Mandate-Based Health Reform and the Labor Market: Evidence from the Massachusetts Reform[*]

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Abstract

We model the labor market impact of the key provisions of the national and Massachusetts "mandate-based" health reforms: individual mandates, employer mandates, and subsidies. We characterize the compensating differential for employer-sponsored health insurance (ESHI) and the welfare impact of reform in terms of "sufficient statistics." We compare welfare under mandate-based reform to welfare in a counterfactual world where individuals do not value ESHI. Relying on the Massachusetts reform, we find that jobs with ESHI pay \$2,812 less annually, somewhat less than the cost of ESHI to employers. Accordingly, the deadweight loss of mandate-based health reform was approximately 8 percent of its potential size.

Keywords: Affordable Care Act, Massachusetts health reform, welfare effects

The Affordable Care Act (ACA) national health reform of 2010 and the Massachusetts state health reform of 2006 both focus on expanding health insurance coverage to near-universal levels. These "mandate-based" reforms rely on three key provisions to expand coverage: 1) a mandate that individuals obtain coverage or pay a penalty, 2) a mandate that employers offer coverage or pay a penalty, and 3) expansions in publicly subsidized coverage. While regulatory policy has long relied on mandates (for example, command and control regulation of technologies to reduce pollution), public policies that mandate that individuals purchase privately supplied goods have little precedent. Such mandates are sufficiently unprecedented, in fact, that uncertainty about whether the individual mandate was constitutional at the national level was not resolved until the Supreme Court upheld it in June 2012. Despite the resolution of legal questions around mandate-based policy, the question of economic efficiency remains.

To inform the application of mandate-based policy in health reform, as well as additional applications in other settings, we develop a simple model of mandate-based health reform. Our model incorporates the three key features of the national and Massachusetts health reforms. Using this model, we characterize the compensating differential for employee-sponsored health insurance (ESHI)—the causal change in wages associated with gaining ESHI—and we derive a set of sufficient statistics that capture the impact of the reforms on the labor market and on welfare. Although these sufficient statistics arise from potentially complex and difficult-to-measure structural parameters that determine individual health insurance and labor supply decisions, we can recover them from easily measured changes in labor market outcomes. Our model builds on the work of Summers (1989) who models a full-compliance employer mandate.[1] We apply the model to current policy by allowing for a pay-or-play employer mandate and adding a pay-or-play individual mandate and expansions in subsidized coverage. The interaction between the employer and individual mandates changes the predictions of the Summers model. The central result that an employer mandate reduces deadweight loss relative to a tax does not hold if there is already an individual mandate in place. This theoretical result is relevant for policy, given that as of this writing, the ACA employer mandate has not yet been enforced.

Based on the structure implied by our theory, we then estimate the relationship between ESHI and the labor market, allowing us to empirically assess the impact of health reform on welfare. Using variation induced by the Massachusetts health reform—which mirrors the national reform in all of the elements of our model—we estimate the empirical analog of our model. We first estimate the compensating differential for health insurance. Our empirical strategy relies on exogenous shifts into and out of ESHI induced by reform. Using longitudinal data from the Survey of Income and Program Participation on wages, employment, and hours worked, we study changes in labor market outcomes for individuals who switch to and from ESHI over the reform period. We incorporate individual fixed effects to control for time-invariant attributes that determine an individual's labor

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[1]The Summers model has been used subsequently by Gruber and Krueger (1991), Gruber (1994), Buchmueller, DiNardo, and Valetta (2011), and Anand (2011), among others, but it has not been used, to our knowledge, to examine implications of the ACA. Krueger and Kuziemko (2013) consider the implications of the ACA on coverage for the uninsured, Pohl (2011) considers the implications of the ACA for the labor supply of single mothers, and Heim and Lurie (2012) consider the implications of the Massachusetts reform for the self-employed.

market outcomes, and we incorporate variation between Massachusetts and other states to control for national trends. We also incorporate variation in firm size to allow some firms to be exempt from the employer mandate and to control for variation in the Massachusetts labor market that is unrelated to the reform. Combining all of these sources of variation and the reform allows us to obtain causal estimates of the compensating differential associated with health insurance.

Adding a small amount of structure to the estimated compensating differential for health insurance, we estimate the sufficient statistics that determine the welfare impact of health reform. Specifically, our model allows us to recover the cost of ESHI to employers, the underlying worker valuation of ESHI, the labor supply and demand elasticities, and the magnitude of the behavioral responses to the key policy provisions of the Massachusetts reform (the individual and employer mandates and subsidies). Once we demonstrate that these parameters are sufficient statistics for welfare analysis, we use our estimates to compute the deadweight loss associated with the mandate-based reform in Massachusetts. We also compare our estimated deadweight loss to the deadweight loss of a counterfactual tax-based insurance expansion that would involve levying a wage tax to pay for the provision of health insurance directly.

We find a compensating differential for ESHI that is of the expected theoretical sign though somewhat smaller in magnitude than the full cost of health insurance, suggesting moderately high average valuation of the benefit among the newly insured. Consistent with the large compensating differential, we find a small hours differential between jobs with and without ESHI, also suggesting moderately high average valuation of the benefit among the newly insured. Translating our estimated compensating and hours differentials into sufficient statistics for welfare analysis, we find that mandate-based reform is a relatively efficient way to expand coverage. We estimate that mandate-based coverage expansion in Massachusetts resulted in a deadweight loss due to distortion of the labor market that was only 7.7 percent of the distortion associated with instead providing health insurance through a tax on wages that workers do not link to receiving insurance. The relative efficiency of mandate-based reform follows from the high estimated valuation of the newly insured; because people were willing to work for ESHI as well as wages, the distortion to the labor market of mandating insurance was relatively small. We examine the robustness of our estimates to a variety of alternative specifications. Although our estimates vary, they always show that mandate-based reform is substantially more efficient than tax-based reform because our finding that individuals value ESHI is very robust.

Apart from our theoretical contributions, our findings contribute to the empirical literature on the incidence of fringe benefits, with health insurance as the largest of those benefits. Typically, the endogeneity of fringe benefits and labor market outcomes leads researchers to find wrong-signed compensating differentials for fringe benefits (see Gruber [2000] and Currie and Madrian [1999] for reviews); most studies find that individuals who receive more fringe benefits also receive higher wages. Existing studies that do not find wrong-signed compensating differentials for health insurance rely on *incremental* changes in the cost of health insurance, such as premium increases due to the addition of mandated maternity benefits (Gruber 1994) or increasing malpractice costs (Baicker and Chandra 2005). By using variation from the Massachusetts reform, we find a compensating

differential for the *full* cost of health insurance; individuals who receive ESHI receive wages that are lower by approximately the amount their employer spends on ESHI.

While our focus is squarely on the application of the model to health insurance and, specifically, the "mandate-based" reforms in the ACA and Massachusetts, our approach has applications more broadly in considering the value of fringe benefits. Our framework ties labor market outcomes to the market for fringe benefits as a function of a small set of parameters. We believe that our model has potential for future applications both in evaluating the roll out of the ACA—where each state will have different underlying conditions in the labor and insurance market—-as well as in other markets (e.g., evaluating changes in pension offerings). Our work also complements recent work by Aizawa and Fang (2013). Their model evaluates similar welfare questions on the interaction between health policy and the labor market but requires additional structure. Thus, our approach provides a complementary set of results that evaluate similar policy questions with fewer assumptions required to both identify the impacts of reform as well as to estimate welfare.

The paper proceeds as follows: Section 1 discusses the provisions of Massachusetts and national reforms that are likely to affect the labor market. Based on these provisions, Section 2 develops a theory of mandate-based health reform that we use to characterize the compensating differential for ESHI and the welfare impact of mandate-based health reform relative to tax-based health reform; Section 3 discusses identification and estimation of the model. Section 4 introduces the data. Section 5 presents results and discusses robustness, and Section 6 concludes.

1   Massachusetts Health Reform, the Affordable Care Act, and the Labor Market

The Massachusetts health reform, passed in April 2006, and the federal Patient Protection and Affordable Care Act (the ACA), passed in March 2010, contain a number of similar provisions that are likely to affect the labor market. We provide a side-by-side comparison in Appendix A. The cornerstone of both reforms is the individual mandate to purchase health insurance. Unlike traditional full-compliance mandates, the individual mandate in both reforms is a "pay-or-play" mandate that allows individuals to pay a penalty if they choose not to comply. The penalty in Massachusetts for those who were unable to demonstrate they had coverage when they filed their taxes was initially $219 per person per year, and it increased to 50 percent of the cost of the least generous ("Bronze") plan available in the Massachusetts health insurance exchange ("the Connector") in 2008.[2] The penalty associated with the ACA individual mandate, which will be phased in beginning in 2014, is the higher of $695 per uninsured member of the household (up to three) or 2.5 percent of household income. Compliance with the individual mandate in Massachusetts has been high—over 97 percent of tax filers submitted the tax form to comply with the individual mandate in 2008, and less than 2 percent reported any spell of uninsurance (Massachusetts Health Connector

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[2]According to the Massachusetts Connector website in 2010, in the zip code 02138 (Cambridge, MA), the cost of a Bronze plan for a family in Cambridge with two 40-year-old parents was $11,000 annually. For a couple with two individuals aged 35, the Bronze plan cost $6,600 annually. A 31-year-old purchasing a Bronze would expect to pay $2,868.

and Department of Revenue 2010).[3]

Second, both reforms include a pay-or-play employer mandate, which requires employers to offer health insurance or pay a penalty. The Massachusetts reform requires employers with 11 or more full-time employees to offer their workers the option to purchase health insurance coverage. Health coverage options must include a plan that allows employees to purchase health insurance using pre-tax wages, and employers must contribute at least 33 percent of the value of the premium or they will be assessed a penalty of $295 per employee per year. The ACA incorporates a similar pay-or-play employer mandate, but it defines large employers as those with 50 or more full-time employees (Kaiser Family Foundation 2010b). The ACA also requires that coverage options be affordable, such that the insurance offered pays at least 60 percent of covered expenses and the employee is not required to pay more than 9.5 percent of family income for individual coverage (Burkhauser, Lyons, and Simon 2011). If the employer does not offer coverage, the penalty is $2,000 per full-time employee, excluding the first 30 employees. If the employer does not offer options that meet the definition of affordable, and employees enroll in subsidized coverage through an exchange, the employer must pay a penalty of $3,000 per employee who obtains subsidized coverage, up to a maximum of $2,000 times the total number of employees minus 30 (Kaiser Family Foundation 2010a). Despite the relatively low penalty, compliance in Massachusetts has been high.[4]

The third cornerstone of both reforms is the expansion of subsidized coverage: fully subsidized coverage through Medicaid and partially subsidized coverage through new programs for low-income individuals who do not qualify for Medicaid. The Massachusetts reform expanded traditional Medicaid (MassHealth) and new fully-subsidized "CommCare" plans to those earning less than 150 percent of the federal poverty level (FPL), (150 percent of FPL for a family of three is $29,685 in 2014).[5] Individuals between 150 and 300 percent of the FPL can purchase CommCare plans with subsidies that decline with income. Similarly, the ACA expands Medicaid eligibility to all those with incomes below 133 percent of poverty.[6] The Supreme Court ruled that states could opt not to expand Medicaid eligibility under the ACA without losing existing Medicaid funding, and about half of states have chosen not to expand at this writing. The ACA also extends subsidized coverage

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[3]To satisfy the mandate, health insurance must meet or exceed a specific value (called "minimum creditable coverage"). See Kaiser Family Foundation (2009) and Raymond (2007). Individuals are automatically exempt from the individual mandate penalty in Massachusetts if they have a gap in creditable coverage of three months or less in a given calendar year, if they claim a religious exemption, or if their annual income is under 150 percent of the federal poverty level (effectively because the lowest cost Connector plan would be free for them). Other individuals can file for an exemption based on affordability using the Certificate of Exemption Application, which also provides details on the definition of "minimum creditable coverage." (The application is available at `https://www.mahealthconnector.org/portal/binary/com.epicentric.contentmanagement.servlet.ContentDeliveryServlet/FindInsurance/Individual/Affordability2520Calculator/2011CertificateofExemption.pdf`, accessed December 1, 2011.)

[4]Only approximately 4.6 percent of employers large enough to be subject to the penalty (12 percent of all Massachusetts employers) were required to pay it in 2010 (Massachusetts DHCFP 2011b). In addition, employers are subject to a separate "free rider surcharge" penalty if they do not offer a plan that allows employees to purchase health insurance using pre-tax wages and instead an employee receives care through the state's uncompensated care pool. The compliance cost for employers to avoid this penalty is minimal. Accordingly, zero employers were liable for the free rider surcharge in fiscal years 2008 and 2009 (Massachusetts DHCFP 2011a).

[5]In the 48 contiguous states and the District of Columbia, the 2014 poverty level is $11,670 for a single individual, and it grows by $4,060 for each additional family member (Federal Register 2014).

[6]Effectively, eligibility will be extended to 138 percent of poverty because there is a special deduction of income under 5 percent of poverty (Kaiser Family Foundation 2010b).

higher up the income distribution to 400 percent of poverty ($79,160 for a family of three). Even though the national reform extends subsidies to families with higher incomes, the Massachusetts subsidies (Commonwealth Connector 2011a,b) are more generous than the national subsidies (Kaiser Family Foundation 2010b) for almost all incomes and family sizes.

2 Model of Mandate-Based Health Reform and the Labor Market

2.1 The Model

We begin by considering labor demand. A representative firm sets wages to maximize profits, resulting in the following labor demand function:

$$L_D^t = L_D^{ESHI,t}(w+b)ESHI_t + L_D^{NoESHI,t}(w+\rho_t b)(1-ESHI_t).$$

Willingness to demand hours of work L in period t is a function $L_D^{ESHI,t}$ or $L_D^{NoESHI,t}$ of the monetary hourly wage w, and other arguments, depending on an indicator for whether the firm provides health insurance $ESHI_t$ at time t.[7] If the firm provides health insurance, labor demand depends solely on the cost of employing an individual in dollar terms—wages and the dollar cost to the employer of a standard health insurance benefit b. There are two periods: $Before$ and $After$. The employer mandate is not in place before reform, so $\rho_{Before} = 0$, but it is in place after reform, so $\rho_{After} = \rho$. If the firm does not provide health insurance, labor demand depends on the wage and the per-worker penalty $\rho_t b$ for not complying with the employer mandate in place in period t (since b is a fixed dollar amount, we express the employer penalty as a fraction of b instead of a fixed dollar amount without loss of generality).

Figure 1 depicts labor demand graphically. Before reform, if the firm provides health insurance, labor demand is lower by b.[8] After reform, if the firm does not provide health insurance, labor demand shifts down by the per-worker penalty for not complying with the employer mandate.

Next consider labor supply. A representative individual chooses how many hours to work to maximize utility, resulting in the following labor supply function:

$$L_S^t \;=\; L_S^{ESHI,t}(w+\alpha b+\lambda_t b-\mu_{xt}b)ESHI_t + L_S^{NoESHI,t}(w)(1-ESHI_t).$$

Willingness to supply hours of work L in period t is a function $L_S^{ESHI,t}$ or $L_S^{NoESHI,t}$ of the hourly wage w. For an individual with ESHI, given by the indicator $ESHI_t$, which is exogenous for now, labor supply is also a function of factors beyond the wage. As shown, it is a function of the cost to the

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[7]We develop the model relying on the simplifying assumption that we can measure L in hours of work including nonworkers with zero hours, ignoring the potential difference between the extensive and intensive margin of employment. When we estimate the model, we relax this assumption in a series of specification checks that allow us to compare the intensive and extensive margin impacts in Massachusetts. We also note that we do not measure L as the probability of employment because only employed workers can have ESHI. We do not measure L as the number of employees because the goal of our model is to make predictions about labor market outcomes for individual workers that we observe in multiple periods.

[8]As depicted, $L_D^{ESHI,t}$ and $L_D^{NoESHI,t}$ have the same linear functional form (the linear functional form is an approximation to a general nonlinear functional form).

Figure 1: Graphical Model
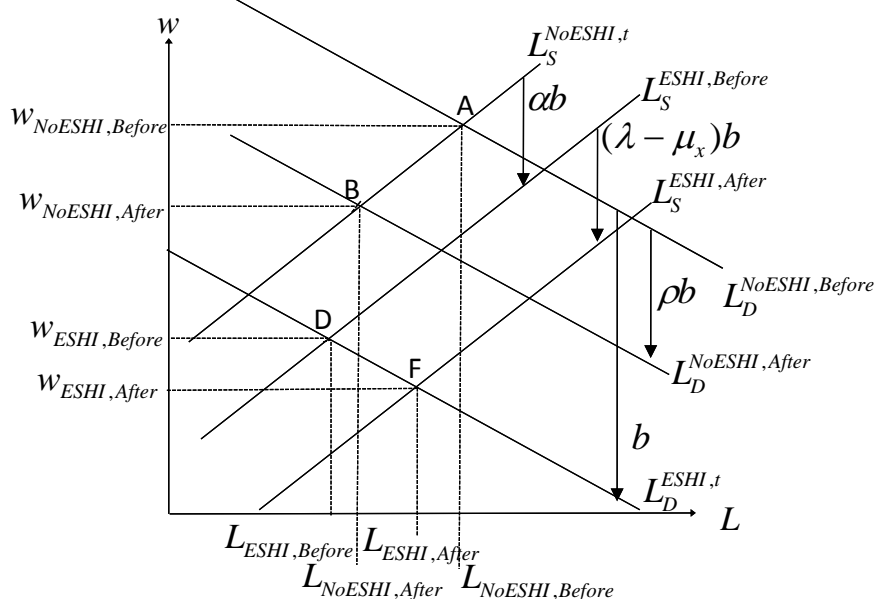
employer for a standard health insurance benefit b, scaled by the amount that an individual values a dollar of ESHI relative to a dollar of wages, α, and policy parameters in place at time t: the individual penalty for not having health insurance λ_t, and the subsidy μ_{xt} available on the individual health insurance market, which varies in generosity based on income group x. The individual mandate and the subsidies are not in place before reform, so $\rho_{Before} = \lambda_{Before} = \mu_{x,Before} = 0$, but they are in place after reform, so $\lambda_{After} = \lambda$, and $\mu_{x,After} = \mu_x$.

Figure 1 depicts labor supply graphically. Before the reform, if an individual moves from not having ESHI to having ESHI, labor supply shifts downward by αb because the individual is willing to work for lower wages in a job that provides ESHI. This shift results because ESHI is not merely a cost to the employer; it also has value to the employee. In the individual's choice problem, several factors can affect the magnitude of the underlying valuation of ESHI relative to a dollar of wages: α. For example, canonical insurance theory demonstrates that willingness to pay for insurance is determined by an individual's wealth, health risk, risk preferences, and the available insurance contract (see, e.g., Arrow [1963] and Rothschild and Stiglitz [1976]). Furthermore, there is a tax preference for ESHI, so we expect the tax preference to increase α as a function of the individual's marginal tax rate. Rather than modeling these factors individually, we model only α, which we will demonstrate to be a sufficient statistic for welfare analysis in the spirit of Chetty (2009).

After the reform, labor supply also reflects the penalty associated with the individual mandate and any subsidy available to that individual for health insurance outside of employment. That is, the individual penalty augments the individual's underlying valuation of ESHI, shifting his labor supply curve further downward for jobs offering ESHI—even if the individual does not value health insurance on its own merits, he will value it at least as much as the penalty that he must pay

for not having it.[9] A subsidy available outside of employment also affects the individual's labor supply if he obtains health insurance *through his employer* because the outside coverage option has changed. He is more willing to work for ESHI instead of wages in the face of a penalty, and he is less willing to work for ESHI instead of wages in the face of a subsidy for health insurance outside of employment. After the reform, if the individual moves from not having ESHI to having ESHI, his labor supply shifts downward by $\alpha + \lambda - \mu_x$, which we call the "penalty-and-subsidy-inclusive valuation" of ESHI, multiplied by the cost of health insurance b. The penalty-and-subsidy-inclusive valuation incorporates underlying preferences for health insurance and the key policy features of mandate-based reform in a simple measure: the shift in labor supply. We will show that the penalty-and-subsidy-inclusive valuation is an important sufficient statistic for our welfare analysis.

Putting the two sides of the market together yields a labor market equilibrium (w, L) in period t that reflects the underlying parameters that determine labor supply and demand. As shown in Figure 1, there are two potential equilibria in each period t, conditional on whether the individual receives health insurance through the employer: D and A are the equilibria for individuals with and without ESHI before the reform, respectively; F and B are the equilibria for individuals with and without ESHI after the reform, respectively. Our remaining theoretical and empirical analysis relies on the distances between points A, B, D, and F. The remainder of this section focuses on translating these distances into parameters of interest that ultimately allow us to analyze welfare.

