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

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Abstract

Theory suggests that the medical costs of obesity should be passed on to obese workers, in the form of lower wages, whenever health coverage is a part of employee compensation. In contrast to existing work on this topic, this paper illustrates that the medical expenditures caused by obesity among working adults are relatively small and that wage offsets should therefore be difficult to detect. The paper supports this claim by exploiting the variation provided by the Affordable Care Act's employer mandate. Findings suggest that obese workers tend to bear the approximate cost of their medical expenditures via lower wages. However, the observed effects are often insignificantly different from zero.

Keywords: Obesity, Wages, Employment-based Health Insurance

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

1 Introduction

Baum and Ford (2004) and Han et al. (2009) show that obese workers in the United States earn lower wages than non-obese workers. However, obesity could affect wages in at least three ways. First, obesity might reduce productivity. Second, obese workers may face discrimination.[1] Third, and the focus of this paper, wages should account for the additional cost of providing employer-sponsored health insurance (ESI) to obese workers.

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[1] These explanations and their consequences (for example, workers should internalize some of these effects and alter their choices about educational attainment and occupation) have received a lot of attention from researchers. Recent examples include Villar and Quintana-Domenque (2009), Johansson et al. (2009), Lindeboom et al. (2010), Han et al. (2011), Mosca (2013), Tafreschi (2015), Larose et al. (2016), Caliendo and Gershitz (2016), and Chu and Ohinmaa (2016).

The cost of ESI matters because ESI is experience rated at the firm level. Experience rating ensures that premiums reflect the medical expenditures of each firms' workers. As a result, ESI creates a cost-wedge between groups of workers with varying medical expenditures unless wages are free to adjust for the difference (Summers, 1989). For that reason, Bhattacharya and Bundorf (2009) and Bailey (2013) examine if obese workers' wages are lower due to the additional cost of providing ESI. Bhattacharya and Bundorf focus on wage differences between obese and non-obese workers at firms with and without ESI. Bailey asks if the cost of state-level mandates on diabetes coverage are passed on to obese workers. In both cases, the authors claim that obese workers experience significant wage offsets due to ESI. However, the wage offsets they observe for obese workers are unlikely to be caused by ESI for two reasons. First, the estimates reported are implausible: Bhattacharya and Bundorf's main estimates are several times what would be predicted based on cost. Second, the identification strategies employed suffer from a lack of exogenous variation.

This paper illustrates and attempts to resolve the issues in the existing literature by examining how the costs of obesity are distributed across obese and non-obese workers using an improved source of identification: the Affordable Care Act's (ACA) employer mandate. The mandate, announced in 2010, requires employers with 50 or more full-time equivalent (FTE) employees to provide ESI from 2014 onward.[2] Experience-rating ensures that the mandate makes some workers relatively more expensive to employ, such as obese workers. Given employment is an ongoing relationship, and that the cost of ESI was to be based on employee characteristics in 2013, theory would predict that the effects of the mandate on obese workers' wages would be apparent prior to the mandate's supposed implementation in 2014. This allows the paper to use data from the Medical Expenditure Panel Survey (MEPS) in a difference-in-difference framework to examine how the relationship between obesity and hourly wages changes for workers at affected employers after the mandate is announced.[3]

Estimates suggest that wages fall for obese workers relative to non-obese workers at employers affected by the employer mandate in the years leading up to its implementation. The observed

[2]See Even and MacPherson (2018) for a detailed explanation of the mandate. Note that the mandate was effectively delayed by 12 months by a decision to delay penalties for non-compliance. See https://www.nytimes.com/2014/02/11/us/politics/health-insurance-enforcement-delayed-again-for-some-employers.html. The delay was announced two months after the initial implementation date.

[3]This forward-looking approach is not unique to this paper (see Section 3).

effect can be considered causal is nothing else effects the wages of obese and non-obese workers differently in the sample period. Robustness checks, focused on wage changes for obese and non-obese workers who work for employers who already offer ESI (or are not covered by the employer mandate) provide support for a causal interpretation. Moreover, in contrast to the existing literature on the effect of ESI on obese workers' wages, the estimated effect on wages is well-aligned with the expected cost of obesity. However, given the ESI-related costs of obesity for an employer are quite small, plus MEPS sample size limitations, the estimated effects on wages are often not statistically different from zero.

The value of using the employer mandate as a source of identification is further demonstrated by examining how the mandate affects wage differences between other groups with differences in medical expenditures. In each case, changes in wages tend to reflect medical expenditure differences between the groups studied. Highlighting how the mandate improves identification, these estimates are contrasted with estimates produced using Bhattacharya and Bundorf's approach.

The remainder of the paper proceeds as follows. Section 2 examines the role of obesity in medical expenditures and the existing literature on how obesity affects wages via the cost of ESI. Section 3 lays out the paper's empirical strategy. Section 4 reports the paper's main findings. Section 5 considers the robustness of these findings and how they contrast to the existing literature. Section 6 concludes.

2 Obesity and ESI

Obese individuals are defined as those who have a body-mass index (BMI) greater than 30.[4] Obesity is itself a disease but medical expenditure data suggests obesity is associated with greater medical expenditures mainly because obese individuals are more likely to require expensive and ongoing treatment for conditions such as diabetes, hypertension, and high cholesterol. Table 1 illustrates this empirical regularity using a sub-sample of the Medical Expenditure Panel Survey. The sub-sample is chosen to represent the characteristics of workers who are affected by the employer mandate: adults aged 27-59 who work for employers with more than 50 employees. MEPS respondents who work for employers with fewer than 50 employees are excluded because the mandate exempted

[4]BMI formula: $703 \times weight/height^2$ when height and weight are in lbs and inches.

