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

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Abstract

Females tend to have higher medical expenditures than males of the same age. As a result, some of the observed gap in wages between males and females in the United States could be a compensating differential for the cost of employer-sponsored health insurance (ESI). This paper first shows that existing work on the role of ESI in the gender wage gap does not separately identify the effect of ESI from plausible alternatives. The paper then uses an improved source of identification to determine how much of the gender wage gap can be attributed to ESI. Findings suggest the portion of the gender wage gap attributable to ESI is smaller than prior estimates in the literature and is statistically no different to zero once individual medical expenses are included as a control. 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.

1 Introduction

This paper asks if differences in medical expenditures contribute to the gap between male and female wages? The question arises because employer-sponsored health insurance (ESI) is experience rated. Experience rating means that insurance premiums for firms are a function of employees' actual medical expenditures. This creates a cost wedge between males and females because females tend to have higher medical expenditures and employee contributions towards ESI cannot legally vary by gender.[1] At the margin, this means that firms who offer ESI will

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[1]The Health Insurance Portability and Accountability Act (HIPAA) forbids discriminatory distinctions in benefit generosity and employee contributions. HIPAA also makes it illegal to assess employee health insurance contributions

prefer to hire males unless female wages are free to adjust to compensate for how their health care expenditures contribute to insurance costs for the firm.

The cost wedge is not trivial. Cylus et al. (2011) found that per capita annual health care spending in 2004 was 32 percent more for females than for males. Bertakis et al. (2000), Woolhandler and Himmelstein (2007), and Bertakis and Azari (2010) also show female spending is higher. The same pattern is evident in the Agency for Healthcare Research and Quality's Medical Expenditure Panel Survey (MEPS). For those aged 18-64 and employed, annual medical expenditures were $3,388 for females and $2,272 for males (using 2006-2014 data) in 2014 dollars.

Cowan and Schwab (2016) ask if the difference in expenditures could be part of the gender wage gap. Using NLSY and MEPS data they find that the gap between male and female wages at firms that offer ESI is wider than at firms who do not offer ESI. They take this as evidence that ESI contributes to the gender wage gap. However, their empirical strategy is not ideal.

Firstly, there could be differences between firms that do and do not offer ESI that magnify wage differences between different groups. One example is firm size (number of employees). In their estimates, Cowan and Schwab control for firm size but do not do so differentially by gender.[2] 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 bigger firms.[3] As ESI is more common in large firms, Cowan and Schwab are potentially picking up a combination of the effect of ESI and firm size on wages.

Secondly, even if the effects on female wages were caused only by ESI, it is not clear if that reflects a causal relationship between gender and wages due to ESI or between individual medical expenditures and wages. Essentially, Cowan and Schwab's approach assumes that the characteristic firms are responding to is gender but it could also be medical expenditures at the individual level while producing the same empirical results. To separate the two, an empirical

based on claims experience, receipt of health care services, genetic information or disability. See https://www.shrm.org/resourcesandtools/tools-and-samples/hr-qa/pages/offeringdifferentbenefitsfordifferentemployees.aspx for more on this.

[2] At least that is what is described in the footnotes to Table 3 in their paper (pp. 108).

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

approach should consider if males and females who tend to have similar medical expenses face similar labor market outcomes.

In either case average female wages will be lower. However, one reflects a relationship between gender and wage outcomes that an individual female cannot affect while the other implies a potentially efficient system of wages and benefits tailored to the individual irrespective of gender. The difference between the two is subtle but important for understanding the policy implications of Cowan and Schwab's findings. Their approach implies that females who have lower 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 Cowan and Schwab's findings reflect market forces: males and females who tend to use more medical services are paid relatively lower wages.

To illustrate and resolve these issues, this paper first confirms Cowan and Schwab's main findings using recent MEPS data. Findings suggest that the gap in wages between men and women is largest at firms that offer ESI. The paper then asks if firm size also increases that gender wage gap, as a falsification test. The goal is not to identify how firm size contributes to the gender wage gap. Instead, this exercise only demonstrates that Cowan and Schwab's strategy may not be accounting for differences between firms who do and do not offer ESI.

The paper also tests the validity of Cowan and Schwab's approach by examining if wages at firms who do and do not offer ESI follow similar patterns for other groups who have differences in medical expenses. For example, MEPS respondents who identify themselves as black tend to have slightly lower medical expenditures but also lower wages than whites. If ESI causes the wage gap between males and females to widen, then ESI should narrow the black-white wage gap. As another example, the college wage premium should be smaller when a firm offers ESI because college graduates have higher medical expenditures than those who do not complete college. In both of these examples, the effect of ESI on wages goes against what Cowan and Schwab's approach would predict (see Section 4). This suggests that comparing wage gaps for

workers at firms who offer ESI to the same gap at firms that do not cannot identify the effect of ESI on the gender wage gap.

The paper then uses the ACA's employer mandate as an improved source of identification. Because the mandate requires all firms with more than 50 employees to offer coverage from 2014 onwards, it creates a natural experiment to test if females are paid less due to employers having to offer ESI. Relying on the employer mandate for identification means that the paper is focused on anticipatory effects. Firms were informed of the Act's mandate in early 2010 and theory would suggest 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.[4]

A forward-looking approach is appropriate in this instance because employment is an ongoing relationship and the employer mandate required coverage to be in place for all full time workers by 2014 (although that date was later relaxed).[5] Forward-looking firms could minimize the cost of compliance with the mandate because coverage costs for 2014 were to be based on the expected costs of each firm's employee pool in 2013.

A focus on anticipatory effects has the advantage of avoiding other ACA provisions which might affect labor market outcomes after 2014. The most obvious one would be the ACA's health insurance exchanges. These exchanges provide affordable coverage options outside of employment.[6] 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 (see Lennon, 2017 for more on this).

Importantly, relying on the employer mandate for identification avoids comparisons across firms who do and do not offer ESI by choice. Moreover, because the MEPS data contains individual medical expenditures, this paper's empirical approach can test if males and females who have similar medical expenses face similar labor market outcomes.

[4]Garrett and Kaestner (2015), Even and MacPherson (2015), and Mathur et al. (2016) also assume employers would be forward-looking. They consider how the ACA affected part-time employment because only workers who work more than 30 hours per week would have to be provided coverage.

[5]In the data used in this paper, more than 60% of workers at firms affected by the employer mandate had employment tenure of 2 years or more.

[6]The estimates presented in the paper include data from 2014 as the employer mandate was later delayed to 2015. Excluding or including 2014 does little to the results presented.