2.2   Characterization of the Compensating Differential for ESHI

We begin by demonstrating how the model allows us to identify the compensating differential for health insurance, defined as the causal difference in wages between jobs with ESHI and jobs without ESHI. We can also characterize the corresponding hours differential using hours in lieu of wages. To obtain compensating and hours differentials, we simply compare wages and hours in the equilibria with ESHI (equilibria D and F) to wages and hours in the equilibria without ESHI (equilibria A and B). The first column of Table 1 shows the four possible comparisons of equilibrium wages and hours that we can use to measure compensating and hours differentials. As shown in Figure 1, because

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[9]We do not expect the individual penalty to increase the total valuation of health insurance for an individual who already values it fully. Therefore, we specify that the magnitude of λ is affected by the underlying valuation α and the statutory penalty Λ as follows:

$$\lambda = \begin{cases} \Lambda, & \text{for } \alpha \leq 1 - \Lambda \\ 1 - \alpha, & \text{for } 1 - \Lambda \leq \alpha \leq 1 \\ 0, & \text{for } \alpha > 1 \end{cases}$$

In the first case, α is small, so λ takes on its statutory value, and the penalty-inclusive valuation, which we define as $\alpha + \lambda$, is less than 1. In the second case, λ is large enough to augment α until the penalty-inclusive valuation is full. In the third case, the individual's underlying valuation of health insurance is higher than the cost to the employer. Such a case could arise if an individual cannot access health insurance outside of employment, perhaps because of preexisting conditions that are excluded on the individual market. Such a case could also arise if health insurance through the employer is cheaper than other insurance, which is likely because of the tax-preference for employer-sponsored health insurance and because employers have more negotiating power than individuals. In this case, the penalty-inclusive valuation of health insurance is his underlying valuation, and the penalty has no impact. We define the subsidy similarly so that it cannot reduce an individual's penalty-and-subsidy-inclusive valuation beyond zero.

health reform shifts labor supply and labor demand, the compensating and hours differentials are different depending on which equilibria we compare.

Table 1: Compensating and Hours Differentials

Compensating Differential	Sufficient Statistics	Coefficients
$w_D - w_A$	$\frac{s-\alpha d}{d-s} b$	$\beta_8 \left[+ \beta_{8e} \right]$
$w_F - w_B$	$\frac{(1-\rho)s-(\alpha+\lambda-\mu_x)d}{d-s} b$	$\beta_1 + \beta_8 \left[+ \beta_{1e} + \beta_{8e} \right]$
$w_D - w_B$	$\frac{(1-\rho)s-\alpha d}{d-s} b$	$\beta_8 - \beta_{11} \left[+ \beta_{8e} \right]$
$w_F - w_A$	$\frac{s-[\alpha+\lambda-\mu_x]d}{d-s} b$	$\beta_1 + \beta_8 + \beta_{11} \left[+ \beta_{1e} + \beta_{8e} \right]$

Hours Differential	Sufficient Statistics	Coefficients
$L_D - L_A$	$\frac{1-\alpha}{d-s} b$	$\gamma_8 \left[+ \gamma_{8e} \right]$
$L_F - L_B$	$\frac{1-\rho-(\alpha+\lambda-\mu_x)}{d-s} b$	$\gamma_1 + \gamma_8 \left[+ \gamma_{1e} + \gamma_{8e} \right]$
$L_D - L_B$	$\frac{1-\rho-\alpha}{d-s} b$	$\gamma_8 - \gamma_{11} \left[+ \gamma_{8e} \right]$
$L_F - L_A$	$\frac{1-(\alpha+\lambda-\mu_x)}{d-s} b$	$\gamma_1 + \gamma_8 + \gamma_{11} \left[+ \gamma_{1e} + \gamma_{8e} \right]$

We can also use the compensating and hours differentials to learn about the valuation of ESHI and the other model parameters. In the second column of Table 1, we express each compensating or hours differential in terms of the sufficient statistics of the model. These expressions follow directly from the geometry of Figure 1. We represent the slope of the labor supply curve with s and the slope of the labor demand curve with d (these slopes are elasticities if we specify w as the logarithm of wages and h as the logarithm of hours).

In our empirical implementation, we will be particularly interested in the compensating and hours differentials for individuals who switch from not having ESHI before the reform (equilibrium A) to having it after the reform (equilibrium F). For these individuals, as shown in the expression in the last row of the each panel of Table 1, if the penalty-and-subsidy-inclusive valuation is full $(\alpha + \lambda - \mu_x = 1)$, then the absolute value of the compensating differential is equal to the cost of ESHI (ESHI decreases wages by b), and the hours differential is zero (ESHI does not distort hours worked). Therefore, if the compensating differential is equal to the cost of the benefit b and the hours differential is zero, then we can infer that the penalty-and-subsidy-inclusive valuation is full.[10]

2.3 Characterization of the Welfare Impact of Mandate-Based Health Reform Using Sufficient Statistics

To this point, we have developed a simple model that allows us to express all of the key parameters of mandate-based reform as well as the set of preferences that determine an individual's valuation

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[10]Previous studies based on the Summers model have stopped at related inferences because they only have enough variation to identify the valuation if it is full. If the compensating differential is less than the cost of the benefit, and the hours differential is nonzero, then they cannot infer the magnitude of the penalty-and-subsidy-inclusive valuation beyond stating that it is not full. However, as we discuss in Section 3, the additional sources of variation in our model enrich the empirical content of the Summers model, allowing us to identify the penalty-and-subsidy-inclusive valuation, regardless of the true magnitude.

of ESHI in terms of labor market equilibria. We are also interested in how the Massachusetts and national mandate-based reform affect welfare and how these policies compare to alternative approaches that could be taken to expand health insurance coverage. Our model allows us to conduct welfare analysis simply with sufficient statistics, building on our estimated compensating differential.

Figure 2: Graphical Model with Deadweight Loss
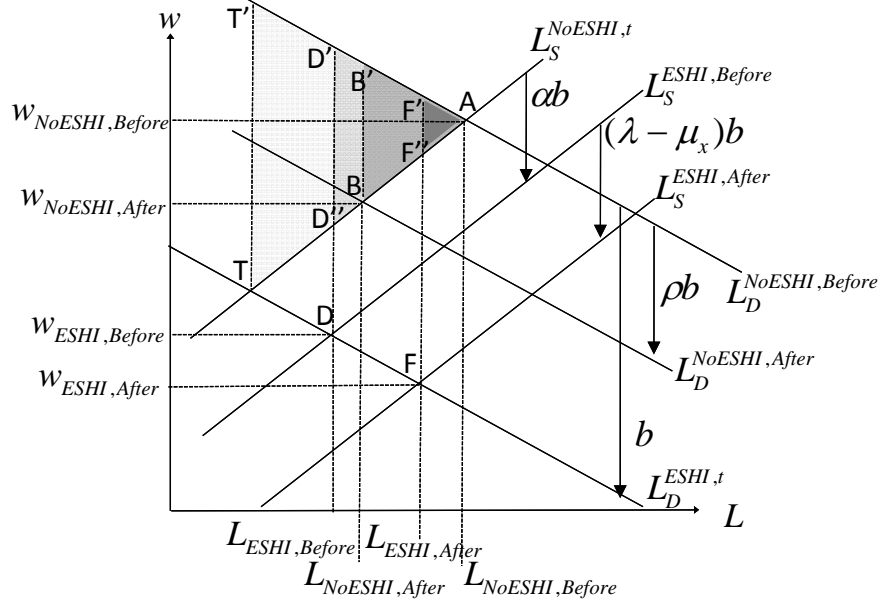
Mandate-based policy can reduce welfare in two ways. First, if it distorts the labor market such that workers are willing to work for wages lower than the market wage and employers are willing to hire workers for more than the market wage, but the transaction does not occur. Second, workers at firms not offering ESHI face a penalty cost. Graphically, the welfare reduction corresponds to a weighted combination of two overlapping triangles shown in Figure 2. The deadweight loss is the triangle given by $F'AF''$ if the representative individual has ESHI after the reform, and it is equal to the triangle given by $B'AB$ if he does not.[11] We can also express these triangles in terms of the key parameters of the model. The combined deadweight loss of the policies of mandate-based health reform (denoted by the subscript m) is as follows:

$$DWL_m = \frac{b^2}{2(s-d)}((1-(\alpha+\lambda-\mu_x))^2 ESHI_{After} + \rho^2(1 - ESHI_{After})). \tag{1}$$

If we know the values for all of the terms in this equation, we can calculate the welfare impact of mandate-based health reform on the labor market. Thus, these terms are sufficient statistics for welfare analysis. For example, if the employer penalty is equal to zero and the penalty-and-subsidy-inclusive valuation is full ($\alpha + \lambda - \mu_x = 1$), then there is no deadweight loss associated with mandate-based health reform.

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[11]As shown, the deadweight loss for individuals with ESHI is smaller, but the relative magnitudes of the triangles can reverse, depending on the magnitudes of the policy parameters.

2.4 Characterization of the Welfare Impact of Mandate-Based to Tax-Based Health Reform

While the welfare impact of mandate-based reform is clearly of interest, we are also interested in comparing the welfare impact of mandate-based reform to the welfare impact of a counterfactual reform to inform policy decisions and to give us a sense of whether the welfare impacts that we find are large or small. Using Equation (1), we can compare the deadweight loss of mandate-based reform to the deadweight loss of an alternative policy. If we can express the key policy elements in terms of labor market equilibria, then we can compare different policies simply by taking the ratio of the deadweight losses. In the tradition of Summers (1989), we compare the welfare impact of mandate-based health reform to the welfare impact of an alternative tax-based health reform.

Before considering the counterfactual policy of the Summers model, we note that the Summers model is a special case of our model. The Summers model incorporates the establishment of a full-compliance employer mandate starting from a regime where no firms provide health insurance. We extend the Summers model to incorporate the establishment of the three elements of mandate-based reform starting from a regime where some firms provide health insurance and others do not. From a modeling perspective, the Summers model is a special case of our model with a single equilibrium in each period and a different policy intervention. In our model, before the policy intervention, there are two equilibria – the equilibrium at A for jobs that do not include ESHI and the equilibrium at D for jobs that include ESHI. In the Summers model, before the policy intervention, there is only one equilibrium at A—no jobs include ESHI. In our model, after the policy intervention, there are two new equilibria because of the imposition of the employer mandate, the individual mandate, and the employer mandate. One occurs at F for firms that provide ESHI, and the other occurs at B for firms that do not provide ESHI and instead pay the penalty. In the Summers model, there is only one equilibrium after the policy intervention. The policy intervention consists of a mandate that all employers must provide health insurance, and it is not a pay-or-play mandate, so there is full compliance with the mandate. The single equilibrium after the full compliance mandate occurs at D, which is equivalent to the ESHI equilibrium before the reform in our model.

As an alternative to the full-compliance mandate, Summers (1989) considers a single counterfactual policy, which he refers to as a "benefit tax". Under this counterfactual policy, again there is a single equilibrium before its implementation at A – no jobs include ESHI. Upon the implementation of the policy, the government levies a tax τ on employers to provide health insurance. Suppose for now that the tax is equal to the cost of providing a standard health insurance benefit b. As shown in Figure 1, labor demand shifts downward by b, and holding labor supply constant, the new labor market equilibrium is at point T. The deadweight loss of the tax-based reform is given by the triangle $T'AT$:

$$DWL_\tau = \frac{\tau^2}{2(s-d)}.$$

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The key Summers (1989) assumption about tax-based reform is that it does not induce a shift in labor supply. An equivalent way of stating this assumption is that workers value insurance,

but because insurance is not linked to work under the tax-based reform, this valuation does not show up in the labor supply curve. This assumption implies that individuals would place no value on health insurance offered by their employers (ESHI) because they already have health insurance from a tax-financed government source. Yet another equivalent way of stating this assumption is that tax-based policy makes the link between taxes and benefits less salient—workers can recognize the link between ESHI and wages under mandate-based health reform, but they do not recognize the link between tax-financed health insurance and the tax that employers pay on wages under tax-based reform.

We compare the welfare impact of mandate-based reform to the welfare impact of the same tax-based reform as Summers (1989). Taking the ratio of the deadweight loss of mandate-based reform to the deadweight loss of tax-based reform, allowing $b \neq \tau$ gives

$$\frac{DWL_m}{DWL\tau} = \frac{b^2}{\tau^2}((1 - (\alpha + \lambda - \mu_x))^2 ESHI_{After} + \rho^2(1 - ESHI_{After})). \tag{2}$$

This equation characterizes the welfare of the combined features of mandate-based reform relative to a tax-based reform in terms of a small number of sufficient statistics: the cost b that employers pay for ESHI compared to the necessary tax revenue τ for the same benefit; the penalty-and-subsidy-inclusive valuation, $\alpha + \lambda - \mu_x$, for individuals who have ESHI after reform; the employer penalty ρ for individuals who do not have ESHI after reform; and the fraction of individuals with ESHI after reform, $ESHI_{After}$. Since the same individuals would be covered by both mandate-based and tax-based reform, underlying health risk is invariant to the plan. Thus the ratio of b to τ is just the relative loading cost of ESHI and government-provided health insurance. We show in Section 3.3 that two of the key sufficient statistics—the cost of the benefit b and the penalty-and-subsidy-inclusive valuation $\alpha + \lambda - \mu_x$—are functions of the compensating and hours differentials from before to after the reform.

Our welfare ratio generalizes the ratio implied by Summers (1989) and offers some insights that are perhaps counter-intuitive. To see this, first consider the ratio implied by Summers (1989): the ratio of the deadweight loss of a full-compliance employer mandate given by the triangle $D'AD''$ to the deadweight loss of tax-based reform given by triangle $T'AT$. Using Equation (2), all agents have ESHI after reform ($ESHI_{After} = 1$), and there are no penalties or subsidies ($\lambda = \mu_x = 0$). Assuming that the tax is equal to the cost of health insurance ($t = b$), the ratio of the deadweight loss of the full-compliance employer mandate to the ratio of the tax-based reform simplifies to $(1 - \alpha)^2$. This special case yields the theoretical contribution of Summers (1989): an employer mandate reduces deadweight loss relative to a tax.

However, our model demonstrates that an employer mandate does not always reduce deadweight loss relative to a tax. If there is already a pay-or-play *individual* mandate in place, then the addition of a full-compliance or a pay-or-play *employer* mandate weakly decreases welfare relative to a tax. Consider the case where there is already an individual pay-or-play mandate in place, and some firms provide ESHI while others do not, but there is no employer mandate of any kind. If the tax is equal to the cost of health insurance ($t = b$), then the deadweight loss of the individual mandate

relative to a tax is given by Equation (2) with $\mu_x = \rho = 0$, which simplifies to $(1 - \alpha)^2 ESHI_{After}$. We can see from this expression that if there is no employer penalty, then there is no distortion to the labor market for firms that do not provide ESHI. Adding a full-compliance employer mandate weakly increases the deadweight loss ratio because after its imposition, all firms must provide ESHI ($ESHI_{After} = 1$); zero distortion without ESHI is no longer possible. Likewise, adding a pay-or-play mandate weakly increases the deadweight loss ratio, which becomes $(1 - \alpha)^2 ESHI_{After} + \rho^2(1 - ESHI_{After})$ because there is now a deadweight loss triangle for firms without ESHI. Intuitively, the individual mandate has a smaller deadweight loss than a tax because it makes individuals willing to work for lower wages if they receive ESHI. When the individual mandate is already in place, the employer mandate results in additional deadweight loss for individuals without ESHI.

Another related implication of our welfare model is that as long as some individuals receive ESHI from their employers, holding existing subsidies and employer mandates constant, the addition of a pay-or-play individual mandate or an increase in the size of the penalty associated with the individual mandate can *reduce* deadweight loss in the labor market. Algebraically, an increase in λ in Equation (1) results in decreased deadweight loss. This implication seems counterintuitive if we generally expect that new or expanded policies increase distortions.[12]

3   Identifying and Estimating the Model

In this section, we develop the empirical analog of our theoretical model. We have shown that we can express the compensating differential for ESHI and the welfare impact of health reform in terms of differences between the four labor market equilibria. Thus, to estimate the model we must identify wages and hours at each equilibrium. To do so, we rely on the variation induced by the reform in Massachusetts. The simplest approach would require only eight pieces of data to estimate the four labor market equilibria in Figure 1: average wages and hours for jobs with and without ESHI before and after reform within Massachusetts. We could then calculate the compensating differential for ESHI and the sufficient statistics for the welfare impact of health reform. However, we need to incorporate additional sources of variation to account for factors outside of the model that would bias our estimates were we to merely compare means in Massachusetts over time across groups. In practice, we also calibrate the sufficient statistics that are the least well-identified by our empirical variation.

3.1   The Estimating Equation

To estimate all of the relevant differences between labor market equilibria, the compensating and hours differentials and the welfare impact of health reform, we specify and estimate wage and hours

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[12]We do note that the addition of an individual mandate can increase distortion in the market for goods and services by requiring individuals to allocate more dollars to health insurance and fewer dollars to other goods. However, we focus on a specific set of distortions to the labor market, assuming that the government has already decided to expand health insurance coverage—as in the case of the Massachusetts reform and the ACA—and is looking for the most efficient way to do so. For a given level of coverage, distortions to the market for goods and services are the same under mandate-based reform and tax-based reform. By taking the ratio of the distortion under mandate-based reform to the distortion under tax-based reform, we can focus on the welfare impact of health reform on the labor market.

equations of the following form:

$$Y_{it} = [\beta_1 (MA * ESHI * After * Large)_{it} + \beta_8 (MA * ESHI * Large)_{it} + \beta_{11}(MA * After * Large)_{it} +$$
$$\beta_{12}(ESHI * After * Large)_{it} + \beta_{19}(ESHI * Large)_{it} + \beta_{22}(After * Large)_{it} +$$
$$\beta_{23}(Large)_{it} + (\phi_s * Large)_{it} +]$$
$$\beta_{1[e]}(MA * ESHI * After)_{it} + \beta_{8[e]}(MA * ESHI)_{it} + \beta_{11[e]}(MA * After)_{it} +$$
$$\beta_{12[e]}(ESHI * After)_{it} + \beta_{19[e]}(ESHI)_{it} + \beta_{22[e]}(After)_{it} + \phi_s + \delta_i + \varepsilon_{it}, \tag{3}$$

where Y_{it} measures wages w or hours L for individual i in state s at time t. We specify wages and hours in levels. The level specification allows us to capture the impact of the reform on the intensive margin of how many hours to work and the extensive margin of whether to work because we can include unemployed workers in the sample, specifying that they have wages and hours of zero. The level specification also allows health insurance to have a realistic additive rather than multiplicative effect on wages, but we also investigate robustness to specifying wages and hours in logarithmic form. MA is an indicator for the state of Massachusetts relative to other states, $ESHI$ is an indicator for ESHI relative to the absence of ESHI, $After$ is an indicator for the period after the reform relative to the period before the reform, and $Large$ is an indicator for large firms relative to firms of known small size that are exempt from the employer mandate. We represent the coefficients of the wage equation with subscripted β coefficients, and we represent the corresponding coefficients of the hours equation with subscripted γ coefficients. The numbers of the coefficients convey that they are a subset of the coefficients of the full equation that we use to separately identify different values of μ_x, which we present in the Online Appendix. We include state fixed effects ϕ_s with a state other than Massachusetts omitted to control for differences in wages across states, and we include individual fixed effects δ_i to control for time-invariant differences across individuals, allowing for individual-specific shocks at time t, ε_{it}. We include a time fixed effect, $After$, to control for changes in the labor market over time.[13] We begin with a baseline specification that excludes all bracketed terms. This specification excludes variation between large and small (exempt) firms. We subsequently include the bracketed terms in our full specification.

Our approach incorporates three key sources of variation in addition to the changes in Massachusetts over time in labor market outcomes by ESHI status. First, we rely on variation within individuals over time by including individual fixed effects. The individual fixed effects are essential because they allow us to control for a myriad of worker characteristics that shift labor supply and demand for a given individual for reasons that are correlated with having ESHI. That is, individuals who have ESHI are likely to differ from those individuals who do not, and those differences also manifest in labor market outcomes. Unobserved differences between individuals with and without ESHI is the standard concern that has plagued the literature on the compensating differential for health insurance. A more subtle but critical reason to incorporate individual fixed effects is the need to address compositional change among those with ESHI in Massachusetts from before to after the

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[13]In all specifications, we also allow for a "during" implementation period that is separate from the before and after periods. In our results tables, we represent the coefficients on during period terms with corresponding d superscripts.

reform. If mandate-based reform differentially increases ESHI rates among individuals with lower wages and/or work hours, without individual fixed effects, we could spuriously estimate a negative relationship between ESHI and wages after the reform.

Second, we incorporate variation between Massachusetts and other states to control for factors that shift labor supply and demand nationally for reasons that are unrelated to Massachusetts health reform. Incorporating this variation allows us to control for any aggregate trends in the relationship between ESHI and labor market outcomes.

Finally, in our full specification, we incorporate variation between small and large firms.[14] This additional source of variation allows us to better identify the impact of the employer penalty by comparing firms that qualified relative to firms that were exempt. In contrast, because it does not include variation by firm size, our baseline specification assumes that all Massachusetts firms are subject to the employer penalty after reform. Incorporating variation by firm size also helps to control for Massachusetts-specific factors unrelated to health reform that could shift labor supply and demand.[15]

Our estimating equations are relatively straightforward, allowing us to estimate them with ordinary least squares. The simplicity of the estimating equations is an advantage of our model relative to alternative structural models because robustness analysis is easier to implement, and the results are more transparent. Furthermore, because the functional form of these equations is relatively simple, we can interpret the coefficients directly as well as the combinations of coefficients that make up the sufficient statistics.