Table 1: **Medical Expenditures and Prevalence of Chronic Conditions by Obesity**

	Non-Obese (BMI<30)				Obese (BMI≥30)			
Selected Conditions	**Prevalence**	**Medical Expenditures**			**Prevalence**	**Medical Expenditures**		
		No	Yes	Difference		No	Yes	Difference
Diabetes	3.97%	$ 2,651	$ 5,969	$ 3,318	11.38%	$ 3,499	$ 6,290	$ 2,791
Hypertension	18.45%	$ 2,526	$ 3,922	$ 1,396	68.67%	$ 2,992	$ 5,123	$ 2,131
Arthritis	11.49%	$ 2,465	$ 5,236	$ 2,771	20.39%	$ 2,978	$ 7,088	$ 4,110
Myocardial Infarction	0.85%	$ 2,762	$ 5,222	$ 2,460	1.85%	$ 3,704	$ 9,783	$ 6,079
High Cholesterol	20.82%	$ 2,482	$ 3,930	$ 1,448	30.86%	$ 3,266	$ 5,050	$ 1,784
Coronary Heart Disease	1.1%	$ 2,714	$ 9,142	$ 6,428	2.34%	$ 3,686	$ 9,275	$ 5,589
Asthma	6.96%	$ 2,641	$ 4,680	$ 2,039	10.55%	$ 3,564	$ 5,956	$ 2,392
Any	44.62%	$ 1,823	$ 3,975	$ 2,152	64.17%	$ 1,885	$ 4,895	$ 3,010
Any - Males	45.77%	$ 1,217	$ 3,345	$ 2,128	62.87%	$ 1,273	$ 3,872	$ 2,599
Any - Females	43.35%	$ 2,460	$ 4,705	$ 2,245	65.51%	$2,558	$ 5,898	$ 2,340
Selected Demographics								
Age	42.5				43.4			
% Male	52.34%				50.54%			
Hourly Wages	$24.53				$21.99			
% College	64.43%				57.52%			
% White	67.75%				65.99%			
ESI Offered 2006-2010	87.4%				88.0%			
ESI Offered 2011-2014	86.2%				87.0%			
Annual Med. Expenditures								
All	$2,783				$3,816			
Males	$2,191				$2,907			
Females	$3,433				$4,746			
Offered ESI	$2,943				$4,043			
Not offered ESI	$1,724				$2,223			
Observations	29,655				14,597			

Note: The table reports annual medical expenditures and prevalence of chronic conditions from the Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59, who work for employers with more than 50 employees. The table excludes those for whom ESI status was not ascertained and those who reported medicaid coverage .

employers with fewer than 50 employees. Those aged 26 and under are excluded from the sample because the ACA's dependent coverage mandate altered younger workers' labor supply decisions.[5] Lastly, those aged 60 and over are excluded due to their proximity to retirement.[6]

While chronic conditions drive expenditure differences between obese and non-obese workers, obesity status is easily observed whereas hypertension and diabetes are not. For that reason, the

[5] See Antwi et al. (2013) and Depew (2015) for more.

[6] Pregnant females are also excluded due to the effects of pregnancy on BMI and labor market outcomes.

difference in medical expenditures between obese and non-obese workers is what should matter to employers if they wish to pass along the costs of ESI to their workers. In the MEPS sample used here, the difference in annual medical expenditure between obese and non-obese workers is $1,043 ($3,816 - $2,783) in 2014 dollars. Van Nuys et al. (2014), using 2004-2009 Thomson Reuters Commercial Claims and Encounters data, suggest that obesity is associated with $1,875 in additional annual medical expenditures.[7] On the other hand, Finkelstein et al. (2003), using 1996 MEPS and 1997 NHIS data, estimated that obesity is related to a $732 difference in annual medical expenditures.

However, the unconditional difference in expenditures is likely to be an upper bound on the ESI-related effect of obesity on wages. First, workers who report no chronic conditions have similar medical expenditures regardless of their BMI. Second, many non-obese workers experience the same chronic conditions as obese workers. That means obesity is not the only cause of these conditions and therefore is not the only source of medical expenditure differences between obese and non-obese workers. In addition, chronic conditions are correlated with age, gender, education, and race but age, gender, education, and race are also correlated with obesity status. In the sample used in this paper, obese workers tend to be older, are more likely to be female, less likely to have a college degree, and less likely to be white. Each of these characteristics is associated with differences in medical expenditures regardless of BMI. As a result, employers should rely on obesity status alone only if they cannot observe other characteristics of their employees. Further limiting how medical expenditures could affect wages, employee medical expenditures (including insurance premiums) are a tax deduction for employers at the marginal corporate tax rate (35% at the federal level during the period studied). Point-of-service cost-sharing would also reduce the cost burden for employers.

Given the complex relationship among expenditures, obesity, and observable characteristics other than obesity, along with the tax treatment of medical expenditures and cost sharing, obesity-related wage offsets should be difficult to detect. Instead, research on this topic claims that obese workers face significantly lower wages than their non-obese colleagues. For example, Bhattacharya and Bundorf (2009) examined the gap between obese and non-obese workers' wages at firms that do and do not offer ESI. Using 2002 NLSY data, they found that mildly obese workers (BMI between

[7] Author's calculation using the data presented in Table 1 of their paper.

30 and 35) appear to earn $1.27 less per hour than non-obese workers. For a full-time worker (2,000 hours per year), that amounts to a $2,540 annual wage offset (in 2002 dollars).[8] Bhattacharya and Bundorf report a wage offset for obese workers of $0.51 when using MEPS data from 2003. This consists of a $0.01 offset for obese males and $1.27 for obese females. It is true that the medical expenditure difference between obese and non-obese workers is largest for females. However, the gap is neither zero for males nor large enough to be the cause of a $1.27 per hour offset for females. Moreover, Bhattacharya and Bundorf's estimates refer only to workers aged 18 to 50. In the MEPS data in this paper, the expenditure difference between obese and non-obese workers aged 18 to 50 is just $619 per year ($449 for males and $705 for females). In other words, Bhattacharya and Bundorf estimate a $2,540 wage offset for females even though the difference based on cost should be no more than $705.

