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ABSTRACT

State Health Insurance Mandates and Labor Market Outcomes: New Evidence on Old Questions*

In this study we re-visit the relationship between private health insurance mandates, access to employer-sponsored health insurance, and labor market outcomes. Specifically, we model employer-sponsored health insurance access and labor market outcomes across the lifecycle as a function of the number of high cost mandates in place at labor market entrance. Our analysis draws on a long panel of workers from the National Longitudinal Survey of Youth 1979 and exploits variation in five high cost state mandates between 1972 and 1989. Four principal findings emerge from our analysis. First, we find no strong evidence that high cost state health insurance mandates discourage employers from offering insurance to employees. Second, employers adjust both wages and labor demand to offset mandate costs, suggesting that employees place some value on the mandated benefits. Third, the effects are persistent, but not permanent. Fourth, the effects are heterogeneous across worker types. These findings have implications for thinking through the full labor market effects of health insurance expansions.

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1. Introduction

Private health insurance has historically been regulated at the state level in the United States (Morrisey, 2014). In particular, states have regulated the generosity and scope of private health insurance coverage through the implementation of 'mandates' since the 1940s. Mandates typically stipulate coverage for specific treatments (e.g., drug abuse), providers (e.g., optometrists), and population categories (e.g., dependents) within the private market (Morrisey, 2014). Pennsylvania passed the first mandate in 1949: specifically, requiring healthcare services provided by osteopaths and dentists (Laugesen et al., 2006). The number of mandates has increased substantially over time. For example, there were roughly 1,000 mandates in 1991 (Gruber, 1994b) and 2,156 mandates in 2010 (Bunce and Wieske, 2010).

The key economic argument for mandates is resolution of the adverse selection problem (Lahey, 2012): only those individuals who expect to utilize a particular healthcare service are willing to pay for insurance that covers this service which leads to a cycle of increasing premiums and a smaller, and less healthy, pool of beneficiaries.[1] Positive externalities from healthcare interventions (e.g., vaccines) also motivate the use of mandates. However, mandate critics argue that these regulations unduly increase labor costs, contribute to the overall rise in healthcare costs, and, consequently, the decline of employer-sponsored health insurance (ESI) within the U.S. (Litow, 2002). Advocates, on the other hand, contend that mandates are welfare enhancing since they compel private health insurers to provide an equitable and appropriate level of coverage (Gruber, 1994b).

The contentious policy debate on the merits and demerits of mandates is not close to resolution, in part, given the conflicting research findings on mandate effects. For example, a series of studies document that mandates increase access to valuable healthcare services (Dave

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[1] In the extreme, the market will enter a 'death spiral' in which the premiums become so high that no individuals are willing to purchase insurance leading to a collapse of the market.

and Mukerjee, 2011, Maclean et al., 2017, Akosa Antwi et al., 2015) and improve health (Courtemanche and Zapata, 2014), without substantially distorting the labor market (Kaestner and Simon, 2002). A different set of studies suggests mandated health insurance benefits in private markets may have negative consequences; such as terminated health insurance coverage (Gabel and Jensen, 1992), increased insurance costs (Bailey and Blascak, 2016, Depew and Bailey, 2015), distortions in the labor market (Bailey and Webber, 2016), and moral hazard (Klick and Stratmann, 2006) with limited impact on access to healthcare services (Pacula and Sturm, 2000, Sturm, 2000, Maclean and Saloner, 2016).

In this study we explore the persistent impact of mandated benefits, specifically high cost private health insurance mandates, on access to employer-sponsored health insurance (ESI) and labor market outcomes among new labor market entrants. As such we add new information to the large economic literature on the impact of mandated benefits on U.S. labor markets.