3.2 Estimating Wages, Hours, and the Compensating Differential for ESHI

To identify the wage and hours associated with different equilibria, we focus on the linear combinations of coefficients that correspond to wage and hours at each equilibrium A, B, D and F, as opposed to focusing on a single coefficient as in a traditional difference-in-differences model. To see the distinction between our estimating equation and a difference-in-differences model, consider that in the baseline specification that omits bracketed terms, the first coefficient β_1 is the traditional difference-in-difference-in-differences coefficient. It gives the impact on wages for individuals in Massachusetts relative to individuals in other states, when they have ESHI relative to when they do not have ESHI, after the reform relative to before the reform. In this specification, individuals in Massachusetts with ESHI after reform are the only group treated by reform, and all other individuals are in control groups. However, our model predicts that individuals in Massachusetts without ESHI after reform have also been treated by the reform—their wages and hours should be lower because of the employer penalty—so we do not want to use them as a control group in our

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[14]We recognize that firm size can be endogenous in the sense that individuals can choose to work at small or large firms in response to health reform or firms that are near the firm size cutoff may endogenously change their size to avoid penalties. However, we want to allow for such behavior to capture the broadest possible impact of reform.

[15]We extend the model to incorporate variation in subsidy amounts in the Online Appendix. This variation allows us to identify separate equilibria for individuals for different subsidy amounts. With these equilibria, we can separately identify λ from μ_x, and we can identify a different value of μ_x for each income eligibility group x. However, because using this variation requires us to divide the data into small groups based on income eligibility thresholds, we do not use this variation in our full specification.

preferred estimate of the compensating differential. Similarly, our full specification that includes bracketed terms resembles a difference-in-differences specification, but we do not want to examine only the resulting β_1 coefficient because doing so would incorporate control groups that should not be incorporated according to our model.[16]

Accounting for differences with relevant control groups, we express the wages associated with each equilibrium in Table 2. The hours associated with each equilibrium are equivalent with γ in place of β. To ease interpretation, we normalize $w_A = 0$ and $L_A = 0$ so that all equilibria are relative to the equilibrium without ESHI before reform. The derivation of these expressions is straightforward. For example, the difference in wages between equilibrium B and A (the equilibrium without ESHI after the reform relative to the equilibrium without ESHI before the reform) is β_{11}, the change in wages from after the reform to before the reform for individuals who remain without ESHI in Massachusetts, relative to individuals in other states who remain without ESHI.[17]

Table 2: Wages in Terms of Coefficients

w_A	NoESHI, Before	0
w_B	NoESHI, After	β_{11}
w_D	ESHI, Before	$\beta_8 \left[\, + \beta_{8e} \, \right]$
w_F	ESHI, After	$\beta_1 + \beta_8 + \beta_{11} \left[\, + \beta_{1e} + \beta_{8e} \, \right]$

Using the expressions for the labor market equilibria in Table 2, we can then express the compensating and hours differentials in terms of regression coefficients in the last column of Table 1. Our preferred measure of the compensating differential, $w_F - w_A$, is the sum of several coefficients: $\beta_1 + \beta_8 + \beta_{11} \left[\, + \beta_{1e} + \beta_{8e} \right]$. These coefficients reflect the change in wages observed for individuals who switch from not having ESHI before the reform to having it after the reform, relative to individuals who have the same switch in ESHI status from before to after reform in other states. Because the most convincing identification comes from changes in ESHI status for a given individual induced by the reform, we focus on this comparison for our preferred estimates of the compensating and hours differentials. In contrast, the first two differentials in Table 1 rely on changes in ESHI status for a given individual *within* the period either before or after reform. The changes in ESHI status that identify these compensating differentials could be endogenous, even after including individual fixed effects, if individuals gain ESHI when they get a better job that includes health insurance.

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[16]As in the baseline specification, we do not want to include individuals in Massachusetts without ESHI after reform as a control group because they are treated by the employer penalty. Even more importantly, we do not want to include individuals in small firms as controls for individuals in large firms in all instances because our model predicts that individuals at firms of both sizes should experience declines in wages when they shift into ESHI. Only individuals in small firms without ESHI are a valid control group because they are not subject to the employer penalty.

[17]In the full specification, which includes the bracketed terms in Equation (3), β_{11} also reflects the difference between individuals in large firms and individuals in small exempt firms, thus controlling for Massachusetts-specific factors after reform.

3.3 Estimating the Welfare Impact of Mandate-Based Health Reform

To estimate the welfare impact of health reform given in Equations (1) and (2), we first estimate the underlying sufficient statistics. We can express most of the sufficient statistics in terms of differences in wages and hours between the four labor market equilibria depicted in Figure 1. Our derivation follows directly from the geometry of the figure. In the first two rows of Table 3, we express the supply and demand curve slopes in terms of wages and hours differences between equilibria. The last column gives equivalent expressions in terms of coefficients. In the subsequent rows of the table, we express other sufficient statistics in terms of the slope of the labor supply and demand curves as well as differences between other equilibria.

Table 3: Sufficient Statistics

Sufficient Statistics	Wages and Hours	Coefficients
s	$\frac{w_B - w_A}{L_B - L_A}$	$\frac{\beta_{11}}{\gamma_{11}}$
d	$\frac{w_F - w_D}{L_F - L_D}$	$\frac{\beta_1 + \beta_{11}\,[+\beta_{1e}]}{\gamma_1 + \gamma_{11} + [\gamma_{1e}]}$
b	$d(L_F - L_A) - (w_F - w_A)$	$d\left(\gamma_1 + \gamma_8 + \gamma_{11} + [\gamma_{1e} + \gamma_{8e}]\right) - (\beta_1 + \beta_8 + \beta_{11}\,[+\beta_{1e} + \beta_{8e}])$
ρ	$\frac{d(L_B - L_A) - (w_B - w_A)}{b}$	$\frac{d(\gamma_{11}) - (\beta_{11})}{b}$
α	$\frac{s(L_D - L_A) - (w_D - w_A)}{b}$	$\frac{s(\gamma_8\,[+\gamma_{8e}]) - (\beta_8\,[+\beta_{8e}])}{b}$
$\lambda - \mu_x$	$\frac{s(L_F - L_D) - (w_F - w_D)}{b}$	$\frac{s(\gamma_1 + \gamma_{11}\,[+\gamma_{1e}]) - (\beta_1 + \beta_{11}\,[+\beta_{1e}])}{b}$
$\alpha + \lambda - \mu_x$	$\frac{s(L_F - L_A) - (w_F - w_A)}{b}$	$\frac{s(\gamma_1 + \gamma_8 + \gamma_{11}\,[+\gamma_{1e} + \gamma_{8e}]) - (\beta_1 + \beta_8 + \beta_{11}\,[+\beta_{1e} + \beta_{8e}])}{b}$

As discussed above, the differences between labor market equilibria that are identified by changes resulting from the Massachusetts reform are arguably best identified. Therefore, some sufficient statistics are identified more convincingly than others. Fortunately, these sufficient statistics are the most important for welfare analysis: the penalty-and-subsidy-inclusive valuation ($\alpha + \lambda - \mu_x$) and the cost of ESHI to employers (b).

The other sufficient statistics are identified in principle, but not as convincingly because they do not depend on changes in ESHI status induced by the reform.[18] In practice, we estimate values

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[18]We can identify the slope of the demand curve d by comparing individuals with ESHI before and after the reform; we can identify the slope of the supply curve s by comparing individuals without ESHI before and after the reform; and we can also identify the employer penalty ρ by comparing individuals without ESHI before and after reform, using a value for d. We can also identify α, λ, and μ_x separately from their sum, which would allow us to analyze the welfare impact of the separate components of health reform independently. As shown in Table 3, identification of α requires a value for s and the comparison of people with and without ESHI before reform. The inclusion of individual fixed effects should help to identify α because we control for time-invariant factors that affect wages and benefits. However, any changes over time that affect both simultaneously will lead to bias. For example, if an individual without health insurance gets promoted to a job with higher wages and ESHI, we will estimate a negative value for α, even if the individual values the benefit such that the true value of α is positive. Such bias is precisely the problem that has hindered previous efforts to identify compensating differentials, which we overcome in identifying the penalty-and-subsidy-inclusive valuation but not in identifying α separately. We can also attempt to separately identify λ and μ_x. As shown in Table 3, identification of the difference $\lambda - \mu_x$ requires a value for s and the comparison of people with ESHI before and after reform. To separately identify μ_x from λ, and to identify different values of

for these parameters that do not accord well with values that we expect based on the literature and the empirical magnitude of the employer penalty. Given that these parameters are not identified by the Massachusetts reform and that their misspecification can affect the estimates of all the other sufficient statistics through the s and d terms in their derivations, we discard the empirical estimates and calibrate them. Reviewing the literature suggests that reasonable magnitudes for labor supply and demand elasticities are 0.1 and -0.2, respectively (Blundell and MaCurdy 1999, Hamermesh 1996).[19] We also calibrate the employer penalty ρ such that the dollar value of the employer penalty ρb is equal to the statutory penalty of \$295 per year.

Given that we calibrate some sufficient statistics, one might be tempted to calibrate most of our model using the statutory values of the policy parameters, rather than estimating any sufficient statistics. However, it is important to estimate the sufficient statistics for two main reasons. First, the individual's underlying valuation α does not have a statutory value. Second, the behavioral response to the policy parameters might be smaller or larger than the statutory policy parameters because of interactions between them and the individual's underlying valuation (see footnote 9), or if individuals over respond if they are averse to paying penalties on moral grounds.[20] Therefore, we only calibrate values when we have reason to believe that identification is not convincing and the empirical results are not consistent with the model.

4 Data and Summary Statistics

4.1 The Survey of Income and Program Participation

For our main analysis, we use the Survey of Income and Program Participation (SIPP), a nationally representative longitudinal survey covering households in the civilian noninstitutionalized population. As we discuss in detail, the longitudinal nature of the SIPP is critical for identification. Individuals selected into the SIPP sample are interviewed once every four months over a four-year panel. Each interview covers information about the previous four-month period, resulting in person-month-level data. Interview months differ across individuals in the sample. Previous research has shown evidence of "seam bias" in the SIPP, whereby individuals tend to give the same responses during one interview for all four months associated with the interview period, but they do change responses from one interview to the next (see Chetty [2008]). To address seam bias, we restrict our data to the interview month in our regression specifications. We use weights in all summary statistics and regressions to account for the SIPP sampling and response unit design.[21]

μ_x for people eligible for different subsidy amounts, we can incorporate variation in subsidy amounts across income eligibility thresholds as we discuss in the Online Appendix.

[19]Because our specification is in levels (not logarithms), we convert these into slopes at the mean wage and hours.

[20]The CBO considered a behavioral response to an individual mandate in their estimates of the impact of the ACA on coverage (Auerbach et al. 2010). They highlight the need to understand responses to the individual mandate in more detail, and our methodology could prove useful.

[21]We use data from the core content of the SIPP. We construct our data by appending the 12 individual-wave files from the 2004 panel and merging longitudinal weights onto the full file by individual person identifiers. Longitudinal panel weights account for people who were in the sample in wave 1 of the panel and for whom data were obtained (either reported or imputed) for every month of the panel. There are four panel weights associated with the 2004 SIPP panel; the first covers people present in waves 1–4, the second covers people in waves 1–7, the third covers

We use the full 2004 SIPP panel, which covers October 2003 to December 2007. A potential limitation of this SIPP panel is that it does not extend for a long time after reform was fully implemented, and it will not be extended further because an entirely new SIPP cohort began in 2008. Despite this potential limitation, we believe the SIPP data are the best-suited to modeling the labor market impact of the Massachusetts reform for a number of reasons.

First and foremost, the SIPP is the largest longitudinal data set that we are aware of that includes labor market outcomes and insurance information. The Medical Expenditure Panel Survey (MEPS) is longitudinal, but it only extends for two and a half years, and the sample size is only approximately 15 percent of the size of the SIPP, with 160 individuals in Massachusetts.[22] The Current Population Survey (CPS) and the American Community Survey (ACS) do not include longitudinal identifiers for health insurance. Although administrative data from Social Security are longitudinal, they do not include information on hours worked or insurance.

Second, while we cannot compare wage and hours trends conditional on individual fixed effects across data sets because of data availability, raw wage and hours trends are very similar in the SIPP to those in other data sets inside and outside of Massachusetts. We present these comparisons in Appendix B.

Third, although we observe a relatively short period of responses after the individual mandate went into effect on July 1, 2007, we also observe a full year of responses during the implementation of the reform. Thus, we are able to observe the response in wages and hours of all individuals who changed ESHI status in Massachusetts at any time after the reform was passed. This is particularly valuable because open-enrollment periods for ESHI are generally in November, with new coverage starting in January. Thus, to satisfy the individual mandate in July 2007 by taking up ESHI, many individuals would have to start coverage in January 2007, well before our data end at the end of December 2007.[23] Using data from the CPS, we find that of the eventual increase in coverage among those 18–64 in Massachusetts by 2010, 87 percent had occurred by the end of 2007 (measured in the March 2008 CPS). The share of the eventual increase in those covered by ESHI by 2010 is even higher at 91 percent, suggesting that our sample period covers the time in which much of the expansion due to the reform had occurred despite our relatively short post-reform period.[24]

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people in waves 1–10, and the fourth covers people who have data for the whole sample (waves 1–12). The panel weighting scheme does not assign weights to people who enter the sample universe after wave 1 (panel weight = 0 if the individual was not in the sample in wave 1, if they have missing data for one or more month(s), or both). In choosing the appropriate weights, there is a tradeoff between length of individual data and reductions in sample size associated with attrition. Our specification does not use panel weights and instead uses individual weights, therefore maximizing the number of respondents. In results not reported, we reestimate our regressions using each panel weight. Reassuringly, the coefficients of interest are relatively robust to these weight changes. Using weights 3 or 4 does lead to substantial loss of precision as the sample size falls when moving from longitudinal weight 1 to 4.

[22]We have run our regressions in the restricted-access MEPS with state identifiers, but the sample size is not large enough for us to obtain reliable results.

[23]We also note that even if we had data beyond December 2007, we would be reticent to rely on it to estimate our model because a recession began at that time. Dubay et al. (2012) present insurance coverage and employment measures in Massachusetts, a set of northeastern control states, and the remainder of the country over time. They show the impact of the recession in both Massachusetts and the various control groups beginning almost immediately in 2008. With 2008 data, we would thus be concerned that the recession could have had a differential impact on Massachusetts relative to other states.

[24]We do, however, note that even though the CPS asks about coverage in the previous year, it is well known that

Despite all of the advantages of the 2004 SIPP panel, one limitation is that the sample size decreases over time, primarily because of interview reductions but also because of attrition. Our group of interest is the population between the ages of 18 and 64. In 2004, there are 72,057 unique individuals in this sample across states, of which 2,047 unique individuals are in Massachusetts. In 2007, there are 28,661 unique individuals in the sample, of which 685 unique individuals are in Massachusetts.

Using the SIPP, we construct our main dependent variables: hourly wages w and hours worked per week L. We convert all wages into 2006 dollars using the Consumer Price Index for all urban consumers (CPI-U) to adjust for inflation. The SIPP allows respondents to report wages and hours for up to two jobs, but our estimates rely on income and hours worked only in the primary job. Because the SIPP data only include monthly income, not monthly wages, we must divide income by a measure of hours worked to obtain monthly wages. However, because our model relies on separate movements in w and L, it would be problematic for both measures to reflect contemporaneous movements in L. To get around this issue, which is related to the division bias problem in the labor economics literature, we divide income by the average hours reported in the first four interviews (representing a 16-month period). Our regression estimates are robust to alternative wage measures because hours vary less than wages.

4.2 Summary Statistics

Before proceeding to our regression results, we assess the empirical validity of comparing Massachusetts to other states by comparing labor market, health insurance, and demographic variables. From Appendix Figures B1 and B2, it is clear that aggregate labor market trends are similar in Massachusetts and other states before reform across a variety of data sets. We also observe similar aggregate labor market trends in Massachusetts and other states *after* reform, which is not surprising given our model. Our model predicts different movements in wages and hours based on ESHI status before and after reform. The overall impact on the labor market, however, is ambiguous and unlikely to be substantial unless a large share of the population changed ESHI status. As shown in Figure 1, we expect individuals who switch from not having ESHI before the reform (equilibrium A) to having ESHI after the reform (equilibrium F) to receive weakly lower wages and to work weakly lower hours. While these individuals are an important group from a policy perspective, they are only a small fraction of the aggregate population in the labor market.[25] We know from prior research that, though the reform resulted in a significant decline in the percentage of people

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responses also reflect current coverage. Thus, the responses from March of 2008 may reflect some share of people who gained coverage in 2008. Nevertheless, because the survey was very early in 2008, we would expect much of the change in coverage to have occurred in our sample period in 2007.

[25]For individuals who continued to have ESHI (equilibrium D to equilibrium F), our model predicts an ambiguous change in hours and wages, depending on the relative magnitudes of the individual penalty λ and the subsidy μ_x. For the small group of other individuals who switched from having ESHI before reform (equilibrium D) to not having it after reform (equilibrium B) (perhaps because subsidies became available), the model also predicts an ambiguous change in wages and hours, depending on the relative magnitudes of the underlying valuation α and the employer penalty ρ. Therefore, while our model makes clear predictions about changes in wages and hours for individuals based on changes in ESHI status, the impact of reform on wages and hours in the *aggregate* labor market is an empirical question.

without health insurance, only approximately 6 percentage points of the population gained health insurance coverage. Of those who gained coverage, roughly half gained ESHI and the other half gained subsidized coverage (Kolstad and Kowalski 2012). Thus, it is unsurprising that the aggregate measures of labor market changes do not reveal an impact of the reform.

Table 4: Summary Statistics

	Full Population		MA		Non-MA		MA-Non-MA
	Before	After	Before	After	Before	After	After-Before
w: Weekly earnings / baseline hours per week	13.998	14.189	17.895	19.164	13.909	14.077	1.101***
w|paid job & w>0	20.247	20.757	24.707	25.259	20.141	20.644	0.049
Log(w|paid job & w>0)	2.736	2.773	2.930	2.973	2.732	2.768	0.007
L: Hours per week	29.237	28.803	30.257	31.053	29.214	28.752	1.258***
L|paid job & L>0	39.055	38.848	38.288	37.817	39.074	38.874	-0.271***
Log(L|paid job & L>0)	3.607	3.606	3.571	3.562	3.608	3.607	-0.008**
Hours per week in all jobs	39.896	39.471	39.452	38.313	39.906	39.499	-0.732***
Paid job	0.783	0.777	0.818	0.841	0.782	0.775	0.029***
Employed by Large Firm	0.853	0.845	0.854	0.825	0.853	0.845	-0.022***
Any Health Insurance	0.830	0.837	0.907	0.952	0.828	0.835	0.038***
ESHI	0.657	0.653	0.741	0.742	0.655	0.651	0.005
<150%FPL†	0.193	0.200	0.130	0.109	0.194	0.202	-0.028***
150-300%FPL†	0.283	0.288	0.207	0.171	0.285	0.291	-0.042***
Age	39.996	40.265	40.499	40.785	39.985	40.254	0.018
Married	0.561	0.552	0.545	0.499	0.562	0.554	-0.038***
Female	0.507	0.507	0.514	0.516	0.507	0.507	0.002*

†FPL category defined for each individual based on status in the Jan-June 2006 period.
2004 SIPP Panel. Monthly weights used.
Full 18-64 population. Only includes interview months.
Before: October 2003 - June 2006; After: July 2007 - December 2007. Statistics are averages over the relevant period.
MA-Non-MA *After-Before* is the coefficient on MA*After from a regression of the outcome on MA*After, MA, and After.
***p<0.01, **p<0.05, *p<0.1, block bootstrapped by state.
w and L measures include individuals without a paid job with w=0 or L=0, respectively, unless noted otherwise.

We report summary statistics in Table 4. We compare the full population, the Massachusetts population, and the non-Massachusetts population before reform and after reform. We exclude the during-reform period here for simplicity. The first row of the table shows our primary measure of w: weekly earnings divided by baseline hours per week, including zero wages for individuals without a paid job. Wages are higher in Massachusetts than they are in other states before and after reform. Netting out the change in wages in other states from before to after reform, as shown in the last column, hourly wages increased by $1.10 in Massachusetts after reform on a base of $17.90 before reform. This increase is statistically significant. Excluding individuals without a paid job in the second and third rows of the table, we see that wages increased by $0.05, or 0.1 percent among the employed, which is less than the wage increase that we see in the full sample, suggesting that part of the wage increase we observe is driven by an increase in the number of people with paid jobs. Indeed, the probability of reporting a paid job increased by 2.9 percentage points in Massachusetts after reform on a base of 81.8 percent before reform.

Results in the fourth row suggest that hours increased by 1.3 hours per week in our preferred measure of L, which includes zero hours for individuals without a paid job. However, the increase in hours appears to entirely reflect an increase in employment. Among individuals with a paid job, hours decreased by 0.27 hours per week on a base of 38.3 hours per week before reform, or by 0.8 percent in the logarithmic specification. The next row shows that by focusing on the first job only

in our primary measure of L, we account for approximately 95 percent of hours in all jobs.