Even if Bhattacharya and Bundorf's estimates were perfectly aligned with the expected costs of obesity, Bailey (2013) notes that their empirical strategy is questionable because employers who offer ESI are different from those not offering it. For example, employers who offer ESI tend be larger, more established, and pay higher wages to all workers. Bailey's specific concern is that other differences between firms with and without ESI could cause the obese/non-obese wage gap to be larger at these firms even if there were no differences in medical expenditures (or no differential pass-through of ESI costs) between the two groups. To try to get around this issue, Bailey relies on the fact that diabetes is more common in obese individuals.[9] Bailey then uses NLSY79 data to compare obese workers' wages in states that did and did not pass diabetes coverage mandates. If obese workers tend to pay for the cost of their care, they should see a wage offset due to these diabetes mandates. Bailey finds a -3.3% effect on hourly wages for obese workers in states that have diabetes mandates.

While Bailey's strategy appears useful, the wage offsets he reports are several times larger than the upper bound of the expected cost of a diabetes mandate. The average hourly wage for workers with ESI (and therefore subject to any diabetes mandates that are passed) is $23.42 (in 2014 dollars) in the data used in this paper. Bailey's estimates would therefore imply a total wage

[8] For those who are morbidly obese (BMI≥35), the hourly offset was as large as $2.22 ($4,440 per year).
[9] See Table 1 earlier in the paper and Mokdad et al. (2003).

offset of $1,545.[10] However, the expected cost of a diabetes mandate is no more than $317 for obese workers in the data used in this paper ($2,791 × 11.38%, see Table 1). That is an upper bound because of cost-sharing at the point of service and the tax treatment of medical expenditures.[11] The expected cost is also lower if some employers already covered diabetes expenditures voluntarily or if they are exempt due to self-insurance.[12] Ultimately, Bailey's estimates suggest diabetes mandates cannot be the cause of the wage offset he observes.

The estimates reported by Bhattacharya and Bundorf (2009) and Bailey (2013) are better-aligned with the cost of obesity determined by Cawley and Meyerhoefer (2012). Cawley and Meyerhoefer note that obesity is endogenous and the right comparison group for determining the cost of obesity is not non-obese workers. Instead, it is an obese worker's fictional non-obese counterpart. Using the weight of a biological relative as an instrument for own weight, they find obesity has a large effect on annual medical expenditures ($2,741 in 2005 dollars). This has important implications for the public policy debate about obesity reduction interventions but it is not the relevant comparison employers and insurers care about. That is, theory suggests wages for obese and non-obese workers should reflect differences in medical expenditures between obese and non-obese workers. The fact that obese workers might have lower medical expenditures than non-obese workers, were they not obese, is not relevant.

3 Empirical Framework

Following Bhattacharya and Bundorf, in a competitive labor market where wages are the only form of compensation, the equilibrium wage of worker i, w_i, should equal the value of her marginal product (MRP_i). If health insurance is mandated as an employment benefit, a competitive labor market would require wages to be modified to account for the new cost of coverage. For simplicity, suppose that premiums are actuarially fair. Then, a worker with medical expenditures e_i will add premium p_{ik} to firm k's costs. In such a case, an employer could pool all medical costs across their

[10] Bailey uses data from every other year from 1990 to 2010 but does not report medical expenditures or wages from the time period studied. Using today's dollars favors Bailey's estimates as medical expenditures have generally risen faster than wages.

[11] Bailey calculates the expected cost as $611 but bases this on data provided by Bhattacharya and Bundorf about the total cost of medical expenditures for diabetics rather than the difference in cost between diabetics and non-diabetics.

[12] Self-insured employers are exempt from these kinds of mandates under the Employee Retirement Income Security Act of 1974. Essentially, self-insurance is not "insurance."

N employees so that wages for worker i at firm k are

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

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 employers. For that reason, the literature has supposed that a firm's N employees can be partitioned into $j \leq N$ subgroups.[13] Let each of the subgroups be denoted as n_j. For $i \in n_j$, then equilibrium 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.[14] 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). These authors typically use difference-in-difference approaches: examining how wages change for one group, relative to another, when ESI becomes more generous/expensive for one of the groups. The way the employer mandate (EM) impacts the labor market also lends itself to a difference-in-difference approach to estimation. The basic estimating equation is as below;

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

In the equation, $Hourly\ Wage_{it}$ is the hourly wage of person i at time t. The right hand side includes controls for the pre-existing relationship between wages and obesity using a dummy for

[13]If $j = N$ then subgroups are individual workers. Generally, authors who study how ESI affects wages have dismissed this possibility without evidence. For more details on the ability of employers to pass along health care costs at the individual level, see Lennon (2018b).

[14]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 paper.

obesity ($Obesity_{it}$) equal to one for those with BMI>30. The estimating equation includes a dummy term ($After\,EM_{it}$) which equals one after the employer mandate is announced and controls for labor market changes that affect all survey respondents after 2010. The coefficient of interest is related to the interaction of the two dummy terms. It provides a measure of the change in obese workers' wages in the period after the employer mandate's announcement. The coefficient has a causal interpretation if nothing else affects the wage difference between obese and non-obese workers without ESI during the sample period. The estimating equation is completed by allowing for a set of typical controls and fixed effects such as age, sex, education, marital status, race, location, and industry.

To produce the paper's main findings, the equation is estimated using MEPS data from 2006 to 2014 for workers who work for employers who have more than 50 employees but do not already offer ESI.[15] Employers must offer ESI to these workers from 2014 onward under the employer mandate or pay hefty financial penalties.[16] MEPS data is ideally suited to this issue because it is a nationally-representative rotating panel of U.S. individuals. Each respondent is part of MEPS for two years and MEPS reports information on each respondent's health and employment status. Importantly, it reports the number of employees where the individual works and whether ESI is offered or not. This allows the researcher to identify which respondents are working at firms who must provide coverage due to the employer mandate. According to the data, about 85 percent of respondents who work for employers with more than 50 workers were offered ESI by their employer. The paper therefore focuses on the remaining 15 percent or so of respondents in each year who work for employers that do not offer coverage but must do so because of the employer mandate.