As a preview of the paper's main findings, the effects of the employer mandate appear to confirm that females are paid less due to ESI. Specifically, at firms that are likely to be affected by the ACA's employer mandate, estimates suggest male employees will earn $1.59 more per hour than a female counterpart due to ESI when controls for individual expenditures are not included. The effect is significant at the 1% level. However, these findings are not robust to controls for individual medical expenditures. In particular, the estimated effect of ESI on the 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. 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 2,000 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 (for example, $3,000 versus $1,500). This relatively small pass-through is perhaps not that surprising given medical expenditures (such as insurance premiums) paid by firms are tax deductible, some of the cost would be borne by the employee via cost sharing, and some firms might have lots of turnover giving them little incentive to prepare for the mandate. In addition, the estimates can reflect only anticipatory effects as firms affected by the employer mandate were not required to provide coverage until January 2014.[7] This dampens the effect individual medical expenditures could have (relative to group level differences) because some medical expenditures in that time period would be random and unpredictable. Medical expenditures associated with one-off events can provide no information on future medical expenditures for the individual, by definition. On the other hand, individuals who have expenditures that are ongoing and predictable might see a greater pass-through of medical expenditures. However, this paper does not attempt to examine these issues. That is, estimating the level and robustness of medical expenditure pass-through at the individual level is not a goal of this paper.

Instead, the paper has two clear goals. One, to show that identifying the effects of employment benefits on various groups using wage gaps across firms that offer ESI and do not offer ESI is not a valid empirical strategy. Two, to show that it is not at all clear that employers pay

[7] In February 2014, the employer mandate deadline was moved to 2015 for firms with more than 100 employees and to 2016 for firms with fewer than 100 employees.

certain groups lower wages, even when using an improved source of identification, because their medical expenditures tend to be higher than other groups. These are the paper's intended contributions.

However, the paper additionally suggests that individual medical expenditures are associated with lower wages, regardless of gender. That finding cannot be related to changes in productivity or reduced absenteeism due to improved health because the mandate does not include a policy change which will alter the current health status of MEPS respondents. The claim is not that firms are able to determine the expected medical expenditures of every individual precisely. An insurance company could not do that for individual customers, either. The idea here is only that (1) medical expenditures during the time period studied are at least a weak predictor of future medical expenditures and (2) employers can infer enough to determine which workers will tend to have higher medical expenditures even after considering gender, race, age, or other easily-observable characteristics.

As a caveat, it is likely that the effect of the employer mandate on wages will increase once ESI is in place and the expected costs of coverage become the actual medical expenditures of employees. However, it is not clear that these will be empirically detectable. Any effort to identify those effects would need to account for anticipatory effects and separate the full implementation of the employer mandate from the effect of other ACA provisions.[8] Indeed, the effect of gender on wages might then be statistically different from zero even after controlling for individual medical expenditures. However, given the other ACA provisions, it might not be possible to causally relate such a finding solely to ESI.

The paper proceeds with a review of the literature on employment benefits and their effects on labor market outcomes in Section 2. The value of this paper's approach and its alternative identification strategy are also laid out. Section 3 explains the data used in this paper and the estimation strategy used to produce the estimates in Section 4. The findings show existing work on this topic is likely picking up unobservable differences between firms that do and do not offer ESI and even if they were not, individual-specific effects rather than gender-specific effects

[8] Appropriate data to examine this issue will not be available until late in 2018. Even then, it will only contain one year of data for all firms affected by the mandate.

are potentially a more important source of wage differences when ESI is offered to employees. Section 5 examines the robustness of these findings. Section 6 concludes.

2 Literature and Background

Summers (1989) was the first to call for research into the empirical regularities of employment benefits. Summers was concerned that unless wages were free to adjust for the different cost of providing mandated benefits to different groups then mandated benefits would exclude certain workers from the labor market.

Authors such as Gruber (1993, 1994), Bhattacharya and Bundorf (2009), Cowan and Schwab (2011), Lahey (2012), Bailey (2013), and others answered Summers' call. They found that workers who are affected by mandated benefits face either negative effects on wages, employment, or both because the benefit makes these workers more costly to employ, all else equal.

Cowan and Schwab (2016) contribute by examining the role of ESI in the gender wage gap. They find that female workers, because they tend to have higher annual medical expenditures, experience relatively lower wages at firms that offer ESI. Their work makes a contribution to a body of research that attempts to explain the empirical gap in wages between men and women including Waldfogel (1998), Altonji and Blank (1999), Blau and Kahn (2000), Mulligan and Rubinstein (2008), Manning and Saidi (2010), Bertrand et al. (2010), and Goldin (2014).

Cowan and Schwab's work is particularly related to Daneshvary and Clauretie (2007) who also tried to tease out how the presence of health insurance could cause some portion of the gender wage gap. Daneshvary and Clauretie find little evidence that females receive lower wages due to ESI. However, their identification strategy relies on spousal job characteristics as an instrumental variable. As a result, their approach is highly sensitive to alternate specifications and has exogeneity concerns. Cowan and Schwab, using data from the National Longitudinal Survey of Youth and the Medical Expenditure Panel Survey, find that the gap between male and female wages is approximately \$0.50–\$1.50 per hour (in 2002 dollars) larger among workers with ESI than it is among workers without ESI.

Cowan and Schwab also find an intensive margin effect among firms that offer ESI: the male-female wage gap changes as coverage costs differ. For example, when a worker is part of a Health Maintenance Organization (HMO) insurance plan Cowan and Schwab find a smaller gap in wages between males and females. Health Maintenance Organization or HMOs are health insurance plans that control costs by coordinating care within a limited network accessed only via referrals from a primary care physician. However, firms who choose an HMO for their employees may not be doing so randomly. That is, Cowan and Schwab do not consider the endogenous reasons why firms choose an HMO nor if it may cause other selection effects.

While Cowan and Schwab's approach to identification seems valid, their conclusions explicitly rely on an assumption that selection into ESI is not a function of gender. They attempt to address this assumption by examining their results by marital status and the presence of children. However, if most people get married and have children at some point in their life, forward-looking agents should behave fairly similarly to those who are married and/or have children (particularly if job switching is not frictionless). There is also an implicit assumption that is not directly addressed by Cowan and Schwab. To understand this implicit assumption, first note (see Table 1) that wages at firms who offer ESI are higher for males and females than at firms who do not offer ESI. This is likely due to a combination of observable characteristics such as firm size and unobservable factors which affect productivity.[9] Cowan and Schwab implicitly assume that those effects do not differ by gender.

The literature on firm size and wages suggests a similarly able worker will earn less at a small firm. The reasons for this are examined by Oi and Idson (1999). For this paper, what matters is that workers with higher productivity appear to earn lower wages at smaller firms compared to larger firms. As ESI is highly correlated with firm size, the previous sentence could replace the words "smaller firms" and "larger firms" with "firms that do not offer ESI" and "firms that offer ESI."[10] Table 1 in the next section highlights the relationship between firm size, ESI, and wages.[11] For example, hourly wages for men who have ESI are $26.93 and just

[9] See Oi and Idson (1999) for an overview of the firm size wage premium. More recent evidence (Even and Macpherson, 2012) suggests the premium may be declining.

[10] See http://kff.org/other/state-indicator/firms-offering-coverage-by-size/.

