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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 three key provisions of the recent Massachusetts and national "mandate-based" health reforms: individual and employer mandates and expansions in publicly-subsidized coverage. Using our model, we characterize the compensating differential for employer-sponsored health insurance (ESHI) -- the causal change in wages associated with gaining ESHI. We also characterize the welfare impact of the labor market distortion induced by health reform. We show that the welfare impact depends on a small number of "sufficient statistics" that can be recovered from labor market outcomes. Relying on the reform implemented in Massachusetts in 2006, we estimate the empirical analog of our model. We find that jobs with ESHI pay wages that are lower by an average of $6,058 annually, indicating that the compensating differential for ESHI is only slightly smaller in magnitude than the average cost of ESHI to employers. Because the newly-insured in Massachusetts valued ESHI, they were willing to accept lower wages, and the deadweight loss of mandate-based health reform was less than 5% of what it would have been if the government had instead provided health insurance by levying a tax on wages.

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1 Introduction

The 2010 United States national health reform and the 2006 Massachusetts state health reform both focus on expanding health insurance coverage to near-universal levels. These "mandate-based" reforms rely on three key provisions to expand coverage: a mandate that employers offer coverage or pay a penalty, a mandate that individuals obtain coverage or pay a penalty, and 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. As we demonstrate, mandate-based policy has the potential for greater efficiency in achieving policy goals if the individuals who gain insurance through their employers value the coverage that they receive.[1]

In this paper, we model and then estimate the relationship between ESHI and the labor market, allowing us to assess the impact of health reform on welfare. First, we extend the Summers (1989) model of a full-compliance employer mandate to incorporate the key features of the national and Massachusetts health reforms. Using this model, we characterize the compensating differential for 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 a set of 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 extensions of the Summers (1989) model incorporate more sources of variation in labor market outcomes, enriching the model's empirical content. Furthermore, our extensions allow us to capture interactions between policy provisions. For example, Summers (1989) gives us the intuition that an employer mandate reduces deadweight loss relative to a tax, but that intuition does not hold if there is already an individual mandate in place.

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 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 variation between Massachusetts and other states to control for national trends, and we incorporate individual fixed effects to control for time-invariant attributes that determine an individual's labor market outcomes. 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

[1] The question of whether the federal government can implement the individual mandate is the critical legal question that has led to court challenges of the national reform. Although the federal government's authority to tax is not questioned, court challenges focus on its authority to mandate the purchase of a private good under the Commerce Clause. We do not comment on the constitutionality of the individual mandate. Rather, we inform the choice of policy instrument by focusing on the efficiency of mandate-based policy relative to traditional tax-based policy.

differential associated with health insurance.

Building on 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 policy parameters of the Massachusetts reform (the employer and individual 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 insurance expansion 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 and slightly smaller in magnitude than the full cost of health insurance, suggesting high average valuation of the benefit among the newly-insured. Consistent with the large decline in wages, we find a small hours differential between jobs with and without ESHI, also suggesting high average valuation of the benefit among the newly insured. 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.

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. Our main estimate suggests that mandate-based coverage expansion in Massachusetts resulted in a deadweight loss due to distortion of the labor market that was only 5% of the distortion associated with instead providing health insurance through a tax on wages. We examine the robustness of our estimates to a variety of alternative specifications. Although our estimates vary, they always show that that mandate-based reform is substantially more efficient than tax-based reform because our finding that individuals value ESHI is very robust.

The paper proceeds as follows: Section 2 discusses the provisions of Massachusetts and national reforms that are likely to affect the labor market. Based on these provisions, Section 3 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; Sections 4 and 5 discuss identification and estimation of the model. Section 6 introduces the data.

Section 7 presents results and discusses robustness, and Section 8 concludes.

2 Massachusetts Health Reform, the ACA, 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. First, both reforms include a "pay-or-play" employer mandate, which requires employers to offer health insurance or pay a penalty. Unlike traditional full-compliance mandates, pay-or-play mandates allow for noncompliance and an associated 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, at minimum, include a plan that allows employees to purchase health insurance using pre-tax wages and employers must contribute at least 33% of the value of the premium or they will be assessed a penalty. This penalty is equal to $295 per employee per year. Compliance has been high. Only approximately 4.6% of employers large enough to be subject to the penalty (0.5% of all employers) paid it from 2007 to 2010 (Massachusetts DHCFP (2011b)).[2]

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 (2010a)). The ACA also requires that coverage options be affordable, such that the insurance offered pays at least 60% of covered expenses and the employee is not required to pay more than 9.5% of family income for individual coverage (Burkhauser et al. (2011)). If the employer does not offer options for coverage, the penalty assessed is $2,000 per full-time employee, excluding the first 30 employees. If the options do not meet the definition of affordable, the employer must pay $3,000 for any employees who enroll in insurance via an exchange and receive a tax credit, also excluding the first 30 employees (Kaiser Family Foundation (2010)).

The second keystone of both reforms is the individual mandate to purchase insurance. The Massachusetts health reform incorporated an individual mandate requiring nearly all residents to purchase health insurance deemed to meet or exceed a specific value (called "minimum creditable coverage"), or pay a penalty if they could have purchased affordable coverage but did not.[3] The penalty in Massachusetts for those who are unable to demonstrate they have coverage when they

[2] 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 — they need only set up a plan without contributing anything to it. Accordingly, zero employers were liable for the free rider surcharge in fiscal years 2008 and 2009 (Massachusetts DHCFP (2011a)).

[3] 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% 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.)

file their taxes is equal to 50% of the cost of the least generous (Bronze) plan available in the Massachusetts health insurance exchange (the Connector).[4] Compliance with the individual mandate in Massachusetts has been high — over 97% of tax filers submitted the tax form to comply with the individual mandate in 2008, and less than 2% reported any spell of uninsurance (Massachusetts Health Connector and Department of Revenue (2010)). The ACA has similar requirements, mandating that individuals purchase qualifying coverage or pay penalties, which will be phased in beginning in 2014. The ACA penalty is the higher of $695 per uninsured member of the household (up to three) or 2.5% of household income.

The third cornerstone of both reforms is the establishment of subsidized coverage for those with incomes too high to qualify for fully subsidized Medicaid coverage. In Massachusetts, this coverage is offered by the state at no premium to those with incomes below 150% of the federal poverty level (FPL) ($27,795 for a family of three in 2011).[5] Those earning less than 100% of FPL have access to traditional Medicaid (MassHealth) or fully subsidized CommCare, depending on categorical eligibility. Individuals between 100 and 150% of FPL receive CommCare coverage but do not pay a premium. Individuals between 150 and 300% of FPL ($27,795 to $55,590 for a family of three) can purchase coverage through the Connector (CommCare plans) with subsidies that decline with income. Similarly, the ACA expands Medicaid eligibility to all those with incomes below 133% of poverty ($24,707 for a family of three).[6] It also extends subsidized coverage higher up the income distribution to 400% of poverty ($74,120 for a family of three).

3 A Model of Mandate-Based Health Reform and the Labor Market

In this section, we incorporate the three key features of the ACA and the Massachusetts reform into a model of mandate-based health reform and the labor market. Using our model, we characterize the compensating differential for health insurance and the welfare impact of health reform. Our model extends the model pioneered by Summers (1989) (hereafter called the "Summers model") and subsequently used by Gruber and Krueger (1991), Gruber (1994), Buchmueller et al. (2011), and Anand (2011), among others. Our extensions capture the salient features of the Massachusetts and national reforms that affect the labor market, bringing the model closer to actual policy. By taking all of the key features of the Massachusetts and national reforms into account, we enrich the empirical content of the model and contribute to the literature on the impact of health reform

[4] 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.

[5] In the 48 contiguous states and the District of Columbia, the 2011 poverty line is $10,890 for a single individual, and it grows by $3,820 for each additional family member (Federal Register (2011)).

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

on the labor market.[7] These extensions also allow us to capture the interactions between policies yielding additional insight. Specifically, we explore the intuition that an employer mandate reduces deadweight loss relative to a tax and find that it does not hold if there is already an individual mandate in place.

3.1 The Model

We begin by characterizing labor supply and demand when some firms do and others do not provide ESHI. 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, which is exogenous for now.[8] 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. 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.

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 the cost to the 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. In the individual's choice problem, several factors can affect 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 Arrow (1963) and Rothschild and Stiglitz (1976)). Furthermore, there is a tax preference for ESHI, so we expect an individual's marginal tax rate to affect his willingness to pay for ESHI. Rather than modeling these factors individually, we model only α, which is a sufficient statistic for welfare analysis in the spirit of

[7] Krueger and Kuziemko (2011) 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.

[8] We develop the model relying on the simplifying assumption that we can measure L in hours of work, ignoring the potential difference between the extensive and intensive margin of employment. We relax this assumption in our empirical implementation.

Chetty (2009). If the worker values ESHI, he is willing to work for lower wages in a job that provides ESHI, and α captures the full magnitude of the downward shift in the individual's supply curve in the absence of an individual penalty or a subsidy.

The individual penalty augments the individual's underlying valuation of ESHI, shifting his labor supply curve further downward — 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. We generally expect $\alpha+\lambda \leq 1$.[9] A subsidy, like the individual mandate, only affects an individual's labor supply if he does obtain health insurance through his employer. However, in the face of a penalty, he is more willing to work for ESHI instead of wages; in the face of a subsidy for health insurance outside of employment, he is less willing to work for ESHI instead of wages. In addition to examining the underlying valuation, the penalty, and the subsidy individually, we can capture the entire shift in the labor supply curve associated with ESHI. The resulting sum, $\alpha + \lambda - \mu_x$, which we call the "penalty-and-subsidy-inclusive valuation" is an important sufficient statistic for our analysis.

The inclusion of intertemporal variation (indexed by t) in the model captures the impact of introducing health reform, allowing assessment of changes in policy as well as tying the model to the empirical setting. We model the impact of health reform by specifying two values of t: $Before$ and $After$. The employer mandate, the individual mandate, and the subsidies are not in place before reform such that $\rho_{Before} = \lambda_{Before}=\mu_{x,Before} = 0$, but they are in place after reform such that $\rho_{After} = \rho$, $\lambda_{After}=\lambda$, and $\mu_{x,After} = \mu_x$. The policy parameters of health reform are the only time-varying elements of the model.

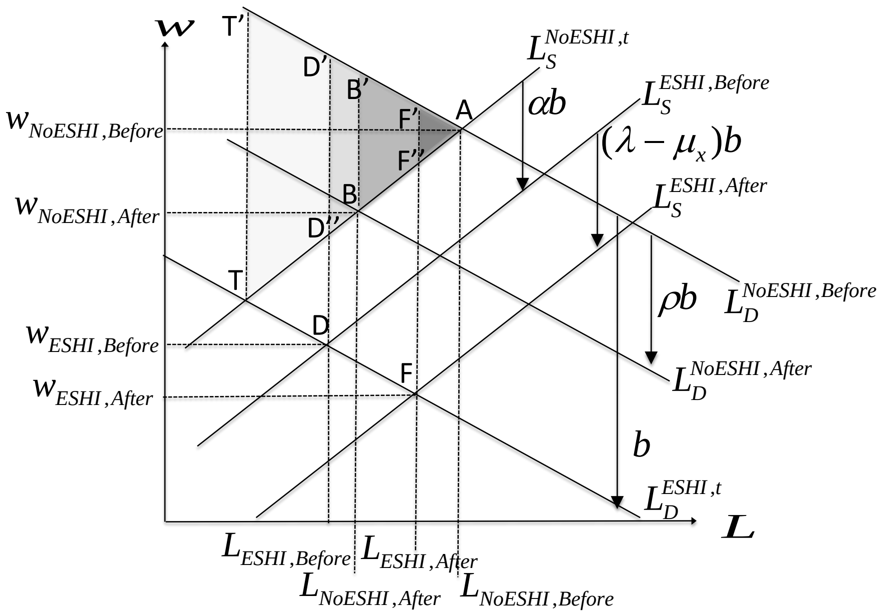
Figure 1 depicts a graphical representation of the model, assuming that $L_D^{ESHI,t}$ and $L_D^{NoESHI,t}$ have the same linear functional form and that $L_S^{ESHI,t}$ and $L_S^{NoESHI,t}$ have the same linear functional form (the linear functional form is an approximation to a general nonlinear functional form). The labor market equilibrium (w, L) in period t is the intersection of labor supply and labor demand. There are two potential equilibria in each period t, conditional on whether the individual

[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, which we see as very unlikely, 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 pre-existing 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.

Figure 1: Graphical Model



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.

The Summers model is a special case of our model with only two equilibria and a different policy intervention. Before the policy intervention, there is only one equilibrium at A — no jobs include ESHI. 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. After the policy intervention, there is only one equilibrium at D — all jobs include ESHI.

3.2 Characterization of the Compensating Differential for ESHI

Using our model, we can characterize 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 lieu of wages. We first characterize the compensating and hours differentials in terms of differences between labor market equilibria, and then we characterize them in terms of sufficient statistics.

The first panel of Table 1 presents the compensating differential for health insurance in terms of differences between equilibria in the first column and sufficient statistics in the third column. The second panel presents the analogous hours differential. These expressions follow directly from the geometry of Figure 1.[10] We represent the slope of the labor supply curve with s and the slope of the labor demand curve with d (these slopes become elasticities if we specify w as the logarithm

[10]To obtain the compensating and hours differentials before the reform, consider D'. Wages at D' are equal to $w_D + b$. Because D' and A are on the demand curve, we can express the slope of the demand curve as the difference in wages at these two points, divided by the difference in hours at these two points:

Table 1: Compensating and Hours Differentials

Compensating Differential	t	Sufficient Statistics	Coefficients
$w_D - w_A$	Before - Before	$\frac{s-\alpha d}{d-s}\,b$	β_8 $[+\beta_{8e}]$
$w_F - w_B$	After - After	$\frac{(1-\rho)s-(\alpha+\lambda-\mu_x)d}{d-s}\,b$	$\beta_1+\beta_8$ $[+\beta_{1e}+\beta_{8e}]$
$w_D - w_B$	Before - After	$\frac{(1-\rho)s-\alpha d}{d-s}\,b$	$\beta_8-\beta_{11}$ $[+\beta_{8e}]$
$w_F - w_A$	After - Before	$\frac{s-[\alpha+\lambda-\mu_x]d}{d-s}\,b$	$\beta_1+\beta_8+\beta_{11}$ $[+\beta_{1e}+\beta_{8e}]$

Hours Differential	t	Sufficient Statistics	Coefficients
$L_D - L_A$	Before - Before	$\frac{1-\alpha}{d-s}\,b$	γ_8 $[+\gamma_{8e}]$
$L_F - L_B$	After - After	$\frac{1-\rho-(\alpha+\lambda-\mu_x)}{d-s}\,b$	$\gamma_1+\gamma_8$ $[+\gamma_{1e}+\gamma_{8e}]$
$L_D - L_B$	Before - After	$\frac{1-\rho-\alpha}{d-s}\,b$	$\gamma_8-\gamma_{11}$ $[+\gamma_{8e}]$
$L_F - L_A$	After - Before	$\frac{1-(\alpha+\lambda-\mu_x)}{d-s}\,b$	$\gamma_1+\gamma_8+\gamma_{11}$ $[+\gamma_{1e}+\gamma_{8e}]$

of wages and h as the logarithm of hours).

The rows of each panel show different expressions for the compensating and hours differentials, depending on the timing of the comparison between individuals with ESHI and individuals without ESHI. The second column indicates the time periods being compared: the first row is the differential before the reform, the second is the differential after the reform, the third is the differential between those with ESHI before the reform and those without ESHI after the reform, and the fourth is the differential between those with ESHI after the reform and those without ESHI before reform. The differential before the reform is equivalent to the corresponding differential from the Summers model. It is a special case of the other three expressions with the policy parameters set to zero. Analysis of the impact of health reform on the labor market, however, requires the policy parameters. We will be particularly interested in the compensating differential for those who switch from not having ESHI before the reform to having it after the reform. For those individuals, shown in the expression in the last row of the each panel, 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 amount of the benefit (ESHI decreases wages by b), and the hours differential is zero (ESHI does not distort hours worked).

Given the expressions in Table 1, we can use the compensating and hours differentials for those who switch to ESHI after reform to learn about the valuation of ESHI. If the compensating differential is equal to the cost of the benefit and the hours differential is zero, then we can infer that the penalty-and-subsidy-inclusive valuation is full. Previous studies based on the Summers

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$$d = \frac{(w_D + b - w_A)}{(L_D - L_A).}$$

Next, consider a point directly below A on the $L_s^{ESHI,Before}$ curve. We can express the slope of the supply curve as follows:

$$s = \frac{(w_D - (w_A - \alpha b))}{(L_D - L_A)}.$$

We obtain the compensating and hours differentials by solving this system of equations.

model have stopped at this point 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 4 on identification, the additional sources of variation in our model extend the empirical content of the Summers model, allowing us to identify the penalty-and-subsidy-inclusive valuation, regardless of the true magnitude.

3.3 Characterization of the Welfare Impact of Health Reform

Using our model, we can also derive the impact of health reform on employer and employee welfare. To do so, we first characterize the welfare impact of health reform in terms of sufficient statistics. We then express the sufficient statistics in terms of differences between labor market equilibria, showing that our key sufficient statistics for the welfare impact of health reform are functions of compensating and hours differentials.

3.3.1 Sufficient Statistics for the Welfare Impact of Health Reform

Distortions to the labor market occur when 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. Summing the area of the employer and employee surplus from the transactions that would have occurred in the absence of the policy, we can express the combined deadweight loss of the policies of mandate-based health reform m 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. Thus, these terms are sufficient statistics for the welfare-impact of mandate-based health reform. Graphically, the welfare impact corresponds to a weighted combination of the two overlapping triangles shown in Figure 1. The deadweight loss is the triangle given by $F'AF''$ if the representative individual has ESHI after the reform, and the deadweight loss is equal to the triangle given by $B'AB$ if he does not. As shown, the deadweight loss for individuals without ESHI is smaller, but the relative magnitudes of the triangles can reverse, depending on the magnitudes of the policy parameters. If the employer penalty is equal to zero and the full cost of ESHI and the penalty-and-subsidy-inclusive valuation is full ($\alpha+\lambda-\mu_x=1$), then there is no deadweight loss associated with health reform.