While our study cannot reconcile the controversy within the mandated benefits literature, we can attempt to shed light on a question that, to the best of our knowledge, has not yet been studied. Specifically, we ask: are there persistent effects of high cost mandates on access to ESI and labor market outcomes for new labor market entrants? In other words, (i) do high cost mandates affect access to ESI and labor market outcomes; and (ii) will there be persistent differences over the lifecycle of two workers, one who enters a labor market characterized by highly regulated private insurance contracts and the other who enters a labor market with limited regulation of private insurance contracts? Economic theories that allow for labor market frictions suggest that shocks to labor demand, such as mandates, at initial labor market entrance can lead to persistent employment effects. Broadly, these theories suggest that a worker's initial compensation package can persistently impact his compensation and labor supply profiles. Thus, shocks to labor demand can have long-lasting implications for new labor market entrants.

3

We consider the following outcomes: whether an employee has access to ESI, hourly wages, and labor supply (weeks worked per year and the probability of fulltime employment). Our contributions to the literature are twofold. First, by using private health insurance mandates data spanning 1973 to 1990, we leverage variation in the number and scope of several costly mandated benefits. Second, we explore dynamics of mandate effects across the lifecycle.

To answer this question, we drawn a long panel of workers from the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 tracks workers from initial labor market entrance through mid-career when workers are in their mid-50s. The mandated benefits we study are: alcohol abuse treatment, illicit drug abuse treatment, mental health treatment, chiropractors, and continuing coverage for terminated employees and their dependents. During our study period health insurance mandates accounted for over 30% of employers' health insurance costs, with these specific mandates generating over 50% of these costs (Gruber, 1994b). We estimate differences-in-differences models that control for time-invariant and time-varying state-level factors that may be correlated with both the passage of high cost mandates, access to ESI and labor market outcomes. Over our study period, numerous states implemented at least one or more of the mandates we study, offering us substantial variation for identification.

Before proceeding to our main analyses of persistent mandate effects, we first explore the effects of the high cost mandates we study on contemporaneous ESI and labor market outcomes. We document that these mandates do indeed reduce wages and measures of labor supply, however, they do not impact ESI propensity. Turning to life course effects, we find no evidence that the state health insurance mandates we study discourage employers from offering ESI but employers reduce wages and labor demand (weeks and hours worked) to offset the cost of mandates. Collectively, these findings suggest that employees value the mandated benefits, but not fully. The effects we estimate are persistent, but not permanent: they dissipate with time

4

spent in the labor market. However, our findings suggest a high degree of persistence as we observe that effects 13 to 34 years, depending on the outcome, after labor market entrance. Lastly, effects are generally concentrated among workers who began their careers with small employers and lesser skill workers (defined as those workers with no college education).

The remainder of this manuscript is organized as follows. Section 2 reviews the economics of mandated benefits and related literature. Section 3 outlines our conceptual framework and hypotheses. Our data, variables, and methods are described in Section 4. Section 5 reports our main results, while extensions to the main analysis and robustness checking are reported in Section 6. Finally, Section 7 concludes.

2. Empirical evidence on health insurance mandate impacts

A comprehensive review of the vast literature on ESI and labor market effects of health insurance mandates is beyond the scope of our study. Thus, for brevity, we focus our attention on studies that are closely related to our research question: those that examine the concurrent effect of mandates on ESI, wages, and labor supply. We refer readers to reviews by Jensen and Morrisey (1999), Monheit et al. (2007), Lahey (2012), and Morrisey (2014) for more details.

2.1. Employer sponsored health insurance (ESI)

Gabel and Jensen (1992) examine the effect of state insurance regulation on a small (<100 workers) employers' decision to offer health insurance coverage. Using data on 1,320 small employers, the authors find mixed evidence on mandate impacts. Continuation-of-coverage mandates decrease the likelihood that a small employer offers ESI whereas drug abuse treatment mandates increase this probability. Overall, implementation of the two mandates reduces the likelihood that a small employer offers ESI. Gruber (1994b) uses data from the Current Population Survey (CPS) and five high cost mandates to investigate the mandate-ESI

5

relationship.[2] Gruber finds no evidence that mandates impact employers' propensity to offer

ESI. Subsequent studies generally support Gruber's null finding (Cseh, 2008, Kaestner and

Simon, 2002, Bao and Sturm, 2004, Sturm, 2000).

