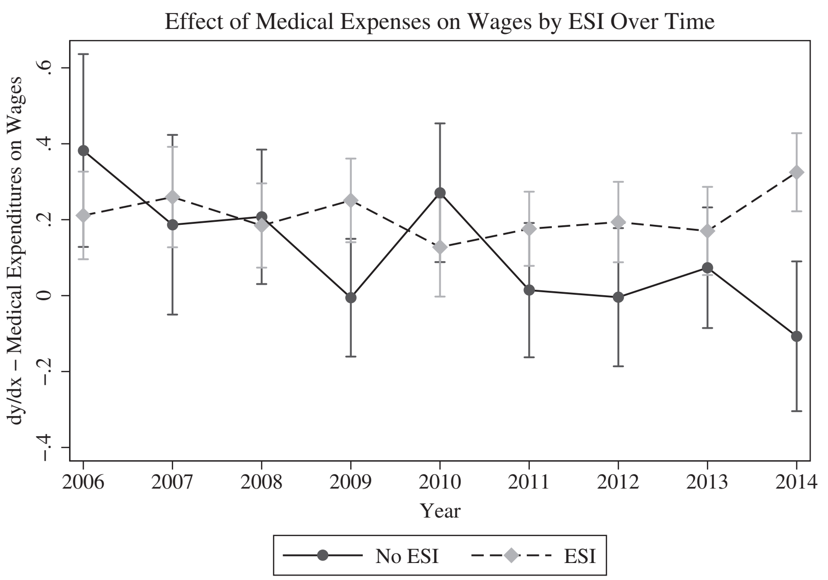

Figure 2. Postestimation Marginal Effect of Medical Expenditures on Wages by Year and by Offer of ESI.

In contrast, for workers at firms that do not offer ESI, a significant gender wage gap begins to appear after 2010 (conditional on observables, it was almost nonexistent up to that point) and the gap is similar to the gap at firms with ESI by 2014 (the implementation date of the mandate).

Figure 2 confirms that it is not only gender that matters. The figure shows the relationship between individual medical expenditures and wages over time, after controlling for observables. It suggests that, at firms that did not offer ESI already, the relationship between wages and individual medical expenditures changes after 2010, while the relationship at firms that already offer ESI is stable.

In sum, Figures 1 and 2 highlight that it is some event that only impacted firms that did not offer ESI around 2010 or 2011 that is the causal force driving the effects seen in section 4. It is possible that event may not have been the announcement of the ACA's employer mandate. However, the patterns in the data and the concentration of changes in wages at firms affected by the ACA's mandate (and on exactly those workers who cost more to cover) suggests that separating the effect of the employer mandate from some other event that could be the cause of the observed patterns would be challenging.

Firm Size and Mandate Impact

The ACA's employer mandate affects only workers at firms that do not already offer ESI and have more than 50 full time employees. For that reason, the estimates presented in Table 4 repeat those in Table 3 but include only MEPS respondents who report that they work at a firm that does not offer ESI but has fewer than 50 employees. As the employer mandate should not affect these workers or their employers, the observed estimates should be similar to the "null" findings in panel B of Table 3. The estimates in column two of Table 4 suggest that is the case. In particular, the estimates in column 2 suggest the gap in hourly wages between males and females decreases by 74 cents per hour (significant at the 10% level) after the employer mandate is announced at firms with fewer than 50 employees who do not offer ESI. That effect is similar to the reduction in the wage gap of \$0.52 observed in column 2 in panel B of Table 3. Because the employer mandate had no direct effect on labor costs for these firms, the estimates cannot be viewed as causal. They are more likely to reflect broader trends in female and male wages during the time period. However, if the gender wage gap was generally getting smaller over this time period at firms not directly affected by the employer mandate, then the main estimates (panel A of Table 3) are biased toward zero.

In addition, Table 4 suggests there are significant associations between medical expenditures (column 3), race (columns 4 and 5), education (columns 6 and 7), and wages. However, these are not statistically different after 2010, again lending support to the claim that the estimates observed in panel A of Table 3 are causally related to the employer mandate.

Last, the fact that firm size is a choice presents a threat to identification: firms close to the 50 employee cut-off might downsize in response to the employer mandate. The estimates in panel A of Table 3 could be biased toward zero if firms that have lots of female employees downsize in order to avoid the mandate. However, the estimates are almost identical when workers at firms with fewer than 75 employees are excluded which suggests that firms downsizing in response to the mandate is not a significant source of bias.