Using this expression, we can compare the deadweight loss of mandate-based reform to the deadweight loss of alternative policies. This approach can be applied to a comparison of any policy, provided we can express the key policy elements in terms of labor market equilibria. We focus on comparison of the deadweight loss from mandate-based reform to the deadweight loss from a tax-based reform that relies on a wage tax to finance a single payer or "Medicare for all" program. The is the relevant welfare comparison if the government has already decided to expand health insurance coverage — as in the case of Massachusetts and the ACA — and is looking for the most efficient way to do so. To compare these policy options, we begin by computing the deadweight loss of a tax. Suppose that before the tax-based reform, there are no penalties or subsidies. No employers provide health insurance to their employees, such that the initial labor market equilibrium is a point A. Now suppose that the government levies a tax τ on employers to provide health insurance (the incidence is the same if the government instead levies the tax on employees). 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. Following Summers (1989), the key assumption about tax-based reform is that it does not induce a shift in labor supply. The deadweight loss of the tax-based reform is the shaded region given by the triangle $T'AT$:

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

Taking the ratio of the deadweight loss of mandate-based reform to the deadweight loss of tax-based reform, allowing $b \neq \tau$ gives:

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

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 and t is just the relative loading cost of ESHI and government-provided health insurance.

Welfare in the Summers model is a special case of welfare in our model. We can capture the ratio of the full-compliance mandate in the Summers model to a tax using equation (2) with no penalties or subsidies ($\lambda = \mu_x = 0$), and all agents in ESHI after reform ($ESHI_{After} = 1$). We can represent the deadweight loss of the full-compliance mandate with a single triangle, $D'AD''$, which is smaller than the triangle associated with a tax if $\alpha > 0$. This special case yields the theoretical contribution of Summers (1989): an employer mandate reduces deadweight loss relative to a tax.

However, the addition of 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, the addition of a full-compliance or a pay-or-play employer mandate weakly decreases welfare. Consider the case where there is already an individual pay-or-play mandate in place, but there is no employer penalty. The deadweight loss is given by equation (1) with $\mu_x = \rho = 0$. Because there is no employer penalty, there is no distortion if the individual does not have ESHI. Adding a full-compliance mandate weakly increases the deadweight loss because the individual must have ESHI and the associated distortion; zero distortion without ESHI is no longer possible. Likewise, adding a pay-or-play mandate weakly increases the deadweight loss because the individual now has a distortion associated with a positive ρ if he does not have ESHI. Our analysis demonstrates that it is important to consider the interactions between policies when assessing welfare.

3.3.2 Sufficient Statistics in Terms of Labor Market Equilibria

Building on the compensating and hours differentials, we can express most of the sufficient statistics in equations (1) and (2) 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. For example, we compute the slope of the labor supply curve by comparing equilibrium A to equilibrium B, and we compute the slope of the labor demand curve by comparing equilibrium D to equilibrium F, as shown in the first two rows of Table 2. In the subsequent rows of the table, we express all other sufficient statistics in terms of the slope of the labor supply and demand curves as well as differences between the equilibria.

Table 2: Sufficient Statistics

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

We can characterize the entire welfare impact of health reform given by equations (1) and (2) with these sufficient statistics and two others: a value for $ESHI_{After}$, and a ratio of b to t. From the table, we see 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.

4 Identification

In this section, we develop the empirical analog of our model. In the previous section, 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 in our model. In this section, we first discuss how we can use the Massachusetts reform to identify the differences between the equilibria empirically. We then discuss the implications for identification of the compensating differential and the welfare impact of health reform.

4.1 Identifying Differences Between Labor Market Equilibria

We can use the Massachusetts reform to identify the differences between labor market equilibria. The simplest approach would require only eight data points from within Massachusetts: average wages and hours for jobs with and without ESHI before and after reform. We could use these data points to plot the four labor market equilibria depicted in Figure 1. We could then calculate the compensating differential for ESHI and the sufficient statistics for the welfare impact of health reform given in Table 2.

However, additional sources of variation are available, and incorporating them into the model can be valuable in two respects. First, we can identify the differences between labor market equilibria more convincingly, given empirical considerations that are as yet outside our model. Second, we can choose to identify more labor market equilibria. Consider the sources of variation that allow us to identify the differences between labor market equilibria more convincingly. 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. We also incorporate variation within individuals over time to control for a myriad of worker characteristics that shift labor supply and demand for a given individual for reasons that are unrelated to Massachusetts health reform.

In addition, we incorporate variation between small and large firms to control for Massachusetts-specific factors that could shift labor supply and demand after the reform in Massachusetts, but that are unrelated to health reform. Because small firms are exempt from the employer mandate, small firms that do not provide health insurance should not shift their labor demand from before to after the reform, and equilibrium B should correspond with equilibrium A. To the extent that equilibrium B does not coincide with equilibrium A for small firms, there could be Massachusetts-specific factors that affect the labor market differentially after the reform that are not due to the reform itself. Given empirical evidence, we control for these factors in our preferred specification.

Finally, we incorporate variation in subsidy amounts to identify more labor market equilibria. 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 primary specification. We extend the model to incorporate variation in subsidy amounts and discuss the associated results in Online Appendix OA1.

For convenience, when we refer in our notation and discussion to a specific equilibrium, we are referring to that equilibrium after netting out differences with the control groups. For example, when we refer to the ESHI equilibrium after the reform (equilibrium F), we imply that we have already netted out the difference between Massachusetts and other states, the difference within individuals over time, and in our preferred specification, the difference by firm size.

4.2 Identification of the Compensating Differential for ESHI

Recall that Table 1 expressed the compensating and hours differentials as differences between labor market equilibria. Therefore, because we can identify differences between labor market equilibria we also identify the compensating and hours differentials. However, identification of some differences between labor market equilibria comes from more plausibly exogenous variation, implying that we identify some differentials more convincingly than others.

The most convincing identification comes from changes in ESHI status for a given individual induced by the reform. Less convincing are the first two differentials in Table 1 that 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 if individuals gain ESHI when they get a better job that includes health insurance. Empirically, consistent with the existing literature, we do not find negative compensating differentials using variation that does not rely on the reform, even after incorporating individual fixed effects, suggesting that ESHI switches that are not due to reform could be endogenous (Gruber (2000) and Currie and Madrian (1999)). However, the reform provides a source of exogenous variation in ESHI status that we can use to identify the differentials in the last two rows of Table 1. We can identify the compensating and hours differentials between equilibrium D and equilibrium B using individuals who switch out of ESHI from before to after the reform because they become eligible for subsidies on the individual market. However, subsidies only affect individuals with certain incomes, and all other individuals will have an incentive to switch into ESHI because of the individual and employer mandates. Therefore, the last compensating and hours differentials in Table 1, which compare equilibrium F and equilibrium A, will be the best identified.

4.3 Identification of the Welfare Impact of Health Reform

If the differences between the labor market equilibria are identified, we can calculate the sufficient statistics using the expressions in Table 2. However, as discussed above, some differences between labor market equilibria are identified more convincingly than others. Therefore, some sufficient statistics are identified more convincingly than others. Specifically, the sufficient statistics that can be derived from a labor market equilibrium before the reform and another labor market equilibrium after the reform with a different ESHI status are the best-identified. Fortunately, these sufficient statistics are the most important for welfare analysis.

From Table 2, we see that we can identify the penalty-and-subsidy-inclusive valuation $\alpha+\lambda-\mu_x$ and the cost of the benefit b using individuals who transition from not having ESHI before the reform to having ESHI after the reform (equilibrium A to equilibrium F) and values for d and s. The differences between these two equilibria are our best-identified measures of the compensating and hours differentials. Therefore, the penalty-and-subsidy-inclusive valuation and the cost of the benefit will be the best-identified sufficient statistics.

The other sufficient statistics are identified, but not as convincingly because they do not depend on changes in ESHI status induced by the reform. For example, 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. In practice, we estimate values 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 convincingly identified 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 (for example Blundell and MaCurdy (1999); Hamermesh (1996)) suggests that reasonable magnitudes for labor supply and demand elasticities are 0.1 and -0.2 respectively. Because our primary specification is in levels (not logarithms) we convert these into slopes at the mean wage and hours. 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, we prefer to estimate the sufficient statistics for several 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). Third, to the extent that we think the behavioral response to the policy parameter should be equal to the statutory value, we can compare the two values in an over identification test.

Because of difficulties with separate identification, we rely mostly on the parameters or sums of parameters that are best-identified: b and $\alpha + \lambda - \mu_x$. As shown, we can characterize the entire welfare impact of health reform given by equations (1) and (2) with these sufficient statistics and two others: a value for $ESHI_{After}$, which we estimate, and a ratio of b to t, which we can calibrate. However, separate estimates of α, λ, and μ_x would allow us to analyze the welfare impact of the separate components of health reform independently, so we proceed in estimating them, keeping in mind that our identification poses challenges.

As shown in Table 2, 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 effort to identify compensating differentials, particularly the compensating differential for entire cost of ESHI. Our identification for the penalty-and-subsidy-inclusive valuation is compelling relative to our identification of α, illustrating the advantage of our approach over the existing literature.

We are similarly interested in separate estimates for λ and μ_x. As shown in Table 2, 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 μ_x for people eligible for different subsidy amounts, we can incorporate variation in subsidy amounts across income eligibility thresholds as we discuss in Online Appendix OA1.

5 Estimation

To estimate all of the relevant differences between labor market equilibria and sufficient statistics that we use to estimate the compensating and hours differentials and the welfare impact of health reform, we specify and estimate wage and hours equations of the following form:

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

where Y_{it} measures wages w or hours L for individual i at time t. In our main specifications, 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. 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 or firms of unknown size relative to small firms that are exempt from the employer mandate. We begin with a baseline specification that excludes all bracketed terms, which reflect variation between large and small (exempt) firms with e coefficient subscripts. We include the bracketed terms in our preferred specification. 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 Online Appendix OA1. 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, and we convert all wages into 2006 dollars using the Consumer Price Index for all urban consumers

(CPI-U) to adjust for inflation. We do not include time fixed effects at a greater level of detail in our main specification because the calculation of our sufficient statistics requires a single period before reform and a single period after reform. In all specifications, we also allow for a "during" implementation period that is separate from the before and after periods.[11]

Our estimating equations are so simple that we can 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.

Although our estimating equations resemble differences-in-differences models, such models are typically used to identify a single coefficient after netting out differences with control groups. In our case, however, we are interested in identifying the four labor market equilibria of our model, from which we derive the compensating and hours differentials and the sufficient statistics for welfare analysis. To obtain the four labor market equilibria of our model, we combine several coefficients from the wage and hours equations, making our analysis richer than traditional difference-in-differences analysis.

5.1 Estimating the Compensating Differential for ESHI

The first step in estimating the compensating differential for ESHI is to express the four labor market equilibria of our model in terms of coefficients from the estimating equations. We express the wages associated with each equilibrium in Table 3. We can express the hours associated with each equilibrium with γ in place of β. 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 over the reform period. In the preferred 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.

Table 3: Wages in Terms of Coefficients

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

[11] To simplify exposition, we omit the terms that correspond to the during period from equation (3). In all of our models, we include a term for the during period that corresponds to each term for the after period. We represent the coefficients on during period terms with corresponding d superscripts.

The next step in estimation of the compensating differential for ESHI is to express the compensating differentials in terms of coefficients. The last column of Table 1 expresses the compensating and hours differentials in terms of coefficients, which we can replace with estimated coefficients.

Our preferred measure of the compensating differential, $w_F - w_A$, is the sum of several coefficients: $\beta_1 + \beta_8 + \beta_{11}$ $[+\beta_{1e} + \beta_{8e}]$. 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. The coefficients in the preferred specification also control for Massachusetts-specific wage changes after reform using variation by firm size.

If there were no employer penalty ($\rho = 0$), we could simplify our preferred estimate of the compensating differential to β_1 $[+\beta_{1e}]$. The employer penalty reduces potential sources of identification for the compensating differential because it affects the labor market equilibrium for workers *without* ESHI after the reform in Massachusetts. If the only impact of the reform were to change the labor market equilibrium for workers with ESHI after the reform, which would be the case if there were no employer penalty, workers without ESHI after the reform could provide an additional control group for workers with ESHI after the reform. Thus, we could control for Massachusetts-specific shocks unrelated to health reform in Massachusetts that occur at the same time as the reform without having to incorporate variation in firm size. In that case, the specification without firm size interactions would simplify into a traditional difference-in-differences-in-differences specification in which β_1 would reflect the compensating differential for ESHI. Instead of being part of the estimated compensating differential, the coefficient β_{11} would control for Massachusetts-specific shocks coincident with reform, and the coefficient β_8 would control for time-invariant differences between individuals with ESHI and individuals without ESHI in Massachusetts relative to other states. The disadvantage of having an employer penalty of zero in terms of identification would be that we would no longer have any labor supply shifters, so we could not identify the slope of the labor supply curve s.

Because the Massachusetts reform included a positive statutory employer penalty, even though it was small relative to the cost of ESHI, we prefer the expression $\beta_1 + \beta_8 + \beta_{11}$ $[+\beta_{1e} + \beta_{8e}]$ for the compensating differential. However, because the statutory employer penalty was small, the compensating differential could, in practice, be very similar to β_1 $[+\beta_{1e}]$. In the model without firm size interactions, as the employer penalty goes to zero, β_8 and β_{11} should approach zero and β_1 should approach the magnitude of the compensating differential. Thus, when we examine the coefficients of our model, we expect β_1 to be large and negative, and we expect β_8 and β_{11} to be close to zero. Further, we expect β_8 to be negative because as we have shown in Table 3, β_8 gives the position of equilibrium D, which according to our model, should be to the lower left of equilibrium A. We also expect β_{11} to be negative because we have shown that β_{11} gives the position of equilibrium B, which should also be to the lower left of equilibrium A if there is a positive penalty. If our estimated β_8 is positive, we will not trust our identification of the compensating differential identified by $w_D - w_A$, and we will continue to prefer the expression for $w_F - w_A$. If our estimated β_{11} is positive, we will want to incorporate the firm size interactions that control for Massachusetts-specific labor market changes coincident with reform.

5.2 Estimating the Welfare Impact of Health Reform

To estimate the welfare impact of health reform, the first step is to express the sufficient statistics in terms of coefficients. We have already expressed the sufficient statistics in terms of differences between labor market equilibria in the second column of Table 2 and the labor market equilibria in terms of coefficients in Table 3. Combining, we express the sufficient statistics in terms of coefficients in the third column of Table 2.

Next, we estimate the deadweight loss of mandate-based reform by replacing terms with their corresponding empirical estimates as follows:

$$DWL_m = \frac{\widehat{b}^2}{2(\widehat{s}-\widehat{d})}\left((1-(\widehat{\alpha+\lambda-\mu_x}))^2\widehat{ESHI_{After}}+\widehat{\rho}^2(1-\widehat{ESHI_{After}})\right). \quad (4)$$

We also estimate the ratio of the deadweight loss of mandate-based reform to the deadweight loss of tax-based reform as follows:

$$\frac{DWL_m}{DWL\tau} = \left(\frac{\widehat{b}}{\tau}\right)^2\left((1-(\widehat{\alpha+\lambda-\mu_x}))^2\widehat{ESHI_{After}}+\widehat{\rho}^2(1-\widehat{ESHI_{After}})\right), \quad (5)$$

where we use $\widehat{\cdot}$ to denote the estimate that corresponds to each parameter. We obtain $\widehat{\alpha+\lambda-\mu_x}$ and $\widehat{b}$ by plugging the estimated values of the compensating and hours differentials and the calibrated values of $\widehat{s}$ and $\widehat{d}$ into Table 2. We calibrate $\widehat{\rho}$ to reflect the statutory employer penalty in Massachusetts. For $\widehat{b/\tau}$, we assume that $b=\tau$. We later relax this assumption and assess robustness to assuming lower loading costs for health insurance provided through tax-based reform. The only remaining parameter is $\widehat{ESHI_{After}}$, which we estimate as the probability of having ESHI in Massachusetts after reform in our data.

6 Data, Summary Statistics, and Examination of Labor Market Trends before Reform

6.1 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 non-institutionalized population.[12] Individuals selected into the SIPP sample are interviewed once every four months over a four-year panel, with each interview covering information about the previous four-month period,

[12]The SIPP is not designed to be representative within the state of Massachusetts; however, the SIPP is the best data available to us on other dimensions, so we proceed by focusing on within-individual variation. We have also run our main regressions in the restricted-access Medical Expenditure Panel Survey (MEPS) with state identifiers, but the MEPS is only approximately 15% of the size of the SIPP, with 160 individuals in Massachusetts, so sample size is not large enough for us to obtain reliable results.

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 SIPP's sampling and response unit design.[13]

We use the full 2004 SIPP panel, which covers October 2003 to December 2007. The main limitation of this SIPP panel is that it does not extend for a long time after reform was fully implemented. Although we observe a relatively short period of responses after all of the aspects of the reform were in effect on July 1, 2007, we also observe a full year of responses during the implementation of the reform. Following our previous work, we refer to the period from July 2006 through June 2007 as the *During* period and the period from July 2007 forward as the *After* period. By doing so, we attain different estimates during the period in which it was initially rolled out and after the period in which it was fully implemented. Furthermore, 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 ends at the end of December 2007.

Another limitation of the 2004 SIPP panel is that the sample size decreases over time, due primarily to interview reductions. Our group of interest is the population under age 65, without military health insurance or Medicare. In 2004, there are 91,771 unique individuals (716,606 observations) in this sample across states, of which 2,690 unique individuals (20,457 observations) are in Massachusetts. In 2007, there are 35,733 unique individuals (320,775 observations) in the sample, of which 847 unique individuals (7,513 observations) are in Massachusetts. However, we still have a large enough sample to find statistically significant results in our main specifications.

The SIPP allows us to construct our main dependent variables: hourly wages w and hours worked per week L. The SIPP allows respondents to report wages and hours for up to two jobs. Our main 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

[13]Our main analysis uses 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 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 preferred specification does not use panel weights and instead uses individual weights, therefore maximizing the number of respondents. In results not reported, we re-estimate our main regressions using each panel weight. Reassuringly, the main 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.

L. To get around this issue, which is related to the common division bias problem from 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 the alternative wage measures, likely because hours move infrequently.

6.2 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. We also compare labor market trends in Massachusetts and other states based on ESHI status, as required by our model. Our identification will be most convincing if labor market trends are similar in Massachusetts and other states before reform.

Our identification can also be convincing if we observe similar aggregate labor market trends in Massachusetts and other states *after* the reform. From prior research, we know that, while the reform resulted in a significant decline in the percentage of people without health insurance, only approximately 6 percentage points of the population gained heath insurance coverage. Of those who gained coverage, roughly half gained ESHI and the other half gained subsidized coverage (Kolstad and Kowalski (2010)). While we do expect potentially large labor market impacts for individuals who switched ESHI status as a result of reform, even very large impacts for these individuals should only result in small changes in the aggregate labor market. To the extent that we do see changes in the aggregate labor market, they could reflect impacts of reform, or they could reflect factors unrelated to reform that differentially affected Massachusetts relative to other states after reform. If we are worried about the latter, we can incorporate variation by firm size.