[11] The summary statistics are in line with Cowan and Schwab's summary statistics using NLSY79 data.

$16.46 at those that do not have ESI. For men who do not have ESI, just 23% work at firms with more than 50 employees. In contrast, 55% of men who have ESI work at firms with more than 50 employees. What is problematic is that the average hourly wage at firms with 50 or fewer employees is $17.18 and $23.24 at firms with more than 50 employees. Cowan and Schwab do control for firm size. However, by not interacting firm size with an indicator for gender, they implicitly assume that firm size affects male and female employees equally. This may not be the case if the empirical gender wage gap is due to occupational choices, individual preferences for in-kind benefits, or unobserved productivity differences.

In particular, if any portion of the gender wage gap is due to productivity differences then the gap between male and female wages will tend to increase with firm size *regardless of ESI*. In other words, firm size will magnify the effect of any existing productivity differences between two groups. If those two groups also have differences in medical expenditures (which may also lead to further productivity differences), the effect of ESI on the wage gap between them becomes indistinguishable from the effect of firm size. The goal here is not to suggest that the effects Cowan and Schwab attribute to ESI are entirely due to firm size. It is only to note that there are potentially many differences across firms who do and do not offer ESI, firm size being just one of them. Specifically, this paper's empirical findings do not imply any causal relationship between firm size and the gender wage gap.

As there are potentially unobserved relationships between gender, wages, productivity, and selection into ESI (see Simon, 2001), the ideal source of identification is an exogenous change in the provision of ESI while holding other firm characteristics constant, including (but not limited to) firm size. 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. Passed in 2010, the Affordable Care Act requires firms with more than 50 full-time equivalent employees to provide health coverage from 2014 onwards to full time workers (those who work over 29 hours in a usual week).[12] While over 80% of workers who work at a firm with more than 50 employees were already offered ESI, many would receive ESI in the immediate

[12]The enforcement of this provision was 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.

future. If females are more costly to cover, then relative demand for male and female workers should change at firms who must provide cover due to the Act's mandate. First, this provides a clean source of identification to re-examine Cowan and Schwab's findings. Second, the MEPS data allow for an examination of how individual medical expenditures rather than just gender affect labor market outcomes. Conveniently for this paper, the ACA's mandate also creates two "control" groups who either (1) already receive coverage from their employer or (2) are not covered by the Act's provisions. The next section describes this data, along with the approach to identification and estimation.

3 Data and Estimation

This section first describes the data used in the paper and why that data was chosen. The section then considers the conceptual economic concepts that can be examined and the estimation framework required to do so.

3.1 Data and Sample Selection

The empirical analysis in this paper uses data from the 2006-2014 Medical Expenditure Panel Survey. The Agency for Healthcare Research and Quality describes the MEPS as "a set of large-scale surveys of families and individuals, their medical providers, and employers across the United States." [13] 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. However, only the two end-of-year interviews are relevant to many researchers as medical expenditures are reported annually. MEPS data is ideal for examining labor market outcomes as a function of medical expenditures because the survey collects detailed information on the specific medical expenditures of individuals (including out-of-pocket expenses and those covered by insurance).

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

Crucially for this paper, the MEPS data identifies workers who are placed in ACA "limbo" from 2011 to the end of 2014. That is, the MEPS data can be used to construct a sample which identifies only workers at firms with more than 50 employees who do not already have ESI. Firm size is constructed via the answers to two MEPS questions: (1) how many employees work at your work location and (2) how many locations does your employer have? This means it is not possible to determine the exact size of every firm each respondent works at. For example, for an individual who reports 30 employees at their location but also more than one firm location it is not clear if that firm would be covered by the employer mandate. There is no way to know if they have 50 full time employees or not. This makes it challenging to use the 50 employee cut off as an alternate source of variation in an RD-style approach.

It would be possible to pursue that approach by ignoring all MEPS respondents who work at firms with more than one location. However, that would examine only the employer mandate's effect at single-location firms. It would also reduce the sample size dramatically. It is not clear if those results would be informative. More problematic is the fact that it would not be a clean RD. Firm size is not immutable: firms close to the 50 employee cut-off might downsize in response to the ACA's mandates. Estimates in Section 4 are biased towards zero if firms downsize in order to avoid the employer mandate. However, the estimates are almost identical when workers at firms with fewer than 75 employees are excluded. That suggests that firm downsizing in response to the mandate is not a significant source of bias in this paper's findings. Relevant summary statistics for males and females at firms who do and do not offer coverage, are presented in Table 1.[14]

This paper's empirical findings mainly rely upon 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. As many crucial variables, such as health care expenditures, are reported as an annual figure, the analysis cannot meaningfully exploit more than the end-of-year interviews reported in the MEPS data. Pooling the sample and ignoring the panel nature of the data is indicated to be problematic by a Breusch-Pagan Lagrange multiplier test. Moreover, Hausman tests indicate a fixed rather than random effects estimation would be appropriate. However, the lack of variation over such a

[14]The data description in this section borrows liberally from Lennon (2017).

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

2006-2010	All	N=57,353	Males without ESI	N=13,609	Males with ESI	N=17,655	Females Without ESI	N=14,368	Females With ESI	N=15,673
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
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 Expenditures										
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	10016.05	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	10582.37	3579.12	7712.59	4359.62	8837.22

2011-2014	All	N=49,529	Males without ESI	N=12,792	Males with ESI	N=14,385	Females Without ESI	N=13,111	Females With ESI	N=12,836
	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	
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 Expenditures										
All	2802.57	9602.28	1648.62	7037.78	2731.69	10587.73	2725.64	9234.81	3986.53	10255.21
White	2940.09	9872.77	1700.40	7276.17	2895.40	10381.15	2917.31	10277.68	4303.35	10544.41
Black	2626.91	9916.06	1441.58	6162.28	2531.95	13732.09	2330.54	5948.46	3576.42	10447.68
High School (or less)	2060.11	8184.70	1225.80	5765.44	2494.48	11355.77	2003.04	7309.38	2943.66	7177.52
College (or more)	3359.82	10508.63	2225.17	8435.81	2881.37	10137.74	3383.39	10643.84	4430.92	11280.67

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 paper. 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.

short time period confounds a fixed effects panel-data approach to the analysis.[15] In particular, a fixed effects approach cannot be used to study the effect of gender as it is time invariant. Instead, the data is treated as a repeated cross-section by dropping the second interview with each respondent (repeating the analysis with just those observations produces similar findings but requires that one year of recent data be ignored as Panel 19 contains just one end-of-year interview at the time of writing).

Those under 27 are excluded from the empirical estimates because the ACA affected them via the dependent coverage mandate. Antwi et al. (2013), Depew (2015), Hahn and Yang (2016), and Goda et al. (2016) examine how the dependent mandate affected younger workers' labor supply.[16] Workers aged 60 and over are excluded because they could be expected to retire prior to or very shortly after the mandate's implementation.