As mentioned earlier, relying on the employer mandate for identification means that the paper is focused on anticipatory effects. A forward-looking approach is appropriate because employment is an ongoing relationship and employers had several years to prepare. Indeed, this paper is not the first to examine the anticipatory effects of the mandate: Garrett and Kaestner (2015), Mathur et al. (2016), and Even and MacPherson (2018) consider how its announcement affected part-time

[15] Data from 2014 is included in the estimates presented in the paper because the mandate was delayed to 2015 in February of 2014. Estimates are little-changed by excluding 2014. However, extending beyond 2014 moves into a period where identification becomes increasingly clouded by the myriad provisions of the ACA.

[16] Enforcement and penalties for non-compliance were delayed to 2015. Employers were unaware of this until February 2014.

employment. They focus on part time employment because the mandate applied only to workers who work more than 30 hours per week. Employers could therefore preemptively shift workers to part-time employment in order to avoid providing ESI. Even and Macpherson suggest that "700,000 additional workers without a college degree are in [involuntary part-time] employment as a result of the ACA employer mandate." On the other hand, Garret and Kaestner and Mathur et al. report a null result.[17] Note that if employers tended to shift obese workers to part-time employment (with no change in hourly wages) then this paper's estimates would be biased towards zero. In addition, focusing on anticipatory effects has the advantage of avoiding the confounding effects of other ACA provisions. The most obvious one 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 ESI-related job lock differentially for obese workers.

4 Main Estimates

Table 2 presents a series of estimates on how obesity affects medical expenditures and wages for MEPS respondents who work for employers affected by the employer mandate. All estimates presented use two year-end observations for each worker (when available) and cluster standard errors at the individual level.

In columns one through four, the dependent variable is medical expenditures. Obesity is associated with a $647.70 difference in medical expenditures in a regression with no demographic controls but only $546.60 of additional medical expenditures once demographic controls are included. In the third column, the effect of obesity is $285.90 but not significantly different from zero when the sample is restricted to just males. That means that the effect of obesity on medical expenditures is driven largely by female medical expenditures (as seen in the fourth column). The estimated effect of obesity of $827.30 is not trivial but the advantageous tax treatment of medical expenditures plus cost sharing at the point of service limits the amount employers would be exposed to.

[17] For more on this specific research question please see https://ldi.upenn.edu/brief/how-has-affordable-care-act-affected-work-and-wages.

Table 2: **Main Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Med. Exp.	Med. Exp.	Med. Exp. Males	Med. Exp. Females	Hr. Wage	Hr. Wage	Hr. Wage Males	Hr. Wage Females
Obese (BMI>30)	647.7***	546.6**	285.9	827.3***	-0.884**	-0.360	-0.124	-0.743*
	(240.7)	(248.9)	(418.5)	(289.2)	(0.385)	(0.319)	(0.524)	(0.388)
After EM					-0.869**	-1.619**	-0.760	-1.570**
					(0.346)	(0.632)	(0.804)	(0.786)
After EM × Obese					-0.151	-0.303	-0.0601	-0.352
					(0.509)	(0.444)	(0.752)	(0.520)
Observations	5,893	5,757	2,626	3,131	5,889	5,757	2,626	3,131
Mean Dep. Var. (in $)	$2,058	$2,058	$1,530	$2,510	$13.94	$13.94	$14.90	$13.12
Mean Dep. Var. (Obese)	$2,488	$2,488	$1,828	$2,996	$13.29	$13.29	$14.92	$12.04
Mean Dep. Var. (Non-Obese)	$1,840	$1,840	$1,390	$2,245	$14.27	$14.27	$14.89	$13.70
Number of Obese	1,986	1,986	865	1,121	1,986	1,986	865	1,121
Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry/Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Flexible Controls	No	No	No	No	No	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for full-time workers aged 27-59. Standard errors are clustered at the individual level. Demographic controls include controls for age (cubic), gender, race, education, and marital status. Where indicated, the specifications also control for location and industry fixed effects. Additionally, the controls for age, gender, and race are interacted with the employer mandate dummy in columns six through eight.

Columns five through eight of Table 2 report regression estimates of how the relationship between obesity and hourly wages changes for workers who work for an employer affected by the employer mandate. Column five presents a parsimonious estimation with no demographic controls. Column six adds a full set of typical controls and fixed effects for age, education, gender, marital status, race, industry, and location. Additionally, age, gender, and race are interacted with the employer mandate dummy in columns six through eight. These personal characteristics are correlated with obesity and also affect medical expenditures. It is therefore important to control for how they affect wages after the employer mandate.[18]

The estimates illustrate that wages tend to be lower for obese workers throughout the sample period, particularly for females. In addition, the negative wage effect for all workers after 2010,

[18]Note that the estimates do not interact all controls and fixed effects with the employer mandate dummy because wage changes by region (four categories), industry (fourteen categories), marital status, or education level (four categories) after 2010 are not likely to also vary by obesity status. This also reduces the potential for over-fitting given the size of the sample.

represented by the coefficient estimates on the "After EM" term, suggests that the employer mandate had bite. Specifically, and supporting the idea that the employer mandate caused the observed effects, the -\$1.57 offset (\$3,140 on an annual basis, \$1.57 × 2,000 hours) for females and -\$0.76 offset for males (\$1,520 on annual basis) align well with the annual medical expenditures of non-obese workers with ESI seen in Table 1. In contrast, estimates in Table B1 (in Appendix B) show that wages at firms who already offered ESI or were exempt from the mandate increased mildly over the same time period.