Taken together, these statistics suggest that Massachusetts experienced increased wages and increased hours overall, with some of the increase in wages and all of the increase in hours operating through increased employment. The increases in wages and hours that we observe are consistent with our model, which predicts small but ambiguously signed impacts on the aggregate labor market, given small numbers of individuals who switch ESHI status. We will need to use our regression framework with individual fixed effects to focus on those individuals who changed coverage due to the reform, isolating the key empirical variation.

Individuals who changed ESHI status as a result of the reform are critical to our identification. In the middle rows of Table 4, we compare insurance coverage in Massachusetts and other states. Massachusetts has higher insurance coverage rates than other states; approximately 91 percent of individuals between the ages of 18 and 64 in Massachusetts had some type of health insurance before reform, increasing to 95 percent after reform. Outside of Massachusetts, health insurance coverage stayed flat at 83 percent. The simple difference-in-differences estimate for the increase in coverage in Massachusetts due to the reform is 3.8 percentage points—slightly lower but consistent with existing estimates (Long 2008; Kolstad and Kowalski 2012; Yelowitz and Cannon 2010). We also see an increase in the rate of ESHI coverage in Massachusetts of 0.5 percentage points; however, this increase is not statistically significant.

Table 5 presents summary statistics for those switching ESHI status from before reform to after reform in Massachusetts and control states. Our estimates likely give us a local average treatment effect for the individuals who are induced to switch ESHI status by the Massachusetts reform, but this local average treatment effect will be policy relevant if similar individuals are induced to switch by the ACA. We see that those who switched ESHI status following reform in Massachusetts had lower hourly wages, fewer working hours, and lower rates of employment before reform than the average Massachusetts resident. The differences between those who switched ESHI status and the rest of the population are relatively small, but they underscore the need for us to use individual fixed effects in our regression specifications to account for compositional change in the population with ESHI. We also see that those switching ESHI status were slightly younger, less likely to be married, and more likely to be male than the population that did not change ESHI status. Finally, the bottom panel shows that those changing ESHI status were roughly equally likely to change from a small to a large firm and vice versa from the period from before reform to after reform. Their rate of firm size switching was also no higher than it was nationally, suggesting that the population that identifies our main results does not systematically move between firms based on the applicability of the employer penalty.

5 Results and Discussion

5.1 Graphical Analysis

We begin by presenting a graphical version of our baseline specification that allows us to investigate trends over time, after incorporating individual fixed effects. To do so, we run a regression analogous

Table 5: Summary Statistics for Individuals Changing ESHI Status in MA vs. Non-MA

	All States		MA	
	All	ESHI Switchers	All	ESHI Switchers
Unique Individuals observed Before and After Reform	23,239	4,030	626	87
w: Weekly earnings / baseline hours per week	14.040	8.487	18.175	9.151
w|paid job & w>0	20.370	15.440	24.877	16.488
Log(w|paid job & w>0)	2.746	2.457	2.936	2.531
L: Hours per week	29.129	23.319	30.643	22.120
L|paid job & L>0	39.008	36.737	38.366	33.162
Log(L|paid job & L>0)	3.607	3.526	3.571	3.344
Hours per week in all jobs	39.786	38.104	39.377	34.280
Paid job	0.781	0.700	0.824	0.709
Employed by Large Firm	0.851	0.825	0.849	0.794
Any Health Insurance	0.832	0.711	0.915	0.804
ESHI	0.656	0.357	0.736	0.314
<150%FPL†	0.197	0.311	0.134	0.311
150-300%FPL†	0.289	0.387	0.199	0.270
Age	40.088	37.556	40.467	38.548
Married	0.559	0.452	0.533	0.303
Female	0.507	0.515	0.513	0.502
FIRM SIZE CHANGES (before to after):				
Large to small	0.056	0.145	0.065	0.201
Small to large	0.077	0.198	0.065	0.171

Notes:
Sample includes 18-64 population, interview months only (4th-reference months).
ESHI switchers reports individuals who switched ESHI status between at least one inteview month before reform and at least one interview month after reform.
MA indicates in MA in at least one interview month.

to our baseline (no bracketed terms) specification given by Equation (3), where the only change is that we replace every instance of $After$ with a vector of all two-month periods in our data, including those before reform, omitting only the last two-month period before reform (May–June 2006).[26]

In Figure 3, we plot the vector of coefficients corresponding to β_{12} with the points connected by the dashed line labeled $ESHI$. This line gives the wage premium for jobs with ESHI relative to jobs without ESHI outside of Massachusetts—the empirical difference in wages between jobs with ESHI and jobs without ESHI. We also plot the vector of coefficients corresponding to β_1 with the points connected by the solid line labeled $MA * ESHI$. This line gives the differential wage premium for ESHI jobs relative to jobs without ESHI in Massachusetts relative to other states. Because individual fixed effects are included in the regressions, the coefficients are identified by people who change ESHI status in the given period relative to the omitted period outside and inside of Massachusetts, respectively. We also show 95 percent confidence intervals for both lines, clustered by state. In Figure 4, we plot the corresponding γ coefficients from a regression with hours as the dependent variable.

Outside of Massachusetts, we see that the wage premium for ESHI jobs stays fairly constant over time in Figure 3. Within Massachusetts before reform, the wage premium appears more variable, likely given the smaller sample size. However, trends in the wage premium are broadly similar in Massachusetts and in the other states before reform, lending support to our identification strategy.

Following the passage of reform in Massachusetts, we see a striking shift in the relationship between ESHI and wages for individuals who switch ESHI status. There is a substantial drop in

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[26]Because incorporating longitudinal variation through individual fixed effects places greater demands on the data, making the trend lines noisier, we combine each monthly response into mutually exclusive two-month periods, and we use all months available in the data instead of just interview months in these figures only.

Figure 3: Wage Premium for ESHI, Controlling for Individual Fixed Effects, MA vs. Non-MA
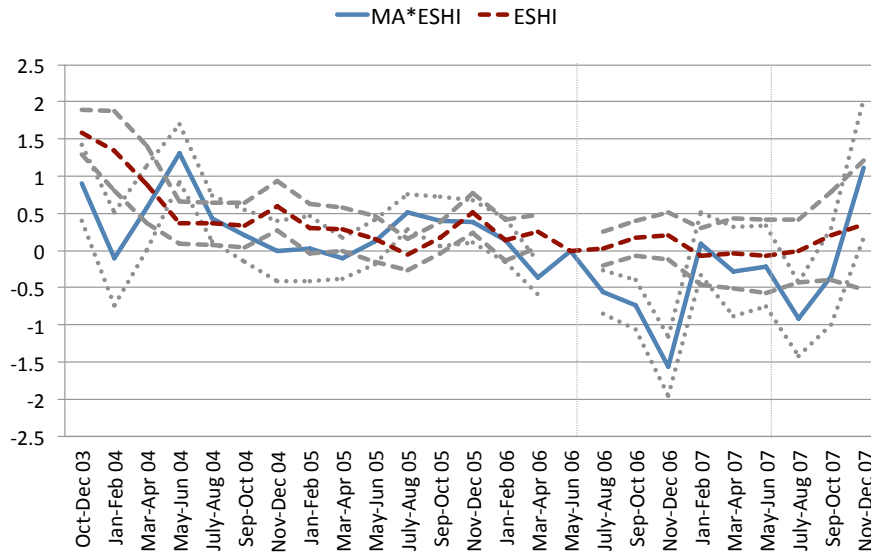
Regression coefficients with w as dependent variable. See text for details.
Wages and ESHI are two-month indicators. May-June 2006 are normalized to zero.

Figure 4: Hours Premium for ESHI, Controlling for Individual Fixed Effects, MA vs. Non-MA
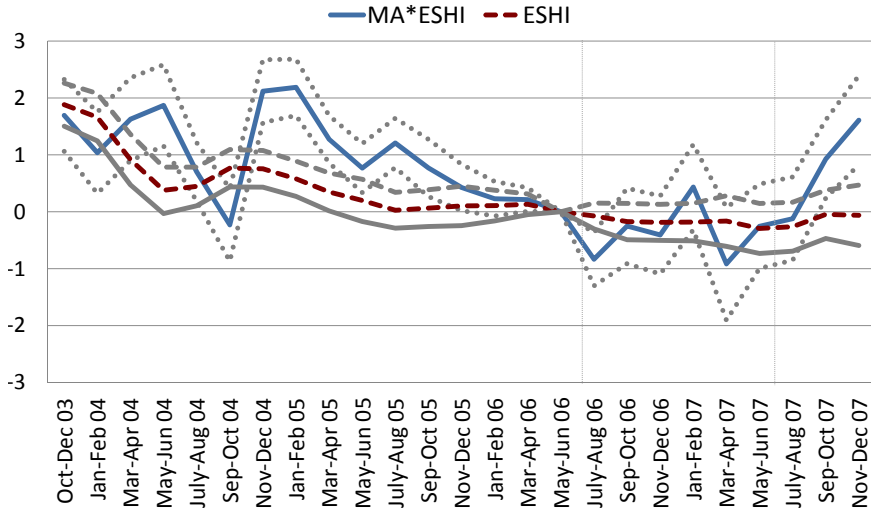
Regression coefficients with h as dependent variable. See text for details.
Hours and ESHI are two-month indicators. May-June 2006 are normalized to zero.

the wage premium for ESHI jobs relative to jobs without ESHI during and after the reform in Massachusetts relative to the period before the reform and relative to other states. We generally see a drop in the wage premium during each of the two-month periods after May–June 2006 except for the last two-month period, which shows a puzzling increase in both figures. The increase seems to be due to sizable attrition from the sample in the last two months and not to a real increase in the wage premium.[27] Our regression specification, which pools all data within the before, during, and after periods separately, places little weight on the visible uptick in the very last two-month period because of the small sample size from which it is drawn.

The results in Figure 4 suggest that, while trends in the hours premium are somewhat noisy in Massachusetts, they do not change dramatically in Massachusetts after the reform. The uptick that we observe in the last period of observation appears to be due to sample attrition, as in Figure 3. The overall small impact that we observe on the hours premium is consistent with the relatively large observed decline in the wage premium; if wages for those with ESHI fell by roughly the full the cost of ESHI to the employer, we would not expect a change in hours worked. The timing of the shift in the wage premium coincides well with the reform.[28]

Although the regression results formalize the magnitude of the decline in the wage premium after reform, we can learn something about the magnitude by examining Figures 3 and 4, keeping in mind that the last point gets the smallest weight. We should also keep in mind that we expect wages for jobs with and without ESHI to fall in Massachusetts after reform, and by analyzing coefficients that correspond to β_1 only, we are assuming that there is no employer penalty. The true compensating differential that takes the employer penalty into account will be larger than the effects we observe. In Figure 3, the magnitude of the decline in the wage premium for ESHI jobs in Massachusetts over the entire period during and after reform appears to be approximately \$1.5/hour, which corresponds to a roughly 7.5 percent decline. These figures provide the first evidence that the Massachusetts reform affected jobs with ESHI, as we predict in our model. They also signal that we have found an exogenous source of variation that will be useful in identifying a compensating differential for ESHI.

The shift that we observe in the relationship between ESHI and wages from before to after

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[27]If we examine the underlying data more closely, we see that there are approximately 3,000 observations (with multiple observations for approximately 1,600 unique individuals) in Massachusetts in each two-month period before reform, and approximately 1,000 observations (about 600 unique individuals) in Massachusetts in each two-month period after reform, with the exception of the last two-month period, Nov to Dec 2007, in which there are only 400 observations (273 unique individuals).

[28]Chandra, Gruber, and McKnight (2011) examine enrollment in the individual health insurance market and find a spike in enrollment in December 2007. The decline in the wage premium that we observe begins before July 2007, but it does not contradict their results because enrollment in ESHI to comply with the individual mandate would have had to occur earlier than enrollment in the individual health insurance market to comply with the individual mandate. Although individuals were free to enroll in the individual health insurance market at any time—allowing them to carefully time enrollment to comply with the rules of the law—employers generally confine enrollment in ESHI to one particular "open season," so individuals would need to enroll in ESHI earlier to satisfy the individual mandate in July 2007. In our data, we cannot observe open seasons, and it is difficult to verify enrollment timing, given issues with seam bias and reduced sample size after reform. Open enrollment frequently occurs on calendar or fiscal years. In either case, to comply by July 1, 2007, one would have had to sign up for ESHI in December 2006 or June 2007. Therefore, we would expect to see much of the impact of the reform prior to the precise implementation of the mandate, in the latter portion of 2006 or the first half of 2007.

reform in Massachusetts is particularly striking because it implies that individuals who switched into ESHI from before to after reform experienced declines in real wages that were larger than inflation, indicating that they experienced declines in nominal wages. The labor economics literature shows that it is very rare for workers who remain in the same job to accept declines in nominal wages. Since these figures do not condition on remaining in the same job, some of the decline could be due to workers who switch ESHI status at the same time that they switch jobs, and these workers could accept lower monetary wages if they receive health insurance. Alternatively, workers could have accepted lower nominal wages in the same job if the firm started providing health insurance. Because the reform in Massachusetts affected the universe of employers, it is plausible that it motivated compensation renegotiations, which allowed for nominal wage reductions. We know of no data on this, so we cannot say much about the detail of the mechanism. We do, however, return to these alternatives in a set of robustness checks that condition on remaining in the same job.

5.2 Regression Results

We report results from the baseline wage and hours equations in Table 6, and we begin our analysis by examining the coefficients directly. Recall that β_1 gives the compensating differential and γ_1 gives the hours differential if we assume that there is no employer penalty such that individuals without ESHI in Massachusetts after reform provide an additional control group for individuals with ESHI in Massachusetts after reform. That is, we do not allow for variation by firm size that distinguishes those who face the employer penalty in the group without ESHI. The estimated β_1 tells us that hourly wages are $0.85 lower for the same individuals when they have ESHI relative to when they do not have ESHI, after the reform relative to before the reform, in Massachusetts relative to other states. This coefficient is statistically significant at the 99 percent level. Annualizing the decrease in hourly wages for a full-time worker, this coefficient implies that the compensating differential for ESHI is $-\$1,759.68 \, (= -0.846 \times 40 \times 52)$ per year. This compensating differential is of the expected sign, standing in contrast to much of the literature.

To get a sense of what we expect the magnitude of the compensating differential for ESHI to be, recall that the absolute value of the compensating differential will be equal to the cost of ESHI to employers b if the employer penalty ρ is equal to zero and the penalty-and-subsidy-inclusive valuation $\alpha + \lambda - \mu_x$ is full. Before estimating b using our model, we can compare the initial results to an estimate from the Kaiser Family Foundation 2007 Survey of Employer Health Benefits (Kaiser Family Foundation 2007b). The average 2007 premium was $4,355 for an individual plan and $11,770 for a family plan (all amounts converted to 2006 dollars). The average employer contribution was 85 percent for an individual and 73 percent for a family plan. Using the share of the SIPP population with ESHI after reform who report having at least one child to calculate the share of family plans, the weighted average annual cost of health insurance was $6,105. Alternatively, using the same family definition and the share of the same SIPP population who switched from not having ESHI in 2006 to having it after reform, the average annualized b was $5,576. Both are larger than $1,760 (the compensating differential we estimate based on β_1); however, the assumption that the employer

Table 6: Results from Baseline Specification

		(1) w Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)		(2) L Hours per week, including individuals without a paid job (hours=0)
MA*ESHI*After	β_1	-0.846*** [-1.321, -0.277]	Y_1	-0.238 [-0.917, 0.301]
MA*ESHI*During	β_1^d	-0.745*** [-1.036, -0.346]	Y_1^d	-1.743*** [-2.201, -1.331]
MA*ESHI	β_8	1.128*** [0.717, 1.418]	Y_8	1.053*** [0.404, 1.383]
MA*After	β_{11}	1.728*** [1.297, 1.993]	Y_{11}	1.559*** [1.085, 2.028]
MA*During	β_{11}^d	1.191*** [0.856, 1.360]	Y_{11}^d	2.433*** [2.092, 2.723]
ESHI*After	β_{12}	-0.298 [-0.790, 0.170]	Y_{12}	-0.611** [-0.942, -0.108]
ESHI*During	β_{12}^d	-0.380** [-0.667, -0.098]	Y_{12}^d	-0.494*** [-0.695, -0.231]
ESHI	β_{19}	3.672*** [3.286, 3.872]	Y_{19}	6.416*** [5.939, 6.703]
After	β_{22}	0.629*** [0.226, 0.928]	Y_{22}	0.314 [-0.189, 0.721]
During	β_{22}^d	0.514*** [0.314, 0.722]	Y_{22}^d	0.302** [0.045, 0.517]
Observations		543,630		499,828
R-squared		0.738		0.805

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block
bootstrapped by state.
Including full 18-64 population.
Only includes interview months.
Individual and state fixed effects included. Monthly weights used.

penalty is zero should bias our estimate toward zero. Therefore, we expect our full estimates will be larger and more in line with actual costs of ESHI to employers.

In the second column of Table 6, our estimate of the hours differential using γ_1 tells us that weekly hours are -0.238 lower for jobs with ESHI relative to jobs without ESHI in Massachusetts relative to other states, after reform relative to before reform. Recall that if the penalty-and-subsidy-inclusive valuation of the benefit is full, the hours differential will be zero. The estimated coefficient is not statistically different from zero, and the standard errors rule out large increases or decreases in hours, consistent with a relatively high valuation of the benefit.

To extend these results, we turn to estimating our full model that incorporates variation by firm size. As we discussed in Section 3, this additional source of variation allows us to separately estimate the employer penalty and to allow for changes in the Massachusetts labor market unrelated to health reform. We also find empirical support for the inclusion of this additional variation in our

full specification.[29]

5.3 Estimates of the Compensating Differential for ESHI

To obtain estimates of the compensating differential and hours differential for ESHI, we estimate our full specification with firm size interaction terms, and we report the results in Table 7. This specification is more complicated than our baseline specification, so it is less intuitive to examine the coefficients directly. However, we can synthesize all of the relevant information in the coefficients by plotting the four empirical equilibria that map to the four theoretical equilibria presented in Figure 1. Figure 5 plots these empirical equilibria. All equilibria are relative to equilibrium A (no ESHI before reform) at the origin.

Figure 5: Empirical Estimates of Wage and Hours Equilibria
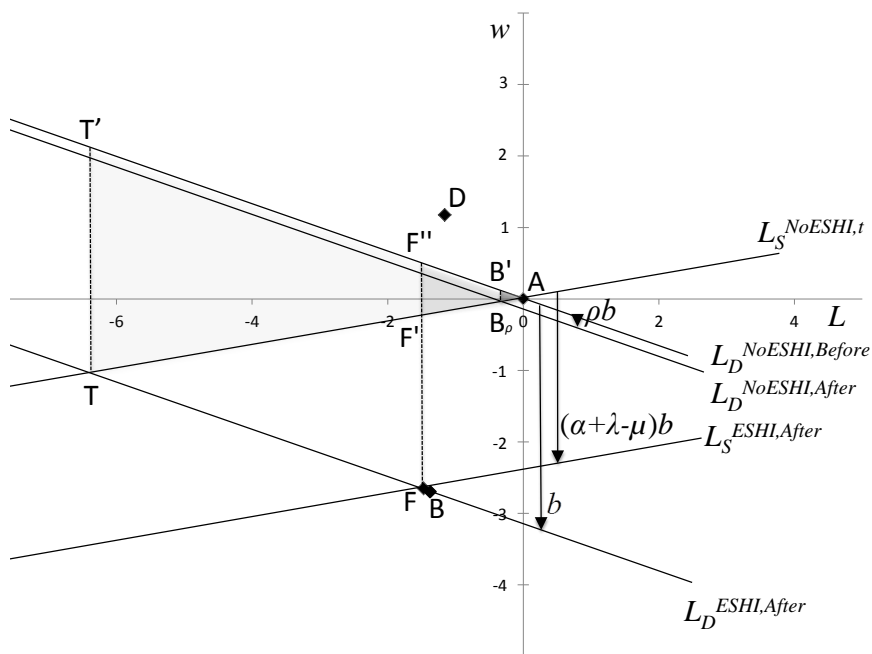
The most important relationship to notice in Figure 5 is that equilibrium F (ESHI after reform) is to the lower left of equilibrium A (no ESHI before reform), as predicted by our theory. The relationship between A and F is the best identified relationship in the figure. The compensating

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[29]If we do not assume that the employer penalty is zero, then the compensating differential from the baseline model is given by $\beta_1 + \beta_8 + \beta_{11} = -0.846 + 1.128 + 1.728 = 2.010$, which is of the wrong theoretical sign. Recall from Section 3.2 that if the employer penalty is small and there are no labor market changes in Massachusetts relative to other states after reform, we expect β_{11} to be small relative to β_1 and negative. Similarly, if our individual fixed effects allow us to identify the compensating differential convincingly without using variation in ESHI induced by reform, we expect β_8 to be small and negative relative to β_1. However, both are positive and of the same order of magnitude as β_1. Our estimated β_{11} suggests that something other than reform differentially affected the labor market in Massachusetts relative to other states (confirming our discussion of summary statistics), so we prefer the specification that incorporates variation by firm size. Our estimated β_8 suggests that we need variation in ESHI induced by reform to estimate the compensating differential for ESHI. For these reasons, we focus on estimates that compare point A to equilibrium F incorporating variation by firm size in the full specification.