The paper relies on an assumption that employers are forward-looking and therefore uses data from the ACA's "pre-implementation" period. This covers the years after the announcement of the employer mandate in 2010 but prior to its full implementation in 2015.[17] Highlighting the importance of a swift response to the law, firms were to be experience-rated based on their employee pool in 2013. As a result, changes to personnel in response to the ACA's requirements would need to be made by late 2012.

Firms could have ignored their responsibilities under the mandate but there would be significant financial penalties for doing so. In particular, firms who did not comply would have to pay an "Employer Shared Responsibility Payment" of $2,000 per employee (employers could exclude 30 full-time employees from the penalty calculation). Given a significant body of empirical evidence (see Section 2) suggests relative wages can be adjusted to pay for ESI (because

[15] Even variables such as age and college education are not significant determinants of wages in fixed effects estimations and non-varying attributes such as race and gender have to be dropped.

[16] The findings in the paper do not change substantially if the analysis is extended to include the relatively small number of individuals aged 26 or younger in the data.

[17] As Lennon (2017) notes the health insurance industry reacted swiftly and there is ample evidence that insurers had developed comprehensive reports advising firms of the Act's regulatory changes and how to prepare for them by mid-2011. A typical example is the Hudson Institute report for franchise owners dated September 2011 http://www.franchise.org/sites/default/files/ek-pdfs/html_page/The_Effects_of_PPACA_on_Franchising_Final_0.pdf. Accessed November 2016.

workers value the coverage) and the existing tax treatment of employee medical expenditures, it would make little sense not to comply with the mandate.[18]

A shorter implementation period might aid identification but this paper is not alone in using the pre-implementation period to study the ACA's effects. Garrett and Kaestner (2015), Mathur et al. (2016), and Even and MacPherson (2015) consider if the ACA caused an increase in part-time employment as the Act only mandates coverage to those who work full-time (more than 30 hours per week). Each of these authors relies on the same assumption that employers are forward-looking and will not wait until the mandate is in place to make changes to the composition of their workforce.

Complicating matters in the other direction, authors interested in the effects of the ACA on the labor market cannot simply wait until after all provisions are in place as (even if employers wait to react) confounding variation will be introduced by the ACA's individual health care exchanges. These exchanges opened in 2014 and render clean identification using the employer mandate difficult as they might affect incentives to participate in the labor market on both the intensive and extensive margins.[19]

3.2 Conceptual Framework

Economic theory predicts that workers, rather than employers, will bear the costs of ESI. Following Bhattacharya and Bundorf (2009) and Lennon (2017), in a competitive market where wages are the only form of compensation, the wage of worker i, w_i, will equal the value of her marginal product (MRP_i). If health insurance is provided as an employment benefit, a competitive labor market would require wages to be modified to account for the cost of coverage. If premiums are actuarially fair a worker with medical expenditures e_i adds premium p_{ik} to firm k's costs and an employer could pool all medical costs across their N employees so that wages for worker i at firm k are

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

[18] Wages for existing workers do not have to go down, simply increase at different rates.

[19] Data from 2014 is included in the results in this paper as (1) the employer mandate did not come into effect until 2015, (2) the health insurance exchanges were slow to sign up enrollees, and (3) few enrollees were full-time employees aged 27-59. Moreover, the findings are little different if 2014 is excluded from the analysis.

In this case, wages are equal to the value of 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. If $j = N$ then subgroups are individual workers. Generally, authors who study how health coverage affects wages have dismissed that possibility without evidence. Usually, their data forces them to do so.

In contrast, this paper uses data which includes individual medical expenditures and does not need to make the same restrictive assumption. Let each of the subgroups be denoted as n_j. For $i \in n_j$, then wages (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.

Many authors have found evidence of this kind of group-specific wage offset, including Gruber (1993), Sheiner (1999), Jensen and Morrisey (2001), Lahey (2012), and Bailey (2014). This paper complements their work by examining how the relationship between gender and wages changes for working adults aged 27-59 at firms affected by the employer mandate in the years after the mandate was announced. This approach improves on the existing literature because the employer mandate requires coverage exogenously at the federal level. As a result, it avoids comparisons across firms who do and do not choose to offer coverage.

3.3 Estimation

The empirical work in this paper follows Cowan and Schwab by focusing on a difference-in-difference approach to estimation. Intuitively, the idea is to first compare the difference in wages for males and females with ESI (or at certain firm sizes) to those without ESI to replicate Cowan and Schwab's analysis. The paper then shows that Cowan and Schwab's findings are replicable

but also that the findings might be explained by other forces. Then, the paper proposes and tests an alternate source of identification using the ACA's employer mandate. This alternate approach focuses only on those who are employed at firms affected by the employer mandate. If Cowan and Schwab are correct then demand for female workers should fall with subsequent effects on wages. The basic estimating equation is of the following form;

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

In the estimating equation, $LaborMarketOutcome_{it}$ stands for labor market outcomes of interest for person i at time t. The dependent variable will be hourly wages to replicate Cowan and Schwab's results but it could be any variable which responds to a change in the demand for labor such as hours worked, annual wages, or unemployment duration. The right hand side of the estimating equation controls for the economy-wide relationship between wages and gender ($Gender_{it}$). It also controls for the presence of ESI (ESI_{it}). The ESI term captures any changes which affect all workers equally at firms that offer ESI. The co-efficient on the interaction of these two terms in the estimating equation gives a measure of the effect of ESI on wages as a function of gender. 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.

The concern with Cowan and Schwab's approach becomes clearer by examining the basic estimating equation common to both this and their paper. While Cowan and Schwab control for economy-wide differences in wages caused by unobservable differences across genders (the reason for that gap is not crucial to the analysis) and also control for wages as a function of ESI, their interaction term is assumed to represent only the effect of ESI on wages by gender. However, if whatever is driving the gender wage gap (discrimination, unobservable productivity, and so on) is also a function of firm characteristics that are correlated with ESI then identification becomes clouded. If there is such a relationship, a difference-in-difference estimation of the effect of ESI and gender on wages will pick up a mechanical association that is not *caused* by ESI.

Cowan and Schwab do 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 use the presence of other fringe benefits (pp. 111) as a check on this theory. The benefits they examine include training programs, profit sharing, retirement plans, childcare, dental insurance, flexible work schedules, life insurance, and parental (not only maternity) leave. They claim these benefits are not generally costlier by gender so that there should be 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 don't is driving their results. However, there are several caveats to their approach.