We report summary statistics in Table 4. We compare the full population, the Massachusetts population, and the non-Massachusetts population before reform (October 2003 to June 2006) and after reform (July 2007 to December 2007). We exclude the during reform period for simplicity. In this table, we include all months, not just interview months. The sample size in each row is different, depending on data availability. In this table and throughout the paper, we deflate wages by the 2006 CPI.

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 \$1.83 in Massachusetts after reform on a base of \$17.86 before reform. This increase is significant at the 99% level. Excluding individuals without a paid job in the second and third rows of the table, we see that wages increased by \$0.30, 1.7% 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 4.5 percentage points in Massachusetts after reform on a base of 80% before 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	13.87	14.60	17.86	20.39	13.78	14.47	1.829***
w|paid job & w>0	20.59	22.37	25.11	27.17	20.49	22.25	0.301
Log(w|paid job & w>0)	2.76	2.84	2.95	3.05	2.75	2.84	0.017**
L: Hours per week	28.01	27.68	29.03	30.45	27.98	27.62	1.775***
L|paid job & L>0	38.65	38.47	37.8	37.42	38.67	38.5	-0.202**
Log(L|paid job & L>0)	3.59	3.59	3.55	3.55	3.59	3.59	-0.002
Hours per week in all jobs	40.82	0.74	40.15	39.06	40.84	40.58	-0.834***
Paid job	0.76	0.76	0.80	0.83	0.76	0.76	0.041***
Employed by Large Firm|paid job	0.88	0.87	0.88	0.85	0.88	0.87	-0.019***
Any Health Insurance	0.85	0.86	0.92	0.96	0.85	0.86	0.035***
ESHI	0.70	0.70	0.78	0.79	0.70	0.70	0.020***
<150%FPL†	0.12	0.10	0.08	0.06	0.12	0.10	0.007*
150-300%FPL†	0.20	0.16	0.15	0.09	0.20	0.16	-0.022***
Age	33.28	33.54	33.22	33.15	33.29	33.55	-0.336***
Married	0.44	0.44	0.43	0.39	0.44	0.44	-0.031***
Female	0.51	0.51	0.51	0.50	0.51	0.51	-0.010***

***p<0.01, **p<0.05, *p<0.1, block bootstrapped by state.
†FPL category defined for each individual based on status in the Jan-June 2006 period.
2004 SIPP Panel. Monthly weights used.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. 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.

Results in the fourth row suggest that hours increased by 1.8 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 0.20 hours per week on a base of 37.8 hours per week before reform, or by 0.2% 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% of hours in all jobs.

Taken together, these results 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 not consistent with the theoretical impact of an increase in ESHI for those who gained coverage, suggesting that mandate-based reform in Massachusetts had little if any impact on the aggregate labor market. To understand what is driving the labor market changes that we observe, we first examine the differences-in-differences impact on health insurance coverage in our sample, and then we examine wage trends at high frequency in Massachusetts and other states by ESHI status.

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 92% of individuals under the age of 65 without Medicare or military health insurance in Massachusetts had some type of health insurance before reform, increasing to 96% after reform. Outside of Massachusetts, health insurance coverage increased slightly from 85% to 86% over the same period. The simple differences-in-differences estimate for the increase in coverage in Massachusetts due to the reform is 3.5 percentage points; slightly lower but consistent with existing estimates (Long (2008); Kolstad and Kowalski (2010); Yelowitz and Cannon (2010)). ESHI coverage rose from 78% to 79% in Massachusetts from pre-reform to post-reform; coverage was steady at approximately 70% in non-Massachusetts states.

The final rows of Table 4 compare demographic characteristics in Massachusetts and other states. We see that roughly the same percent of individuals with paid jobs work for large firms in Massachusetts and other states (88% before reform). Massachusetts residents have higher incomes, so smaller numbers of individuals in Massachusetts qualify for Medicaid and CommCare under the Massachusetts reform thresholds. Other demographic characteristics reported look similar. We will control for time-invariant demographic characteristics in our regressions using individual fixed effects.

6.3 Wage Trends by ESHI Status

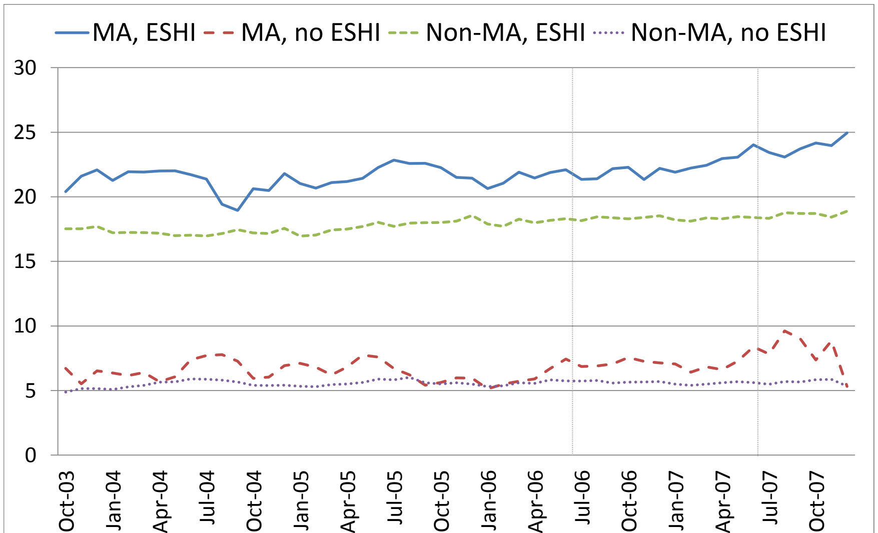
Figures 2 and 3 show wage trends over time by ESHI status in Massachusetts and other states, using the level of wages and the logarithm of wages, respectively. In these figures, as in the previous table, we use data from all months, not just interview months.

These figures give us several insights into the empirical relationship between ESHI and labor market outcomes in Massachusetts and other states. First, inside and outside of Massachusetts, wages are higher for jobs with ESHI than for jobs without ESHI, consistent with the literature that finds no compensating differential for ESHI. Second, within jobs with the same ESHI status, wages were higher in Massachusetts than other states before reform, but they were trending similarly. Massachusetts trends are noisier because they are based on a smaller sample of respondents. The trends are noisiest at the end of the sample period, when the sample size decreases. Third, after the reform, there is a barely visible increase in the slope of the Massachusetts trend lines relative to the trend lines outside of Massachusetts, which was reflected in our previous difference-in-differences estimates. Because of this issue, we incorporate variation by firm size in our preferred specifications.

Before incorporating variation by firm size, we explore the impact of incorporating longitudinal variation on wage trends by ESHI status for Massachusetts compared with other states. Because incorporating longitudinal variation places greater demands on the data, making the trend lines noisier, we combine each monthly response into mutually exclusive two-month periods. We run a regression analogous 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, omitting only the last two-month period before reform (May-June 2006).

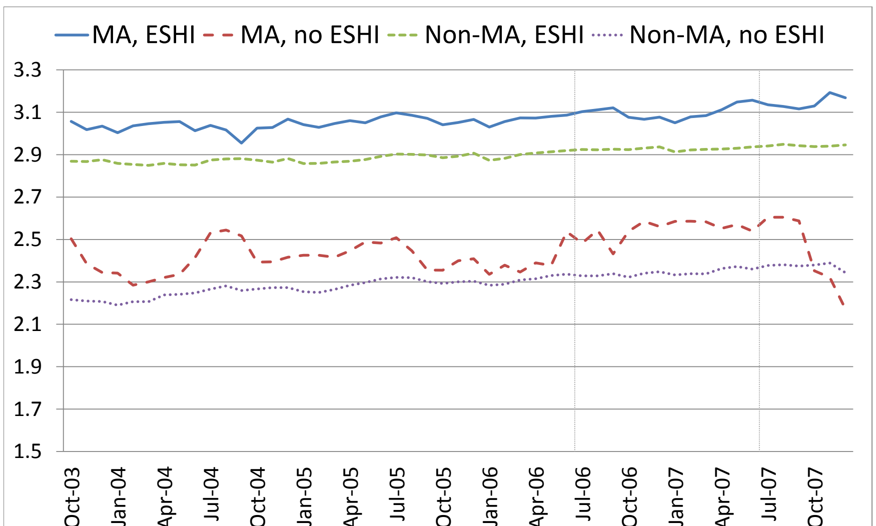
In Figure 4, we plot the vector of coefficients corresponding to β_{12} with the points connected by the dashed line labeled $ESHI$. This line gives the wage premium for jobs with ESHI relative to jobs without ESHI outside of Massachusetts. We define the wage premium as the empirical difference in wages between jobs with ESHI and jobs without ESHI; this might not be equal to the compensating differential in our model in the absence of adequate identification. We also plot the vector of coefficients corresponding to β_1 with the points connected by the solid line labeled

Figure 2: Wage Trends, MA vs. Non-MA



Weekly earnings / baseline hours per week, including without paid job (wage=0).
Full <65 population, excluding those with Medicare or military health insurance
Monthly weights are used to calculate means.

Figure 3: Log Wage Trends, MA vs. Non-MA



Log (weekly earnings / baseline hours per week | paid job & weekly earnings > 0).
Full <65 population, excluding those with Medicare or military health insurance
Monthly weights are used to calculate means.

Figure 4: Longitudinal Wage Premium for Jobs with ESHI Relative to Jobs Without ESHI, MA vs. Non-MA

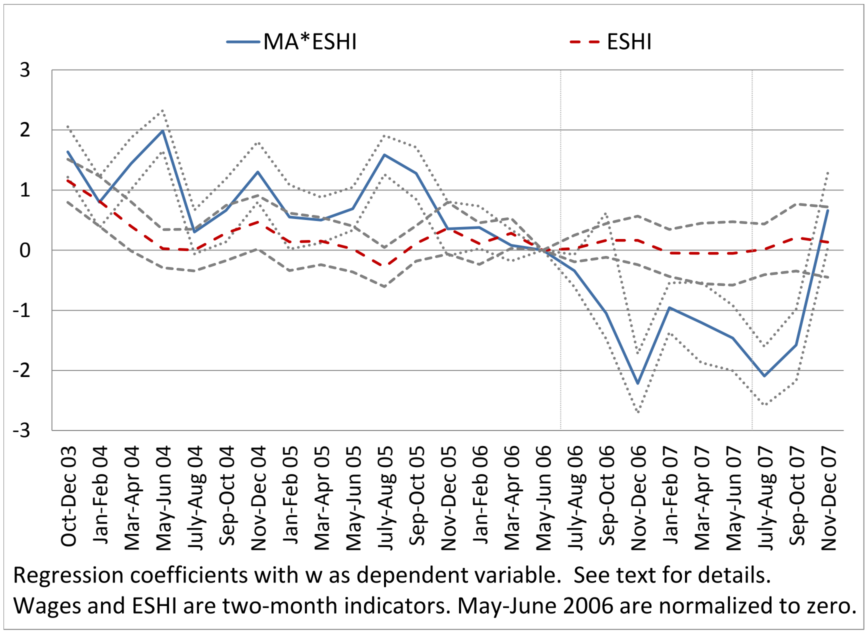
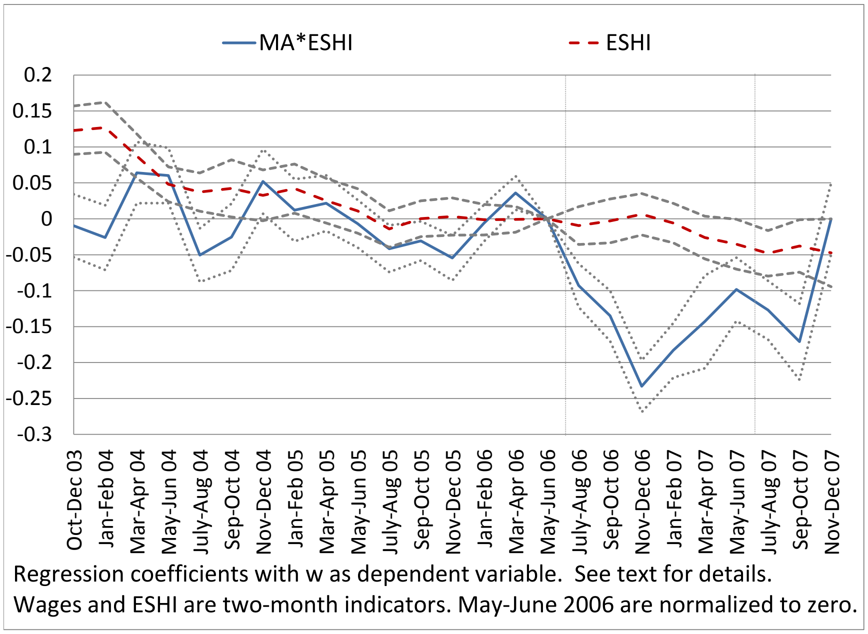


Figure 5: Longitudinal Log Wage Premium for Jobs with ESHI Relative to Jobs Without ESHI, MA vs. Non-MA



$MA * ESHI$. This line gives the differential wage premium for ESHI jobs relative to jobs without ESHI in Massachusetts, relative to other states. We also show 95% confidence intervals for both lines, block clustered by state. In Figure 5, we plot the corresponding coefficients from a regression with the logarithm of wages as the dependent variable, estimated only on individuals with a paid job. Because individual fixed effects are included in the regressions, the coefficients corresponding to β_{12} are identified by people outside of Massachusetts who change ESHI status in the given period relative to the omitted period. The coefficients corresponding to β_1 are identified by people within Massachusetts who change ESHI status in the given period relative to the omitted period or by people who move between Massachusetts and other states in the given period relative to the omitted period.

In the omitted period, the wage premium for jobs with ESHI relative to jobs without ESHI is normalized to zero. Outside of Massachusetts, we see that the wage premium for ESHI jobs increases over time in Figure 4. However, when we examine the log wage premium in Figure 5, we see a downward trend, suggesting that wages increase faster in jobs without ESHI, reflecting the secular increase in the cost of health insurance relative to inflation over this time period.

Within Massachusetts, as shown in Figure 4, the premium for ESHI jobs relative to jobs without ESHI is higher before the reform relative to other states. However, it trends similarly for individuals with paid jobs before reform, as shown in Figure 4. The similar trends in Massachusetts and other states before reform lend 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 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. This is true for all of the two-month periods after May-June of 2006 except for for the last two-month period, which shows a puzzling increase in both figures. The increase seems to be due to sizeable attrition from the sample in the last two months and not to a real increase in the wage premium. If we examine the underlying data more closely, we see that there are approximately 4,000 observations (with multiple observations for approximately 2,000 unique individuals) in Massachusetts in each two-month period before reform, and approximately 1,500 observations (about 725 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 511 observations (350 unique individuals). Our baseline regression, 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.

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 4 and 5, 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 4, 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 $2/hour, which corresponds to a roughly 10% decline in Figure 5. These figures provide the first evidence that the Massachusetts reform affected jobs with ESHI, as we predict in our model. They also signal that this can be an exogenous sources of variation that will be useful in identifying a compensating differential for ESHI.

7 Results and Discussion

We report results from the baseline wage and hours equations in Table 5, 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 and, therefore, individuals without ESHI in Massachusetts after reform provide an additional control group for individuals with ESHI in Massachusetts after reform. The estimated β_1 tells us that hourly wages are $2.61 lower for individuals with ESHI relative to individuals without ESHI, after the reform relative to before the reform, in Massachusetts relative to other states. This coefficient is statistically significant at the 99% level. Annualizing the decrease in hourly wages for a full-time worker, this coefficient implies that the compensating differential for ESHI is $-5,426.72(= -2.609 * 40 * 52)$ dollars 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 calculate a comparison 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% for an individual and 73% for a family plan. We weight our estimate of the annualized b to reflect the likely share of individual vs. family plans in the SIPP population. 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 average annualized b 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. In this context, the magnitude of the compensating differential we estimate based on β_1 seems reasonable, especially considering that the assumption that the employer penalty is zero should bias our estimate toward zero, suggesting that our preferred estimate will be larger and even more in line with actual costs of ESHI to employers.

If we do not assume that the employer penalty is zero, our preferred estimate of the compensating differential from the baseline model is $\beta_1 + \beta_8 + \beta_{11} = -2.609 + 2.054 + 3.215 = 2.660$, which is of the wrong theoretical sign. Recall from Section 5.1 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

Table 5: 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	-2.609*** [-3.068, -2.057]	γ_1	-1.183*** [-1.824, -0.568]
MA*ESHI*During	β_1^d	-2.241*** [-2.540, -1.725]	γ_1^d	-2.377*** [-2.762, -1.872]
MA*ESHI	β_8	2.054*** [1.656, 2.412]	γ_8	1.409*** [0.609, 1.729]
MA*After	β_{11}	3.215*** [2.774, 3.480]	γ_{11}	2.308*** [1.764, 2.787]
MA*During	β_{11}^d	2.393*** [1.972, 2.619]	γ_{11}^d	3.010*** [2.566, 3.301]
ESHI*After	β_{12}	-0.122 [-0.614, 0.397]	γ_{12}	-0.982*** [-1.343, -0.402]
ESHI*During	β_{12}^d	-0.178 [-0.565, 0.114]	γ_{12}^d	-0.775*** [-0.993, -0.459]
ESHI	β_{19}	3.847*** [3.392, 4.099]	γ_{19}	6.495*** [6.091, 6.730]
After	β_{22}	0.536*** [0.198, 0.831]	γ_{22}	1.243*** [0.697, 1.662]
During	β_{22}^d	0.413*** [0.174, 0.711]	γ_{22}^d	0.966*** [0.621, 1.219]
Observations		495,420		479,374
R-squared		0.758		0.832

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries.
Only includes interview months.
Individual and state fixed effects included. Monthly weights used.

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 (our estimated compensating differential is \$3.22 in Massachusetts before reform, netting out differences with other states), so we focus on our preferred estimate that compares equilibrium A to equilibrium F within our preferred specification.

In the second column of Table 5, our estimate of the hours differential using γ_1 tells us that weekly hours are -1.183 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. This estimate is statistically different from zero at the 99% level, suggesting that the penalty-and-subsidy-inclusive valuation will be less than full, and we need to turn to our model to calculate it. Analysis of the γ_8 and γ_{11} coefficients parallels analysis of the β_8 and β_{11} coefficients, suggesting that the specification with firm size interactions will be our preferred specification.

7.1 Estimates of the Compensating Differential for ESHI

To obtain our preferred estimates of the compensating differential and hours differential for ESHI, we estimate our preferred specification with firm interaction terms, and we report the results in Table 6. To isolate small firms that are not subject to the employer mandate, we categorize individuals without paid jobs as working for a large firm. 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 empirical equilibria that map to the theoretical equilibria presented in Figure 1. Figure 6 plots the empirical equilibria. All equilibria are relative to equilibrium A (no ESHI before reform) at the origin.