Table 7: Results from Full Specification

		(1) w Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)		(2) L Hours per week, including individuals without a paid job (hours=0)		(3) Compensating and Hours Differentials, Sufficient Statistics, and Welfare Impact of Health Reform
MA*ESHI*After*Large	β_1	-1.264* [-2.328, 0.00400]	Y_1	-0.593 [-1.871, 0.425]	$w_D - w_A$	1.193*** [0.801, 1.482]
MA*ESHI*During*Large	β_1^d	-1.535*** [-2.485, -0.535]	Y_1^d	0.433 [-0.197, 1.125]	$w_F - w_B$	0.136 [-0.409, 0.542]
MA*ESHI*Large	β_8	1.059*** [0.558, 1.515]	Y_8	-2.001*** [-2.819, -1.332]	$w_D - w_B$	2.682*** [1.987, 3.341]
MA*After*Large	β_{11}	-1.488*** [-2.156, -0.869]	Y_{11}	-1.142** [-2.010, -0.233]	$w_F - w_A$	-1.352*** [-2.447, -0.528]
MA*During*Large	β_{11}^d	1.107*** [0.318, 2.025]	Y_{11}^d	-0.768*** [-1.438, -0.223]		
ESHI*After*Large	β_{12}	0.301 [-0.787, 1.249]	Y_{12}	-0.0484 [-1.083, 0.968]	$L_D - L_A$	0.42 [-0.293, 0.811]
ESHI*During*Large	β_{12}^d	0.561 [-0.322, 1.459]	Y_{12}^d	-0.414 [-0.896, 0.114]	$L_F - L_B$	0.185 [-0.939, 0.788]
ESHI*Large	β_{19}	1.844*** [1.365, 2.178]	Y_{19}	7.212*** [6.580, 7.800]	$L_D - L_B$	1.563*** [0.341, 2.286]
After*Large	β_{22}	-0.394 [-1.029, 0.165]	Y_{22}	-0.688** [-1.540, -0.00700]	$L_F - L_A$	-0.957** [-2.279, -0.115]
During*Large	β_{22}^d	-0.413 [-1.300, 0.119]	Y_{22}^d	-0.257 [-0.837, 0.151]		
Large	β_{23}	-2.458*** [-4.593, -2.045]	Y_{23}	-12.754** [-14.515, -11.061]		
MA*Large	β_{24}	-2.136 [-2.414, 0.183]	Y_{24}	3.124** [1.259, 5.140]		
MA*ESHI*After	β_{1e}	0.207 [-1.166, 1.497]	Y_{1e}	0.358 [-0.584, 1.255]		
MA*ESHI*During	β_{1e}^d	0.607 [-0.323, 1.716]	Y_{1e}^d	-1.853*** [-2.432, -1.211]	s	0.190 -
MA*ESHI	β_{8e}	0.135 [-0.442, 0.595]	Y_{8e}	2.422*** [1.664, 2.885]	d	-0.380 -
MA*After	β_{11e}	2.895*** [2.152, 3.581]	Y_{11e}	2.247*** [1.489, 2.963]	b	1.715*** [0.684, 3.059]
MA*During	β_{11e}^d	0.189 [-0.689, 0.812]	Y_{11e}^d	2.799*** [2.182, 3.345]	ρ	0.083*** [0.046, 0.208]
ESHI*After	β_{12e}	-0.528 [-1.728, 0.747]	Y_{12e}	-0.512 [-1.296, 0.297]	$\alpha + \lambda - \mu_x$	0.682*** [0.405, 0.936]
ESHI*During	β_{12e}^d	-0.843** [-1.829, -0.0150]	Y_{12e}^d	-0.149 [-0.559, 0.308]	$ESHI_{After}$	0.740 -
ESHI	β_{19e}	2.193*** [1.682, 2.603]	Y_{19e}	0.589*** [0.135, 0.952]	$\dfrac{b}{\tau}$	1.000 -
After	β_{22e}	0.925*** [0.296, 1.546]	Y_{22e}	0.829** [0.261, 1.487]	DWL_m	0.198*** [0.01, 1.1]
During	β_{22e}^d	0.842*** [0.292, 1.632]	Y_{22e}^d	0.523*** [0.105, 1.046]	$\dfrac{DWL_m}{DWL_\tau}$	0.077*** [0.012, 0.265]
Observations		543,630		499,828		
R-squared		0.739		0.815		

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Including full 18-64 population.
Only includes interview months.
Individual and state fixed effects included. Monthly weights used.

differential for ESHI from Table 1 is the negative of the vertical distance between equilibrium A and equilibrium F. As depicted in Figure 5, the third column of Table 7 shows that $w_F - w_A$ is equal to $-\$1.35$ per hour. Annualizing the point estimate for a full-time worker, the implied compensating differential is $-\$2,812$ per year, which corresponds closely to the average cost of ESHI to employers. This suggests that the magnitude of our estimate is in a plausible range and that the penalty-and-subsidy-inclusive valuation is high. The annualized 95 percent confidence interval on the compensating differential is $-\$5,090$ to $-\$1,098$ per year, obtained by block-bootstrapping by state.[30]

We obtain our preferred estimate of the hours differential for ESHI by taking the negative of the horizontal distance between equilibrium A and equilibrium F. As depicted in Figure 5, the third column of Table 7 shows that $L_F - L_A$ is equal to -0.96 hours per week. While small in magnitude, this estimate is statistically different from zero at the 5 percent level.

Another key relationship to notice is that equilibrium B (no ESHI after reform) is also to the lower left of equilibrium A (no ESHI before reform), as predicted by our theory. However, the theory also predicts that equilibrium B should have higher wages than equilibrium F when the penalty is small (the hours comparison is ambiguous because it also depends on the magnitude of the penalty-and-subsidy-inclusive valuation), but empirically we see that equilibrium B has slightly lower wages than equilibrium F, leading to a wrong-signed compensating differential $w_F - w_B$. Recall, however, that the relationship between equilibrium B and equilibrium A is not actually identified by changes in ESHI status due to reform. When we compute the welfare impact of reform, we identify B by calibrating the employer penalty ρ, and we plot the analog of equilibrium B that corresponds to the calibrated ρ as the point B_ρ.

Finally, theory tells us that equilibrium D (ESHI after reform) should be to the lower left of equilibrium A (no ESHI before reform), but the empirical equilibrium D is to the upper left of equilibrium A. As shown in the top portions of the third column of Table 7, which do not depend on calibration, the compensating and hours differentials, based on equilibrium D are wrong-signed. The observed relationship between equilibrium D and equilibrium A is yet another manifestation of the well-known issue in the literature that it is difficult to find a correctly signed compensating differential without exogenous variation in benefits. The estimate of D relative to A comes from individuals who switch into and out of ESHI before reform. Because we cannot convincingly identify D, we are not able to separately identify the components of the penalty-and-subsidy-inclusive valuation of health insurance, α and $\lambda - \mu$, and our separate estimates of these parameters are unreasonable. Although these parameters would be interesting to analyze, we do not need to separately

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[30]To obtain all confidence intervals, we perform a simple nonparametric block bootstrap. We first draw a sample of 51 states with replacement—the data include all 50 U.S. states and the District of Columbia—drawing all observations within a state as a block. We then estimate the wage and hours equations on the same sample, thus taking into account that the same individuals are used to estimate the wage and hours equations. To include as much data as possible, we drop observations with missing values of either outcome after selecting the replication sample, thereby assuming that individuals with missing data have the same behavior as those without missing data. We repeat the sampling process for 350 replications. For each quantity in column 3, we report the 0.025 quantile and the 0.975 quantile as our 95 percent confidence interval. We report significance when this confidence interval or the analogous 90 percent or 99 percent confidence interval does not include zero.

identify them to identify the aggregate welfare impact of mandate-based health reform.

5.4 Estimates of the Welfare Impact of Health Reform

We next translate our preferred estimates of the compensating and hours differentials into the welfare impact of health reform. Up to this point, our results have come directly from the regression coefficients, and we have not made any calibrations. In theory, all of the sufficient statistics for the deadweight loss for health reform given by Equation (1) are identified. However, as we discuss above, we have reason to believe that the identification for equilibrium B and equilibrium D is not convincing, and plotting the empirical equilibria gives us further cause to doubt their identification. For this reason, we rely only on the difference between labor market equilibria that is identified by variation due to the Massachusetts reform (equilibrium F relative to equilibrium A), and calibrate other sufficient statistics as shown in Table 8.[31]

Table 8: Calibrated Sufficient Statistics

Calibrated Sufficient Statistics	Calibrated Values with Wages and Hours
s	0.190
d	-0.380
b	$\frac{d(L_F - L_A) - (w_F - w_A)}{(295/(52*40))}$
ρ	$\frac{b}{b}$
$\alpha + \lambda - \mu_x$	$\frac{s(L_F - L_A) - (w_F - w_A)}{b}$
$ESHI_{After}$	0.74
b/τ	1
DWL_m	$\frac{b^2}{2(s-d)}((1 - (\alpha + \lambda - \mu_x))^2 ESHI_{After} + \rho^2(1 - ESHI_{After}))$
$DWL_m/DWL\tau$	$(\frac{b}{\tau})^2((1 - (\alpha + \lambda - \mu_x))^2 ESHI_{After} + \rho^2(1 - ESHI_{After}))$

The bottom portion of the third column of Table 7 gives the corresponding estimated values. As shown, we obtain a value of 1.72 for b, which translates into \$3,566 annually for a full-time worker. This number is somewhat smaller than the rough cost of $ESHI$ estimated from the Kaiser survey data of \$6,105. We obtain an estimated value of 0.68 for the penalty-and-subsidy-inclusive valuation, which suggests that workers value health expenditures made by their employers at about \$0.68 per \$1.[32] The magnitude of the penalty-and-subsidy-inclusive valuation is quite high. In fact,

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[31]We discuss our choices of calibrated values in Section 3.3. The formulas in this table are drawn from the formulas in Table 3 and Equations (1) and (2). The formulas in the lower rows of the table reference values calibrated in the upper rows.

[32]As discussed above, the estimate of \$0.68 per \$1 includes the tax preference for ESHI. Based on income, the vast majority of respondents in our sample fall in the 10 and 15 percent brackets (less than 20 percent fall in the other possible 25, 28, 33, and 35 percent brackets). However, these tax brackets overstate actual marginal tax rates because approximately 46 percent of households actually pay no federal income tax because of exemptions (Johnson et al. 2011). State and payroll taxes increase all marginal tax rates by approximately 10 percent. Therefore, most of our respondents have a penalty-and-subsidy-inclusive valuation of at least 48 cents on the dollar, excluding the tax preference, which is still quite high.

the 95 percent confidence interval for the penalty-and-subsidy-inclusive valuation ranges up to 0.94.

We find that the deadweight loss of mandate-based health reform is equal to $10 per year ($= 0.198 \times 12$). Relative to tax-based health reform, mandate-based health reform is substantially more efficient: we calculate $DWL_m/DWL_\tau = 0.077$; the distortions that mandate-based health reform induces are only 8 percent of the distortions induced by tax-based health reform.[33] The confidence interval reported in Table 7 suggests that we are 95 percent certain that the deadweight loss of mandate-based health reform is between 1.2 percent and 26.5 percent of the deadweight loss of tax-based health reform. This substantial efficiency is perhaps not surprising as it follows directly from our estimate of a relatively high penalty-and-subsidy-inclusive valuation for $ESHI$.

5.5   Robustness of the Empirical Results

In Appendix C, we consider the robustness of the empirical results. In Appendix C.1, we examine the robustness of our empirical results to the calibrated values, and we show that our main finding that the deadweight loss of mandate-based health reform is substantially smaller than the deadweight loss of tax-based health reform is robust to a wide range of calibrated values. In Appendix C.2, we examine robustness to the estimation sample and show that the results are similar when we restrict our sample to individuals in New England and married individuals. When we restrict our sample to individuals in New England, we find that the ratio of the deadweight loss of mandate-based health reform to the ratio of the deadweight loss of tax-based health reform is 0.1 percent—which is smaller but qualitatively similar to our preferred estimate of 7.7 percent. In Appendix C.3, we examine continuous workers separately from workers who move into and out of employment, and we find that we still observe a compensating differential among continuous workers. Finally, in Appendix C.4, we show that our results are *not* robust to the exclusion of individual fixed effects—individual fixed effects are essential to identification.

5.6   Implications for National Reform

The impact of mandate-based reform in Massachusetts is, of course, interesting its own right. However, because the Massachusetts reform bore all of the same key features as national health reform, we can use our model and our estimates to reach some conclusions about the potential welfare impact of the ACA. While we do not model all of the underlying structure that would be required to fully predict the impact of national reform, because of the many similarities between both reforms, given our sufficient statistics approach, we expect the welfare impacts to be similar to the extent the policy parameters are equivalent in Massachusetts and under the ACA. Since the magnitudes

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[33]We find that the deadweight loss of tax-based health reform is $134 per year. A tax of size $\tau = b =$ $1.71 per hour, would raise $2,429 per person per year ($1.71 per hour $\times$ (30.26 hours per week in Massachusetts before reform - 3.01 hours per week after reform) $\times$ 52 weeks per year), which would not be large enough to finance the estimated average annual cost of ESHI per worker of $3,566. Even if we only require that tax-based reform insure as many individuals as mandate-based reform, 95 percent of individuals in the Massachusetts experience, the government would still need to collect $3,387 per worker ($3,566 \times 0.95$). Therefore, we are conservative in setting $\tau = b$. Under the tax, the ratio of the deadweight loss to revenue raised is 0.06, which is on the lower end of the range but consistent with prominent estimates from the literature such as Ballard, Shoven, and Whalley (1985) and Feldstein (1999).

of the individual penalty, employer penalty, and subsidies differ across the reforms, we can use our model to consider the robustness of our main conclusions. We can also use our model to consider the impact of other differences between Massachusetts and the rest of the nation.

The relatively larger individual penalty under the ACA suggests that the national reform could be even more efficient than the Massachusetts reform. The individual penalty increases the penalty-and-subsidy-inclusive valuation, decreasing distortion. Thus, while the labor market distortion of mandate-based health reform in Massachusetts was only 8 percent of the distortion of tax-based health reform, a larger individual penalty would decrease the ratio even further. On the other hand, the employer penalty is also larger under ACA than it is in Massachusetts, which increases distortion. In practice, the size of the employer penalty is unlikely to change our basic conclusions on the efficiency of mandate-based reform. As we show in Appendix C.1, our finding that mandate-based health reform is substantially more efficient than tax-based health reform is robust to the larger value of the penalty.

Subsidies under national reform extend to a larger population because incomes are lower outside of Massachusetts and subsidy thresholds are higher under national reform, increasing distortion to the labor market. However, the subsidy dollar amounts are lower under national reform, decreasing distortion. Adding further complexity to the comparison, the recent Supreme Court decision on the ACA eliminated the ability of the federal government to require that states expand Medicaid or face a complete elimination of existing Medicaid funds. This decision might reduce subsidies available in some states under the ACA, decreasing distortion.

Even if the statutory parameters were the same, firms and individuals outside of Massachusetts could respond differently to reform. For example, some analysts predict that firms will be less likely to offer ESHI and individuals will be less likely to take up ESHI under national reform. Our model treats a firm's decision to offer health insurance and an individual's decision to take up health insurance as exogenous, largely because we do not observe whether a firm offers health insurance in our data—we only observe individuals who take up ESHI. However, were we to extend our model, we would predict that the larger employer penalties under the ACA would result in larger ESHI offer rates. We would also predict that the different tax incentives under the ACA would result in larger employee ESHI take up rates—under the ACA, the tax advantage for the employee portion of premiums will only be available for plans offered by employers, even though Massachusetts employees could pay pretax for plans offered by the Connector (Massachusetts DHCFP 2011a). The larger ESHI offer rates would increase labor market distortion, but the larger valuation of ESHI would decrease labor market distortion relative to what we observed in Massachusetts.

Finally, there could be general equilibrium changes to health insurance markets under national reform that our analysis of the Massachusetts reform does not capture. For example, compliance with the reform in Massachusetts was high, mitigating adverse selection in the market for health insurance outside of employment (see Hackmann, Kolstad, and Kowalski [2012,2015] for evidence of adverse selection in Massachusetts prior to the reform). If compliance with national reform is not as high, adverse selection could remain high in the market for health insurance outside of employment, making the outside alternative to ESHI less attractive. In terms of our model,

although adverse selection in the non-employer-sponsored market should not affect the cost of health insurance to employers b, it could affect the value of a dollar of ESHI relative to a dollar of wages α because employees will value ESHI more if their outside health insurance option is more expensive. In that case, more adverse selection in the market for health insurance outside of employment nationally could actually decrease the reform-induced distortion to the labor market relative to what we observed in Massachusetts.

6 Conclusion

The recent Massachusetts and national health reforms are the most profound changes to health policy in the United States since the introduction of Medicare and Medicaid in 1965. Since employers sponsor the majority of health insurance coverage for the nonelderly in the United States, changes to health policy can affect the labor market profoundly. To study the relationship between health reform and the labor market, we develop a model that incorporates the three key elements of mandate-based health reform: employer and individual pay-or-play mandates and expansions in subsidized coverage. Using our model, we characterize the compensating differential for ESHI. We also characterize the welfare impact of the labor market distortion induced by health reform in terms of a small number of sufficient statistics that can be recovered from labor market outcomes. By modeling all three elements at once, we show that if an individual mandate is already in place, then the addition of an employer mandate increases deadweight loss. This theoretical result has some relevance to the ACA employer mandate, which has not yet been enforced as of this writing.

Using variation from the Massachusetts reform—which includes the same mandate-based reform elements as the national reform—we estimate our model using longitudinal data from the Survey of Income and Program Participation. We find evidence of a substantial compensating differential for ESHI: full-time workers who gained coverage because of the Massachusetts reform earned lower wages than they would have had they not gained ESHI by $2,812 per year, a significant portion of the average cost of their health insurance to their employers. Our finding stands in stark contrast to the results from the extensive literature that searches for a compensating differential for ESHI but does not find one. Because of difficulties with identification, studies generally find that individuals with ESHI have *higher* wages than those without. A small number of studies do find evidence in favor of a compensating differential, showing that wages for workers with ESHI decrease as health insurance costs increase. However, these studies use variation in *incremental changes* in the cost of health insurance. We identify the compensating differential using variation in the *entire* cost of health insurance using reform-induced exogenous transitions into and out of ESHI.

Building on our estimated compensating differential, we estimate the welfare impact of the labor market distortion induced by health reform. Our large estimated compensating differential indicates that individuals who gained ESHI were willing to accept lower wages because they valued the coverage that they received. We estimate that individuals who gained coverage through their employers valued approximately $0.68 of every dollar that their employers spent on their coverage. Because individuals valued ESHI, mandate-based health reform in Massachusetts resulted in signif-

icantly less distortion to the labor market than it would have under alternative policies to expand insurance coverage. We estimate that if the government had instead increased insurance coverage by establishing a wage tax to pay for health insurance, the distortion to the labor market would have been roughly 13 times as large. Our results suggest that mandate-based reform has the potential to be a very efficient approach for expanding health insurance coverage nationally.

Finally, our results help to explain why ESHI coverage increased in Massachusetts relative to other states following reform, despite anticipation that other sources of coverage would crowd out ESHI (see Kolstad and Kowalski [2012]). Although it seemed plausible that employers would stop offering coverage, instead of electing to pay the small employer penalty, between 2005 and 2009, the rate of employers offering ESHI increased from 70 percent to 76 percent in Massachusetts while it remained flat nationally (Massachusetts DHCFP 2011a). Our model and results suggest that the individual mandate, combined with the large penalty-and-subsidy-inclusive valuation of ESHI, encouraged workers to demand ESHI from their employers, which they paid for out of decreased wages. If employers have a comparative advantage in offering health plans that their employees value, then we expect crowd-in to ESHI—precisely what we observed in Massachusetts. To the extent that ESHI is even more valuable relative to other coverage under national reform than it was in Massachusetts, perhaps because of the difference in the relative tax preference between reforms, we could see even greater crowd-in to employer-sponsored coverage under national reform than we did in Massachusetts.