The coefficient on the interaction term, After EM × Obese, represents the additional wage offset for obese workers after the mandate was announced. In column six, the estimate of -0.303 suggests obesity is associated with a 30.3 cent fall in hourly wages relative to the 2006-2010 period. For an obese worker, this amounts to a \$606 annual wage offset (\$0.303 ×2,000). However, the effect is not statistically different from zero. The magnitude of the estimate is also more than the \$546.60 conditional difference in medical expenditures that is associated with obesity in column two. However, expenditure differences between obese and non-obese workers may be magnified after ESI is in place for these workers. Additionally, the estimates in columns three and four suggest that obese females are the primary source of expenditure differences between obese and non-obese workers. This means that, if the employer mandate is a valid source of identification, effects should be concentrated on female workers. The point estimates on the interaction term in the seventh (males only) and eighth (females only) columns confirm that prediction but these effects are noisy and contingent on specification.[19]

The estimates in Table 2 were recreated for two groups of MEPS respondents that were unaffected by the employer mandate. The first group is workers who work for employers that have fewer than 50 employees. The employer mandate did not apply to those workers and therefore wages should not change after 2010 at these firms. The second group is workers already offered ESI by their employer. For these workers, any ESI-related wage offsets should be reflected in wages before the employer mandate was announced. The estimates from those exercises are presented in

[19]For example, in estimates where age, race, and gender are not interacted with the employer mandate term, the estimated effect on obese female wages is larger than obese male wages. Upon examination, obese males tend to be older (1.2 years on average) and more likely to be white (3 percentage points) than non-obese males, whereas obese females are more likely to be black (10 percentage points) but no older than non-obese females. As a result, in estimates that do not interact these demographic characteristics with the employer mandate, the coefficient on the obese male term also reflects the effect of age and race-based differences in medical expenditures.

Table A1 in Appendix A. The estimates show that obese workers earn less than non-obese workers but there is no change in that difference after the employer mandate is announced. This suggests that the employer mandate, rather than broad labor market trends, is responsible for the effects observed in Table 2.

5 Validation and Robustness

To illustrate the value of using the employer mandate as a source of identification, Table 3 examines the effect of the employer mandate on wage gaps for five different groups. These groups include obese workers, males, white workers, college educated workers, and smokers. The goal of this exercise is to illustrate that the employer mandate caused wages for groups with larger medical expenditures to fall.[20] In Table 3, the estimated coefficient corresponding to ”After EM × Group Offset” is the coefficient of interest as it reflects the additional change in wages after the employer mandate is announced for that group.

The interaction term in each of the estimates shows that obese workers, females, whites, those with a college degree, and smokers suffer a wage penalty after 2010 at firms where coverage must be offered due to the employer mandate. On the other hand, the size of the wage offset for each group does not perfectly align with the medical expenditure differences among the groups. In particular, the estimated effect for white workers appears much too large to be due only to medical expenditure differences. However, the main effect of the employer mandate drops to essentially zero in that specification suggesting that black workers experience no wage offset due to the employer mandate. Therefore, the entire effect of the employer mandate, rather than only the difference in the effect across race, is loading onto the white interaction term. The estimates suggest that employers do not intend to offer ESI to black workers in the sample. Perhaps black workers in the sample are more likely to be seasonal or are working in jobs with significant employee turnover? Perhaps more of them they are working “off the books” or can be shifted to part-time once the mandate is implemented? This empirical finding is worthy of further examination but is beyond the scope of this paper.

[20]Table 3 reports the unconditional mean hourly wages and annual medical expenditures for each sub-group (males/females, smokers/non-smokers, and so on) below the corresponding estimates.

Table 3: After EM Wage Offsets for Various Groups using MEPS 2006-2014 Data

	(1)	(2)	(3)	(4)	(5)
Dep. Variable	Hourly Wages	Hourly Wages	Hourly Wages	Hourly Wages	Hourly Wages
Group=1	Obese	Male	White	College	Smoke
Group Wage Offset	-0.360	1.750***	1.361***	5.938***	-1.205***
	(0.319)	(0.357)	(0.369)	(0.424)	(0.324)
After EM	-1.619**	-1.725***	-0.0481	-1.377**	-2.021***
	(0.632)	(0.613)	(0.625)	(0.644)	(0.663)
After EM × Group Offset	-0.303	0.976**	-1.531***	-0.736	-0.355
	(0.444)	(0.484)	(0.515)	(0.560)	(0.455)
Observations	5,757	5,757	5,292	5,757	5,757
Hourly Wages					
Group=1	\$13.29	\$14.90	\$14.02	\$17.26	\$12.14
Group=0	\$14.27	\$13.12	\$12.86	\$11.69	\$14.34
Annual Medical Expenditures					
Group=1	\$2,488	\$1,530	\$2,267	\$2,532	\$2,046
Group=0	\$1,840	\$2,510	\$2,081	\$1,736	\$2,061

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. All estimates include controls for education, marital status, race, gender, a cubic in age, and location and industry fixed effects. In each specification, age, gender, and race are interacted with the employer mandate dummy term. Standard errors are clustered at the individual level. The specification in column three limits the sample to black and white workers.

The pattern of estimates in Table 3 can be compared to those in Table B1 in Appendix B. Those estimates are produced by mimicking Bhattacharya and Bundorf's empirical approach to examine the effect of ESI on wages for the same five groups. Bhattacharya and Bundorf's estimating equation takes the following form;

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

In their estimating equation, $Hourly\ wage_{it}$ refers to hourly wages for person i at time t. The right hand side controls for the general labor market relationship between wages and the group of interest ($Group_{it}$, for Bhattacharya and Bundorf that group is obese workers). A dummy for ESI (ESI_{it}) captures differences which affect all workers equally at firms that offer ESI. The coefficient on the interaction of these two terms measures the ESI-related effect of obesity on wages. This has a causal interpretation if the identifying assumption - that nothing affects the relative wages of the two groups of workers differently at employers with and without ESI - is satisfied. The estimating equation is completed by allowing for a set of typical demographic controls X_{it}.