The most important relationship to notice in Figure 6 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. Our preferred estimate of the compensating differential for ESHI from Table 1 is the negative of the vertical distance between equilibrium A and equilibrium F. As depicted in Figure 6, the third column of Table 6 shows that $w_F - w_A$ is equal to -\$2.91 per hour. Annualizing the point estimate for a full-time worker, the implied compensating differential is -\$6,058 per year, which is only slightly smaller in magnitude than 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 less than full. We obtain the annualized 95% confidence interval on the compensating differential of -\$8,611 to -\$4,098 per year by block-bootstrapping by state.[14]

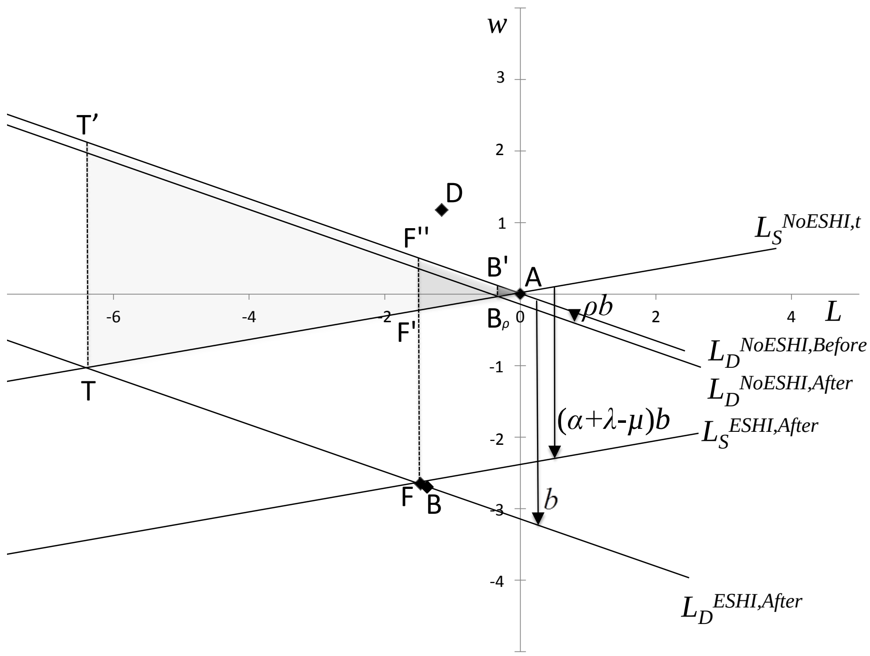
[14]To obtain all confidence intervals, we perform a simple nonparametric block bootstrap. We first draw a sample

Table 6: Results from Preferred 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	0.933**	γ_1	0.189	$w_D - w_A$	2.157***
		[0.031, 2.680]		[-0.936, 1.289]		[1.788, 2.455]
MA*ESHI*During*Large	β_1^d	-0.444	γ_1^d	-0.277	$w_F - w_B$	-0.231
		[-1.926, 1.079]		[-0.765, 0.610]		[-0.634, 0.230]
MA*ESHI*Large	β_8	1.002**	γ_8	-1.159***	$w_D - w_B$	4.839***
		[0.231, 1.826]		[-2.044, -0.618]		[4.102, 5.865]
MA*After*Large	β_{11}	-2.682***	γ_{11}	-1.375**	$w_F - w_A$	-2.913***
		[-3.752, -2.021]		[-2.333, -0.459]		[-4.140, -1.970]
MA*During*Large	β_{11}^d	0.815	γ_{11}^d	0.144		
		[-0.328, 2.185]		[-0.669, 0.663]		
ESHI*After*Large	β_{12}	0.354	γ_{12}	-0.698	$L_D - L_A$	0.891**
		[-0.922, 1.394]		[-1.639, 0.247]		[0.039, 1.250]
ESHI*During*Large	β_{12}^d	0.567	γ_{12}^d	-0.780**	$L_F - L_B$	-0.095
		[-0.712, 1.899]		[-1.366, -0.255]		[-1.117, 0.511]
ESHI*Large	β_{19}	2.148***	γ_{19}	7.097***	$L_D - L_B$	2.266***
		[1.366, 2.732]		[6.563, 7.589]		[1.002, 2.924]
After*Large	β_{22}	-0.848***	γ_{22}	-0.137	$L_F - L_A$	-1.470***
		[-1.540, -0.113]		[-1.079, 0.521]		[-2.933, -0.678]
During*Large	β_{22}^d	-0.725*	γ_{22}^d	0.099		
		[-2.008, 0.095]		[-0.497, 0.567]		
MA*Large	β_{23}	0.544	γ_{23}	3.868***	s	0.19
		[-0.689, 3.426]		[0.705, 5.370]		-
Large	β_{24}	-3.903***	γ_{24}	-14.581***	d	-0.38
		[-6.599, -3.057]		[-15.765, -11.404]		-
MA*ESHI*After	β_{1e}	-3.322***	γ_{1e}	-1.175**	ρ	0.041***
		[-5.130, -2.192]		[-1.969, -0.393]		[0.028, 0.057]
MA*ESHI*During	β_{1e}^d	-1.799**	γ_{1e}^d	-1.787***	b	3.471***
		[-3.254, -0.238]		[-2.442, -1.147]		[2.509, 5.001]
MA*ESHI	β_{8e}	1.155**	γ_{8e}	2.050***	α	-0.573***
		[0.374, 1.915]		[1.359, 2.476]		[-0.860, -0.376]
MA*After	β_{11e}	5.378***	γ_{11e}	3.169***	$\lambda - \mu_x$	1.331***
		[4.626, 6.259]		[2.411, 3.838]		[1.077, 1.588]
MA*During	β_{11e}^d	1.628**	γ_{11e}^d	2.543***	$\alpha + \lambda - \mu_x$	0.759***
		[0.218, 2.688]		[1.925, 3.123]		[0.587, 0.878]
ESHI*After	β_{12e}	-0.447	γ_{12e}	-0.307	$ESHI_{After}$	0.79
		[-1.631, 1.058]		[-1.001, 0.507]		-
ESHI*During	β_{12e}^d	-0.678	γ_{12e}^d	-0.11	$\frac{b}{\tau}$	1
		[-2.065, 0.566]		[-0.583, 0.460]		-
ESHI	β_{19e}	2.121***	γ_{19e}	0.705***		
		[1.379, 2.753]		[0.281, 1.022]		
After	β_{22e}	1.301***	γ_{22e}	1.227***	DWL_m	0.490***
		[0.552, 1.991]		[0.677, 1.904]		[0.107, 1.940]
During	β_{22e}^d	1.058***	γ_{22e}^d	0.849***	$\frac{DWL_m}{DWL_\tau}$	0.046***
		[0.241, 2.320]		[0.440, 1.362]		[0.012, 0.135]
Observations		495,420		479,374		
R-squared		0.759		0.842		

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. Only includes interview months.
All specifications include individual, state, and state*large firm fixed effects. Monthly weights used.
Large firm defined as >25 employees.

Figure 6: Empirical Estimates of Wage and Hours Equilibria



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 6, the third column of Table 6 shows that $L_F - L_A$ is equal to -1.47 hours per week. This estimate is statistically different from zero at the 99% level, also suggesting that the penalty-and-subsidy-inclusive valuation is less than full.

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 $w_F - w_B$. Because the relationship between equilibrium B and equilibrium A is not identified by changes in ESHI status due to reform, we have reason to doubt our identification of B.

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

of 51 states with replacement — the data include all 50 US 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% confidence interval. We report significance when this confidence interval or the analogous 90% or 99% confidence interval does not include zero.

equilibrium A. As shown in the third column of Table 6, 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 the benefit. 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 $\lambda - \mu$. As shown in the third column of Table 6, our separate estimates of these parameters are nonsensical. Although these parameters would be interesting to analyze, we do not need to separately identify them to identify the aggregate welfare impact of mandate-based health reform.

7.2 Estimating 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 best-identified (equilibrium F relative to equilibrium A), and calibrate other sufficient statistics as necessary.

We first translate our estimated compensating and hours differentials into our key sufficient statistics — the cost of ESHI to employers b and the penalty-and-subsidy-inclusive valuation $\alpha + \lambda - \mu_x$. We can see from the expressions in Table 2 and the geometry of Figure 6 that an estimate of the slope of the demand curve is sufficient to translate the compensating and hours differentials into b, and an estimate of the slope of the supply curve is sufficient to translate the compensating and hours differentials into $\alpha + \lambda - \mu_x$. Using elasticity estimates from the literature, discussed in Section 4.3, and average hours and wages for the full SIPP sample before reform, we set the slope of the supply curve equal to 0.19, and we set the slope of the demand curve equal to -0.38.[15] As shown in Figure 6 and in the third column of Table 6, we obtain a value of 3.47 for b, which translates into $7,220 annually for a full-time worker. We obtain a value of 0.759 for the penalty-and-subsidy-inclusive valuation, which suggests that workers value health expenditures made by their employers at about 76 cents per dollar.

Next, we translate the penalty-and-subsidy-inclusive valuation of health insurance into the deadweight loss of mandate-based health reform for individuals who have ESHI after reform, depicted as triangle $F'AF''$ in the theoretical and empirical figures. The area of the triangle is equal to 0.616, which translates into an annual deadweight loss of $32 (=0.616*52) for a full-time worker. This deadweight loss is only 6% of the size of the deadweight loss triangle associated with tax-based reform TAT', assuming that the tax τ is equal to the cost of the benefit to employers b.

[15] We report the calibrated values of s and d in Table 6 because we use them to calibrate the subsequent statistics. The estimated value for s is 1.95 with a 95% confidence interval of [1.12,6.13]***, and the estimated value for d is 2.15 with a 95% confidence interval of [1.65,3.25]***.

Finally, we calculate the deadweight loss of mandate-based health reform for individuals who do not have ESHI after reform, which requires an estimate of ρ. Rather than estimating ρ using equilibrium B, we calibrate it such that the dollar value of the employer penalty ρb is equal to the statutory penalty of $295/year. We plot the analog of equilibrium B that corresponds to the calibrated ρ as the point B_ρ. The associated welfare triangle is given by $B_\rho AB'$. The empirical area of this triangle is 0.0176, which translates into an annual deadweight loss of $0.92 (=0.0176*52) for a full-time worker.

To obtain the deadweight loss of mandate-based health reform, DWL_m, we weight the two triangles by ESHI status in Massachusetts after reform, setting $ESHI_{After}$ equal to 0.79, according to our table of summary statistics. Putting everything together using equation (4), we find that the deadweight loss of mandate-based health reform is equal to $25 per year for a full-time worker. Relative to tax-based health reform, mandate-based health reform is substantially more efficient: using equation (5), we calculate $DWL_m/DWL_\tau = .046$; the distortions that mandate-based health reform induces are less than 5% of the distortions induced by tax-based health reform.[16]

7.3 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. The confidence interval reported in Table 6 suggests that we are 95% certain that the deadweight loss of mandate-based health reform is between 1.2% and 13.5% of the deadweight loss of tax-based health reform. However, 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 0.041, reflecting that the statutory employer penalty of $295 is approximately 4.1% of the estimated cost of ESHI $\widehat{b}$. If we increase ρ such that the penalty is instead 25% 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 6% 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 2, 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 42% of the estimated cost of ESHI. However, the deadweight loss for individuals without ESHI

[16] We find that the deadweight loss of tax-based health reform is $550 per year for a full-time worker. A tax of size $\tau = b$=$3.47 per hour, would raise $4,140 per person per year ($3.47 per hour*(29.03 hours per week in Massachusetts before reform - 6.09 hours per week after reform)*52 weeks per year), which would not be large enough to finance the estimated average annual cost of ESHI per worker of $7,220. Therefore, we are conservative in setting $\tau = b$. Under the tax, the ratio of the deadweight loss to revenue raised is 0.13, which is on the lower end of the range but consistent with prominent estimates from the literature such as Ballard et al. (1985) and Feldstein (1999).

after mandate-based reform is only 17% 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 8.2% 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 preferred specification. Suppose that ESHI costs 10% 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 \$454 annually, but the deadweight loss of mandate-based health reform is still only 5.6% as large. Even if b/τ=1.50 such that ESHI costs 50% more to provide than government-provided health insurance, the deadweight loss of mandate-based health reform is still only 10.4% 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: $(1 - \widehat{\alpha + \lambda - \mu_x})$. Using the expressions in Table 2, 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 $s = 0.1 * 27.68/14.60 = 0.19$ to $s = 0.38$), the relative deadweight loss increases to 8.2%. If we increase it further to 0.5 ($s = 0.95$), the relative deadweight loss increases to 25%. Alternatively, holding supply constant, if we decrease the calibrated labor demand elasticity from -0.2 to -0.4 (from $d = -0.2 * 27.68/14.60 = -0.38$ to $d = -0.76$), the relative deadweight loss increases to 9.5%. If we decrease it further to -1.2 ($d = -2.28$), the relative deadweight loss increases to 26.5%. 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 6, 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.

7.4 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 0.76 from our preferred specification suggests that individuals who take up ESHI because of reform value it at 76 cents on the dollar on average (even after taking the tax-preference for ESHI into account), so they would not have taken it up in the absence of reform.

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 preferred specifications on three subsets of the full population: individuals in New England, those earning more than 300% of the FPL, and those who are married.

In the first column of Tables 7 and 8, 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 7 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 95% confidence intervals on both coefficients include the respective coefficients from the main sample. Furthermore, the compensating and hours differentials from the preferred specification are very similar to those from the main sample. In the sample that includes only New England, the annualized estimate of the cost of ESHI b is \$9,703, slightly larger than our main estimate (\$6,058), but the penalty-and-subsidy-inclusive valuation of 0.69 is very similar to our main estimate (0.76). 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 7.7% — which is similar to our main estimate of 4.6%.

In the second column of Table 7, we restrict our estimation sample to include only individuals above 300% of the federal poverty level before reform. We classify individuals into income groups using the first period of available data to avoid regressing wages on contemporaneous measures of income. Individuals above 300% of FPL before reform are not eligible for any subsidies, which

Table 7: Results from Baseline Specification on Different Samples

		(1a)	(1b)	(1c)		(2a)	(2b)	(2c)
		w	w	w		L	L	L
		Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)	Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)	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)	Hours per week, including individuals without a paid job (hours=0)	Hours per week, including individuals without a paid job (hours=0)
SAMPLE		*New England*	*300%FPL+*	*Married*		*New England*	*300%FPL+*	*Married*
MA*ESHI*After	β_1	-4.519***	-2.253***	-3.107***	γ_1	-2.564*	-1.288***	0.135
		[-8.666, -1.993]	[-2.763, -1.578]	[-3.919, -2.630]		[-4.076, 0.012]	[-2.041, -0.442]	[-0.559, 0.885]
MA*ESHI*During	β_1^d	-1.197	-2.306***	0.211	γ_1^d	-2.762**	-3.337***	-1.791***
		[-4.981, 2.383]	[-2.645, -1.530]	[-0.360, 0.559]		[-5.190, -0.376]	[-3.786, -2.628]	[-2.491, -1.341]
MA*ESHI	β_8	3.343***	1.479***	1.385***	γ_8	2.09	0.836	0.105
		[1.762, 6.234]	[0.893, 2.111]	[0.838, 2.047]		[-1.130, 3.967]	[-0.149, 1.330]	[-0.694, 0.551]
MA*After	β_{11}	3.543***	3.650***	3.681***	γ_{11}	2.094*	2.167***	0.790**
		[1.062, 7.300]	[2.979, 3.970]	[3.214, 4.309]		[-0.447, 3.703]	[1.297, 2.729]	[0.144, 1.441]
MA*During	β_{11}^d	0.844	2.768***	0.369*	γ_{11}^d	3.225**	3.612***	2.211***
		[-2.631, 4.889]	[2.014, 3.048]	[-0.044, 0.945]		[0.982, 5.361]	[2.808, 4.004]	[1.793, 2.895]
ESHI*After	β_{12}	1.811	-0.239	-0.309	γ_{12}	0.428	-0.958**	-0.565
		[-0.591, 5.922]	[-0.906, 0.380]	[-0.863, 0.262]		[-1.994, 2.161]	[-1.471, -0.189]	[-1.080, 0.268]
ESHI*During	β_{12}^d	-1.186	-0.325	-0.323	γ_{12}^d	-0.358	-0.701***	-0.567*
		[-4.582, 2.568]	[-0.979, 0.115]	[-0.675, 0.152]		[-2.662, 2.112]	[-1.193, -0.149]	[-0.883, 0.158]
ESHI	β_{19}	2.283*	4.320***	3.936***	γ_{19}	5.184***	6.338***	5.975***
		[-0.304, 3.895]	[3.625, 4.735]	[3.326, 4.259]		[3.013, 7.783]	[5.660, 6.684]	[5.307, 6.356]
After	β_{22}	-0.017	0.594**	-0.309*	γ_{22}	1.239	1.114***	-0.725**
		[-3.745, 2.249]	[0.095, 1.100]	[-0.878, 0.063]		[-0.494, 3.713]	[0.440, 1.687]	[-1.501, -0.127]
During	β_{22}^d	1.837	0.718***	-0.046	γ_{22}^d	0.554	0.907***	-0.252
		[-2.200, 5.082]	[0.334, 1.303]	[-0.530, 0.283]		[-1.647, 2.646]	[0.395, 1.402]	[-0.945, 0.137]
Observations		28,857	345,287	258,222		27,925	333,106	246,483
R-squared		0.715	0.764	0.826		0.841	0.83	0.852

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. Only includes interview months.
Individual and state fixed effects included. Monthly weights used.
New England states include MA, CT, NH, VT, ME, RI.
300%FPL+ sample includes those who DO NOT fall into the <150%FPL or 150-300%FPL categories

means that their penalty-and-subsidy-inclusive valuation should be higher. As we show in the second column of Table 8, the point estimate for the penalty-and-subsidy-inclusive valuation in this sample is indeed much higher than it is in the main specification — it is almost full. Because the penalty-and-subsidy-inclusive valuation in this sample is so large, mandate-based health reform causes minimal distortion to the labor market. The ratio of the deadweight loss of mandate-based health reform to that of tax-based health reform is 0.3%.

In the third columns of Tables 7 and 8, 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 8 that the valuation of ESHI for married individuals is approximately 0.80, with a 95% confidence interval of 0.74 to 0.88, slightly larger than the valuation of ESHI for the full sample.