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Appendix A Massachusetts and National Reform Comparison

Table A1: Summary of Labor Market Provisions in Massachusetts and National Reforms

	Massachusetts Health Care Reform, April 2006	Patient Protection and Affordable Care Act (PPACA), March 2010
"Large" Employer	At least 11 employees[1,2]	At least 50 full-time employees[3]
Provisions Affecting Large Employers	Must either: • Offer employees the option to purchase health coverage,[5] OR • Pay an annual penalty per employee[1] In addition, employers: • Must offer the option to pay the premium using pre-tax wages[5] • Are not required to contribute towards the premium (but may pay penalties if they do not)[5]	Must either: • Offer employees affordable health coverage options,[4] OR • Pay an annual penalty per employee[3] Affordable coverage defined as: • Insurance coverage at least 60% of covered expenses,[3] AND • Employees not required to pay more than 9.5% of family income for coverage[3,4]
"Small" Employer	Fewer than 11 employees	Fewer than 50 employees
Provisions Affecting Small Employers	May purchase coverage for employees via the Commonwealth Health Insurance Connector, which:[1] • Offers access to health insurance options approved by a State board • Merges the individual and small business insurance markets	Very small businesses (fewer than 25 employees) may: • Be eligible for a tax credit for offering health insurance if average wages are under $50,000[3,4]
Penalties (Large Employers)	Must pay a penalty of $295 per employee per year, if: • The employer does not offer health insurance options,[1] OR • The employer contributes less than 33% of the premium[2] Must also pay a penalty if employees use the uncompensated care pool[2]	Two types of penalties: • Must pay $2,000 per full-time employee for not offering **any** insurance options[3,4] • Must pay $3,000 (up to a maximum) for not offering affordable coverage, for all employees receiving a tax credit for insurance purchased on exchange[3,4] Penalties increase annually for premium growth. Not assessed for first 30 employees[3,4]
Provisions Affecting Individuals	Individuals are required either to: • Buy creditable health insurance,[1,7] OR • Pay a penalty, if the cost of coverage has been deemed affordable[6,7] Individuals with incomes below 300% of poverty can access subsidized health insurance:[7] • <150% of poverty pay no premium[1] • 151-200% pay $35 per month[1] • Up to 300% receive subsidies	Individuals are required either to: • Purchase "qualifying" health coverage,[8] OR • Pay a penalty, with some exemptions available[8] Provides subsidies/access to coverage for low-income individuals: • <133% of poverty become eligible for Medicaid coverage, effectively 138% after deducting 5% of poverty[8,9] • Up to 400% receive premium/cost-sharing credits towards purchase via the exchanges. Credits increase with income, limiting contributions from 2% to 9.5% of income[8]
Penalties (Individuals)	Individuals who do not purchase affordable coverage, but are in income brackets with affordable coverage available, face penalties:[7] • Initially, $219 per individual • Starting in 2008, up to 50% of the cost of the least expensive coverage	Individuals not purchasing coverage face a penalty of the greater of: • $695 (annually) to a maximum of three times this amount,[8] OR • 2.5% of household income[8] These amounts phased in beginning in 2014[8]

Notes: (1) Kaiser Family Foundation (2007a), (2) Felland et al (2007), (3) Kaiser Family Foundation (2010d), (4) Anonymous (2011), (5) Commonwealth Connector (2007), (6) Kaiser Family Foundation (2009), (7) Kaiser Family Foundation (2010b), (8) Kaiser Family Foundation (2010c).

Appendix B Comparison of SIPP to Alternative Survey Data

Figures B1 and B2 plot wages and hours in Massachusetts and non-Massachusetts states between 2004 and 2011 for three data sets in addition to the SIPP: the CPS, the MEPS, and the ACS. We also include data from the 2008 SIPP panel. It is clear that the 2004 SIPP panel captures the same trends as the other datasets, with the only deviation coming from an uptick in the last period of observation. As we discuss, the last period of observation is affected by attrition from the sample in the last survey month and though an uptick is visible, it does not impact our estimation substantially due to the weighting of the regression results. The 2008 SIPP panel continues with trends similar to the remainder of the country and to the 2004 SIPP panel.

We also note that the last period of the 2008 SIPP panel appears to have an uptick in wages, suggesting that the attrition from the sample we see in the 2004 panel is a general issue with the SIPP and does not reflect an unobserved change in Massachusetts the last two months of 2007. Taken together, these trends suggest that the 2004 SIPP is representative of Massachusetts and control states and does not obscure substantial change in Massachusetts in the period after our data ends (2008 onwards).

Figure B1: Wage Levels in MA vs. Non-MA from Survey Data

Weekly earnings / baseline hours per week, including without paid job (wage=0) [converted to 2006$].
Full 18-64 population. Survey weights are used to calculate means.
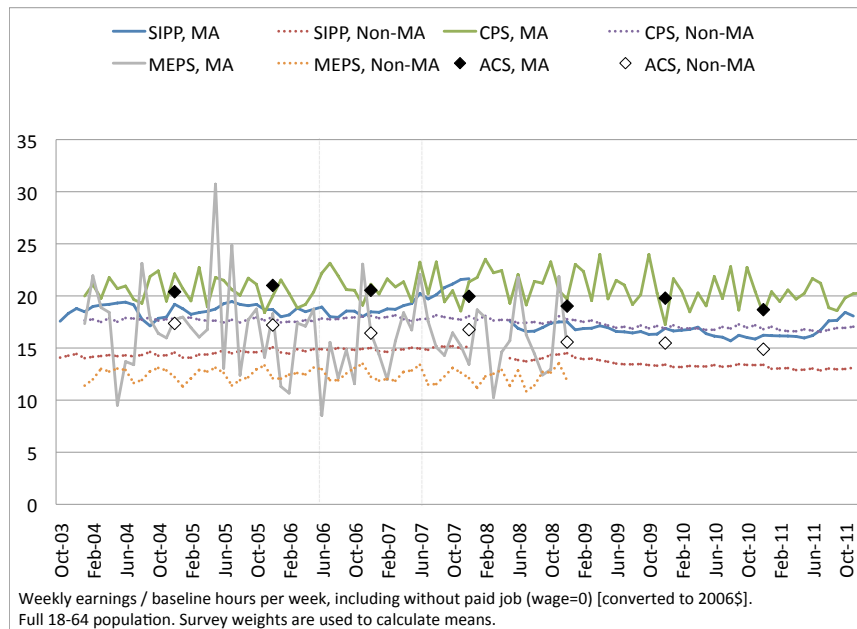
Figure B2: Hours Levels in MA vs. Non-MA from Survey Data

Hours per week. Full 18-64 population. Hours=0 if no paid job with the exception of MEPS data (set to missing). Survey weights are used to calculate means.
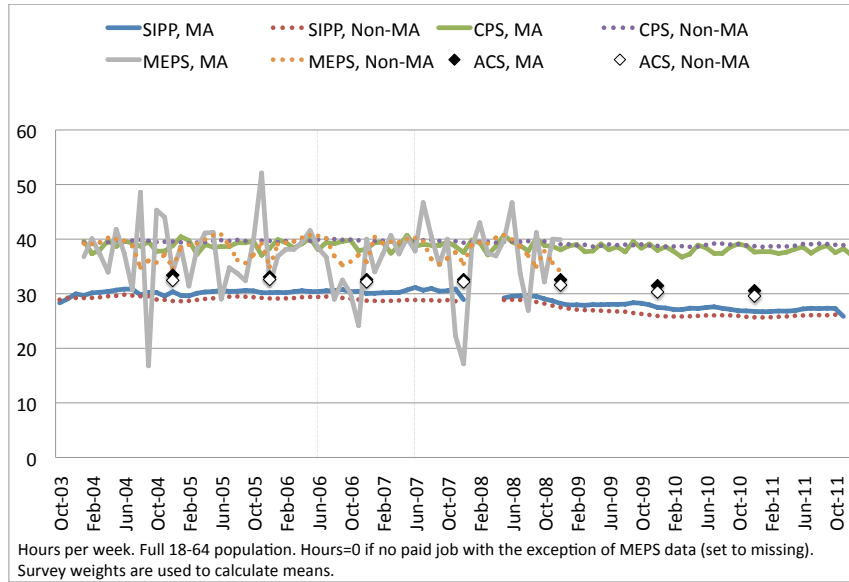
Appendix C ROBUSTNESS OF THE EMPIRICAL RESULTS

Appendix C.1 Robustness to Calibrated Values

Thus far, we have discussed point estimates for the welfare impact of health reform, but we are also interested in their robustness. Although the reported confidence intervals should be of the correct size for the compensating and hours differentials, the other confidence intervals should be too small because they reflect calibrated values, which were themselves estimated elsewhere. Therefore, it is instructive to consider robustness to alternative calibrated values.

First consider alternative values of ρ and $ESHI_{After}$. These values have little impact on our overall conclusion that mandate-based health reform is substantially more efficient than tax-based health reform in Massachusetts. Our preferred calibrated ρ is 0.083, reflecting that the statutory employer penalty of $295 is approximately 8.3 percent of the estimated cost of ESHI $\widehat{b}$. If we increase ρ such that the penalty is instead 31.8 percent of the estimated cost of ESHI, the deadweight loss for individuals without ESHI after reform is equal to the deadweight loss for individuals with ESHI after reform, which is approximately 10 percent of the deadweight loss of mandate-based health reform. In this case, $ESHI_{After}$ has no impact because the size of both triangles is the same.

As discussed in Section 1, employer penalties under the ACA are substantially larger than those under the Massachusetts reform, up to a maximum of $3,000 per employee annually, approximately 84 percent of the estimated cost of ESHI. However, the deadweight loss for individuals without

ESHI after mandate-based reform is only 7.7 percent of the deadweight loss of tax-based reform; this is because triangle $B_\rho AB'$ grows with the square of the penalty, but triangle TAT' grows with the square of the cost of ESHI. Taking into account the triangle $F'AF''$, the overall welfare cost of mandate-based reform is only 25.9 percent of the welfare cost of tax-based reform.

Next, consider alternative values for the loading cost of ESHI relative to the loading cost of government-provided health insurance, b/τ, keeping all other values the same as in our full specification. Suppose that ESHI costs 10 percent more to provide than government-provided health insurance because the government has economies of scale relative to employers, so b/τ=1.1. The deadweight loss of tax-based health reform decreases to \$111 annually, but the deadweight loss of mandate-based health reform is still only 9.3 percent as large. Even if b/τ=1.50 such that ESHI costs 50 percent more to provide than government-provided health insurance, the deadweight loss of mandate-based health reform is still only 17.2 percent of the deadweight loss of tax-based health reform.

The last calibrated values to consider are the slope of the supply curve s and the slope of the demand curve d. To examine the effect of s and d on the ratio of the deadweight loss of mandate-based health reform to the deadweight loss of tax-based health reform, we see from Equation (2) that the ratio of the deadweight loss of mandate-based health reform to the deadweight loss of tax-based health reform grows with the square of the percentage of the cost of ESHI that workers do not value: $(1 - \alpha + \widehat{\lambda - \mu_x})$. Using the expressions in Table 3, we can express this percentage in terms of the compensating and hours differentials, the slope of the demand curve, and the slope of the supply curve. We find that the relative deadweight loss of mandate-based reform increases as the slope of the labor supply curve increases (becomes more inelastic) and increases as the slope of the labor demand curve decreases (becomes more elastic). Holding demand constant, if we increase the calibrated labor supply elasticity from 0.1 to 0.2 (from $s = 0.19$ to $s = 0.38$), the relative deadweight loss increases to 14.5 percent. If we increase it further to 0.3 ($s = 0.57$), the relative deadweight loss increases to 20.7 percent. Alternatively, holding supply constant, if we decrease the calibrated labor demand elasticity from -0.2 to -0.4 (from $d = -0.38$ to $d = -0.76$), the relative deadweight loss increases to 14.3 percent. If we decrease it further to -1.2 ($d = -2.28$), the relative deadweight loss increases to 33.1 percent. Thus, the finding that mandate-based health reform is efficient relative to tax-based health reform is robust to changes in calibrated labor supply and demand.

The slopes of the supply and demand curves do, however, fix the incidence of the deadweight loss of health reform on employees versus their employers. As we can see from Figure 5, as supply becomes less elastic, a larger fraction of each deadweight loss triangle is below the L axis, demonstrating that employees bear more of the burden of reform. Conversely, as demand becomes less elastic, a larger fraction of each deadweight loss triangle is above the L axis, demonstrating that employers bear more of the burden of reform.

Appendix C.2   Robustness to Different Estimation Samples

Thus far, our model has taken individual ESHI takeup decisions as exogenous. Therefore, individuals who switched into ESHI because of reform are representative of all individuals, and we have estimated the penalty-and-subsidy-inclusive valuation for the population. However, we can extend our model to make ESHI status endogenous by allowing underlying valuations, and thus penalty-and-subsidy-inclusive valuations, to vary across individuals. In this extended model, after allowing for some optimization error, individuals with a penalty-and-subsidy-inclusive valuation above a certain threshold purchase health insurance in each period. Individuals with the highest intrinsic valuation of health insurance α already have health insurance before reform. The reform will increase penalty-and-subsidy-inclusive valuations for some individuals, leading them to take up ESHI. Interpreted in light of the extended model, our estimates then reflect the average penalty-and-subsidy-inclusive valuation among individuals who take up ESHI. Therefore, our estimated valuation of 0.68 from our full specification suggests that individuals who take up ESHI because of reform value it at 68 cents on the dollar on average (after incorporating the individual penalty and taking the tax-preference for ESHI into account).

Under the extended model with endogenous takeup of ESHI, we can test whether the penalty-and-subsidy-inclusive valuation (and thus the incidence of reform among employees) varies across different populations by estimating our model on subsets of our estimation sample. Under our original model, the same specifications test the robustness of our estimates to alternative samples and control groups. We examine our baseline and full specifications on two subsets of the full population: individuals in New England and individuals who are married.

In the first column of Tables C1 and C2, we restrict our estimation sample to include only individuals in New England, on the grounds that Massachusetts might be more similar to other New England states than it is to the rest of the country. Table C1 reports results from the baseline specification on the New England sample. The estimates of β_1 and γ_1 (the compensating and hours differentials assuming that the employer penalty is zero, respectively), are slightly larger in magnitude than the corresponding estimates from the baseline specification. However, the compensating and hours differentials from the full specification are very similar to those from the full sample. In the sample that includes only New England, the annualized estimate of the cost of ESHI b is $10,997, much larger than our preferred estimate ($3,566), but the penalty-and-subsidy-inclusive valuation of 0.89 is also closer to 1 than our preferred estimate of 0.68. Furthermore, the ratio of the deadweight loss of mandate-based health reform to the ratio of the deadweight loss of tax-based health reform is 0.1 percent—which is smaller but qualitatively similar to our preferred estimate of 7.7 percent.

In the second columns of Tables C1 and C2, we restrict our estimation sample to include only married individuals. Married individuals could value ESHI less than other individuals if they have health insurance options available through their spouses; alternatively, they could also value it more if their spouse relies on them for insurance. Empirically, we see in Table C2 that the valuation of ESHI for married individuals is approximately 0.56, with a 95 percent confidence interval of 0.47 to

Table C1: Results from Baseline Specification on Different Samples

		(1a)	(1b)		(2a)	(2b)
		w			L	
		Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)			Hours per week, including individuals without a paid job (hours=0)	
SAMPLE		*New England*	*Married*		*New England*	*Married*
MA*ESHI*After	β_1	-4.415***	-0.232	Y_1	-1.739**	1.977***
		[-7.288, -2.776]	[-0.908, 0.459]		[-3.235, -0.111]	[1.282, 2.662]
MA*ESHI*During	β_1^d	-1.862**	1.392***	Y_1^d	-2.318**	-0.441**
		[-4.124, -0.00600]	[0.941, 1.783]		[-4.508, -0.237]	[-1.040, -0.0510]
MA*ESHI	β_8	2.148**	0.762***	Y_8	2.149**	-0.912***
		[0.330, 6.524]	[0.310, 1.254]		[0.373, 3.995]	[-1.616, -0.498]
MA*After	β_{11}	3.214***	1.280***	Y_{11}	1.171	-1.077***
		[1.885, 4.442]	[0.750, 1.801]		[-0.368, 2.421]	[-1.662, -0.403]
MA*During	β_{11}^d	1.405**	-0.820***	Y_{11}^d	2.497**	0.844***
		[0.187, 3.277]	[-1.258, -0.386]		[0.459, 4.371]	[0.511, 1.445]
ESHI*After	β_{12}	3.276***	-0.623*	Y_{12}	0.742	-0.312
		[1.661, 6.114]	[-1.309, 0.061]		[-0.869, 2.373]	[-0.781, 0.407]
ESHI*During	β_{12}^d	0.806	-0.522***	Y_{12}^d	-0.0560	-0.291
		[-0.901, 3.097]	[-0.896, -0.132]		[-1.945, 2.164]	[-0.576, 0.285]
ESHI	β_{19}	2.276	3.580***	Y_{19}	4.912***	5.383***
		[-1.403, 4.127]	[3.077, 3.823]		[2.901, 6.511]	[4.803, 5.780]
After	β_{22}	-1.107	0.231	Y_{22}	0.613	-0.929***
		[-2.166, 0.174]	[-0.220, 0.681]		[-0.777, 2.121]	[-1.670, -0.435]
During	β_{22}^d	0.120	0.311	Y_{22}^d	0.171	-0.484***
		[-1.713, 1.314]	[-0.0690, 0.665]		[-1.754, 2.179]	[-1.071, -0.160]
Observations		31,131	302,175		28,589	277,477
R-squared		0.670	0.760		0.814	0.834

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.

Including full 18-64 population; only includes interview months.

Individual and state fixed effects included. Monthly weights used.

New England states include MA, CT, NH, VT, ME, RI.

		(1a)	(1b)		(2a)	(2b)		(3a)	(3b)
		w			L			Compensating and Hours Differentials, Sufficient Statistics, and Welfare Impact of Health Reform	
		Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)			Hours per week, including individuals without a paid job (hours=0)				
SAMPLE		*New England*	*Married*		*New England*	*Married*		*New England*	*Married*
MA*ESHI*After*Large	β_1	-2.064	-2.135***	Y_1	-4.552	3.268***	$w_D - w_A$	1.792**	0.244
		[-5.701, 3.948]	[-4.271, -0.604]		[-10.591, 3.707]	[0.945, 4.444]		[0.337, 5.02]	[-0.143, 0.68]
MA*ESHI*During*Large	β_1^d	-1.440	-0.820	Y_1^d	0.109	2.078***	$w_F - w_B$	-3.109	-0.573**
		[-4.548, 3.210]	[-1.763, 0.463]		[-5.145, 4.501]	[0.767, 2.766]		[-6.794, 1.599]	[-1.277, -0.117]
MA*ESHI*Large	β_8	0.231	-2.641***	Y_8	-2.189	-7.236***	$w_D - w_B$	3.58	5.718***
		[-2.923, 2.564]	[-3.807, -1.869]		[-5.624, 1.584]	[-8.146, -6.311]		[-0.854, 11.208]	[4.279, 6.978]
MA*After*Large	β_{11}	-1.788	-5.473***	Y_{11}	0.997	-6.723***	$w_F - w_A$	-4.897**	-6.046***
		[-6.598, 1.833]	[-6.556, -4.119]		[-5.149, 6.431]	[-7.756, -4.417]		[-8.274, -1.726]	[-7.458, -4.838]
MA*During*Large	β_{11}^d	-0.0916	-0.905**	Y_{11}^d	-1.576**	-2.644***			
		[-4.982, 2.871]	[-1.676, -0.0350]		[-3.195, -0.116]	[-3.191, -1.255]			
ESHI*After*Large	β_{12}	1.092	0.613	Y_{12}	3.777	-0.294	$L_D - L_A$	1.025	-2.293***
		[-4.543, 4.444]	[-0.728, 2.164]		[-4.366, 9.645]	[-1.378, 1.737]		[-0.846, 2.314]	[-3.14, -1.867]
ESHI*During*Large	β_{12}^d	0.574	0.357	Y_{12}^d	0.0871	-0.411	$L_F - L_B$	-2.02**	0.16
		[-3.709, 3.352]	[-0.845, 1.217]		[-4.205, 4.488]	[-1.114, 0.775]		[-3.239, -1.011]	[-1.132, 1.02]
ESHI*Large	β_{19}	2.521**	1.822***	Y_{19}	7.145***	6.663***	$L_D - L_B$	0.028	4.43***
		[0.491, 5.487]	[1.045, 2.720]		[3.691, 10.522]	[5.728, 7.512]		[-5.781, 4.955]	[2.127, 5.673]
After*Large	β_{22}	-0.358	-0.138	Y_{22}	-2.982	-0.861	$L_F - L_A$	-1.023	-6.564***
		[-3.577, 4.219]	[-1.500, 0.873]		[-8.119, 2.691]	[-2.938, 0.138]		[-6.464, 4.311]	[-7.554, -5.091]
During*Large	β_{22}^d	0.488	-0.107	Y_{22}^d	0.162	-0.199			
		[-2.085, 5.376]	[-0.871, 0.527]		[-0.966, 1.615]	[-1.463, 0.501]			
Large	β_{23}	-9.536***	-2.616***	Y_{23}	-16.532**	-15.919**			
		[-12.211, -2.407]	[-5.088, -1.644]		[-20.432, -10.877]	[-17.111, -6.294]			
MA*Large	β_{24}	5.094	1.146*	Y_{24}	7.034***	14.250**			
		[-2.004, 7.877]	[-0.0190, 3.646]		[1.269, 11.24]	[4.544, 15.502]			
MA*ESHI*After	β_{1e}	-2.838*	1.318	Y_{1e}	1.506	-0.815			
		[-7.661, 0.267]	[-0.596, 3.581]		[-4.096, 7.407]	[-1.633, 0.788]			
MA*ESHI*During	β_{1e}^d	-0.605	2.066***	Y_{1e}^d	-2.442	-2.000***	s	0.190	0.190
		[-5.702, 1.446]	[0.876, 2.980]		[-6.461, 1.676]	[-2.543, -1.273]		-	-
MA*ESHI	β_{8e}	1.561	2.886***	Y_{8e}	3.214	4.943***	d	-0.380	-0.380
		[-1.779, 7.743]	[2.053, 4.098]		[-1.795, 6.294]	[3.976, 5.604]		-	-
MA*After	β_{11e}	4.770**	6.231***	Y_{11e}	0.737	4.735***	b	5.285**	8.54***
		[1.590, 9.243]	[4.948, 7.412]		[-4.736, 4.965]	[3.100, 5.494]		[0.605, 10.495]	[7.089, 9.969]
MA*During	β_{11e}^d	1.442	-0.0845	Y_{11e}^d	3.874**	2.859***	ρ	0.027**	0.017***
		[-1.125, 6.659]	[-0.958, 0.734]		[0.915, 6.152]	[1.977, 3.345]		[0.013, 0.157]	[0.014, 0.02]
ESHI*After	β_{12e}	2.517*	-1.163	Y_{12e}	-1.721	-0.0956	$\alpha + \lambda - \mu_x$	0.89**	0.562***
		[-0.262, 7.354]	[-3.114, 0.516]		[-7.306, 3.845]	[-1.306, 0.731]		[0.353, 3.012]	[0.474, 0.643]
ESHI*During	β_{12e}^d	0.333	-0.825	Y_{12e}^d	0.128	0.0194	$ESHI_{After}$	0.740	0.740
		[-1.509, 5.335]	[-1.759, 0.247]		[-3.698, 4.330]	[-0.646, 0.570]		-	-
ESHI	β_{19e}	0.507	2.119***	Y_{19e}	-0.442	0.0171	<u>b</u>	1.000	1.000
		[-4.973, 3.708]	[1.065, 2.814]		[-3.416, 4.156]	[-0.658, 0.601]	τ	-	-
After	β_{22e}	-0.980	0.343	Y_{22e}	2.377	-0.155	DWL_m	0.225***	9.09***
		[-5.060, 1.829]	[-0.697, 1.561]		[-1.625, 7.625]	[-0.876, 1.217]		[0.007, 9.272]	[5.471, 12.038]
During	β_{22e}^d	-0.335	0.386	Y_{22e}^d	-0.269	-0.271	<u>DWL_m</u>	0.009***	0.142***
		[-5.164, 2.076]	[-0.325, 1.224]		[-2.620, 2.289]	[-0.789, 0.524]	DWL_τ	[0, 5.488]	[0.095, 0.205]
Observations		31,131	302,175		28,589	277,477			
R-squared		0.671	0.761		0.823	0.841			

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.