Changes in Female Labor Supply

Cowan and Schwab consider if selection could be driving their findings. The concern they have is that females may value having ESI more than males. If so, their labor supply toward firms

Table 4. Falsification Tests (<50 Employees), MEPS 2006–2014 Data

	(1)	(2)	(3)	(4)	(5)	(4)	(5)	(6)	(7)
	Hourly Wage	Hourly Wage	Hourly Wage	Log Wage	Log Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage
Employer mandate	0.0687	2.138	1.988	0.0879	0.0755	−0.00994	0.288	−0.645*	2.248
	(0.416)	(4.780)	(4.772)	(0.296)	(0.296)	(0.585)	(4.917)	(0.361)	(4.847)
Male	2.473***	2.798***	3.007***	0.171***	0.184***		2.136***		2.532***
	(0.275)	(0.310)	(0.318)	(0.0158)	(0.0164)		(0.201)		(0.208)
EM × Male	−0.646*	−0.743*	−0.816**	−0.0257	−0.0293				
	(0.367)	(0.402)	(0.412)	(0.0215)	(0.0222)				
Log annual med. exp.			0.0772***		0.00465***				
			(0.0251)		(0.00134)				
EM × med. exp.			−0.0228		−0.00108				
			(0.0320)		(0.00184)				
Caucasian						0.977**	0.324		
						(0.396)	(0.374)		
EM × caucasian						−0.227	−0.330		
						(0.506)	(0.488)		
College educated								4.524***	4.312***
								(0.342)	(0.327)
EM × college educated								−0.699	−0.405
								(0.444)	(0.420)
Observations	9153	9077	9077	9077	9077	8328	8266	9153	9153
Demographic controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Industry fixed effect	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Firm size	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Region fixed effect	No	Yes	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. See notes for Table 3. EM: employer mandate.
***$p < 0.01$.
**$p < 0.05$.
*$p < 0.1$.

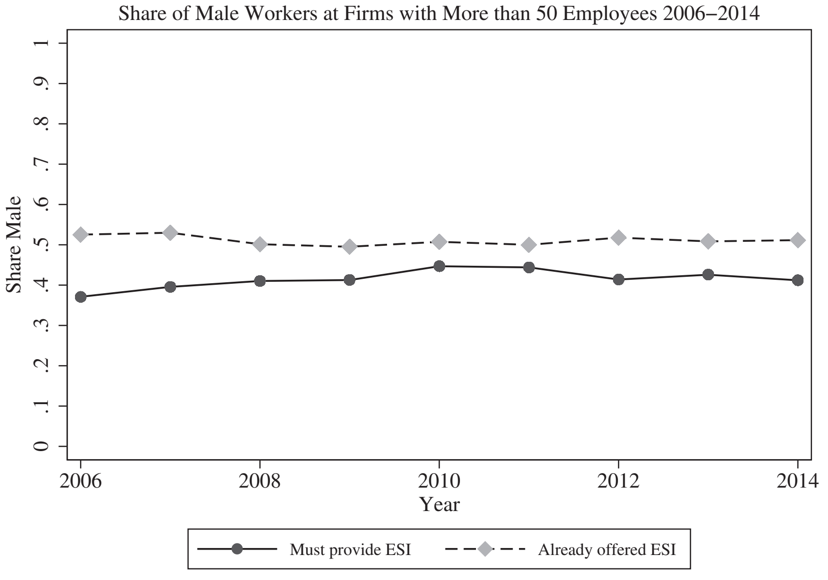

Figure 3. Share of Male Workers by Year.

that offer ESI will be relatively higher than males. If that is the case, it means females pay for their coverage via a labor supply response rather than labor demand. The same would be true in this article if females increased their relative supply of labor to affected firms in advance of the mandate's implementation. However, changes in labor supply do not undermine the main takeaway. In either case, females earn less due to ESI.

For several reasons, it is unlikely that the effects seen in this article are due to changes in labor supply. One reason is an asymmetry between the knowledge of employers and their employees. The employer is likely to know if they will be subject to the mandate: will they have 50 full-time employees or not after implementation, can they move toward more part-time workers to avoid providing coverage, can they increase their reliance on capital rather than labor to reduce their exposure, what will coverage cost the firm, how much will contributions be from employees, and so on. In addition, many individuals who work at firms without coverage would qualify for heavily subsidized coverage on the individual exchanges.[25] As a result, the chance of obtaining employer-based coverage may not be a sufficient incentive to alter a worker's relative labor supply.

These claims are supported by the data which shows little change in the proportion of males and females working at firms affected by the mandate after 2010. Figure 3 shows the share of male workers over time at firms with 50 employees or more by ESI. Firms that do not have ESI already in place for employees are denoted as "Must Provide ESI." There is little change in the share of male workers at these firms: if anything it is higher after 2010 than before. If females were increasing labor supply toward these firms, the share of males should decline.