Table 8: Results from Preferred Specification on Different Samples

		(1a)	(1b)	(1c)		(2a)	(2b)	(2c)		(3a)	(3b)	(3c)
		w	w	w		L	L	L				
		Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)	Weekly earnings / baseline hours per week, including individuals without a paid job (wage=0)	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)	Hours per week, including individuals without a paid job (hours=0)	Hours per week, including individuals without a paid job (hours=0)		Compensating and Hours Differentials, Sufficient Statistics, and Welfare Impact of Health Reform		
SAMPLE		*New England*	*300%FPL+*	*Married*		*New England*	*300%FPL+*	*Married*		*New England*	*300%FPL+*	*Married*
MA*ESHI*After*Large	β_1	0.443	0.252	1.470*	γ_1	-2.879	0.699	-0.548	$w_D - w_A$	3.080***	1.817***	0.909***
		[-4.665, 2.914]	[-1.525, 2.648]	[-0.100, 3.070]		[-8.848, 5.634]	[-0.730, 2.170]	[-2.976, 0.790]		[1.904, 4.632]	[1.276, 2.382]	[0.278, 1.453]
MA*ESHI*During*Large	β_1^d	-0.618	-2.623***	-0.34	γ_1^d	0.425	-3.615***	1.123*	$w_F - w_B$	-1.414	-0.143	-2.223***
		[-3.897, 5.692]	[-5.158, -0.474]	[-1.575, 1.176]		[-4.104, 3.762]	[-4.583, -2.684]	[-0.420, 1.927]		[-5.975, 1.283]	[-0.581, 0.478]	[-2.945, -1.804]
MA*ESHI*Large	β_8	-0.213	1.992***	-2.252***	γ_8	-2.6283	1.104**	-7.098***	$w_D - w_B$	5.359*	2.806***	8.155***
		[-4.935, 4.052]	[0.829, 3.034]	[-3.503, -1.245]		[-5.602, -0.375]	[0.043, 1.799]	[-8.091, -6.006]		[-0.480, 11.195]	[1.384, 4.219]	[6.920, 9.401]
MA*After*Large	β_{11}	-2.278	-0.989	-7.246***	γ_{11}	0.252	-0.205	-2.598**	$w_F - w_A$	-3.692	-1.132	-9.468***
		[-7.478, 2.931]	[-2.563, 0.393]	[-8.574, -5.837]		[-5.775, 4.938]	[-1.443, 1.085]	[-3.813, -0.013]		[-9.708, 2.639]	[-2.893, 0.530]	[-11.035, -8.134]
MA*During*Large	β_{11}^d	0.837	3.435***	-0.822	γ_{11}^d	-1.480**	3.224***	-1.521*				
		[-5.600, 4.714]	[1.648, 6.208]	[-1.950, 0.487]		[-3.037, -0.288]	[2.474, 3.970]	[-2.200, 0.209]				
ESHI*After*Large	β_{12}	0.972	0.234	0.347	γ_{12}	2.628	-0.246	-0.356	$L_D - L_A$	1.042	0.593	-1.499***
		[-1.160, 5.105]	[-1.620, 1.885]	[-1.144, 1.521]		[-5.688, 8.279]	[-1.403, 0.977]	[-1.584, 1.803]		[-2.438, 2.844]	[-0.388, 1.037]	[-2.370, -1.037]
ESHI*During*Large	β_{12}^d	0.906	1.385	-0.225	γ_{12}^d	-0.953	-0.07	-0.592	$L_F - L_B$	-2.812**	0.198	-1.335***
		[-5.213, 3.806]	[-0.467, 3.688]	[-1.549, 0.862]		[-4.236, 2.744]	[-0.825, 0.658]	[-1.456, 0.750]		[-4.211, -1.566]	[-0.878, 0.813]	[-2.579, -0.319]
ESHI*Large	β_{19}	3.205	2.227***	2.413***	γ_{19}	8.0353	6.621***	7.216***	$L_D - L_B$	0.789	0.798	1.099
		[-0.429, 7.525]	[1.151, 3.007]	[1.439, 3.469]		[6.186, 10.857]	[5.999, 7.349]	[6.194, 8.141]		[-5.022, 5.373]	[-0.563, 1.617]	[-1.410, 2.395]
After*Large	β_{22}	-1.596	-0.628	-0.472	γ_{22}	-2.219	-0.597	-0.897	$L_F - L_A$	-2.560	-0.007	-3.932***
		[-6.807, 3.098]	[-2.050, 0.609]	[-1.749, 0.591]		[-6.483, 3.405]	[-1.859, 0.412]	[-3.246, 0.425]		[-7.679, 1.929]	[-1.352, 0.978]	[-5.101, -2.140]
During*Large	β_{22}^d	-1.082	-1.221	0.038	γ_{22}^d	1.031**	-0.383	-0.091				
		[-4.545, 4.924]	[-3.704, 0.172]	[-1.189, 0.937]		[0.258, 2.935]	[-1.076, 0.228]	[-1.675, 0.828]				
MA*Large	β_{23}	4.388	-0.659	2.571**	γ_{23}	8.518**	2.827	14.843***	s	0.190	0.190	0.190
		[-2.335, 7.951]	[-6.595, 1.810]	[0.434, 4.757]		[1.823, 11.647]	[-2.444, 4.605]	[2.420, 16.196]		-	-	-
Large	β_{24}	-7.528**	-4.424	-3.117***	γ_{24}	-18.8453	-15.181***	-17.351***	d	-0.380	-0.380	-0.380
		[-10.796, -0.855]	[-6.572, 1.538]	[-5.075, -1.383]		[-21.619, -12.260]	[-16.691, -10.002]	[-18.652, -4.772]		-	-	-
MA*ESHI*After	β_{1e}	-4.937**	-2.212***	-4.601***	γ_{1e}	-0.975	-1.093*	0.712*	ρ	0.030	0.125	0.013***
		[-7.889, -0.929]	[-4.533, -0.347]	[-6.318, -3.244]		[-5.927, 4.459]	[-2.043, 0.008]	[-0.218, 2.285]		[-0.141, 0.247]	[-0.948, 2.214]	[0.011, 0.015]
MA*ESHI*During	β_{1e}^d	-0.732	-0.034	0.588	γ_{1e}^d	-3.079	0.388	-2.374***	b	4.665	1.135	10.963***
		[-8.839, 3.209]	[-2.018, 2.522]	[-0.822, 1.718]		[-6.888, 0.223]	[-0.335, 1.364]	[-3.094, -1.595]		[-2.107, 12.087]	[-0.521, 3.077]	[9.699, 12.515]
MA*ESHI	β_{8e}	3.294	-0.174	3.161***	γ_{8e}	3.669*	-0.511*	5.599***	α	-0.618	-1.502	-0.109***
		[-1.224, 9.607]	[-1.254, 0.960]	[2.101, 4.483]		[-0.634, 6.720]	[-1.279, 0.060]	[4.449, 6.306]		[-5.598, 2.991]	[-30.958, 13.484]	[-0.168, -0.052]
MA*After	β_{11e}	5.446*	4.220***	10.257***	γ_{11e}	2.602	1.554***	2.979***	$\lambda - \mu_x$	1.305	2.499	0.904***
		[-0.818, 11.705]	[2.751, 5.555]	[9.113, 11.408]		[-2.333, 6.404]	[0.473, 2.244]	[1.227, 3.747]		[-1.476, 6.058]	[-15.340, 27.383]	[0.840, 1.035]
MA*During	β_{11e}^d	0.206	-0.172	0.968*	γ_{11e}^d	4.4463	0.26	3.119***	$\alpha + \lambda - \mu_x$	0.687*	0.996*	0.796***
		[-4.579, 8.676]	[-2.787, 1.350]	[-0.161, 2.044]		[1.649, 6.775]	[-0.532, 0.791]	[2.141, 3.712]		[-0.666, 2.233]	[-2.116, 3.341]	[0.740, 0.878]
ESHI*After	β_{12e}	1.085	-0.495	-0.717	γ_{12e}	-0.714	-0.692	-0.274	$ESHI_{After}$	0.790	0.790	0.790
		[-2.534, 3.976]	[-2.256, 1.542]	[-1.913, 0.905]		[-5.726, 4.226]	[-1.546, 0.256]	[-1.616, 0.623]		-	-	-
ESHI*During	β_{12e}^d	-1.853	-1.545*	-0.207	γ_{12e}^d	0.747	-0.711*	-0.103	$\frac{b}{\tau}$	1.000	1.000	1.000
		[-5.510, 5.839]	[-3.905, 0.246]	[-1.259, 1.062]		[-2.202, 4.526]	[-1.459, 0.025]	[-0.814, 0.643]		-	-	-
ESHI	β_{19e}	-0.167	2.543***	2.065***	γ_{19e}	-1.148	0.999**	0.269				
		[-5.941, 3.934]	[1.528, 3.501]	[0.798, 2.863]		[-4.292, 2.466]	[0.179, 1.530]	[-0.424, 0.925]				
After	β_{22e}	1.29	1.206*	0.218	γ_{22e}	1.948	1.527***	0.056	DWL_m	1.479***	0.004***	3.486***
		[-4.078, 7.095]	[-0.103, 2.519]	[-0.857, 1.228]		[-1.737, 6.424]	[0.776, 2.400]	[-0.770, 1.600]		[0.012, 13.313]	[0.004, 0.468]	[1.035, 5.864]
During	β_{22e}^d	2.64	1.800***	0.012	γ_{22e}^d	-0.653	1.270***	-0.131	$\frac{DWL_m}{DWL_\tau}$	0.077***	0.003***	0.033***
		[-5.150, 7.209]	[0.427, 4.190]	[-0.846, 1.092]		[-3.404, 1.811]	[0.692, 1.962]	[-0.755, 0.712]		[0.004, 2.198]	[0.001, 18.630]	[0.012, 0.053]
Observations		28,857	345,287	258,222		27,925	333,106	246,483				
R-squared		0.716	0.764	0.826		0.849	0.838	0.858				

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. 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.
300%FPL+ sample includes those who DO NOT fall into the <150%FPL or 150-300%FPL categories

7.5 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.

We now investigate whether we observe responses on the intensive margin and whether the distinction between the intensive and extensive margins affects our findings. We first restrict our sample to 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 9 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 9% to 12% of income, broadly consistent with our main results. The estimates of γ_1, however, do not show any evidence in any of the samples of an hours differential that is significant at the 95% level. This result for individuals with paid jobs suggests suggests that much of the decline in hours that we observe in the main sample is driven by the extensive margin decision of whether to work.

In the results from the preferred specification restricted to workers, shown in Table 10, we continue 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 4.2% to 18.5% across all six specifications, and the largest upper bound of the 95% confidence interval is 29%.

Overall, our results that include only the intensive margin decision are consistent with our main results, suggesting that the extensive margin decision of whether to work does not drive our

Table 9: 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	-4.580***	-0.116***	-4.401***	-0.097***	-5.482***	-0.120***
		[-5.201, -3.899]	[-0.142, -0.085]	[-5.099, -3.658]	[-0.122, -0.066]	[-6.153, -4.624]	[-0.147, -0.083]
MA*ESHI*During	β_1^d	-3.958***	-0.213***	-4.101***	-0.233***	-4.414***	-0.244***
		[-4.515, -2.929]	[-0.235, -0.177]	[-4.633, -3.118]	[-0.266, -0.200]	[-5.065, -3.418]	[-0.281, -0.203]
MA*ESHI	β_8	3.552***	0.107***	3.385***	0.108***	4.033***	0.123***
		[2.986, 3.851]	[0.083, 0.124]	[2.680, 3.779]	[0.073, 0.130]	[3.215, 4.467]	[0.088, 0.148]
MA*After	β_{11}	4.881***	0.139***	4.960***	0.122***	5.624***	0.135***
		[4.372, 5.356]	[0.112, 0.166]	[4.324, 5.434]	[0.095, 0.144]	[4.942, 6.089]	[0.101, 0.158]
MA*During	β_{11}^d	3.808***	0.217***	4.224***	0.240***	4.505***	0.257***
		[2.913, 4.250]	[0.185, 0.234]	[3.346, 4.704]	[0.210, 0.268]	[3.601, 5.051]	[0.223, 0.287]
ESHI*After	β_{12}	-0.674**	-0.069***	-0.57	-0.055***	-0.546	-0.054***
		[-1.319, -0.053]	[-0.097, -0.037]	[-1.242, 0.240]	[-0.080, -0.031]	[-1.317, 0.199]	[-0.088, -0.027]
ESHI*During	β_{12}^d	-0.663**	-0.039***	-0.617**	-0.033**	-0.698**	-0.030*
		[-1.384, -0.177]	[-0.063, -0.021]	[-1.349, -0.105]	[-0.057, -0.006]	[-1.550, -0.069]	[-0.058, 0.005]
ESHI	β_{19}	2.368***	0.171***	2.258***	0.167***	2.145***	0.155***
		[1.920, 2.754]	[0.150, 0.182]	[1.846, 2.590]	[0.143, 0.179]	[1.711, 2.601]	[0.132, 0.168]
After	β_{22}	2.198***	0.130***	1.842***	0.104***	1.534***	0.090***
		[1.663, 2.595]	[0.098, 0.152]	[1.356, 2.175]	[0.079, 0.123]	[1.068, 2.003]	[0.062, 0.117]
During	β_{22}^d	1.600***	0.082***	1.406***	0.071***	1.272***	0.057***
		[1.091, 2.287]	[0.060, 0.104]	[0.944, 2.087]	[0.043, 0.094]	[0.737, 2.061]	[0.022, 0.084]
Observations		327,388	327,388	281,457	281,457	262,988	262,988
R-squared		0.792	0.776	0.806	0.791	0.809	0.8

			(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	γ_1	-0.207	-0.011	0.29	0.022**	-0.622*	-0.018
		[-0.580, 0.256]	[-0.025, 0.009]	[-0.181, 0.792]	[0.003, 0.043]	[-1.218, 0.004]	[-0.042, 0.004]
MA*ESHI*During	γ_1^d	-1.890***	-0.046***	-1.681***	-0.017*	-2.285***	-0.044***
		[-2.181, -1.525]	[-0.057, -0.031]	[-2.111, -1.304]	[-0.033, 0.001]	[-2.788, -1.897]	[-0.065, -0.026]
MA*ESHI	γ_8	1.750***	0.047***	2.251***	0.076***	2.367***	0.082***
		[1.158, 2.160]	[0.026, 0.059]	[1.626, 2.758]	[0.053, 0.095]	[1.708, 2.955]	[0.059, 0.103]
MA*After	γ_{11}	0.802***	0.037***	-0.106	-0.008	0.667*	0.026**
		[0.253, 1.127]	[0.019, 0.050]	[-0.739, 0.342]	[-0.029, 0.008]	[-0.146, 1.193]	[0.002, 0.047]
MA*During	γ_{11}^d	2.307***	0.060***	1.988***	0.032***	2.788***	0.068***
		[1.809, 2.560]	[0.043, 0.069]	[1.543, 2.346]	[0.014, 0.045]	[2.320, 3.168]	[0.049, 0.082]
ESHI*After	γ_{12}	-1.035***	-0.040***	-0.773***	-0.028***	-0.713**	-0.025**
		[-1.451, -0.655]	[-0.054, -0.024]	[-1.193, -0.349]	[-0.040, -0.012]	[-1.284, -0.279]	[-0.040, -0.008]
ESHI*During	γ_{12}^d	-0.722***	-0.025***	-0.702***	-0.024***	-0.644***	-0.022***
		[-0.913, -0.553]	[-0.032, -0.015]	[-0.941, -0.423]	[-0.035, -0.014]	[-0.947, -0.324]	[-0.033, -0.009]
ESHI	γ_{19}	2.002***	0.072***	1.935***	0.066***	1.818***	0.062***
		[1.713, 2.166]	[0.061, 0.079]	[1.604, 2.098]	[0.054, 0.073]	[1.446, 2.058]	[0.049, 0.070]
After	γ_{22}	1.231***	0.056***	0.705***	0.031***	0.573**	0.026***
		[0.869, 1.623]	[0.042, 0.068]	[0.306, 1.146]	[0.017, 0.043]	[0.131, 1.219]	[0.009, 0.040]
During	γ_{22}^d	0.953***	0.038***	0.819***	0.032***	0.694***	0.028***
		[0.764, 1.174]	[0.029, 0.045]	[0.536, 1.105]	[0.021, 0.043]	[0.358, 0.990]	[0.014, 0.039]
Observations		320,720	320,720	268,488	268,488	251,033	251,033
R-squared		0.769	0.754	0.754	0.737	0.765	0.749

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. Only includes interview months.
Individual and state fixed effects included. Monthly weights used.