Firstly, each of their regression estimates also controls for ESI. If ESI is generally also offered by firms who offer those other benefits, then it becomes difficult to separate the two effects. As an example, imagine 100 workers, 80 of whom work at a firm with ESI and 20 at a firm that does not. Of those with ESI, suppose 60 are also offered life insurance while none of the non-ESI firms offers other fringe benefits. Then, assume the gender wage gap is truly larger at firms who offer ESI (but perhaps due to other job characteristics). Lastly, suppose that at firms that offer ESI and life insurance the wage gap is the same as those that offer only ESI. Any difference-in-difference estimation that examines the effect on wages of life insurance interacted with gender would find a significant effect of life insurance on the gender wage gap if controls for ESI are dropped but not otherwise. If this example is typical of Cowan and Schwab's data then their approach is not a valid falsification test. An alternate approach would have been to ignore all those who had ESI. Then, for those who do not have ESI, examine if other fringe benefits are associated with a larger gender wage gap to ensure it is not other firm characteristics that drive the findings. However, there may be very few observations where ESI is not offered but other fringe benefits are offered. Of course, if that is the case, then the approach taken to falsification cannot do what Cowan and Schwab are asking it to do.

Secondly, if unobservable productivity differences drive Cowan and Schwab's findings, then the tax treatment of ESI compared to other benefits in the United States means finding no relationship between benefits such as retirement and life insurance and the gender wage gap reveals little. ESI has historically been a substitute for higher wages and is heavily subsidized by employers. Other employment benefits are both less costly and less subsidized (such as dental

insurance) or rare (such as childcare) and therefore their direct effect on wages could be hard to detect even if they are present.[20]

4 Empirical Findings

The first two columns in Table 2 are a replication of Cowan and Schwab's main findings both with and without demographic, region, and industry controls. The estimates use 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 Coverage × Male" shows males receive relatively higher wages when employed at firms who offer coverage. That is, as Cowan and Schwab found, females receive relatively lower wages when they work at a firm which offers ESI. Strictly speaking, Cowan and Schwab use "holds coverage" but the results change little using one or the other as take-up is 85-90%.[21] The results using "offered coverage" are presented here to align better with later estimates using the ACA's employer mandate for identification.[22]

In the second pair of estimates (columns three and four) the estimating equation is altered slightly. It substitutes a continuous measure of firm size (in 100s of employees) for the dummy for ESI. The third column shows an estimation without typical controls while the fourth column includes a complete set of controls. There, the term "Male × No. of Employees" shows that for every one hundred employees hourly wages increase $1.10 per hour for all workers and by an additional $0.15 for men. The trouble this finding causes is that it is hard to explain these findings without suggesting firm size and productivity are related. However, if that is the case, then we would not expect two workers with similar ability to have the same gap in their wages across firm sizes. Rounding out the problem, as ESI is typically offered at all larger firms it is not clear that ESI is the *cause* of Cowan and Schwab's findings (as replicated in the first two columns of the table). Again, note that this paper is not attempting to explain away Cowan and

[20] As mentioned earlier, Cowan and Schwab also consider if the gender gap between ESI and no-ESI firms could be caused by selection *into* ESI firms by females who might value coverage more (and are then willing to accept lower wages). They find little evidence for that theory but there are again certain caveats to their approach (see Section 2).

[21] See Table 1.

[22] As the mandate changes the requirement to *offer* coverage, that is the variable of interest.

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

	(1) Hourly Wages	(2) Hourly Wages	(3) Hourly Wages	(4) Hourly Wages	(5) Hourly Wages	(6) Hourly Wages	(7) Hourly Wages	(8) Hourly Wages
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 (in hundreds)			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
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size	No	Yes	No	Yes	No	Yes	No	Yes
Region Fixed Effect	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The demographic controls include gender, marital status, age (cubic), race, and education as appropriate given the co-efficient of interest in the specification. The firm size effect uses binary dummies for the firm size buckets presented in Table 1.

Schwab's findings via firm size. The paper is agnostic about the source of firm size-related wage differences. The point of the exercise is simply to show that holding ESI is not the only *different* determinant of wages workers who are studied in Cowan and Schwab's paper experience.

In addition to firm size, their overall hypothesis is testable by appealing to other groups of workers that have patterns in wages and medical expenditures that differ from the patterns of men and women. For example, white and black workers have a well-established wage gap but blacks tend to have similar or slightly lower medical expenses (see Altonji and Blank, 1999 for an overview). In Table 1, the summary statistics from the 2006-2014 MEPS data show white workers are paid more per hour than black workers but medical expenditures are slightly lower for blacks. In contrast, females are paid less than men but have higher medical expenses. This means that 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 and sixth columns of Table 2.[23] As can be seen, even though the prediction would be that firms with ESI should have a similar or perhaps slightly smaller black-white wage gap, the effect is the opposite. Moreover, the effect is larger in size than the gender effect seen in columns one and two. This suggests that firms with ESI are different in ways that magnify existing differences in wages between groups. That is, there is a larger gender wage gap at firms who offer ESI but it may not be because of ESI.

Another group with different wages and medical expenditures are college graduates and non-graduates. College graduates have both higher medical expenditures *and* higher wages (see Goldin and Katz, 2007 for an overview). This means that ESI should reduce the wage premium for college-educated employees if Cowan and Schwab's approach to identification is correct. However, if differences in wages are due to firm characteristics and productivity, then the estimates should be the opposite. In columns seven and eight, the estimates show that ESI is associated with a larger college wage premium. Again, this suggests Cowan and Schwab are picking up the fact that, at firms that offer ESI, differentially productive workers will see bigger gaps in their wages not because of ESI but because of other characteristics of firms that offer ESI.

While not presented here, the same basic analysis using any two groups that tend to have different wages (young versus old workers, married versus single workers, migrants versus non-migrants, and so on) finds that ESI is associated with larger wage differences between the two groups regardless of average medical expenditures across the groups.[24]

None of the preceding estimations directly imply that ESI has no effect on wages. They only highlight that other factors may be overwhelming the effect of ESI in existing empirical approaches. 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.

[23] The sample is restricted only to whites and blacks for those estimations.

[24] Cowan and Schwab find similar patterns of wage gaps for those who smoke and those who are obese which they find reassuring. The logic is that if medical spending causes wage offsets then variables correlated with health should be important. However, both may affect worker productivity directly and via absenteeism: If firms that offer ESI "allow" more productive workers to earn larger wages due to firm characteristics, then obese workers and smokers may not be able to take as much advantage of those characteristics as non-obese and non-smokers can.

Table 3 reports estimates of the ESI-related gender wage gap using the ACA's employer mandate for identification. The estimates presented use a similar difference-in-difference estimating equation as in Table 2. However, instead of comparing the gender wage gap at firms that do and do not offer ESI, it uses the ACA's mandate to examine how the wage gap between males and females changes after the ACA is announced for workers at firms affected by the mandate. The estimating equation is as follows;

$$LaborMarketOutcome_{it} = \beta_0 + \beta_1 ACA_{=1 \ if \ year>2010} + \beta_2 Gender_{it} + \beta_3 ACA \times Gender_{it} + \Pi X_{it} + \epsilon_{it}$$

In the estimating equation, ACA=1 for the years after 2010. All else is as described in Section 3. Panel A of Table 3 uses this estimaitng equation with data from MEPS that included only employees who are not already offered ESI but who report working at a firm with more than 50 employees - those in ACA "limbo." These are the main estimates of interest. If females are costlier to cover, then male wages at firms who must provide ESI will increase relative to females.