Figure 4 shows the employment tenure of workers over time at firms affected by the employer mandate. Identification would be threatened if female workers' tenure patterns were very different to male workers or if they changed after 2010. The figure shows employment tenure is a little noisy (there are only a few hundred observations each year) particularly for males. For females, tenure

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

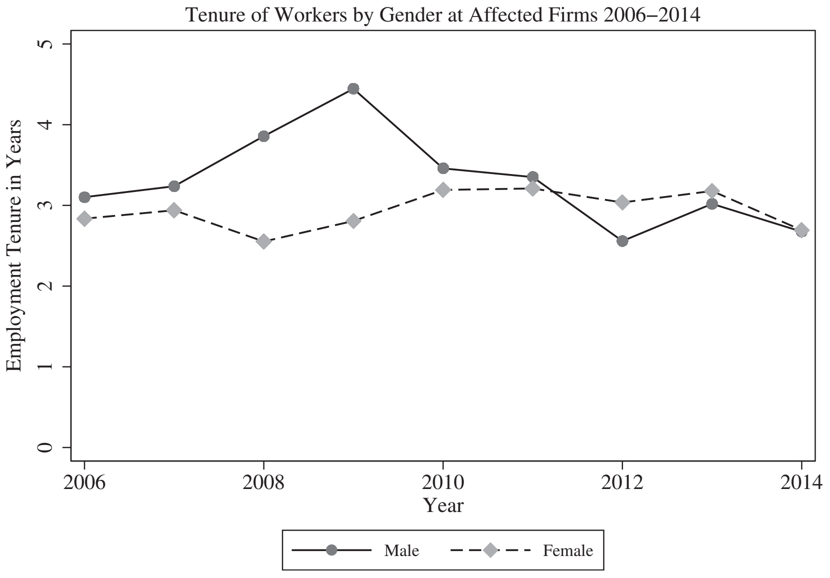

Figure 4. Employment Tenure of Workers by Year.

patterns are little different after 2010. Given the patterns in Figures 3 and 4 it is unlikely that the article's findings are due to changes in female labor supply.

6. Conclusions

This article builds upon work by Cowan and Schwab (2016), which suggests female wages are lower because females disproportionately add to the cost of ESI. First, the article shows that Cowan and Schwab's identification strategy may have led them to uncover a mechanical relationship between ESI and wages that is caused by other firm characteristics. The article then tests for such a mechanical relationship by replicating their approach using other groups that have well-documented wage gaps but differences in medical expenditures which should reduce rather than exacerbate the wage gap when a firm offers ESI. Examples include blacks versus whites and having a college education or not. Estimates using these alternate groups show that the presence of ESI increases the gap in wages between groups when it should have no or the opposite effect.

As females should, in theory, be paid less than males in the presence of ESI, this article then tests that theory using an alternate source of identification: the ACA's employer mandate. The exogenous nature of the mandate makes it ideal for studying the question at hand and indeed, there appears to be a relationship between the gender wage gap and ESI. However, the article then shows that the effect fades in size and significance once controls for individual medical expenditures are introduced. That is, males and females with lower medical expenditures should expect a smaller wage offset due to ESI.

Robustness checks show that the gender wage gap increases only at firms with more than 50 employees that did not already offer ESI to their employees after 2010.[26] These are precisely the

[26] It is tempting to try to use the 50 employee cut off as an alternate source of variation in an RD-style empirical strategy. The available data precludes such an approach. Firm size is not reported directly and is not immutable.

firms that are affected by the announcement of the employer mandate and its implementation in 2014. The timing of the observed effects and the fact that the resulting wage gaps appear to converge toward the gaps at firms that already offered ESI suggest the ACA's mandate is what is driving the observed effects. Further checks show that different anticipatory responses in labor supply or changes in tenure patterns are not causing these findings.

As a caveat, the effect of the employer mandate on wages could increase once the mandate is fully implemented and actual medical expenditures become known. However, as suggested earlier, cleanly identifying wage offsets caused by ESI after 2014 will be challenging. Any effort to identify those effects would need to account for anticipatory effects and separate the effect of the employer mandate from other ACA provisions.

In summary, ESI is likely associated with observable and unobservable firm characteristics that affect wages and productivity. This ensures identification of ESI's effect on wages is challenging. The employer mandate resolves most of these issues (but perhaps has issues of its own). However, using the mandate for identification shows that requiring ESI causes relative wages to fall for groups that are more costly to cover and vice-versa. However, the findings are weaker once controls for individual medical expenditures are introduced. This suggests that ESI potentially provides coverage to individuals at the "right" price: lower wages for those with larger expenditures and higher wages for those who do not cost employers as much to cover, regardless of gender.

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