Table 10: Results from Preferred 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	1.526***	0.410***	4.032***	0.710***	3.209***	0.709***
		[0.507, 3.098]	[0.358, 0.480]	[2.989, 5.594]	[0.647, 0.774]	[2.134, 4.899]	[0.655, 0.779]
MA*ESHI*During*Large	β_1^d	-1.568*	-0.006	-1.178	0.067**	-0.589	0.097***
		[-2.938, 0.169]	[-0.048, 0.048]	[-2.839, 0.788]	[0.006, 0.124]	[-2.504, 1.444]	[0.029, 0.160]
MA*ESHI*Large	β_8	3.355***	0.112***	3.611***	0.103***	3.710***	0.111***
		[2.472, 4.354]	[0.079, 0.137]	[2.678, 4.609]	[0.062, 0.141]	[2.513, 4.817]	[0.068, 0.145]
MA*After*Large	β_{11}	-3.254***	-0.471***	-6.806***	-0.757***	-6.580***	-0.782***
		[-4.458, -2.011]	[-0.547, -0.404]	[-7.869, -5.482]	[-0.826, -0.690]	[-7.781, -5.288]	[-0.853, -0.723]
MA*During*Large	β_{11}^d	1.068	0.001	-0.144	-0.099***	-0.747	-0.133***
		[-0.259, 2.840]	[-0.051, 0.045]	[-1.546, 1.938]	[-0.151, -0.039]	[-2.285, 1.626]	[-0.194, -0.063]
ESHI*After*Large	β_{12}	-0.895	-0.077**	-0.541	-0.063*	0.054	-0.044
		[-2.225, 0.344]	[-0.138, -0.010]	[-1.684, 0.628]	[-0.118, 0.005]	[-1.254, 1.258]	[-0.098, 0.028]
ESHI*During*Large	β_{12}^d	-0.251	-0.074***	0.54	-0.048	0.747	-0.039
		[-1.702, 1.450]	[-0.118, -0.020]	[-1.067, 2.243]	[-0.099, 0.014]	[-1.089, 2.685]	[-0.097, 0.024]
ESHI*Large	β_{19}	1.298***	0.090***	1.170**	0.074***	0.858	0.060***
		[0.398, 1.923]	[0.064, 0.110]	[0.245, 1.941]	[0.046, 0.106]	[-0.220, 1.723]	[0.034, 0.092]
After*Large	β_{22}	1.018	0.057	1.024	0.053	0.702	0.042
		[-0.361, 2.254]	[-0.015, 0.123]	[-0.310, 1.906]	[-0.016, 0.107]	[-0.681, 1.760]	[-0.019, 0.099]
During*Large	β_{22}^d	0.371	0.048	-0.221	0.028	-0.22	0.029
		[-1.359, 1.408]	[-0.008, 0.093]	[-2.010, 0.933]	[-0.033, 0.074]	[-2.306, 1.086]	[-0.033, 0.078]
MA*Large	β_{23}	0.712	0.094	0.87	0.106	-0.549	0.075
		[-2.178, 4.198]	[-0.020, 0.211]	[-3.162, 3.531]	[-0.133, 0.185]	[-4.742, 2.907]	[-0.223, 0.172]
Large	β_{24}	-1.131	-0.091	-1.675	-0.187	-0.508	-0.147
		[-4.431, 1.882]	[-0.198, 0.024]	[-4.027, 2.640]	[-0.253, 0.044]	[-3.687, 3.923]	[-0.227, 0.146]
MA*ESHI*After	β_{1e}	-4.972***	-0.354***	-6.511***	-0.578***	-6.777***	-0.577***
		[-6.371, -4.065]	[-0.401, -0.312]	[-7.743, -5.543]	[-0.636, -0.530]	[-8.039, -5.810]	[-0.636, -0.534]
MA*ESHI*During	β_{1e}^d	-2.777***	-0.205***	-3.006***	-0.264***	-3.615***	-0.286***
		[-4.109, -1.085]	[-0.249, -0.149]	[-4.481, -1.279]	[-0.318, -0.207]	[-5.301, -1.835]	[-0.351, -0.225]
MA*ESHI	β_{8e}	0.953*	0.017	0.508	0.022	1.169**	0.035
		[-0.003, 1.736]	[-0.015, 0.047]	[-0.488, 1.362]	[-0.021, 0.064]	[0.012, 1.975]	[-0.008, 0.076]
MA*After	β_{11e}	6.775***	0.431***	9.480***	0.643***	9.829***	0.655***
		[5.814, 7.540]	[0.376, 0.482]	[8.464, 10.341]	[0.593, 0.693]	[8.674, 10.725]	[0.604, 0.710]
MA*During	β_{11e}^d	3.059***	0.214***	4.283***	0.299***	4.887***	0.330***
		[1.266, 4.072]	[0.161, 0.255]	[2.321, 5.358]	[0.247, 0.349]	[2.695, 6.025]	[0.271, 0.383]
ESHI*After	β_{12e}	-0.179	-0.019	-0.393	-0.015	-0.829	-0.03
		[-1.294, 1.130]	[-0.068, 0.025]	[-1.461, 0.739]	[-0.067, 0.030]	[-2.012, 0.289]	[-0.080, 0.015]
ESHI*During	β_{12e}^d	-0.522	0.012	-0.988	0.001	-1.261	-0.005
		[-2.148, 0.765]	[-0.038, 0.049]	[-2.601, 0.428]	[-0.048, 0.050]	[-3.065, 0.464]	[-0.058, 0.052]
ESHI	β_{19e}	1.399***	0.103***	1.386***	0.111***	1.515***	0.110***
		[0.664, 2.213]	[0.079, 0.125]	[0.535, 2.215]	[0.075, 0.136]	[0.635, 2.397]	[0.071, 0.137]
After	β_{22e}	1.599***	0.096***	1.252***	0.074***	1.179***	0.068***
		[0.865, 2.419]	[0.055, 0.146]	[0.495, 2.162]	[0.039, 0.119]	[0.301, 2.281]	[0.031, 0.117]
During	β_{22e}^d	1.363***	0.053***	1.509***	0.055***	1.396**	0.042*
		[0.440, 3.055]	[0.022, 0.098]	[0.501, 3.317]	[0.016, 0.101]	[0.271, 3.442]	[-0.006, 0.091]
Observations		327,388	327,388	281,457	281,457	262,988	262,988
R-squared		0.793	0.776	0.806	0.792	0.809	0.801

Table 10: Results from Baseline Specification to Investigate Intensive Margin (Continued)

		(2a)	(2b)	(2c)	(2d)	(2e)	(2f)
		L	Log(L)	L	Log(L)	L	Log(L)
		Hours per week	Hours per week	Hours per week	Hours per week	Hours per week	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	γ_1	5.857***	0.253***	13.670***	0.597***	13.557***	0.583***
		[5.445, 6.683]	[0.239, 0.286]	[12.620, 14.873]	[0.564, 0.641]	[12.685, 14.890]	[0.550, 0.631]
MA*ESHI*During*Large	γ_1^d	2.112***	0.073***	5.977***	0.208***	5.444***	0.188***
		[1.573, 2.724]	[0.054, 0.098]	[5.009, 6.769]	[0.173, 0.245]	[4.356, 6.216]	[0.151, 0.225]
MA*ESHI*Large	γ_8	1.412***	0.054***	-0.104	0.007	0.039	0.01
		[0.771, 1.817]	[0.034, 0.065]	[-0.842, 0.466]	[-0.022, 0.024]	[-0.722, 0.763]	[-0.022, 0.030]
MA*After*Large	γ_{11}	-6.594***	-0.319***	-13.807***	-0.647***	-13.516***	-0.634***
		[-7.328, -5.928]	[-0.347, -0.301]	[-14.858, -12.692]	[-0.686, -0.615]	[-14.485, -12.555]	[-0.675, -0.604]
MA*During*Large	γ_{11}^d	-2.285***	-0.109***	-6.299***	-0.246***	-5.645***	-0.221***
		[-3.021, -1.678]	[-0.136, -0.089]	[-7.108, -5.407]	[-0.282, -0.215]	[-6.473, -4.736]	[-0.259, -0.188]
ESHI*After*Large	γ_{12}	-1.369***	-0.056***	-1.125***	-0.042***	-1.030***	-0.039***
		[-1.923, -0.774]	[-0.080, -0.035]	[-1.957, -0.221]	[-0.073, -0.012]	[-1.905, -0.359]	[-0.077, -0.013]
ESHI*During*Large	γ_{12}^d	-0.997***	-0.040***	-0.890***	-0.031**	-0.718**	-0.027*
		[-1.341, -0.523]	[-0.054, -0.022]	[-1.466, -0.222]	[-0.057, -0.004]	[-1.376, -0.018]	[-0.055, 0.000]
ESHI*Large	γ_{19}	1.091***	0.038***	1.071***	0.036***	0.736***	0.026***
		[0.799, 1.353]	[0.028, 0.048]	[0.762, 1.388]	[0.024, 0.051]	[0.381, 1.149]	[0.010, 0.044]
After*Large	γ_{22}	0.808**	0.024**	0.797	0.025	0.759**	0.021
		[0.143, 1.320]	[0.003, 0.041]	[-0.237, 1.530]	[-0.007, 0.054]	[0.012, 1.401]	[-0.007, 0.055]
During*Large	γ_{22}^d	0.590**	0.016	0.644*	0.016	0.565	0.014
		[0.009, 0.973]	[-0.004, 0.031]	[-0.107, 1.175]	[-0.015, 0.040]	[-0.249, 1.154]	[-0.019, 0.041]
MA*Large	γ_{23}	4.259	0.186	4.538	0.147	3.592	0.137
		[-4.833, 5.149]	[-0.239, 0.217]	[-7.003, 5.542]	[-0.392, 0.185]	[-6.421, 4.804]	[-0.169, 0.179]
Large	γ_{24}	-4.252	-0.164	-3.874	-0.118	-2.91	-0.109
		[-4.691, 4.798]	[-0.182, 0.265]	[-4.283, 7.711]	[-0.139, 0.421]	[-3.491, 7.130]	[-0.139, 0.198]
MA*ESHI*After	γ_{1e}	-3.909***	-0.166***	-9.124***	-0.384***	-9.681***	-0.401***
		[-4.810, -3.322]	[-0.201, -0.146]	[-10.457, -8.263]	[-0.427, -0.354]	[-11.112, -8.808]	[-0.452, -0.366]
MA*ESHI*During	γ_{1e}^d	-3.306***	-0.091***	-5.648***	-0.150***	-5.724***	-0.157***
		[-3.884, -2.747]	[-0.113, -0.073]	[-6.442, -4.889]	[-0.178, -0.125]	[-6.568, -5.017]	[-0.186, -0.131]
MA*ESHI	γ_{8e}	0.562*	0	2.128***	0.061***	2.209***	0.068***
		[-0.027, 1.171]	[-0.022, 0.022]	[1.249, 2.985]	[0.032, 0.093]	[1.273, 3.182]	[0.037, 0.105]
MA*After	γ_{11e}	5.119***	0.247***	9.416***	0.440***	9.687***	0.451***
		[4.372, 5.809]	[0.222, 0.272]	[8.260, 10.362]	[0.406, 0.476]	[8.595, 10.748]	[0.417, 0.493]
MA*During	γ_{11e}^d	3.855***	0.134***	6.232***	0.198***	6.411***	0.209***
		[3.172, 4.473]	[0.113, 0.156]	[5.440, 6.990]	[0.170, 0.227]	[5.579, 7.195]	[0.179, 0.240]
ESHI*After	γ_{12e}	-0.068	0.001	-0.028	0.001	-0.054	0.002
		[-0.668, 0.542]	[-0.020, 0.028]	[-0.744, 0.801]	[-0.022, 0.032]	[-0.820, 0.780]	[-0.023, 0.038]
ESHI*During	γ_{12e}^d	-0.009	0.006	-0.108	-0.002	-0.186	-0.003
		[-0.357, 0.326]	[-0.009, 0.019]	[-0.534, 0.434]	[-0.018, 0.019]	[-0.613, 0.396]	[-0.020, 0.021]
ESHI	γ_{19e}	1.171***	0.042***	1.135***	0.038***	1.265***	0.042***
		[0.824, 1.388]	[0.026, 0.052]	[0.690, 1.437]	[0.019, 0.050]	[0.733, 1.630]	[0.018, 0.057]
After	γ_{22e}	0.740***	0.041***	0.24	0.016	0.145	0.014
		[0.215, 1.501]	[0.022, 0.065]	[-0.481, 1.097]	[-0.010, 0.039]	[-0.517, 1.081]	[-0.015, 0.038]
During	γ_{22e}^d	0.588***	0.028***	0.437*	0.023**	0.369	0.020*
		[0.249, 1.021]	[0.016, 0.042]	[-0.054, 0.897]	[0.003, 0.042]	[-0.146, 0.856]	[-0.003, 0.039]
Observations		320,720	320,720	268,488	268,488	251,033	251,033
R-squared		0.77	0.754	0.755	0.738	0.765	0.75

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. 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 10: Results from Baseline Specification to Investigate Intensive Margin (Continued)

	(3a)	(3b)	(3c)	(3d)	(3e)	(3f)
	w,L	Log(w),Log(L)	w,L	Log(w),Log(L)	w,L	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*	
$w_D - w_A$	4.308***	0.129***	4.119***	0.125***	4.879***	0.146***
	[3.781, 4.671]	[0.104, 0.145]	[3.376, 4.578]	[0.096, 0.145]	[4.023, 5.398]	[0.113, 0.170]
$w_F - w_B$	0.861**	0.185***	1.640***	0.257***	1.311***	0.277***
	[0.194, 1.293]	[0.137, 0.227]	[0.929, 2.205]	[0.217, 0.290]	[0.450, 1.854]	[0.231, 0.316]
$w_D - w_B$	7.562***	0.600***	10.926***	0.882***	11.459***	0.928***
	[6.232, 8.730]	[0.529, 0.687]	[9.488, 12.057]	[0.807, 0.960]	[9.962, 12.744]	[0.857, 1.014]
$w_F - w_A$	-2.393***	-0.287***	-5.166***	-0.500***	-5.268***	-0.505***
	[-3.535, -1.285]	[-0.346, -0.228]	[-6.356, -4.244]	[-0.565, -0.441]	[-6.447, -4.188]	[-0.564, -0.456]
$L_D - L_A$	1.974***	0.054***	2.025***	0.069***	2.248***	0.079***
	[1.373, 2.419]	[0.032, 0.067]	[1.455, 2.514]	[0.047, 0.084]	[1.657, 2.732]	[0.056, 0.095]
$L_F - L_B$	3.923***	0.141***	6.571***	0.282***	6.124***	0.261***
	[3.344, 4.373]	[0.121, 0.158]	[5.945, 7.032]	[0.260, 0.300]	[5.458, 6.769]	[0.235, 0.285]
$L_D - L_B$	8.569***	0.373***	15.832***	0.716***	15.764***	0.713***
	[7.695, 9.406]	[0.347, 0.402]	[14.537, 16.898]	[0.674, 0.759]	[14.599, 17.023]	[0.671, 0.763]
$L_F - L_A$	-2.672***	-0.178***	-7.236***	-0.366***	-7.392***	-0.374***
	[-3.543, -1.862]	[-0.207, -0.153]	[-8.244, -6.274]	[-0.406, -0.336]	[-8.477, -6.541]	[-0.414, -0.345]
s	0.170	0.100	0.170	0.100	0.170	0.100
	-	-	-	-	-	-
d	-0.340	-0.200	-0.340	-0.200	-0.340	-0.200
	-	-	-	-	-	-
ρ	0.043***	0.440***	0.019***	0.247***	0.018***	0.245***
	[0.031, 0.070]	[0.366, 0.545]	[0.016, 0.022]	[0.221, 0.277]	[0.016, 0.021]	[0.220, 0.270]
b	3.302***	0.322***	7.627***	0.573***	7.781***	0.580***
	[2.037, 4.625]	[0.260, 0.388]	[6.459, 9.043]	[0.512, 0.642]	[6.644, 9.070]	[0.526, 0.644]
α	-1.203***	-0.383***	-0.495***	-0.206***	-0.578***	-0.238***
	[-1.949, -0.813]	[-0.489, -0.279]	[-0.572, -0.381]	[-0.252, -0.151]	[-0.656, -0.447]	[-0.273, -0.187]
$\lambda - \mu_x$	1.790***	1.217***	1.011***	1.015***	1.093***	1.044***
	[1.463, 2.449]	[1.115, 1.316]	[0.924, 1.089]	[0.962, 1.055]	[0.980, 1.153]	[0.989, 1.078]
$\alpha + \lambda - \mu_x$	0.587***	0.834***	0.516***	0.809***	0.516***	0.807***
	[0.395, 0.699]	[0.803, 0.850]	[0.431, 0.588]	[0.790, 0.821]	[0.421, 0.580]	[0.786, 0.818]
$ESHI_{After}$	0.790	0.790	0.790	0.790	0.790	0.790
	-	-	-	-	-	-
$\frac{b}{\tau}$	1.000	1.000	1.000	1.000	1.000	1.000
	-	-	-	-	-	-
DWL_m	1.442***	0.011***	10.553***	0.023***	11.010***	0.024***
	[0.703, 2.532]	[0.010, 0.012]	[7.934, 13.695]	[0.020, 0.027]	[8.622, 14.482]	[0.021, 0.027]
$\frac{DWL_m}{DWL_\tau}$	0.135***	0.062***	0.185***	0.042***	0.185***	0.042***
	[0.072, 0.290]	[0.049, 0.092]	[0.134, 0.256]	[0.036, 0.050]	[0.139, 0.265]	[0.037, 0.050]

results. Unfortunately, our model does not allow us to examine extensive margin decisions directly. 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. If we instead aggregate our data, defining L as the fraction of individuals with a paid job, we cannot take advantage of longitudinal variation.

7.6 Implications for National Reform

We have shown that our model applies to both the Massachusetts and national reforms because they share the same key features. The model shows that mandate-based health reform can be more efficient than tax-based health reform, and we find empirical evidence confirming the model for Massachusetts. Using our results from Massachusetts, we can predict the likely impact of national reform, subject to some caveats.

First, although the Massachusetts and national reforms share the same key features, the statutory values of the policy parameters differ. We have already demonstrated that our main result that mandate-based reform is much more efficient than tax-based reform holds if we increase the employer penalty to the statutory level in national reform, increasing labor market distortion. Our result will also be robust to the larger statutory values of the individual penalty in national reform because the individual penalty increases the penalty-and-subsidy-inclusive valuation, decreasing distortion. Subsidies in national reform are somewhat larger and extend to a larger population because incomes are lower outside of Massachusetts and subsidy thresholds are higher under national reform. Larger subsidies increase labor market distortion. Although the extension of our model in Online Appendix OA1 clearly demonstrates the impact of varying subsidy amounts on welfare, we are not able to separately identify behavioral responses to different subsidy amounts because our cell sizes are so small.[17]

A second caveat in applying our results to national reform is that our model has treated a firm's decision to offer health insurance as exogenous. We do not extend the model to incorporate the firm's decision to offer health insurance because we do not observe whether a firm offers health insurance in our data — we only observe ESHI for individuals who have it. To the extent that firms respond differently to national reform than they do to Massachusetts reform, our results might not generalize.

Furthermore, there could be general equilibrium changes to the 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 et al. (2012) for evidence of adverse selection in Massachusetts). Suppose that compliance with national reform is not as high, leaving higher prices in the market for health insurance outside of employment. In terms of our model, although adverse selection in the non-employer-sponsored market should not affect the cost of

[17] Rather than relying on our estimates from Massachusetts, we could use our model and exclusively calibrate values to predict the impact of national reform. However, without a separate estimate for α, this approach requires very strong assumptions.

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 national non-employer-sponsored market could actually decrease the reform-induced distortion to the labor market relative to what we observed in Massachusetts.

Another issue in applying our results to national reform is that we only consider the welfare impact of health reform through distortions to the labor market. Requiring individuals to purchase health insurance through mandate-based reform or providing it to them directly through a tax-based reform also distorts the market for goods and services by requiring individuals to allocate more dollars to health insurance and fewer dollars to other goods. Our welfare analysis starts from the standpoint that policy makers want to expand coverage to near-universal levels and then examines the efficiency of mandate-based reform and tax-based reform in achieving that policy goal. Because mandate-based reform and tax-based reform would result in the same distortion to the product market, when we compare them, we do not need to take product market distortions into account when we analyze the Massachusetts or national reforms. However, if we choose to examine the welfare-impact of mandate-based reform alone, we need to consider that distortions to the market for goods and services create an additional welfare loss. Because Massachusetts started with a higher level of coverage than the rest of the nation, distortions to the market for goods and services could be larger under national reform than they were under the Massachusetts reform. Furthermore, the individual penalty under Massachusetts reform is smaller than the individual penalty under national reform. We have shown that the distortion to the labor market should be greater under the smaller Massachusetts individual penalty; however, the larger national individual penalty could increase distortion to the market for goods and services by providing a larger incentive for individuals to substitute health insurance for other goods.