If Bhattacharya and Bundorf's strategy is valid then, all else equal, the difference in the wage gap between groups of workers with and without ESI should depend only on the difference in medical expenditures between the groups. Instead, Table B1 shows that the wage offset associated with ESI, using the Bhattacharya and Bundorf approach, is either implausibly large or has the wrong sign in each specification.[21] As an example, Table B1 suggests that obese workers earn 76 cents less per hour than non-obese workers wherever ESI is offered. That is, the approach to identification taken by Bhattacharya and Bundorf, using this paper's sample, gives an estimated annual wage offset caused by ESI of $1,514 even though the medical expenditure difference between obese and non-obese workers with ESI in the sample is only $1,008. The sign of the effect supports their approach but the magnitude of the effect is implausible. The disparity suggests something other than ESI is affecting wage differences between obese and non-obese workers wherever ESI is offered. This means that Bhattachrya and Bundorf's approach ensures that the portion of the wage offset, if any, actually caused by ESI is unknown. In contrast, Table 3, using the employer mandate for identification, estimated a $606 wage offset ($-0.303 \times 2,000$ hours per year) while the annual medical expenditure difference between obese and non-obese workers affected by the employer mandate is $648 ($2,488 minus $1,840).

Moreover, the estimates in Table B1 are just examples; the same basic analysis using groups that tend to have different wages and medical expenditures (young versus old workers, married versus single workers, and so on) finds that ESI is associated with larger wage gaps between groups regardless of the medical expenditures of the two groups. That suggests that whatever causes a wage gap between groups of workers is magnified at firms that offer ESI, but potentially not because of ESI. Only an exogenous source of variation in ESI-provision can overcome the identification challenges that presents.

[21] Note that some of the estimates in Table B1 are similar to Cowan and Schwab (2011) and Cowan and Schwab (2016) who used Bhattacharya and Bundorf's approach to examine how ESI affects the gender wage gap and wages for smokers. Cowan and Schwab find and acknowledge a similar pattern in their work. However, they implicitly assume that the wage offsets they observe contain a full ESI-related wage offset. Therefore, only a portion of the observed offset is unexplained. Such a conclusion is not clearly supported by their estimates.

6 Conclusion

Theory suggests obese workers should bear the additional cost of their own medical expenditures in the form of lower wages. However, finding evidence of this relationship has proven difficult because obesity potentially affects wages in several ways. To get around these identification challenges Bhattacharya and Bundorf (2009) studied wage differences between obese and non-obese workers at firms that do and do not offer ESI while Bailey (2013) examined the effect of obesity on wages indirectly via diabetes coverage mandates. However, their empirical strategies are questionable and the effect sizes they report are implausible.

This paper has its own limitations but contributes to the literature by highlighting that the medical costs of obesity are small for working adults and any effect on wages due to ESI should be hard to detect. The paper supports this claim using the employer mandate as a source of identification. The costs of the mandate can be reduced by making adjustments prior to the mandate's implementation in 2014. In particular, employers should increase their relative willingess-to-pay for non-obese workers. This exogenous variation in demand for workers allows the researcher to examine how the relationship between obesity and wages changes after 2010 for workers who work for these employers in order to understand if and how the medical costs of obesity are passed on to workers. As expected, the ESI-related effects of obesity on wages are small and often statistically insignificant. However, the magnitude of the wage offsets align well with the expected costs of obesity.

Indeed, one of the paper's contributions is showing that the relationship between obesity and wages is more complex than previously considered by those who studied how ESI affects obese workers' wages. Obese workers differ on observables: they are older and more likely to be female but less likely to be white or college educated. Each of these characteristics would be expected to affect both wages and medical expenditures, sometimes in the same direction and sometimes in opposing directions. This creates several problems for empirical strategies which do not rely on sharp exogenous variation. The employer mandate resolves those problems because the mandate affected a specific group - obese and non-obese workers without ESI who work at employers who have more than 50 employees - allowing the paper to isolate how the medical costs of obesity affect wages via ESI. On the other hand, the way the employer mandate will interact with other components of the ACA limits the paper to an examination of anticipatory effects. The estimates presented should therefore be properly viewed as a lower bound. That is, the paper finds a lower wage offset due to obesity than the extant literature. Over time, the wage offset may become more pronounced. However, other provisions of the ACA ensure that it will not be possible to obtain clean identification once all of the ACA's provisions are in place.

Lastly, the findings in this paper could be explained by a labor supply response: obese workers might be more willing to work at employers affected by the employer mandate in anticipation of receiving ESI in the future. Even if this is the case, it means the ESI-related costs of obesity are borne by obese workers. However, anticipatory effects on labor supply due to the mandate are theoretically ambiguous. Any worker who chooses to work at an affected firm in anticipation of receiving coverage would need to be very well-informed of the ACA's requirements, their employer's future plans (especially in terms of firm size and growth), and the cost of the coverage that would be offered. Moreover, employers could charge employees up to 9.5% of their salary for coverage and still be compliant with the mandate. This means that an informed worker might have found working at a firm with fewer than 50 employees (even at a lower wage) and obtaining coverage via the ACA's insurance exchanges to be a preferable option. This would reduce their willingness to work at firms affected by the mandate. Easing any concerns, the relative share of obese workers and employment tenure patterns do not change at affected employers in the years after 2010 (see Appendix Figures A1 and A2).