Including full 18-64 population; only includes interview months.

All specifications include individual, state, and state*large firm fixed effects. Monthly weights used.

Large firm defined as >25 employees.

New England states include MA, CT, NH, VT, ME, RI.

0.64, slightly smaller than the valuation of ESHI for the full sample.

Appendix C.3 Robustness to Intensive Margin Only

Thus far, we have not distinguished the extensive margin decision of whether to work at all from the intensive margin decision to work a different number of hours in our measure of L. Instead, we have attempted to capture the broadest possible impact of reform by allowing for responses on the intensive and extensive margins. However, previous research, including Cutler and Madrian (1998), shows that ESHI could have different impacts on employment than it does on hours because ESHI has a fixed cost, regardless of hours worked. Furthermore, part-time employees often do not receive health insurance from their employers.

We now investigate whether we observe responses on the intensive margin and whether the distinction between the intensive and extensive margins affects our findings. Unfortunately, our model does not allow us to examine extensive margin decisions directly.[34] Instead, we restrict our sample to individuals who worked; effectively limiting the response to the intensive margin. Comparing these estimates to our preferred results allows us to test whether the distinction between extensive and intensive margin effects drives our findings. We first include only individuals with a paid job and positive wages in a given period. We then further restrict our sample to include only individuals with a paid job and positive wages over the entire period, and then further to include only individuals with no job switch over the entire period. We adjust the calibrated values of s and d to reflect the higher average wages and hours. Because these three samples only include people with positive wages and hours, we can also estimate logarithmic specifications without losing any information. In the logarithmic specifications, our theoretical graph stays the same, the axes change from w to $log(w)$ and from L to $log(L)$. With the change in axes, the compensating differential and the cost of the benefit are percentages of wages instead of dollar amounts, and the hours differential is a percentage of hours. However, the units of the penalty-and-subsidy-inclusive valuation and the deadweight loss ratio do not change.

Table C3 presents results from the baseline specification on the three samples of workers, using levels and logarithms of the dependent variables. In all samples, our estimates of β_1 give evidence of a compensating differential, assuming no employer penalty. The logarithmic specifications show a compensating differential from 6 percent to 10 percent of income, broadly consistent with our preferred results. The estimates of γ_1, however, do not show any evidence of a negative hours differential. If anything, the effect is positive in the only specification with a coefficient estimate that is significant at the 95 percent confidence level. These results suggest that much of the small decline in hours that we observe in the full sample is driven by the extensive margin decision of whether to work.

In the results from the full specification restricted to workers, shown in Table C4, we continue

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[34]Within our model, we cannot redefine L as an indicator variable for having a paid job, because all individuals with ESHI must have a paid job, so equilibrium D would always be above equilibrium F. Alternatively, if we instead aggregate our data, defining L as the fraction of individuals with a paid job, we cannot take advantage of longitudinal variation.

Table C3: Results from Baseline Specification to Investigate Intensive Margin

		(1a) w Weekly earnings / baseline hours per week	(1b) Log(w) Weekly earnings / baseline hours per week	(1c) w Weekly earnings / baseline hours per week	(1d) Log(w) Weekly earnings / baseline hours per week	(1e) w Weekly earnings / baseline hours per week	(1f) Log(w) Weekly earnings / baseline hours per week
SAMPLE		Workers with a paid job and wages>0 in the given period		Workers with a paid job and wages>0 over the entire period		Workers with a paid job and wages>0 and no job switch over the entire period	
MA*ESHI*After	β_1	-2.376***	-0.100***	-2.652***	-0.0612***	-3.729***	-0.0729***
		[-2.811, -1.831]	[-0.124, -0.0630]	[-3.422, -2.283]	[-0.0850, -0.0280]	[-4.233, -3.031]	[-0.109, -0.0366]
MA*ESHI*During	β_1^d	-2.044***	-0.182***	-2.496***	-0.206***	-2.554***	-0.206***
		[-2.426, -1.388]	[-0.202, -0.148]	[-2.917, -1.736]	[-0.233, -0.169]	[-3.084, -1.824]	[-0.243, -0.169]
MA*ESHI	β_8	1.501***	0.108***	1.474***	0.100***	1.631***	0.106***
		[1.051, 1.688]	[0.0820, 0.121]	[0.815, 1.778]	[0.0710, 0.115]	[0.906, 1.959]	[0.0717, 0.141]
MA*After	β_{11}	2.902***	0.126***	3.153***	0.0831***	3.720***	0.0788***
		[2.526, 3.179]	[0.0950, 0.144]	[2.887, 3.705]	[0.0510, 0.102]	[3.221, 4.070]	[0.0402, 0.117]
MA*During	β_{11}^d	2.229***	0.190***	2.706***	0.212***	2.627***	0.215***
		[1.741, 2.457]	[0.161, 0.203]	[2.093, 3.008]	[0.175, 0.233]	[2.061, 3.001]	[0.183, 0.246]
ESHI*After	β_{12}	-0.441*	-0.0704***	-0.319	-0.0562***	-0.273	-0.0588***
		[-1.001, 0.005]	[-0.104, -0.0430]	[-0.828, 0.353]	[-0.0830, -0.0290]	[-0.855, 0.235]	[-0.0913, -0.0264]
ESHI*During	β_{12}^d	-0.551***	-0.0421***	-0.483**	-0.0361**	-0.533**	-0.0342**
		[-0.999, -0.236]	[-0.0630, -0.0220]	[-1.033, -0.0370]	[-0.0560, -0.00300]	[-1.070, -0.0250]	[-0.0622, -0.00624]
ESHI	β_{19}	1.827***	0.157***	1.786***	0.148***	1.753***	0.136***
		[1.527, 2.124]	[0.141, 0.166]	[1.491, 2.171]	[0.129, 0.163]	[1.381, 2.163]	[0.119, 0.152]
After	β_{22}	1.452***	0.129***	1.251***	0.103***	0.955***	0.0933***
		[1.072, 1.743]	[0.0990, 0.155]	[0.739, 1.460]	[0.0720, 0.124]	[0.565, 1.245]	[0.0606, 0.126]
During	β_{22}^d	1.058***	0.0801***	0.982***	0.0705***	0.865***	0.0583***
		[0.759, 1.430]	[0.0590, 0.100]	[0.618, 1.450]	[0.0360, 0.0900]	[0.447, 1.353]	[0.0293, 0.0873]
Observations		370,996	370,996	307,438	307,438	281,624	281,624
R-squared		0.792	0.749	0.790	0.762	0.794	0.771

		(2a) L Hours per week	(2b) Log(L) Hours per week	(2c) L Hours per week	(2d) Log(L) Hours per week	(2e) L Hours per week	(2f) Log(L) Hours per week
SAMPLE		Workers with a paid job and wages>0 in the given period		Workers with a paid job and wages>0 over the entire period		Workers with a paid job and wages>0 and no job switch over the entire period	
MA*ESHI*After	Υ_1	-0.255	-0.00409	0.734***	0.0346***	-0.0480	0.0102
		[-0.594, 0.180]	[-0.0190, 0.0130]	[0.417, 1.286]	[0.0240, 0.0590]	[-0.489, 0.601]	[-0.0135, 0.0338]
MA*ESHI*During	Υ_1^d	-1.694***	-0.0304***	-2.217***	-0.0405***	-2.397***	-0.0461***
		[-1.955, -1.366]	[-0.0420, -0.0180]	[-2.431, -1.635]	[-0.0480, -0.0170]	[-2.864, -2.017]	[-0.0664, -0.0258]
MA*ESHI	Υ_8	1.659***	0.0433***	1.981***	0.0618***	2.191***	0.0690***
		[1.070, 1.960]	[0.0240, 0.0530]	[1.384, 2.348]	[0.0410, 0.0740]	[1.517, 2.557]	[0.0418, 0.0961]
MA*After	Υ_{11}	0.657***	0.0181**	-0.643***	-0.0252***	-0.0980	-0.00888
		[0.156, 0.971]	[0.00100, 0.0300]	[-1.301, -0.378]	[-0.0490, -0.0160]	[-0.868, 0.329]	[-0.0336, 0.0159]
MA*During	Υ_{11}^d	2.137***	0.0439***	2.445***	0.0521***	2.798***	0.0636***
		[1.670, 2.371]	[0.0280, 0.0520]	[1.825, 2.556]	[0.0260, 0.0550]	[2.388, 3.153]	[0.0475, 0.0796]
ESHI*After	Υ_{12}	-0.827***	-0.0328***	-0.687***	-0.0267***	-0.567**	-0.0248**
		[-1.190, -0.493]	[-0.0460, -0.0180]	[-1.045, -0.229]	[-0.0370, -0.00900]	[-1.137, -0.193]	[-0.0435, -0.00615]
ESHI*During	Υ_{12}^d	-0.614***	-0.0235***	-0.594***	-0.0232***	-0.520***	-0.0201***
		[-0.805, -0.454]	[-0.0310, -0.0160]	[-0.810, -0.354]	[-0.0310, -0.0130]	[-0.783, -0.237]	[-0.0319, -0.00834]
ESHI	Υ_{19}	1.833***	0.0684***	1.763***	0.0642***	1.651***	0.0606***
		[1.593, 1.983]	[0.0600, 0.0740]	[1.431, 1.984]	[0.0530, 0.0720]	[1.343, 1.884]	[0.0498, 0.0714]
After	Υ_{22}	0.857***	0.0393***	0.580**	0.0266***	0.361*	0.0225**
		[0.516, 1.223]	[0.0260, 0.0520]	[0.0850, 0.950]	[0.00800, 0.0360]	[-0.0550, 0.977]	[0.00423, 0.0408]
During	Υ_{22}^d	0.728***	0.0311***	0.660***	0.0286***	0.518***	0.0234***
		[0.540, 0.952]	[0.0220, 0.0400]	[0.376, 0.886]	[0.0170, 0.0370]	[0.223, 0.832]	[0.0107, 0.0361]
Observations		344,315	344,315	287,144	287,144	263,033	263,033
R-squared		0.720	0.697	0.707	0.678	0.718	0.691

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Including full 18-64 population; only includes interview months.
Individual and state fixed effects included. Monthly weights used.

Table C4: Results from Full Specification to Investigate Intensive Margin

		(1a) w Weekly earnings / baseline hours per week	(1b) Log(w) Weekly earnings / baseline hours per week	(1c) w Weekly earnings / baseline hours per week	(1d) Log(w) Weekly earnings / baseline hours per week	(1e) w Weekly earnings / baseline hours per week	(1f) Log(w) Weekly earnings / baseline hours per week
SAMPLE		Workers with a paid job and wages>0 in the given period		Workers with a paid job and wages>0 over the entire period		Workers with a paid job and wages>0 and no job switch over the entire period	
MA*ESHI*After*Large	β_1	1.682*** [0.999, 2.756]	0.358*** [0.320, 0.419]	2.920*** [1.825, 3.971]	0.615*** [0.570, 0.679]	2.611*** [1.192, 3.823]	0.656*** [0.581, 0.732]
MA*ESHI*During*Large	β_1^d	-0.532 [-1.420, 0.619]	0.0213 [-0.0160, 0.0720]	-0.0587 [-1.375, 1.120]	0.0869** [0.0320, 0.133]	0.615 [-0.871, 1.745]	0.119*** [0.0595, 0.178]
MA*ESHI*Large	β_8	3.124*** [2.681, 3.610]	0.118*** [0.0910, 0.141]	3.564*** [3.043, 4.244]	0.113*** [0.0890, 0.157]	3.264*** [2.564, 3.876]	0.104*** [0.0653, 0.143]
MA*After*Large	β_{11}	-2.635*** [-3.584, -1.615]	-0.356*** [-0.415, -0.309]	-4.742*** [-5.720, -3.166]	-0.650*** [-0.710, -0.597]	-4.363*** [-5.596, -2.736]	-0.691*** [-0.770, -0.612]
MA*During*Large	β_{11}^d	0.518 [-0.307, 1.654]	-0.000265 [-0.0530, 0.0380]	-0.242 [-1.228, 1.392]	-0.112*** [-0.159, -0.0590]	-0.679 [-1.592, 1.071]	-0.140*** [-0.195, -0.0849]
ESHI*After*Large	β_{12}	-0.599 [-1.487, 0.274]	-0.0695*** [-0.119, -0.0150]	-0.293 [-1.054, 0.912]	-0.0681* [-0.107, 0.00300]	0.186 [-0.891, 1.370]	-0.0625* [-0.136, 0.0113]
ESHI*During*Large	β_{12}^d	-0.00128 [-0.936, 0.958]	-0.0539** [-0.0950, -0.00800]	0.420 [-0.643, 1.671]	-0.0400 [-0.0810, 0.0180]	0.478 [-0.698, 1.792]	-0.0367 [-0.0920, 0.0185]
ESHI*Large	β_{19}	0.677** [0.190, 1.054]	0.0736*** [0.0510, 0.0950]	0.617** [0.033, 1.241]	0.0689*** [0.0390, 0.0990]	0.616* [-0.0500, 1.184]	0.0651*** [0.0349, 0.0952]
After*Large	β_{22}	0.785 [-0.302, 1.678]	0.0507* [-0.00700, 0.102]	0.814 [-0.757, 1.764]	0.0570 [-0.00900, 0.0960]	0.579 [-0.789, 1.789]	0.0579 [-0.0203, 0.136]
During*Large	β_{22}^d	0.203 [-0.968, 0.967]	0.0344 [-0.0130, 0.0750]	-0.123 [-1.639, 0.822]	0.0222 [-0.0350, 0.0580]	0.000 [-1.586, 1.004]	0.0271 [-0.0275, 0.0817]
Large	β_{23}	1.140 [-2.200, 1.807]	0.00659 [-0.110, 0.0630]	0.738 [-2.727, 1.618]	-0.0705 [-0.135, 0.0350]	2.128 [-2.930, 2.768]	-0.0133 [-0.116, 0.0896]
MA*Large	β_{24}	-1.243 [-1.975, 2.199]	-0.0222 [-0.0880, 0.108]	-1.865 [-2.739, 1.715]	-0.0336 [-0.155, 0.0320]	-3.712 [-4.437, 1.479]	-0.0804 [-0.190, 0.0288]
MA*ESHI*After	β_{1e}	-3.124*** [-4.075, -2.436]	-0.321*** [-0.365, -0.288]	-4.002*** [-5.176, -3.271]	-0.454*** [-0.506, -0.420]	-4.805*** [-5.881, -3.592]	-0.486*** [-0.545, -0.426]
MA*ESHI*During	β_{1e}^d	-1.672*** [-2.408, -0.763]	-0.196*** [-0.231, -0.151]	-2.285*** [-3.218, -1.152]	-0.245*** [-0.284, -0.193]	-2.755*** [-3.774, -1.680]	-0.261*** [-0.315, -0.207]
MA*ESHI	β_{8e}	-0.873*** [-1.444, -0.531]	0.0167 [-0.0150, 0.0400]	-1.234*** [-2.051, -0.739]	0.00925 [-0.0430, 0.0330]	-0.749** [-1.571, -0.197]	0.0260 [-0.0245, 0.0765]
MA*After	β_{11e}	4.489*** [3.625, 5.099]	0.347*** [0.298, 0.381]	6.086*** [5.306, 7.042]	0.506*** [0.463, 0.545]	6.300*** [5.240, 7.104]	0.520*** [0.464, 0.576]
MA*During	β_{11e}^d	1.879*** [0.693, 2.471]	0.186*** [0.139, 0.222]	2.774*** [1.276, 3.552]	0.274*** [0.223, 0.310]	2.916*** [1.450, 3.716]	0.289*** [0.238, 0.340]
ESHI*After	β_{12e}	-0.116 [-0.934, 0.688]	-0.0240 [-0.0670, 0.0120]	-0.276 [-1.291, 0.579]	-0.0123 [-0.0650, 0.0190]	-0.597 [-1.660, 0.393]	-0.0214 [-0.0710, 0.0281]
ESHI*During	β_{12e}^d	-0.587 [-1.424, 0.219]	-0.00417 [-0.0440, 0.0300]	-0.794 [-1.797, 0.261]	-0.00678 [-0.0480, 0.0400]	-0.929* [-1.956, 0.133]	-0.00966 [-0.0578, 0.0384]
ESHI	β_{19e}	1.317*** [0.907, 1.717]	0.101*** [0.0810, 0.119]	1.327*** [0.677, 1.861]	0.0960*** [0.0680, 0.123]	1.304*** [0.655, 1.817]	0.0870*** [0.0600, 0.114]
After	β_{22e}	0.969*** [0.390, 1.650]	0.0985*** [0.0660, 0.141]	0.760** [0.0610, 1.605]	0.0690*** [0.0410, 0.113]	0.630 [-0.201, 1.528]	0.0603*** [0.0158, 0.105]
During	β_{22e}^d	0.928*** [0.321, 1.983]	0.0589*** [0.0280, 0.100]	1.043*** [0.293, 2.320]	0.0568*** [0.0190, 0.100]	0.859** [0.0400, 2.202]	0.0425* [-0.00371, 0.0886]
Observations		370,996	370,996	307,438	307,438	281,624	281,624
R-squared		0.792	0.750	0.790	0.763	0.794	0.772

Table C4: Results from Full Specification to Investigate Intensive Margin (Continued)