Panel B is the first falsification exercise. It focuses on those who work at a firm that offers ESI already. These estimates are presented to help ensure the effects seen in Panel A are causally related to the ACA's mandate. For firms that already offered ESI, the employer mandate does not change their incentives and therefore estimates should not show any ACA-related effects.

The estimates in the first two columns of Panel A show a positive relationship between male wages and the mandated coverage required by the ACA. The effect is statistically significant at the 5% level in the specification with a full set of controls in column two. Given they 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).[25] Column four shows similar effects on wages when using log wages as the dependent variable.

The estimates in columns six through nine provide confidence in the 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 race and college education was the opposite to what medical

[25] Using the CPI (www.bls.gov) to convert from 2002 to 2014, Cowan and Schwab's estimates would be \$0.66-\$1.97.

Table 3: Estimates of ESI-induced Gender Wage Gap, ACA Mandate, MEPS 2006-2014 Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A - No ESI	Hourly Wage	Hourly Wage	Hourly Wage	Log Wage	Log Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage
Affordable Care Act	-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)
ACA × Male	1.077	1.593**	1.175	0.0934**	0.0730*				
	(0.777)	(0.776)	(0.791)	(0.0399)	(0.0421)				
Log Medical Expenditure			0.191***		0.00915***				
			(0.0469)		(0.00261)				
ACA × Med. Exp.			-0.162***		-0.00797**				
			(0.0619)		(0.00369)				
Caucasian						1.851***	0.820		
						(0.595)	(0.578)		
ACA × Caucasian						-0.910	-1.072		
						(0.803)	(0.814)		
College Educated								7.217***	6.406***
								(0.629)	(0.614)
ACA × College Educated								-2.130***	-1.827**
								(0.820)	(0.787)
Observations	2,738	2,722	2,722	2,722	2,722	2,533	2,518	2,738	2,738
Panel B - ESI	Hourly Wage	Hourly Wage	Hourly Wage	Log Wage	Log Wage	Hourly Wage	Hourly Wage	Hourly Wage	Hourly Wage
Affordable Care Act	-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)
ACA × Male	-1.011**	-0.521	-0.550	-0.0105	-0.0111				
	(0.476)	(0.434)	(0.442)	(0.0152)	(0.0154)				
Log Medical Expenditure			0.197***		0.00892***				
			(0.0319)		(0.00118)				
ACA × Med. Exp.			-0.0104		7.81e-06				
			(0.0446)		(0.00168)				
Caucasian						4.642***	1.963***		
						(0.373)	(0.332)		
ACA × Caucasian						-0.586	-0.283		
						(0.535)	(0.487)		
College Educated								12.84***	11.63***
								(0.275)	(0.278)
ACA × 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
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Firm Size	No	Yes	No	Yes	No	Yes	No	Yes	
Region Fixed Effect	No	Yes	Yes	No	Yes	No	Yes	No	Yes

The estimates presented here use a similar estimating equation as in Table 2. However, instead of comparing the gender wage gap at firms that do and do not offer ESI, it uses the ACA's mandate to examine how the gender wage gap changes after the ACA is announced. Panel A focuses only on those who are not already offered ESI. These are the main estimates of interest. Panel B focuses on those who work at a firm that offers ESI already to ensure the effects seen in Panel A are because of the ACA's mandate. The employer mandate does not change the incentives for firms that already offer ESI and therefore the estimates should not show any ACA-related effects. All estimations include a year fixed effect to ensure overall economic conditions are not driving observed changes. A quadratic time trend produces similar results. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The demographic controls include controls for gender, marital status, age (cubic), race, and education as appropriate given the co-efficient of interest in the specification. The firm size effect uses binary dummies for the firm size buckets presented in Table 1.

expenditures would predict. Given Caucasians tend to have slightly higher medical expenditures the passage of the employer mandate should slightly lower the black-white wage gap at firms who must offer ESI in the near future. While not significantly different from zero the direction of the "ACA × Caucasian" term is as expected. The gap between medical expenditures is small so it is not surprising that its effect is not detectable in a sample of 2,500 or so. In contrast, the gap between medical expenditures for college and non-college graduates is quite large and the direction and size of the "ACA × College Educated" effect in columns eight and nine is reassuring. Note, again, that Cowan and Schwab's approach to the question of how ESI affects wages led to the opposite findings.

These estimates resolve concerns with the existing literature on this topic but do not address the important question of group versus individual effects. In column three of Panel A (and B) of Table 3, the same estimation as in column two is repeated with a control for medical expenditures at the individual level and its interaction with the passage of the ACA's mandate.[26] In the specification, the "ACA × Male" effect decreases in size and is no longer statistically significant when controls for individual medical expenditures are introduced. Column five presents the same estimation, with similar observed effects, using log hourly wages as the dependent variable. The estimates suggest that gender is acting 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.

What is interesting is that research on the effect of ESI has used differences in health status and/or medical expenditure across groups to identify wage effects. None of those papers, 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 all members of the group they study are affected equally. Instead, some of the results here suggest employers might form accurate expectations of individual costs (this could be based on absenteeism, physical characteristics visible at interview, and/or employee behavior). This

[26] Only observations from 2011 and onwards are considered post-ACA as many employment and salary decisions were already in place for 2010 before the ACA was announced.

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.[27]

Panel B is presented as an initial falsification test. The estimates in Panel B use data from employees who work at a firm that already offers ESI. For these firms, the employer mandate does not change their incentives and the estimates seen in Panel B reflect that. In column two (in log form in column four), a specification with a full set of typical controls, the effect of "ACA × Male" is negative but not statistically different from zero. Moreover, in column four, the interaction term suggests there are no changes in the effect of individual medical expenditures after the ACA is announced at firms who already offer coverage, as expected. Interestingly, the estimates seen in columns six through nine suggest that the ACA also affected the wages of college educated and Caucasian workers negatively - but not by as much - at firms who already offered ESI. This suggests that some but not all of the effects seen in columns six through nine Panel A reflect changes in the labor market that are unrelated to the ACA.

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 much better 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 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 robustness and 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.

[27] See Lennon (2017) for more discussion and examples of how employers may come to be aware of individual medical expenditures.