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A Appendix

A.1 Control Group Estimates

Panel A in Table A1 focuses on MEPS respondents who work for employers who were not covered by the employer mandate.[A1] The table provides the difference-in-difference estimate (using the same approach used to produce the estimates presented in Table 2) for how the hourly wages of obese workers changed after the employer mandate for those employed at employers with fewer than 50 workers. Panel B presents the same estimates for MEPS respondents who work at a firm that already offers ESI (voluntarily, prior to the employer mandate requiring them to do so). In both panels, the estimates show obese workers' wages were lower across the sample period but that the gap did not change after the employer mandate. Notice that the coefficient on the "After Employer Mandate" term is positive (albeit, generally not statistically significantly different from zero) in all but one specification. In contrast, in Table 2 in the body of the paper, the corresponding coefficient was generally negative, in line with claim that the employer mandate reduced affected employers' labor demand (for all workers).

A.2 Anticipatory Changes in Labor Supply

Figure A1 shows the share of obese workers over time at firms with 50 employees or more by ESI status. Employers who do not have ESI in place are denoted as "Must Provide Coverage." There is no noticeable change in the share of obese workers who work for employers who are required to offer ESI. This suggests there are no changes in the labor supply patterns of obese workers towards employers who must offer ESI.

Figure A2 shows the employment tenure of obese and non-obese workers over time at employers affected by the employer mandate. Identification would be threatened if obese workers' tenure patterns were very different to non-obese workers or if they changed after 2010. The figure shows employment tenure for both obese and non-obese workers is a little noisy (there are only a few hundred observations each year). However, there does not appear to be any clear change in tenure patterns after 2010.

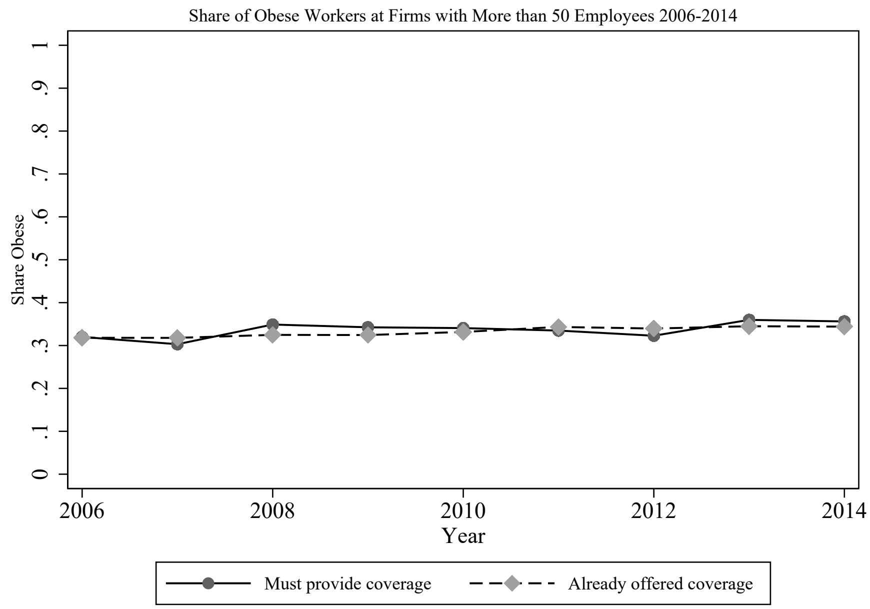
[A1] Note that those who report fewer than 50 employees but that their employer has more than one work location are excluded from these estimates due to the firm size ambiguity it creates.

Table A1: **Additional Estimates**

	(1)	(2)	(3)	(4)
	Hr. Wage	Hr. Wage	Hr. Wage	Hr. Wage
Panel A: Not Covered by Employer Mandate (< 50 FTEs)			Males Only	Females Only
Obese	-1.225***	-0.238	-0.0761	-0.600***
	(0.197)	(0.163)	(0.255)	(0.201)
After Employer Mandate	0.911***	0.464	0.387	0.0480
	(0.178)	(0.319)	(0.412)	(0.384)
After Employer Mandate × Obese	0.0153	0.0560	0.105	0.0699
	(0.287)	(0.236)	(0.369)	(0.292)
Observations	32,718	32,415	16,405	16,010
Mean Dep. Var. (in $)	$16.65	$16.65	$18.20	$15.06
Mean Dep. Var. (Obese)	$15.52	$15.52	$17.77	$13.97
Mean Dep. Var. (Non-Obese)	$17.03	$17.03	$18.39	$15.57
Number of Obese	10,110	10,110	4,921	5,189
Demographic Controls	No	Yes	Yes	Yes
Industry and Census Region FE	No	Yes	Yes	Yes
Panel B: ESI Voluntarily Offered to Worker				
Obese	-2.231***	-0.580***	-0.613***	-0.641***
	(0.177)	(0.145)	(0.211)	(0.195)
After Employer Mandate	1.390***	0.142	-0.221	0.376
	(0.168)	(0.327)	(0.433)	(0.415)
After Employer Mandate × Obese	-0.279	0.0398	0.168	-0.0854
	(0.263)	(0.215)	(0.317)	(0.287)
Observations	51,848	51,464	26,591	24,873
Mean Dep. Var. (in $)	$21.79	$21.79	$23.47	$19.99
Mean Dep. Var. (Obese)	$20.21	$20.21	$22.04	$18.38
Mean Dep. Var. (Non-Obese)	$22.56	$22.56	$24.14	$20.81
Number of Obese	16,951	16,951	8,513	8,438
Demographic Controls	No	Yes	Yes	Yes
Industry and Census Region FE	No	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. Wages adjusted to 2014 dollars using the CPI (www.bls.gov). Standard errors are clustered at the individual level. When indicated, regressions include controls/fixed effects for age (cubic), gender, location, race, industry category, education, and marital status. Race, age, and gender, when included, are further interacted with the employer mandate dummy to control for the relationship between obesity and other demographic characteristics. Panel A focuses on workers who work for employers with fewer than 50 workers. These employers are exempt from the employer mandate. Panel B focuses on workers who are already offered ESI, any wage offsets that are due to obesity status should be unaffected by the employer mandate.