		(2a) L Hours per week	(2b) Log(L) Hours per week	(2c) L Hours per week	(2d) Log(L) Hours per week	(2e) L Hours per week	(2f) Log(L) Hours per week
SAMPLE		Workers with a paid job and wages>0 in the given period		Workers with a paid job and wages>0 over the entire period		Workers with a paid job and wages>0 and no job switch over the entire period	
MA*ESHI*After*Large	Y_1	5.761***	0.215***	11.883***	0.480***	12.771***	0.498***
		[5.358, 6.567]	[0.204, 0.245]	[11.085, 13.147]	[0.454, 0.527]	[11.780, 13.719]	[0.462, 0.534]
MA*ESHI*During*Large	Y_1^d	2.377***	0.0787***	5.711***	0.199***	5.638***	0.206***
		[1.755, 2.903]	[0.0540, 0.100]	[4.670, 6.083]	[0.154, 0.220]	[4.536, 6.233]	[0.162, 0.251]
MA*ESHI*Large	Y_8	1.244***	0.0544***	-0.134	0.0218	0.106	0.0248*
		[0.665, 1.585]	[0.0350, 0.0650]	[-0.786, 0.372]	[-0.00400, 0.0370]	[-0.624, 0.720]	[-0.00241, 0.0519]
MA*After*Large	Y_{11}	-6.243***	-0.260***	-12.60***	-0.544***	-12.21***	-0.541***
		[-7.000, -5.584]	[-0.288, -0.240]	[-13.544, -11.280]	[-0.580, -0.508]	[-13.201, -11.216]	[-0.578, -0.504]
MA*During*Large	Y_{11}^d	-2.557***	-0.0885***	-6.162***	-0.231***	-5.647***	-0.220***
		[-3.209, -1.923]	[-0.114, -0.0660]	[-6.680, -5.331]	[-0.253, -0.199]	[-6.384, -4.731]	[-0.253, -0.187]
ESHI*After*Large	Y_{12}	-0.828***	-0.0275***	-0.766	-0.0279	-0.692*	-0.0307*
		[-1.436, -0.256]	[-0.0510, -0.00900]	[-1.584, 0.199]	[-0.0560, 0.00300]	[-1.540, 0.0170]	[-0.0632, 0.00182]
ESHI*During*Large	Y_{12}^d	-0.833**	-0.0288**	-0.779*	-0.0266*	-0.629*	-0.0246*
		[-1.228, -0.294]	[-0.0460, -0.00800]	[-1.229, -0.152]	[-0.0470, 0.00200]	[-1.254, 0.0940]	[-0.0514, 0.00226]
ESHI*Large	Y_{19}	0.794***	0.0240***	0.651***	0.0178***	0.452**	0.0117
		[0.530, 1.121]	[0.0120, 0.0350]	[0.400, 1.096]	[0.00600, 0.0350]	[0.0400, 0.897]	[-0.00757, 0.0310]
After*Large	Y_{22}	0.509	0.0110	0.586	0.0177	0.502	0.0185
		[-0.159, 1.110]	[-0.0100, 0.0320]	[-0.558, 1.366]	[-0.0210, 0.0440]	[-0.286, 1.255]	[-0.0147, 0.0518]
During*Large	Y_{22}^d	0.544*	0.0120	0.601*	0.0151	0.550	0.0144
		[-0.0420, 0.977]	[-0.0140, 0.0330]	[-0.0310, 1.046]	[-0.0120, 0.0350]	[-0.285, 1.120]	[-0.0134, 0.0423]
Large	Y_{23}	-2.100	-0.0621	-1.527	-0.0328	-0.501	-0.0143
		[-2.597, 4.995]	[-0.0760, 0.258]	[-2.015, 7.011]	[-0.0520, 0.392]	[-0.999, 6.232]	[-0.0492, 0.0206]
MA*Large	Y_{24}	2.053	0.0565	2.330	0.0429	1.400	0.0336
		[-5.041, 2.853]	[-0.263, 0.0840]	[-6.070, 3.228]	[-0.375, 0.0800]	[-5.303, 2.368]	[-0.0123, 0.0794]
MA*ESHI*After	Y_{1e}	-3.789***	-0.131***	-6.904***	-0.268***	-8.296***	-0.303***
		[-4.529, -3.252]	[-0.160, -0.112]	[-8.162, -6.135]	[-0.311, -0.246]	[-9.240, -7.228]	[-0.342, -0.264]
MA*ESHI*During	Y_{1e}^d	-3.202***	-0.0792***	-5.816***	-0.161***	-5.896***	-0.170***
		[-3.724, -2.629]	[-0.0970, -0.0590]	[-6.137, -4.876]	[-0.170, -0.125]	[-6.503, -5.110]	[-0.198, -0.142]
MA*ESHI	Y_{8e}	0.613**	-0.00170	1.913***	0.0380***	2.032***	0.0460**
		[0.0220, 1.127]	[-0.0210, 0.0160]	[1.133, 2.608]	[0.0130, 0.0650]	[1.087, 2.836]	[0.00541, 0.0867]
MA*After	Y_{11e}	4.601***	0.182***	7.600***	0.331***	7.656***	0.340***
		[3.841, 5.159]	[0.159, 0.205]	[6.397, 8.355]	[0.295, 0.360]	[6.571, 8.542]	[0.302, 0.378]
MA*During	Y_{11e}^d	3.796***	0.101***	6.437***	0.201***	6.321***	0.200***
		[3.102, 4.357]	[0.0780, 0.121]	[5.483, 6.824]	[0.166, 0.212]	[5.506, 6.995]	[0.172, 0.228]
ESHI*After	Y_{12e}	-0.241	-0.0122	-0.177	-0.00713	-0.114	-0.00333
		[-0.834, 0.356]	[-0.0300, 0.0100]	[-0.904, 0.647]	[-0.0290, 0.0220]	[-0.856, 0.604]	[-0.0319, 0.0252]
ESHI*During	Y_{12e}^d	-0.0257	-0.00173	-0.0786	-0.00428	-0.132	-0.00297
		[-0.425, 0.286]	[-0.0150, 0.0110]	[-0.517, 0.326]	[-0.0220, 0.0110]	[-0.589, 0.326]	[-0.0192, 0.0133]
ESHI	Y_{19e}	1.218***	0.0491***	1.265***	0.0500***	1.296***	0.0507***
		[0.876, 1.427]	[0.0370, 0.0590]	[0.814, 1.499]	[0.0310, 0.0590]	[0.758, 1.656]	[0.0298, 0.0717]
After	Y_{22e}	0.542**	0.0326***	0.225	0.0158	0.0720	0.0115
		[0.0140, 1.244]	[0.0130, 0.0530]	[-0.504, 0.949]	[-0.0100, 0.0380]	[-0.551, 1.023]	[-0.0145, 0.0375]
During	Y_{22e}^d	0.385**	0.0236***	0.298	0.0196**	0.201	0.0151
		[0.00900, 0.844]	[0.0100, 0.0390]	[-0.107, 0.676]	[0.00400, 0.0330]	[-0.263, 0.691]	[-0.00387, 0.0341]
Observations		344,315	344,315	287,144	287,144	263,033	263,033
R-squared		0.720	0.698	0.708	0.679	0.718	0.691

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Including full 18-64 population; only includes interview months.
All specifications include individual, state, and state*large firm fixed effects. Monthly weights used.
Large firm defined as >25 employees.

Table C4: Results from Full Specification to Investigate Intensive Margin (Continued)

	(3a) w,L	(3b) Log(w),Log(L)	(3c) w,L	(3d) Log(w),Log(L)	(3e) w,L	(3f) Log(w),Log(L)
	Compensating and Hours Differentials, Sufficient Statistics, and Welfare Impact of Health Reform					
	Workers with a paid job and wages>0 in the given period		*Workers with a paid job and wages>0 over the entire period*		*Workers with a paid job and wages>0 and no job switch over the entire period*	
$w_D - w_A$	2.251*** [1.797, 2.465]	0.135*** [0.11, 0.146]	2.33*** [1.775, 2.628]	0.123*** [0.098, 0.138]	2.515*** [1.789, 2.842]	0.130*** [0.108, 0.156]
$w_F - w_B$	0.809*** [0.418, 1.09]	0.172*** [0.125, 0.217]	1.248*** [0.266, 1.371]	0.284*** [0.244, 0.312]	0.321 [-0.467, 0.725]	0.300*** [0.253, 0.338]
$w_D - w_B$	4.886*** [3.655, 5.84]	0.49*** [0.432, 0.557]	7.072*** [4.968, 8.184]	0.773*** [0.714, 0.839]	6.878*** [4.802, 8.225]	0.821*** [0.747, 0.903]
$w_F - w_A$	-1.826*** [-2.779, -0.903]	-0.184*** [-0.234, -0.138]	-3.494*** [-4.675, -2.768]	-0.366*** [-0.425, -0.319]	-4.042*** [-5.132, -3.158]	-0.391*** [-0.488, -0.381]
$L_D - L_A$	1.857*** [1.264, 2.205]	0.053*** [0.033, 0.063]	1.779*** [1.215, 2.149]	0.06*** [0.04, 0.073]	2.138*** [1.551, 2.509]	0.071*** [0.056, 0.093]
$L_F - L_B$	3.829*** [3.27, 4.293]	0.137*** [0.117, 0.154]	6.758*** [6.138, 7.214]	0.271*** [0.247, 0.294]	6.613*** [5.963, 7.122]	0.266*** [0.247, 0.294]
$L_D - L_B$	8.1*** [7.198, 8.819]	0.313*** [0.285, 0.341]	14.379*** [12.895, 15.24]	0.604*** [0.561, 0.64]	14.348*** [13.063, 15.45]	0.612*** [0.572, 0.665]
$L_F - L_A$	-2.414*** [-3.197, -1.649]	-0.123*** [-0.153, -0.098]	-5.842*** [-6.664, -4.743]	-0.273*** [-0.308, -0.241]	-5.598*** [-6.601, -4.718]	-0.275*** [-0.303, -0.240]
s	0.170 -	0.100 -	0.170 -	0.100 -	0.170 -	0.100 -
d	-0.350 -	-0.200 -	-0.350 -	-0.200 -	-0.350 -	-0.200 -
b	2.671*** [1.523, 3.838]	0.209*** [0.161, 0.262]	5.538*** [4.629, 6.933]	0.421*** [0.369, 0.484]	6.001*** [4.992, 7.319]	0.446*** [0.404, 0.519]
ρ	0.053*** [0.037, 0.094]	0.68*** [0.542, 0.88]	0.026*** [0.02, 0.031]	0.337*** [0.293, 0.384]	0.024*** [0.019, 0.028]	0.318*** [0.287, 0.364]
$\alpha + \lambda - \mu_x$	0.53*** [0.304, 0.641]	0.823*** [0.778, 0.846]	0.451*** [0.379, 0.552]	0.806*** [0.785, 0.824]	0.515*** [0.436, 0.579]	0.815*** [0.796, 0.829]
$ESHI_{After}$	0.740 -	0.740 -	0.740 -	0.740 -	0.740 -	0.740 -
$\underline{b}$	1.000	1.000	1.000	1.000	1.000	1.000
τ	-	-	-	-	-	-
DWL_m	1.126*** [0.529, 1.971]	0.01*** [0.01, 0.011]	6.572*** [4.333, 8.549]	0.017*** [0.015, 0.019]	6.033*** [4.291, 8.39]	0.017*** [0.015, 0.018]
$\dfrac{DWL_m}{DWL_\tau}$	0.164*** [0.096, 0.361]	0.144*** [0.096, 0.235]	0.223*** [0.149, 0.285]	0.057*** [0.047, 0.069]	0.174*** [0.131, 0.236]	0.052*** [0.045, 0.061]

to observe a compensating differential. Interestingly, our compensating differential findings do not appear to be driven exclusively by individuals who switch ESHI status by changing jobs—we still estimate a compensating differential when we only use variation from individuals who switch ESHI status within the same job. In all three samples, the penalty-and-subsidy-inclusive valuation is smaller in the level specifications and larger in the log specifications. The ratio of the deadweight loss of mandate-based health reform to the deadweight loss of tax-based health reform varies from 5.2 percent to 22.3 percent across all six specifications, and the largest upper bound of the 95 percent confidence interval is 36.1 percent. Overall, our results that include only the intensive margin decision are consistent with our preferred results, suggesting that the extensive margin decision of whether to work does not drive our key findings.

Appendix C.4 Robustness to Elimination of Individual Fixed Effects

We have argued that individual fixed effects are essential to our identification. Indeed, empirically, we show that when we exclude individual fixed effects in the baseline and full specifications our estimate of the compensating differential becomes substantially smaller or loses statistical significance. We present these results in Appendix Tables C5 and C6. These tables also show that when we exclude individual fixed effects but instead control for age, gender, marital status, race, education, and industry, our estimated compensating differential is only slightly larger and is not statistically significant. These results demonstrate that even with a large set of demographic controls, there are still unobserved factors that bias the comparison of individuals with ESHI to those without before compared to after reform. Therefore, the longitudinal nature of the SIPP is very important to our analysis.

Table C5: Results from Baseline Specification Without Individual Fixed Effects

		(1a)	(1b)		(2a)	(2b)
		w			L	
		Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)			Hours per week, including individuals without a paid job (hours=0)	
MA*ESHI*After	β_1	-0.132	-0.660*	Y_1	1.166***	3.600***
		[-0.646, 0.368]	[-1.345, 0.0470]		[0.484, 1.851]	[2.824, 4.109]
MA*ESHI*During	β_1^d	-0.424*	0.200	Y_1^d	-0.455**	0.151
		[-0.872, 0.0430]	[-0.540, 1.112]		[-0.869, -0.0870]	[-0.276, 0.462]
MA*ESHI	β_8	3.639***	2.323***	Y_8	1.956***	2.342***
		[2.899, 4.600]	[1.631, 3.255]		[1.347, 2.451]	[2.012, 2.626]
MA*After	β_{11}	1.126***	1.238***	Y_{11}	0.214	-3.024***
		[0.772, 1.470]	[0.612, 1.768]		[-0.471, 0.905]	[-3.552, -2.261]
MA*During	β_{11}^d	0.326*	-0.464*	Y_{11}^d	1.095***	0.363***
		[-0.0560, 0.598]	[-1.245, 0.0760]		[0.770, 1.534]	[0.0550, 0.729]
ESHI*After	β_{12}	-0.0148	-0.904**	Y_{12}	1.038***	-0.197
		[-0.491, 0.468]	[-1.530, -0.252]		[0.371, 1.763]	[-0.692, 0.380]
ESHI*During	β_{12}^d	-0.0802	-0.818***	Y_{12}^d	0.734***	-0.166
		[-0.532, 0.347]	[-1.540, -0.254]		[0.361, 1.111]	[-0.429, 0.149]
ESHI	β_{19}	12.131***	4.599***	Y_{19}	15.38***	3.195***
		[11.189, 12.879]	[3.993, 5.103]		[14.894, 16.009]	[2.960, 3.453]
After	β_{22}	0.254	0.814***	Y_{22}	-1.080***	-0.103
		[-0.0870, 0.595]	[0.268, 1.325]		[-1.747, -0.445]	[-0.651, 0.414]
During	β_{22}^d	0.193	0.660***	Y_{22}^d	-0.669***	0.0140
		[-0.0570, 0.544]	[0.237, 1.348]		[-1.068, -0.344]	[-0.285, 0.244]
Controls?		No	Yes		No	Yes
Observations		543,630	400,502		499,828	369,745
R-squared		0.076	0.169		0.15	0.164

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Including full 18-64 population.
Only includes interview months.
State fixed effects included, no individual fixed effects. Monthly weights used.
Estimations with controls control for age, gender, marital status, race, education, and industry.

Table C6: Results from Full Specification Without Individual Fixed Effects

		(1a)	(1b)		(2a)	(2b)		(3a)	(3b)
		w			L			Compensating and Hours Differentials, Sufficient Statistics, and Welfare Impact of Health Reform	
		Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)			Hours per week, including individuals without a paid job (hours=0)				
MA*ESHI*After*Large	β_1	0.912 [-0.317, 2.147]	-1.452** [-2.302, -0.244]	Y_1	-1.666** [-2.904, -0.570]	0.154 [-0.779, 1.288]	$w_D - w_A$	3.657*** [2.854, 4.702]	1.977*** [1.225, 3.067]
MA*ESHI*During*Large	β_1^d	-2.418*** [-3.527, -1.287]	-2.075*** [-3.415, -0.672]	Y_1^d	-0.277 [-0.789, 0.258]	0.615* [-0.0570, 1.190]	$w_F - w_B$	3.863*** [2.763, 5.064]	1.313** [0.165, 2.724]
MA*ESHI*Large	β_8	0.710*** [0.320, 1.349]	-0.836* [-1.361, 0.0250]	Y_8	-0.340 [-1.256, 0.406]	0.829*** [0.284, 1.273]	$w_D - w_B$	6.012*** [5.275, 6.934]	2.67*** [1.695, 3.888]
MA*After*Large	β_{11}	-2.355*** [-3.019, -1.653]	-0.693 [-1.732, 0.333]	Y_{11}	1.626*** [0.593, 2.819]	-1.406*** [-2.282, -0.530]	$w_F - w_A$	1.508** [0.396, 3.195]	0.62 [-0.781, 2.319]
MA*During*Large	β_{11}^d	1.198*** [0.632, 1.794]	-0.246 [-1.243, 0.630]	Y_{11}^d	0.596** [0.0520, 1.147]	-0.993*** [-1.484, -0.388]			
ESHI*After*Large	β_{12}	0.296 [-0.951, 1.457]	-1.372*** [-2.294, -0.450]	Y_{12}	0.568 [-0.594, 1.651]	-0.625 [-1.635, 0.379]	$L_D - L_A$	1.395*** [0.78, 1.914]	2.43*** [2.048, 2.725]
ESHI*During*Large	β_{12}^d	0.693 [-0.410, 1.776]	-0.361 [-1.663, 0.804]	Y_{12}^d	0.411 [-0.152, 0.936]	-0.550** [-1.057, -0.0530]	$L_F - L_B$	2.708*** [1.793, 3.652]	6.415*** [5.611, 6.91]
ESHI*Large	β_{19}	6.317*** [5.685, 6.753]	2.230*** [1.646, 2.635]	Y_{19}	16.770*** [16.024, 17.685]	2.029*** [1.646, 2.512]	$L_D - L_B$	-0.231 [-1.539, 0.941]	3.836*** [2.877, 4.608]
After*Large	β_{22}	-0.0896 [-0.815, 0.561]	1.348*** [0.560, 1.971]	Y_{22}	-0.844 [-1.999, 0.242]	0.258 [-0.607, 1.009]	$L_F - L_A$	4.334*** [3.272, 5.337]	5.009*** [3.831, 5.79]
During*Large	β_{22}^d	-0.383 [-0.963, 0.246]	0.575 [-0.200, 1.576]	Y_{22}^d	-0.474* [-1.045, 0.0630]	0.367 [-0.226, 0.839]			
Large	β_{23}	-4.987*** [-9.625, -4.398]	0.972 [-2.351, 1.708]	Y_{23}	-19.596** [-26.838, -17.745]	1.755 [-0.408, 2.186]			
MA*Large	β_{24}	-0.565 [-1.389, 4.066]	1.243* [-0.0180, 4.564]	Y_{24}	1.634 [-0.118, 8.972]	-0.954 [-1.445, 1.168]			
MA*ESHI*After	β_{1e}	-0.706 [-1.764, 0.264]	0.788* [-0.115, 1.706]	Y_{1e}	2.979*** [1.843, 3.909]	3.832*** [2.750, 4.684]			
MA*ESHI*During	β_{1e}^d	1.577*** [0.638, 2.579]	2.124*** [1.168, 3.366]	Y_{1e}^d	-0.231 [-0.748, 0.289]	-0.130 [-0.560, 0.351]	s	0.190 -	0.190 -
MA*ESHI	β_{8e}	2.948*** [2.159, 3.845]	2.813*** [2.079, 3.511]	Y_{8e}	1.735*** [1.345, 2.232]	1.601*** [1.202, 2.016]	d	-0.380 -	-0.380 -
MA*After	β_{11e}	2.941*** [2.349, 3.490]	1.608*** [0.833, 2.323]	Y_{11e}	-1.615*** [-2.519, -0.612]	-2.126*** [-2.834, -1.092]	b	-3.155*** [-4.926, -1.958]	-2.524*** [-4.123, -1.046]
MA*During	β_{11e}^d	-0.618*** [-1.078, -0.226]	-0.428* [-1.127, 0.0400]	Y_{11e}^d	0.602** [0.0980, 1.071]	0.992*** [0.536, 1.404]	ρ	-0.045*** [-0.072, -0.029]	-0.056*** [-0.136, -0.034]
ESHI*After	β_{12e}	-0.284 [-1.279, 0.757]	-0.0596 [-0.923, 0.740]	Y_{12e}	0.443 [-0.469, 1.604]	0.278 [-0.516, 1.347]	$\alpha + \lambda - \mu_x$	0.217 [-0.199, 0.498]	-0.131 [-1.616, 0.394]
ESHI*During	β_{12e}^d	-0.675 [-1.626, 0.249]	-0.652 [-1.614, 0.146]	Y_{12e}^d	0.287 [-0.214, 0.797]	0.231 [-0.142, 0.675]	$ESHI_{After}$	0.740 -	0.740 -
ESHI	β_{19e}	7.036*** [6.152, 7.797]	2.705*** [2.100, 3.411]	Y_{19e}	2.136*** [1.642, 2.560]	1.506*** [1.074, 1.878]	$\frac{b}{\tau}$	1.000 -	1.000 -
After	β_{22e}	0.352 [-0.204, 0.938]	0.0193 [-0.561, 0.632]	Y_{22e}	-0.287 [-1.182, 0.607]	-0.256 [-1.076, 0.584]	DWL_m	3.966*** [2.266, 6.012]	5.296*** [3.1, 7.075]
During	β_{22e}^d	0.520** [0.0750, 0.968]	0.324 [-0.156, 0.766]	Y_{22e}^d	-0.189 [-0.671, 0.313]	-0.221 [-0.641, 0.227]	$\frac{DWL_m}{DWL_\tau}$	0.454*** [0.187, 1.066]	0.948*** [0.272, 5.071]
Controls?		No	Yes		No	Yes		No	Yes
Observations		543,630	400,502		499,828	369,745			
R-squared		0.081	0.172		0.214	0.171			

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Including full 18-64 population; only includes interview months.
All specifications include state and state*large firm fixed effects, no individual fixed effects. Monthly weights used.
Estimations with controls control for age, gender, marital status, race, education, and industry.
Large firm defined as >25 employees.