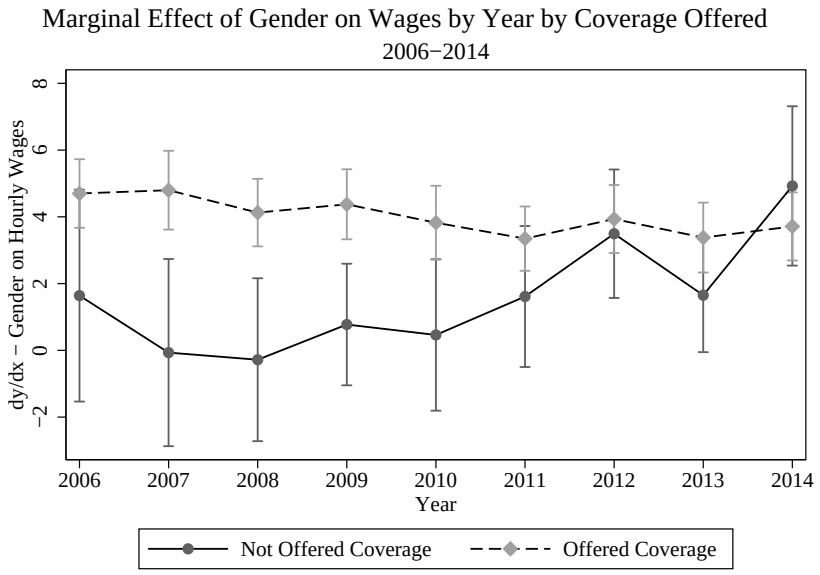

Figure 1: Post-estimation Marginal Effect of Gender on Wages by Year and by Offer of ESI

5 Robustness

This section focuses on ensuring the ACA's mandate, rather than labor market trends or other events, is responsible for the effects observed.

5.1 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 trends in the labor market that are unrelated to the ACA. Figures 1 and 2 present post-estimation 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, the relationships of interest are plotted by year. Figure 1 shows the additional wages earned by men at firms that offer ESI and do not offer ESI. In Figure 2 the effect of medical expenditures on wages is plotted by year at firms with ESI and without ESI. They both suggest

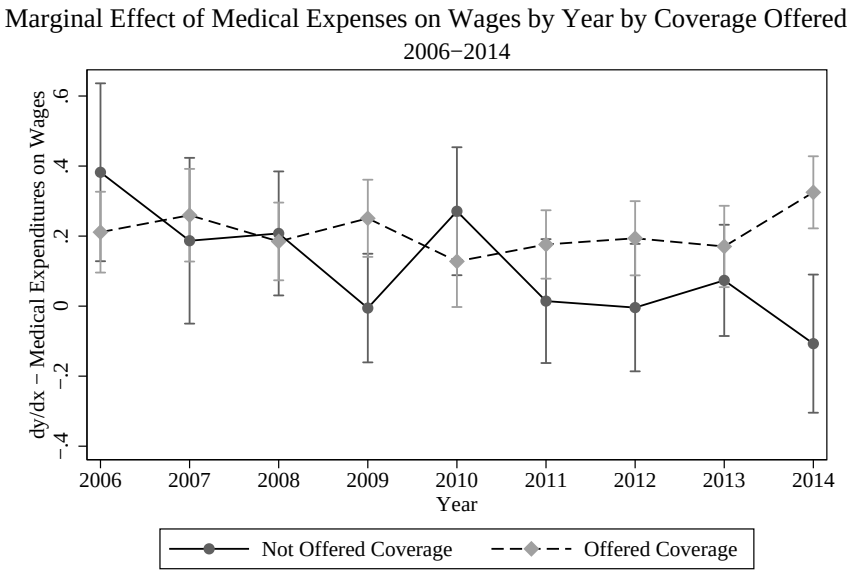

Figure 2: Post-estimation Marginal Effect of Medical Expenditures on Wages by Year and by Offer of ESI

firms who did not offer ESI experience changes in the relationships of interest after 2010. The same changes are not seen for firms who already offered ESI. This pattern eases concerns that the ACA is correlated with trends in the labor market which explain the paper's findings.

Specifically, in Figure 1 the dotted line shows that the gender wage gap was basically no different before and after 2010 for employees at firms who already offer ESI. This suggests that the ACA 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, it is therefore likely that there were also no other (unaccounted for) trends affecting the labor market that affected the gender wage gap.

In contrast, the solid line shows that at firms that do not offer ESI, the gender wage gap begins to converge to that of the firms with ESI as it approaches 2014 (the implementation date of the ACA's mandate). This is suggestive that the effect of the employer mandate was to make firms who did not offer coverage begin to treat their employees the same as firms who did offer ESI.

In sum, Figure 1 suggests that it is some event that only impacted firms who 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.

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 who already offer ESI is stable (the relationships differ markedly by 2014 after being relatively similar from 2006 to 2010). These figures help to be sure the ACA's changes are the source of the effects seen in Section 4.

5.2 Firm Size and Mandate Impact

The ACA's mandate applies only to firms with more than 50 full time employees. For that reason, the estimates presented in Table 4 repeat those in Table 3 but include only those who work at a firm with fewer than 50 employees who does not offer ESI. As the employer mandate does not apply to those firms the estimates should mimic the "no effect" findings in Panel B of Table 3 which focused on firms who already offer coverage (the ACA's mandate did not change their incentives).

Table 4 shows that the effects on wage gaps at firms with fewer than 50 employees are remarkably similar to Panel B of Table 3 across all specifications. They show no statistically significant changes in the relationship between gender, race, education, or medical expenditures and wages. This suggests that labor market outcomes after 2010 were not affected both for employees of firms who already offered ESI and for those who work at firms not affected by the ACA due to their small size (fewer than 50 employees).

This provides reassurance that the estimates in Tables 2 and 3 can be considered as the effects of the Affordable Care Act's employer mandate. Those tables show wages for males are higher than for females once firms affected by the ACA know they must provide ESI but no

Table 4: Falsification Tests of ACA's Effects (Firms with <50 employees), MEPS 2006-2014 Data

	(1) Hourly Wage	(2) Hourly Wage	(3) Hourly Wage	(4) Log Wage	(5) Log Wage	(6) Hourly Wage	(7) Hourly Wage	(8) Hourly Wage	(9) Hourly Wage
Affordable Care Act	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 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)
ACA × Male	-0.646*	-0.743*	-0.816**	-0.0257	-0.0293				
	(0.367)	(0.402)	(0.412)	(0.0215)	(0.0222)				
Log Medical Expenditure			0.0772***		0.00465***				
			(0.0251)		(0.00134)				
ACA × Med. Exp.			-0.0228		-0.00108				
			(0.0320)		(0.00184)				
Caucasian						0.977**	0.324		
						(0.396)	(0.374)		
ACA × Caucasian						-0.227	-0.330		
						(0.506)	(0.488)		
College Educated								4.524***	4.312***
								(0.342)	(0.327)
ACA × College Educated								-0.699	-0.405
								(0.444)	(0.420)
Observations	9,153	9,077	9,077	9,077	9,077	8,328	8,266	9,153	9,153
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

The estimates presented here repeat those in Panel A of Table 3. The difference is that the survey respondents represented in these estimates report working at a firm with fewer than 50 employees. The ACA's employer mandate does not apply to those firms and therefore the estimates should not show the same effects as those seen for employees at firms with more than 50 employees. All estimations include a year fixed effect to ensure overall economic conditions are not driving observed changes. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All dollar amounts were adjusted to 2014 dollars using the CPI (www.bls.gov). The demographic controls include controls for gender, marital status, age (cubic), race, and education as appropriate given the co-efficient of interest in the specification. The firm size effect uses binary dummies for the firm size buckets presented in Table 1.

similar effect at any firm who already offered coverage or was too small to be covered by the mandate. As the effects for other groups with different expenses and for firms who are not impacted by the ACA's mandate follow patterns we would expect to see (for example, there is a small but statistically insignificant fall in the black-white wage gap as a result of having to offer ESI in the near future, in line with the medical expenditure of those groups) this eases any concerns that the ACA's employer mandate is being afforded an importance that it does not deserve.