Figure A1: Share of Obese Workers Over Time



The figure shows the share of obese workers over time at employers with 50 employees or more by insurance coverage status. Employers who do not have coverage in place for employees prior to 2014 are denoted as "Must Provide Coverage".

Figure A2: Employment Tenure of Workers Over Time

The figure shows the tenure of workers over time at employers with 50 employees or more by obesity status.

B Appendix

B.1 Comparison to Bhattacharya and Bundorf

The first column of estimates in Table B1 is a difference-in-difference estimate of the wage differences between obese and non-obese workers with and without ESI using this paper's 2006-2014 MEPS sample. It is therefore a "replication" of Bhattacharya and Bundorf (2009) main result albeit with a different data set and differences in specification mainly because of variables unavailable to the researcher (such as AFQT scores and granular location data).

As mentioned in the main text, the first column of estimates suggests that obese workers earn 76 cents less per hour than non-obese workers wherever ESI is offered. This gives an estimated annual wage offset of $1,514 even though the medical expenditure difference is only $1,008. This suggests that something else also affects wages differently for obese and non-obese workers and that Bhattachrya and Bundorf's approach ensures that the portion of the wage offset, if any, actually caused by ESI is unknown.

The second set of estimates presented in Table B1 examine the gender wage gap at employers that offer ESI. Because males have lower medical medical spending, Bhattacharya and Bundorf's approach would predict male wages would be relatively higher wherever ESI is offered. Again, the estimated effect is the right sign but the difference in the gender wage gap, $2,294 per year, is 1.7 times the medical expenditure difference between males and females.[B1]

Medical expenditure differences (presented towards the end of the table) suggest that ESI should reduce or at least not exacerbate any black/white, college/non-college, and smoker/non-smoker wage gaps. The estimates do not align well with such a prediction. Specifically, estimates suggest white workers earn $2.14 per hour more than black workers at employers with ESI even though black workers' annual medical expenditures tend to be lower, in the MEPS data, than white workers.[B2] College graduates have higher medical expenditures *and* tend to have higher wages.[B3] This means that ESI should reduce the wage "premium" for college-educated employees if Bhattacharya and Bundorf's approach to identification is correct. Instead, the estimates show the opposite: college graduates who are offered ESI earn $8,964 more per year (relative to non-college graduates with ESI) even though they tend to have medical expenditures that are $760 greater

[B1] The discussion in this section borrows from Lennon (2018a). Note that it is not possible, due the empirical strategies used, to employ the same MEPS sub-sample to produce the estimates in Tables 3 and B1.

[B2] As in Table 3, the sample is restricted only to whites and blacks for the estimations when examining the black-white wage gap.

[B3] See Goldin and Katz (2007) for an overview of the college wage premium.

Table B1: ESI-Related Wage Offsets for Various Groups using MEPS 2006-2014 Data

	(1)	(2)	(3)	(4)	(5)
Dep. Variable	Hourly Wages	Hourly Wages	Hourly Wages	Hourly Wages	Hourly Wages
Group=1	Obese	Male	White	College	Smoke
Group Wage Offset	-0.314	2.182***	-0.135	5.819***	-0.611**
	(0.261)	(0.288)	(0.322)	(0.307)	(0.274)
ESI	6.948***	6.162***	4.995***	5.176***	6.818***
	(0.209)	(0.205)	(0.299)	(0.165)	(0.189)
ESI × Group Offset	-0.757**	1.147***	2.138***	4.482***	-0.905***
	(0.303)	(0.322)	(0.353)	(0.345)	(0.337)
Observations	38,611	38,611	34,329	38,611	38,611
Hourly Wages					
Group=1	$21.17	$24.68	$23.18	$27.20	$18.99
Group=0	$23.75	$21.05	$19.88	$16.18	$23.55
Annual Med. Expenditures (workers with ESI)					
Group=1	$3,988	$2,662	$3,486	$2,835	$2,998
Group=0	$2,980	$4,010	$3,234	$3,595	$3,364

*** p<0.01, ** p<0.05, * p<0.1. Note: Estimates based on Medical Expenditure Panel Survey, 2006-2014, for workers aged 27-59. All estimates include controls for education, marital status, race, gender, industry, census region, and a cubic in age. Standard errors are clustered at the individual level. Controls include age (cubic), gender, location, race, industry, education, and marital status.

than their colleagues without a college education. On the other hand, the estimates presented in Table B1 are causal only if the identifying assumption holds: only ESI affects wage differences between the groups of workers examined. This assumption is questionable in the case of college graduates because MEPS does not contain a measure of ability (such as standardized test scores) and education is a choice variable. As a result, the effect of ability on education, productivity, and the probability of having a job that offers ESI is poorly accounted for.

For smokers (the final column of estimates), ESI is associated with lower wages of about $0.91 per hour ($1,810 per year) even though the observed medical expenditure difference in the sample is $366 in the opposite direction.[B4] The small difference between the medical expenditure differences of smokers and non-smokers is likely because not all else is equal: smokers and non-smokers are different in observable (and unobservable) ways that also affect medical expenditures.

[B4] It appears that smokers have become increasingly selected over time so that the medical expenditure gap has reversed relative to what Cowan and Schwab (2011) observed with 2000-2005 MEPS data.

The estimates in Table B1 are presented only to suggest that there are issues with the identification strategy used by Bhattacharya and Bundorf. It is worth noting that Bhattacharya and Bundorf support their approach to identification mainly by examining how wages in the NLSY79 respond to the presence of other employment benefits (such as dental insurance, life insurance, and training programs). The idea is that these benefits are not more costly by obesity status. As a result, it allows Bhattacharya and Bundorf to "determine if the results we find for health insurance are driven by omitted factors relating to worker productivity that affect the availability of all types of benefits." Unfortunately, MEPS data does not contain the same information on employment benefits to repeat that test.