Finally, in comparing mandate-based health reform to tax-based health reform, we have assumed that mandate-based policy makes the linkage between taxes and benefits more 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 wages under tax-based reform. In this light, our theory demonstrates that if individuals recognize the linkage between the taxes they pay and the benefits that they receive, and if they value those benefits, then the labor market distortion is smaller than it would be otherwise. Our theory can be applied in a variety of other contexts in which the salience of the linkage between costs and benefits varies. Our results from health reform in Massachusetts suggest that individuals did recognize the linkage between wages and health insurance and that they placed some value on the health insurance that they received. If individuals outside of Massachusetts place a larger value on health insurance, the labor market distortion could be smaller under national reform than it was under the Massachusetts reform.

8 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 non-elderly 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. Our model accounts for the complex set of underlying preferences for insurance, capturing them simply as the willingness to trade off monetary wages for employer-sponsored health insurance coverage. 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 that gained coverage due to the Massachusetts reform earned wages that were lower than they would have been had they not gained ESHI by $6,055 per year, nearly the entire 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 76 cents of every dollar that their employers spent on their coverage. Because individuals valued ESHI, mandate-based health reform in Massachusetts resulted in significantly less distortion to the labor market than it would have otherwise. 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 more than 20 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.

A Appendix: 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 (2007), [2] Felland et al (2007), [3] Kaiser Family Foundation (2010d), [4] Anonymous (2011), [5] Commonwealth Connector (2007), [6] Kaiser Family Foundation (2009), [7] Raymond (2007), [7] Kaiser Family Foundation (2010b), [8] Kaiser Family Foundation (2010c).

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OA1 Online Appendix: Variation in Subsidy Amounts

We can extend our model to incorporate variation in the health insurance subsidy amounts that Massachusetts and ACA extend to subsets of the population. Thus far, we have modeled these subsidies as a share of the total cost of health insurance, μ_x, that is paid by the government. The subscript x indexes the magnitude of the subsidy, which varies based on income. Here, we specify three different income categories as values of x: I for the income category that is not eligible for any subsidies, II for the income category that is eligible for partial subsidies, and III for the income category that is eligible for full subsidies. Before reform, some individuals receive subsidies μ_{III}. After reform, some individuals receive fully subsidized coverage, and others with higher incomes receive partial subsidies μ_{II}. Individuals in the highest income categories do not qualify for any subsidies; therefore, $\mu_I = 0$.

As in the case of the individual mandate, the subsidies only affect an individual's labor supply if he *does* obtain health insurance through his employer. In the face of a penalty, he is *more* willing to work for health benefits instead of wages. However, if he is eligible for a subsidy for health insurance outside of employment, he is is *less* willing to work for employer health insurance benefits instead of wages.[1]

Figure OA1: Graphical Model with Variation in Subsidy Amounts

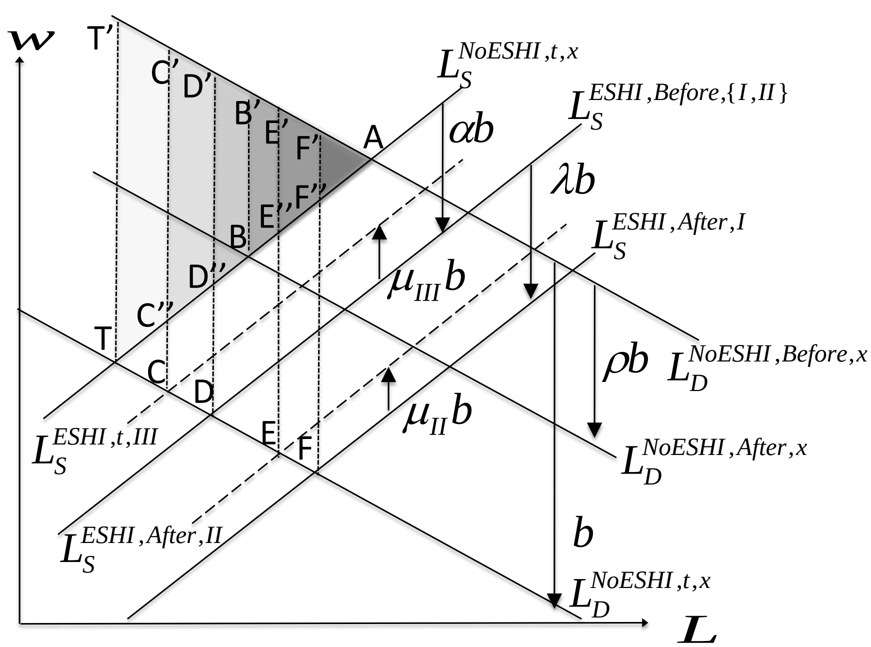

Figure OA1 shows the graphical model that incorporates variation in the subsidy amounts. Individuals who are eligible for a full subsidy but forgo it for ESHI before the reform have wages and hours given by C. They remain at C after the reform because the individual penalty does not

[1]This discussion assumes that health insurance does not affect an individual's ability to work. To the extent that it does, we could introduce separate L_S^{NoESHI} curves for individuals with and without any health insurance. We are unlikely to have enough variation to separate these two curves in our empirical implementation.

apply to them. Individuals who become eligible for a partial subsidy but forgo it for ESHI after the reform have wages and hours given by point E. Individuals with ESHI who are not eligible for any subsidies after the reform have wages and hours given by point F. The other equilibria are unchanged from the original model.

Figure OA1 incorporates the universe of equilibria under mandate-based health reform with three separate subsidy eligibility categories. There are six possible labor market equilibria (A through F) depicted, which depend on ESHI status and the values of the parameters. However, for any given individual, there are only four possible equilibria - equilibria with and without ESHI before and after reform. The first two columns of Table OA1 list the four relevant equilibria for each category of subsidy eligibility.

Table OA1: Wages in Terms of Coefficients by Subsidy Amounts

I: 300 FPL+

w_{AI}	noeshi, before	0
w_{BI}	noeshi, after	β_{11}
w_{DI}	eshi, before	β_8 [$+\beta_{8e}$]
w_{FI}	eshi, after	$\beta_1 + \beta_8 + \beta_{11}$ [$+\beta_{1e} + \beta_{8e}$]

II: 150 to 300 FPL

w_{AII}	noeshi, before	0
w_{BII}	noeshi, after	$\beta_7 + \beta_{11}$
w_{DII}	eshi, before	$\beta_5 + \beta_8$ [$+\beta_{5e} + \beta_{8e}$]
w_{EII}	eshi, after	$\beta_1 + \beta_3 + \beta_5 + \beta_7 + \beta_8 + \beta_{11}$ [$+\beta_{1e} + \beta_{3e} + \beta_{5e} + \beta_{8e}$]

III: <150 FPL

w_{AIII}	noeshi, before	0
w_{BIII}	noeshi, after	$\beta_6 + \beta_{11}$
w_{CIII}	eshi, before	$\beta_4 + \beta_8$ [$+\beta_{4e} + \beta_{8e}$]
w_{CIII}	eshi, after	$\beta_1 + \beta_2 + \beta_4 + \beta_6 + \beta_8 + \beta_{11}$ [$+\beta_{1e} + \beta_{2e} + \beta_{4e} + \beta_{8e}$]

OA1.1 Identification

For identification of the additional parameters, we incorporate variation across subsidy eligibility categories. Table OA2 presents all of the sufficient statistics in terms of differences between equilibria. Within each subsidy eligibility category, we can estimate all possible sufficient statistics from the original model. For example, we can derive the slope of the supply curve within each of the three eligibility categories, resulting in sufficient statistics s_I, s_{II}, and s_{III}, as shown in the first three rows of the table. As drawn, our graphical model assumes that the slopes of the labor supply curves are the same within each category. We can test the assumption by allowing the slopes to vary within each eligibility category. We can identify all sufficient statistics identified by the main model, discussed in Section 4 of the paper, using the same variation in the extended model.

Table OA2: Sufficient Statistics in Terms of Differences between Equilibria by Subsidy Amounts

Sufficient statistic	Expression in wages and employment
s_I	$\frac{w_{BI}-w_{AI}}{L_{BI}-L_{AI}}$
s_{II}	$\frac{w_{BII}-w_{AII}}{L_{BII}-L_{AII}}$
s_{III}	$\frac{w_{BIII}-w_{AIII}}{L_{BIII}-L_{AIII}}$
d_I	$\frac{w_{FI}-w_{DI}}{L_{FI}-L_{DI}}$
d_{II}	$\frac{w_{EII}-w_{DII}}{L_{EII}-L_{DII}}$
d_{III}	$\frac{w_{CIII}-w_{DIII}}{L_{CIII}-L_{DIII}}$
ρ_I	$\frac{d_I(L_{BI}-L_{AI})-(w_{BI}-w_{AI})}{b}$
ρ_{II}	$\frac{d_{II}(L_{BII}-L_{AII})-(w_{BII}-w_{AII})}{b_{II}}$
ρ_{III}	$\frac{d_{III}(L_{BIII}-L_{AIII})-(w_{BIII}-w_{AIII})}{b_{III}}$
b_I	$d_I(L_{FI}-L_{AI})-(w_{FI}-w_{AI})$
b_{II}	$d_{II}(L_{EII}-L_{AII})-(w_{EII}-w_{AII})$
b_{III}	$d_{III}(L_{CIII}-L_{AIII})-(w_{CIII}-w_{AIII})$
α_I	$\frac{s(L_{DI}-L_{AI})-(w_{DI}-w_{AI})}{b_I}$
α_{II}	$\frac{s_{II}(L_{DII}-L_{AII})-(w_{DII}-w_{AII})}{b_{II}}$
λ_I	$\frac{s(L_{FI}-L_{DI})-(w_{FI}-w_{DI})}{b_I}$
$\alpha_I+\lambda_I$	$\frac{s(L_{FI}-L_{AI})-(w_{FI}-w_{AI})}{b_I}$
$\alpha_{II}+\lambda_I-\mu_{II}$	$\frac{s_{II}(L_{EII}-L_{AII})-(w_{EII}-w_{AII})}{b_{II}}$
$\alpha_I-\mu_{III}$	$\frac{s_{III}(L_{CIII}-L_{AIII})-(w_{CIII}-w_{AIII})}{b_{III}}$
μ_{III}	$\alpha_I-\frac{s_{III}(L_{CIII}-L_{AIII})-(w_{CIII}-w_{AIII})}{b_{III}}$
μ_{II}	$\alpha_{II}+\lambda_I-\frac{s_{II}(L_{EII}-L_{AII})-(w_{EII}-w_{AII})}{b_{II}}$

In addition, we can separately identify the subsidy parameters from the other parameters by comparing across categories. For example, to identify μ_{III}, we first identify $\alpha-\mu_{III}$ by comparing individuals who are eligible for full subsidies who move from not having ESHI before the reform (equilibrium A) to having ESHI after the reform (equilibrium C). Next, we net this sum out of the value of α obtained from the category that is not eligible for any subsidies. We identify μ_{II} with a similar comparison across categories. We do note, however, that separate identification of each μ_x requires the assumption that α does not vary by subsidy eligibility group. Were we to relax that assumption, as we do in the pooled estimation in Section 7.4 , we cannot separately identify subsidy effects from differences in the underlying valuation.

OA1.2 Estimation

To estimate all of the relevant parameters of our model, we specify and estimate wage and hours equations of the following form:

$$Y_{it} = [\beta_1 MA * ESHI * After * Large + \beta_2 MA * ESHI * 1(< 150FPL) * After * Large +$$

$$\beta_3 MA * ESHI * 1(150to300FPL) * After * Large + \beta_4 MA * ESHI * 1(<150FPL) * Large +$$
$$\beta_5 MA * ESHI * 1(150to300FPL) * Large + \beta_6 MA * 1(<150FPL) * After * Large +$$
$$\beta_7 MA * 1(150to300FPL) * After * Large + \beta_8 MA * ESHI * Large +$$
$$\beta_9 MA * 1(<150FPL) * Large + \beta_{10} MA * 1(150to300FPL) * Large +$$
$$\beta_{11} MA * After * Large + \beta_{12} ESHI * After * Large +$$
$$\beta_{13} ESHI * 1(<150FPL) * After * Large + \beta_{14} ESHI * 1(150to300FPL) * After * Large +$$
$$\beta_{15} ESHI * 1(<150FPL) * Large + \beta_{16} ESHI * 1(150to300FPL) * Large +$$
$$\beta_{17} 1(<150FPL) * After * Large + \beta_{18} 1(150to300FPL) * After * Large +$$
$$\beta_{19} ESHI * Large + \beta_{20} 1(<150FPL) * Large + \beta_{21} 1(150to300FPL) * Large + \beta_{22} After * Large +$$
$$\beta_{23} large + \phi_g * Large +]$$
$$\beta_{1[e]} MA * ESHI * After + \beta_{2[e]} MA * ESHI * 1(<150FPL) * After +$$
$$\beta_{3[e]} MA * ESHI * 1(150to300FPL) * After + \beta_{4[e]} MA * ESHI * 1(<150FPL) +$$
$$\beta_{5[e]} MA * ESHI * 1(150to300FPL) + \beta_{6[e]} MA * 1(<150FPL) * After +$$
$$\beta_{7[e]} MA * 1(150to300FPL) * After + \beta_{8[e]} MA * ESHI +$$
$$\beta_{9[e]} MA * 1(<150FPL) + \beta_{10[e]} MA * 1(150to300FPL) +$$
$$\beta_{11[e]} MA * After + \beta_{12[e]} ESHI * After +$$
$$\beta_{13[e]} ESHI * 1(<150FPL) * After + \beta_{14[e]} ESHI * 1(150to300FPL) * After +$$
$$\beta_{15[e]} ESHI * 1(<150FPL) + \beta_{16[e]} ESHI * 1(150to300FPL) +$$
$$\beta_{17[e]} 1(<150FPL) * After + \beta_{18[e]} 1(150to300FPL) * After +$$
$$\beta_{19[e]} ESHI + \{\beta_{20[e]} 1(<150FPL)\} + \{\beta_{21[e]} 1(150to300FPL)\} + \beta_{22[e]} After +$$
$$\phi_g + \delta_i + \varepsilon_{it}$$

where all of the terms are as defined in the main estimating equation, which is a special case of this equation. Because our data do not include information on subsidy eligibility, we proxy for subsidy eligibility with income categories. We assume that people above 300% of the federal poverty line (FPL) are not eligible for any subsidies before or after reform. People between 150 and 300% of FPL are eligible for partial subsidies after the reform, and we represent them with the indicator $1(150to300FPL)$. People under 150% of FPL are eligible for full subsidies before and after the reform, and we represent them with the indicator $1(<150FPL)$. We classify individuals into income groups using the first period of available data to avoid regressing wages on contemporaneous measures of income. Therefore, β_{20e} and β_{21e} are shown in $\{\}$ and omitted because they are collinear with the individual fixed effects. From the estimated coefficients, we can derive each of the sufficient statistics as shown in Table OA3.

Table OA3: Sufficient Statistics in Terms of Coefficients by Subsidy Amounts

Sufficient statistic	Expression in coefficients
s_I	$\frac{\beta_{11}}{\gamma_{11}}$
s_{II}	$\frac{\beta_7+\beta_{11}}{\gamma_7+\gamma_{11}}$
s_{III}	$\frac{\beta_6+\beta_{11}}{\gamma_6+\gamma_{11}}$
d_I	$\frac{\beta_1+\beta_{11}\,[+\beta_{1e}]}{\gamma_1+\gamma_{11}\,[+\gamma_{1e}]}$
d_{II}	$\frac{\beta_1+\beta_3+\beta_7+\beta_{11}\,[+\beta_{1e}+\beta_{3e}]}{\gamma_1+\gamma_3+\gamma_7+\gamma_{11}\,[+\gamma_{1e}+\gamma_{3e}]}$
d_{III}	$\frac{\beta_1+\beta_2+\beta_6+\beta_{11}\,[+\beta_{1e}+\beta_{2e}]}{\gamma_1+\gamma_2+\gamma_6+\gamma_{11}\,[+\gamma_{1e}+\gamma_{2e}]}$
ρ_I	$\frac{d_I(\gamma_{11})-(\beta_{11})}{b_I}$
ρ_{II}	$\frac{d_{II}(\gamma_7+\gamma_{11})-(\beta_7+\beta_{11})}{b_{II}}$
ρ_{III}	$\frac{d_{III}(\gamma_6+\gamma_{11})-(\beta_6+\beta_{11})}{b_{III}}$
b_I	$d_I\left(\gamma_1+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{8e}]\right) - \left(\beta_1+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{8e}]\right)$
b_{II}	$d_{II}\left(\gamma_1+\gamma_3+\gamma_5+\gamma_7+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{3e}+\gamma_{5e}+\gamma_{8e}]\right) - \left(\beta_1+\beta_3+\beta_5+\beta_7+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{3e}+\beta_{5e}+\beta_{8e}]\right)$
b_{III}	$d_{III}\left(\gamma_1+\gamma_2+\gamma_4+\gamma_6+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{2e}+\gamma_{4e}+\gamma_{8e}]\right) - \left(\beta_1+\beta_2+\beta_4+\beta_6+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{2e}+\beta_{4e}+\beta_{8e}]\right)$
α_I	$\frac{s_I(\gamma_8\,[+\gamma_{8e}])-(\beta_8\,[+\beta_{8e}])}{b_I}$
α_{II}	$\frac{s_{II}(\gamma_5+\gamma_8\,[+\gamma_{5e}+\gamma_{8e}])-(\beta_5+\beta_8\,[+\beta_{5e}+\beta_{8e}])}{b_{II}}$
λ_I	$\frac{s_I(\gamma_1+\gamma_{11}\,[+\gamma_{1e}])-(\beta_1+\beta_{11}\,[+\beta_{1e}])}{b_I}$
$\alpha_I+\lambda_I$	$\frac{s_I(\gamma_1+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{8e}])-(\beta_1+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{8e}])}{b_I}$
$\alpha_{II}+\lambda_I-\mu_{II}$	$\frac{s_{II}(\gamma_1+\gamma_3+\gamma_5+\gamma_7+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{3e}+\gamma_{5e}+\gamma_{8e}])-(\beta_1+\beta_3+\beta_5+\beta_7+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{3e}+\beta_{5e}+\beta_{8e}])}{b_{II}}$
$\alpha_I-\mu_{III}$	$\frac{s_{III}(\gamma_1+\gamma_2+\gamma_4+\gamma_6+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{2e}+\gamma_{4e}+\gamma_{8e}])-(\beta_1+\beta_2+\beta_4+\beta_6+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{2e}+\beta_{4e}+\beta_{8e}])}{b_{III}}$
μ_{III}	$\alpha_I-\frac{s_{III}(\gamma_1+\gamma_2+\gamma_4+\gamma_6+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{2e}+\gamma_{4e}+\gamma_{8e}])-(\beta_1+\beta_2+\beta_4+\beta_6+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{2e}+\beta_{4e}+\beta_{8e}])}{b_{III}}$
μ_{II}	$\alpha_{II}+\lambda_I-\frac{s_{II}(\gamma_1+\gamma_3+\gamma_5+\gamma_7+\gamma_8+\gamma_{11}\,[+\gamma_{1e}+\gamma_{3e}+\gamma_{5e}+\gamma_{8e}])-(\beta_1+\beta_3+\beta_5+\beta_7+\beta_8+\beta_{11}\,[+\beta_{1e}+\beta_{3e}+\beta_{5e}+\beta_{8e}])}{b_{II}}$

OA1.3 Results

We present results from the preferred specification with firm size interaction terms in Table OA4. Following our main approach, we calibrate s and d in each category. We also calibrate ρ as above in computing our measures of deadweight loss. Our estimates are very noisy, illustrating the advantage of pooling all income categories in our main specifications. Despite the noise, some patterns emerge.