It is tempting to try to use the 50 employee cut off as an alternate source of variation in an RD-style empirical strategy. However, as mentioned in Section 3, the available data precludes such an approach. Firm size is not reported directly and is not immutable.

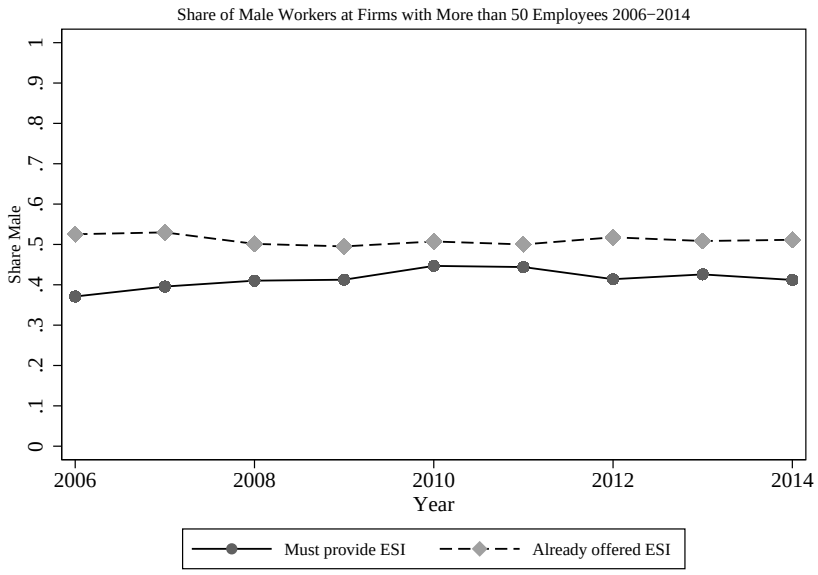

Figure 3: Share of Male Workers by Year

5.3 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 towards firms who 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 paper if females increased their relative supply of labor to firms affected by the ACA's mandate in advance of the mandate. 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 paper 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 towards more part-time workers to avoid providing coverage, can they increase their reliance on capital rather than labor in order 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

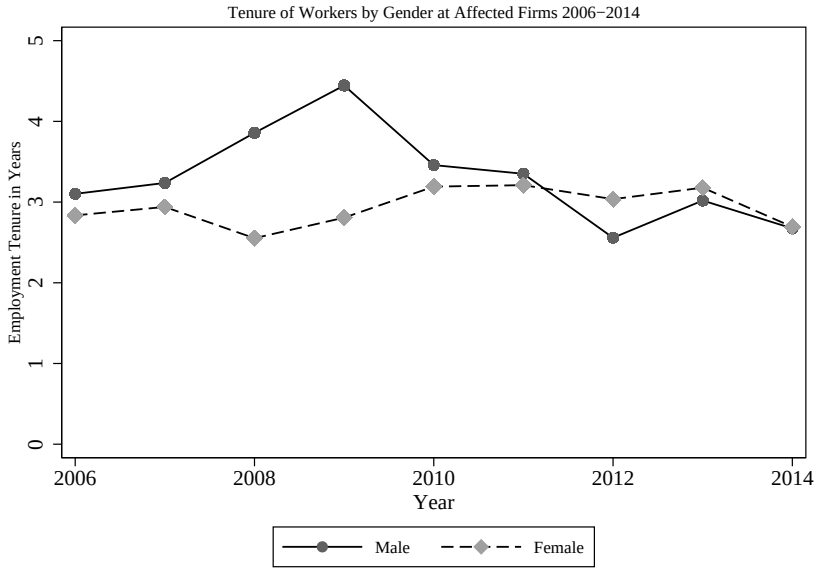

Figure 4: Employment Tenure of Workers Over Time

qualify for heavily-subsidized coverage on the individual exchanges.[28] As a result, the chance of obtaining employer-based coverage may not be a sufficient incentive to increase 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 who 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 towards these firms, the share of males should decline.

Figure 4 shows the employment tenure of workers over time at firms affected by the ACA 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 patterns are little different after 2010. Given the patterns in Figures 3 and 4 it is unlikely that the results seen in the paper are due to changes in female labor supply.

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

6 Conclusion

This paper builds upon work by Cowan and Schwab (2016) which suggests female wages are lower because females disproportionately add to the cost of employee health coverage. First, the paper shows that Cowan and Schwab's identification strategy is not ideal and may have led them to uncover a mechanical relationship between ESI and wages that is actually determined by other firm characteristics such as firm size.

The paper then tests for this mechanical relationship by repeating Cowan and Schwab's approach using other groups who have well-documented wage gaps but differences in medical expenditures which should reduce rather than exacerbate the 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. This means that Cowan and Schwab's findings are, at least in part, driven by a mechanical association between firm size, productivity, ESI, and wages.

As females should, in theory, be paid less than males in the presence of ESI, this paper then tests for the same effects 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 paper then shows that the effect fades in size and significance once controls for individual medical expenditures are introduced. That is, males and females with similar levels of expenditures can expect a similar wage offset due to ESI.

The robustness of these estimates is considered in Section 5. The section mainly focuses on whether or not the ACA can be considered a valid source of identification. The section illustrates that the relationships studied only change at firms with more than 50 employees that did not already offer ESI to their employees and only after 2010. These are precisely the firms who are affected by the ACA and precisely the time period the ACA impacts. Moreover, the firms affected by the mandate begin to treat employees like firms who already offered ESI. The timing

of the observed effects and the fact that they appear to converge towards the behavior of firms who already offered ESI suggest the ACA's mandate is what is driving the observed effects.

The figures in Section 5 consider how the gender wage gap and the relationship between wages and medical expenditures change on a year-by-year basis. This gives the reader a sense of the effect placebo tests on the Act's timing would uncover because they highlight the importance of the post-2010 (post-ACA) time period to the findings. As there is no evidence of other changes affecting the labor market the findings can be considered causal.

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 ACA's employer mandate resolves most of these issues (but perhaps has issues of its own, such as the multi-year pre-implementation period). Using the mandate for identification shows that requiring ESI causes relative wages to fall for groups who are more costly to cover and vice-versa. However, these findings are not robust to controlling for individual medical expenditures suggesting the effects of ESI on individual wages reflect market forces and incentives. The system of 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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