Combining the compensating and hours differential estimates with calibrated labor supply and demand slopes yields estimates for b that are correctly signed. Our estimates for b_I and b_{III} are \$1.13 and \$2.50 per hour, respectively. The estimate for b_{II}, however, is \$15.55. This does not accord well with the cost of ESHI. This is a first indication that estimated effects for this group likely reflect the relatively small sample size for individuals between 150% of FPL and 300% of FPL in Massachusetts in the SIPP. Because the SIPP over samples among the poor, sample size is not a problem for the population who qualify for full subsidies, μ_{III}. We also have sufficient observations for income category I, given the large share of the population earning more than 300% of FPL.

Translating our estimates for the compensating and hours differentials into penalty-and-subsidy-inclusive valuations, we find relatively high estimates for all groups. To maintain internal consistency by group, the penalty-and-subsidy-inclusive valuation for each group is taken relative to the estimated cost of b for that group. All of the estimates are of a reasonable magnitude, though we note that only $\alpha_I - \mu_{III}$ and $\alpha_I + \lambda_I$ are significantly different from zero. The estimates suggest a relatively high penalty-and-subsidy-inclusive valuation across subsidy levels. In fact, the estimate of $\alpha_I + \lambda_I$ is not significantly different from 1. These findings suggest that the behavioral response to qualifying for a subsidy is not a strong driver of reduced valuation for ESHI. However, given the noisy estimates, particularly for those who qualify for partial subsidies, we do not emphasize the precision of this conclusion.

Turning to our separate estimates for each of the μ_x terms, which are not identified as convincingly, we do not find compelling estimates. Our estimate for μ_{III}, corresponding to a full subsidy, is -2.56. This estimate is both of the wrong sign and far larger than we would predict given that it is measured as a share of the cost of the benefit, b. The estimate for μ_{II} has the expected sign but is also larger than we would expect, given that μ_{II} is only a partial subsidy.

As in our earlier estimates, we are still able to compute aggregate measures for the cost of ESHI and the penalty-and-subsidy-inclusive valuation, even if the separate parameters are not measured well. Therefore, we can still compute the deadweight loss of mandated-based reform for each of the groups who qualify for different subsidies. Our estimates for the deadweight loss associated with mandate-based reform for group I and III are substantially smaller than our estimates in the aggregate population. The estimated deadweight loss of mandate-based reform for the partial subsidy group, on the other hand, is much larger. We find that the annual deadweight loss of mandate-based reform (DWL_m) for individuals eligible for no, partial, and full subsidies is \$0.57, \$355 and \$0.26, respectively. We are also able to compute the relative deadweight loss of mandate-based reform compared to tax-based reform for each of the subsidy eligible groups. The estimated deadweight loss of mandate-based reform is 1%, 3.2%, and 0.1% of tax based reform for groups

I, II, and III, respectively. These findings suggest that our main deadweight loss findings are broadly robust to subgroup specific estimates, though we note that group II in particular seems to have very noisy estimates.

Taken together, these results, which allow for different responses for individuals who qualify for different subsidies, are consistent with our main findings. Because these specifications allow us to identify additional parameters, specifically the magnitude of the behavioral response to qualifying for subsidized health insurance outside of an employer, we could use these estimates as our main specification. However, we do not emphasize these results because we lose a considerable amount of power when we further divide the SIPP data into income categories.

Table OA4: Results from Preferred Specification by Subsidy Amounts

		(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	0.319	γ_1	0.791	$w_{DI} - w_{AI}$	1.833***
		[-1.240, 2.473]		[-0.741, 2.332]		[1.284, 2.364]
MA*ESHI*During*Large	β_1^d	-2.634***	γ_1^d	-3.331***	$w_{FI} - w_{BI}$	0.038
		[-5.101, -0.476]		[-4.519, -2.401]		[-0.393, 0.660]
MA*ESHI*After*<150FPL*Large	β_2	0.004	γ_2	-0.137	$w_{DI} - w_{BI}$	2.914***
		[-4.147, 3.812]		[-8.886, 5.032]		[1.502, 4.224]
MA*ESHI*During*<150FPL*Large	β_2^d	-1.631	γ_2^d	6.712*	$w_{FI} - w_{AI}$	-1.043
		[-5.409, 2.305]		[-0.484, 10.780]		[-2.667, 0.502]
MA*ESHI*After*150-300FPL*Large	β_3	-1.283	γ_3	-5.674***	$w_{DII} - w_{AII}$	0.586
		[-4.240, 1.536]		[-8.395, -2.025]		[-0.418, 1.026]
MA*ESHI*During*150-300FPL*Large	β_3^d	1.959*	γ_3^d	11.535***	$w_{EII} - w_{BII}$	0.158
		[-0.380, 5.219]		[9.449, 14.496]		[-0.855, 0.724]
MA*ESHI*<150FPL*Large	β_4	-2.299*	γ_4	-7.338***	$w_{DII} - w_{BII}$	0.926
		[-3.716, 0.017]		[-10.044, -5.159]		[-0.583, 1.878]
MA*ESHI*150-300FPL*Large	β_5	0.223	γ_5	-1.645	$w_{EII} - w_{AII}$	-0.183
		[-1.039, 1.761]		[-3.134, 0.339]		[-1.564, 0.819]
MA*After*<150FPL*Large	β_6	3.343***	γ_6	-0.241	$w_{CIII} - w_{AIII}$	6.730***
		[1.198, 5.323]		[-3.143, 1.760]		[6.226, 7.250]
MA*During*<150FPL*Large	β_6^d	-0.383	γ_6^d	0.041	$w_{CIII} - w_{BIII}$	-1.148**
		[-2.731, 1.224]		[-2.165, 1.562]		[-2.248, -0.063]
MA*After*150-300FPL*Large	β_7	-11.225***	γ_7	-8.704***	$w_{CIII} - w_{BIII}$	19.036***
		[-13.575, -9.242]		[-10.712, -6.781]		[17.787, 20.959]
MA*During*150-300FPL*Large	β_7^d	-10.503***	γ_7^d	-13.655***	$w_{CIII} - w_{AIII}$	-13.455***
		[-13.720, -8.552]		[-16.176, -11.613]		[-15.789, -11.673]
MA*ESHI*Large	β_8	2.181***	γ_8	0.942*	$L_{DI} - L_{AI}$	1.497**
		[1.068, 3.178]		[-0.062, 1.679]		[0.264, 2.117]
MA*<150FPL*Large	β_9	3.697***	γ_9	9.528***	$L_{FI} - L_{BI}$	3.538***
		[2.370, 4.853]		[7.955, 11.906]		[2.080, 5.663]
MA*150-300FPL*Large	β_{10}	5.135***	γ_{10}	1.468	$L_{DI} - L_{BI}$	10.542***
		[3.749, 6.220]		[-0.460, 3.202]		[8.245, 12.428]
MA*After*Large	β_{11}	-1.081	γ_{11}	-0.341	$L_{FI} - L_{AI}$	-5.506***
		[-2.533, 0.224]		[-1.632, 0.989]		[-7.277, -3.456]
MA*During*Large	β_{11}^d	3.457***	γ_{11}^d	3.006***	$L_{DII} - L_{AII}$	-0.773***
		[1.764, 6.129]		[2.227, 3.979]		[-2.019, -0.280]
ESHI*After*Large	β_{12}	0.075	γ_{12}	-0.322	$L_{EII} - L_{BII}$	-4.791***
		[-1.598, 1.578]		[-1.464, 0.890]		[-7.264, -2.714]
ESHI*During*Large	β_{12}^d	1.292	γ_{12}^d	-0.205	$L_{DII} - L_{BII}$	-3.034***
		[-0.492, 3.531]		[-0.901, 0.575]		[-4.580, -1.749]
ESHI*After*<150FPL*Large	β_{13}	0.44	γ_{13}	-0.081	$L_{EII} - L_{AII}$	-2.529***
		[-2.742, 2.901]		[-4.494, 2.925]		[-5.755, -0.667]
ESHI*During*<150FPL*Large	β_{13}^d	-3.494***	γ_{13}^d	-2.068	$L_{CIII} - L_{AIII}$	2.344***
		[-6.670, -1.257]		[-5.128, 0.334]		[0.718, 3.615]
ESHI*After*150-300FPL*Large	β_{14}	0.647	γ_{14}	-0.272	$L_{CIII} - L_{BIII}$	0.656
		[-2.074, 3.324]		[-2.635, 2.219]		[-5.671, 3.140]
ESHI*During*150-300FPL*Large	β_{14}^d	-1.539	γ_{14}^d	-1.192*	$L_{CIII} - L_{BIII}$	2.926***
		[-3.869, 0.606]		[-2.775, 0.180]		[0.809, 5.546]
ESHI*<150FPL*Large	β_{15}	-0.379	γ_{15}	0.966	$L_{CIII} - L_{AIII}$	0.074
		[-2.002, 0.847]		[-0.514, 2.520]		[-7.029, 2.207]
ESHI*150-300FPL*Large	β_{16}	0.526	γ_{16}	-0.042	s	0.19
		[-0.773, 1.954]		[-1.676, 1.437]		-
After*<150FPL*Large	β_{17}	-0.628	γ_{17}	1.452*	d	-0.38
		[-2.365, 1.555]		[-0.242, 3.733]		-
During*<150FPL*Large	β_{17}^d	1.287**	γ_{17}^d	1.214*	ρ_I	0.127
		[0.035, 3.449]		[-0.133, 2.676]		[-1.389, 1.334]
After*150-300FPL*Large	β_{18}	-0.987	γ_{18}	-0.12	ρ_{II}	0.009***
		[-2.803, 1.223]		[-1.764, 1.451]		[0.008, 0.011]
During*150-300FPL*Large	β_{18}^d	0.465	γ_{18}^d	0.422	ρ_{III}	0.057**
		[-1.165, 2.517]		[-0.753, 1.453]		[0.016, 0.235]
ESHI*Large	β_{19}	2.232***	γ_{19}	6.714***	b_I	1.113
		[1.235, 3.007]		[6.065, 7.400]		[-0.484, 3.032]
Large*<150FPL	β_{20}	1.154***	γ_{20}	-2.732***	b_{II}	15.547***
		[0.382, 2.172]		[-3.746, -1.601]		[13.444, 18.161]
Large*150-300FPL	β_{21}	-0.183	γ_{21}	0.482	b_{III}	2.501**
		[-1.280, 0.887]		[-0.902, 2.302]		[0.096, 7.976]
After*Large	β_{22}	-0.476	γ_{22}	-0.493	α_I	-1.547
		[-1.801, 0.644]		[-1.767, 0.469]		[-15.454, 17.748]

Table OA4: Results from Preferred Specification by Subsidy Amounts (Continued)

During*Large	β_{22}^{d}	-1.152	γ_{22}^{d}	-0.289	α_{II}	-0.415***
		[-3.515, 0.219]		[-1.033, 0.338]		[-0.521, -0.348]
MA*large	β_{23}	-1.064	γ_{23}	1.735	λ_I	2.454
		[-2.398, 2.087]		[-1.458, 3.443]		[-12.905, 18.055]
Large	β_{24}	-4.230***	γ_{24}	-14.000***	$\alpha_I + \lambda_I$	0.906
		[-6.998, -3.309]		[-15.416, -10.914]		[-0.431, 3.318]
MA*ESHI*After	β_{1e}	-2.114***	γ_{1e}	-1.218**	$\alpha_{II} + \lambda_I - \mu_{II}$	0.798***
		[-4.324, -0.398]		[-2.231, -0.043]		[0.740, 0.867]
MA*ESHI*During	β_{1e}^{d}	0.157	γ_{1e}^{d}	0.278	$\alpha_I - \mu_{III}$	1.017**
		[-1.689, 2.709]		[-0.454, 1.296]		[0.308, 2.071]
MA*ESHI*After*<150FPL	β_{2e}	-2.226	γ_{2e}	-1.123	μ_{III}	-2.564
		[-4.851, 0.835]		[-5.420, 2.825]		[-18.402, 20.616]
MA*ESHI*During*<150FPL	β_{2e}^{d}	6.221***	γ_{2e}^{d}	-2.098*	μ_{II}	1.241
		[2.952, 8.774]		[-4.854, 0.181]		[-14.137, 16.713]
MA*ESHI*After*150-300FPL	β_{3e}	-4.800***	γ_{3e}	8.143***	$ESHI_{After}$	0.79
		[-7.251, -2.841]		[5.316, 10.723]		-
MA*ESHI*During*150-300FPL	β_{3e}^{d}	-7.109***	γ_{3e}^{d}	-8.062***	b_I/τ	1
		[-10.272, -5.326]		[-10.753, -5.966]		-
MA*ESHI*<150FPL	β_{4e}	-0.307	γ_{4e}	9.097***	b_{II}/τ	1
		[-3.479, 1.001]		[6.999, 11.489]		-
MA*ESHI*150-300FPL	β_{5e}	4.674***	γ_{5e}	2.557**	b_{III}/τ	1
		[3.146, 6.016]		[0.280, 4.168]		-
MA*After*<150FPL	β_{6e}	-3.544***	γ_{6e}	3.161***	DWL_{mI}	0.011***
		[-5.420, -1.474]		[1.563, 5.834]		[0.004, 0.555]
MA*During*<150FPL	β_{6e}^{d}	-1.783	γ_{6e}^{d}	-0.985	DWL_{mII}	6.830***
		[-3.270, 0.420]		[-2.278, 0.620]		[2.693, 11.930]
MA*After*150-300FPL	β_{7e}	7.843***	γ_{7e}	4.379***	DWL_{mIII}	0.005***
		[6.065, 10.224]		[2.814, 6.399]		[0.005, 11.129]
MA*During*150-300FPL	β_{7e}^{d}	9.785***	γ_{7e}^{d}	11.624***	$\frac{DWL_{mI}}{DWL_\tau}$	0.010***
		[8.124, 12.685]		[10.433, 14.162]		[0.001, 5.030]
MA*ESHI	β_{8e}	-0.348	γ_{8e}	-0.357	$\frac{DWL_{mII}}{DWL_\tau}$	0.032***
		[-1.418, 0.736]		[-1.174, 0.143]		[0.014, 0.053]
MA*<150FPL	β_{9e}	1.658	γ_{9e}	-0.323	$\frac{DWL_{mIII}}{DWL_\tau}$	0.001***
		[-4.009, 7.585]		[-16.317, 11.692]		[0.001, 5.970]
MA*150-300FPL	β_{10e}	-6.432**	γ_{10e}	-4.329		
		[-12.749, -1.972]		[-13.410, 5.062]		
MA*After	β_{11e}	4.115***	γ_{11e}	1.682***		
		[2.690, 5.289]		[0.602, 2.392]		
MA*During	β_{11e}^{d}	-0.378	γ_{11e}^{d}	0.327		
		[-2.935, 1.100]		[-0.603, 0.958]		
ESHI*After	β_{12e}	-0.334	γ_{12e}	-0.652		
		[-1.962, 1.626]		[-1.550, 0.318]		
ESHI*During	β_{12e}^{d}	-1.435	γ_{12e}^{d}	-0.633*		
		[-3.738, 0.234]		[-1.463, 0.060]		
ESHI*After*<150FPL	β_{13e}	-0.105	γ_{13e}	0.908		
		[-2.983, 3.127]		[-2.121, 4.705]		
ESHI*During*<150FPL	β_{13e}^{d}	2.601**	γ_{13e}^{d}	1.64		
		[0.251, 5.848]		[-0.798, 4.648]		
ESHI*After*150-300FPL	β_{14e}	-0.388	γ_{14e}	0.808		
		[-2.444, 2.120]		[-1.169, 3.254]		
ESHI*During*150-300FPL	β_{14e}^{d}	1.181	γ_{14e}^{d}	1.031		
		[-0.893, 3.454]		[-0.303, 2.436]		
ESHI*<150FPL	β_{15e}	-0.591	γ_{15e}	0.074		
		[-1.897, 1.246]		[-1.252, 1.632]		
ESHI*150-300FPL	β_{16e}	-1.695***	γ_{16e}	-0.433		
		[-2.938, -0.514]		[-1.772, 1.221]		
After*<150FPL	β_{17e}	0.284	γ_{17e}	-0.759		
		[-1.892, 1.913]		[-2.845, 0.579]		
During*<150FPL	β_{17e}^{d}	-1.753***	γ_{17e}^{d}	-1.127**		
		[-3.777, -0.534]		[-2.467, -0.058]		
After*150-300FPL	β_{18e}	1.084	γ_{18e}	-0.23		
		[-0.912, 2.830]		[-1.803, 1.444]		
During*150-300FPL	β_{18e}^{d}	-0.811	γ_{18e}^{d}	-0.368		
		[-2.839, 0.773]		[-1.428, 0.865]		
ESHI	β_{19e}	2.522***	γ_{19e}	0.980**		
		[1.510, 3.395]		[0.226, 1.478]		
After	β_{22e}	1.049*	γ_{22e}	1.462***		
		[-0.207, 2.331]		[0.686, 2.372]		
During	β_{22e}^{d}	1.703***	γ_{22e}^{d}	1.219***		
		[0.395, 4.014]		[0.615, 1.951]		
Observations		495,352		479,310		
R-squared		0.759		0.842		

***p<0.01, **p<0.05, *p<0.1, 95% confidence intervals reported; CIs block bootstrapped by state.
Excluding >=age 65, Medicare beneficiaries, military health insurance beneficiaries. Only includes interview months.
All specifications include individual, state, and state*large firm fixed effects. Monthly weights used.
Large firm defined as >25 employees.