2.2 Wages and labor supply

In a seminal study, Gruber (1994a) evaluates the labor market response to state and

federal mandates for comprehensive coverage for childbirth among married women of

childbearing age. Using CPS data, Gruber considers the effect of the mandate on wages, hours

worked, and employment of married women of childbearing age (i.e., the target population of

this mandate). He finds evidence that employers shift the cost of mandated maternity benefits to

married women of childbearing age, but no evidence that mandates impact such women's

employment propensity. The decrease in wages combined with no change in employment

propensity suggests that these women value the mandated benefit.

Cutler and Madrian (1996) document that the number of state mandates increase average

hours worked, suggesting that due to the increased cost per employee, employers opt to extract

more work time from current employees rather than hiring new employees. Kaestner and Simon

(2002), using CPS data, find that the number of state-mandated health insurance benefits have

no effect on wages, weeks of work, and group insurance coverage. However, the authors show

that the number of mandates increases weekly work hours.

Cseh (2008) studies the impact of state mental health mandates on labor market

outcomes and finds that these mandates reduce wages, suggesting that employees value the

mandated benefit, but no evidence that measures of labor supply are impacted by this mandate.

Andersen (2015) builds on Cseh's work to study heterogeneity in the effect of mental health

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[2] Mandated minimum benefits for alcohol abuse treatment, drug abuse treatment, and mental illness; mandated coverage for chiropractic services; and mandated continuation of health insurance benefits for terminated employees and their dependents. We examine the same high cost mandates in our study.

mandates on labor market outcomes. Specifically, among employees with poor mental health, mandates increase wages, working hours, and the probability of insurance coverage. Relatedly, Lahey (2012) finds that wages are unaffected by infertility mandates though labor supply decreases by 1.07 weeks per year among women of childbearing age.

Overall, the literature on the effects of mandates on health insurance and labor market outcomes is mixed.[3] However, while mixed, the literature does suggest some scope for mandates to impact the labor market outcomes examined in this study: wages and labor supply.

3. Conceptual framework

We next review several strands of economic theory that point toward (i) contemporaneous impacts of mandates on access to ESI and labor market outcomes, and (ii) persistent impacts of mandates on these outcomes.

3.1 A brief review of the economics of health insurance mandates

Summers (1989) provides one of the early economic analyses of health insurance mandates on labor market outcomes. Prior to implementation of a mandate, the labor market is in equilibrium at the intersection D_1 and S_1, with employment level E_1 and wages of W_1 as depicted in Figure 1. Summers, assuming that the cost of the mandate is a per hour rate, argues that health insurance mandates increase labor costs and should, all else equal, lead to a decrease in demand for labor among employers by the cost of the mandate, from D_1 to D_2. Thus, the mandate should lead to a lower level of employment and wages (E_2', W_2'). However, if employees value the mandate, then this valuation will lead to an increase labor supply (from S_1 to S_2). The labor supply increase will have two effects: it will attenuate the employment decline and increase the wage decline. At the new equilibrium, wages and employment will fall

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[3] Lahey (2012) argues that the lack of consensus is not surprising. Because mandates vary in terms of scope, costs to employers, affected population, and characteristics, economic theory necessarily predicts the mixed results observed within the literature.

(E_2, W_2). The extent to which the mandate impacts wages and employment levels is determined

by employees' valuation of the benefit.

If employees fully value the benefit, the incidence of the mandate will be entirely passed

on to the employees in terms of lower wages and there will be no impact on employment levels

(as employers experience no increase in labor costs). Alternatively, if employees do not value

the benefit to any extent, the cost of the mandate will be fully born by the employers, wages will

be unchanged, and overall employment will decline to offset the mandate cost. Intermediate

valuations of the benefit by employees will lead to both lower wages and employment levels,

with the relative magnitudes of these effects determined by employee preferences. Summers

(1989) notes that several features of the U.S. labor market, such as minimum wages and anti-

discrimination laws, may limit employers' ability to reduce wages to offset mandate costs.

Subsequent work offers a number of potential extensions to the Summers framework.

Sloan and Conover (1998) highlight the possibility that, if mandates become too costly,

employers may elect to self-insure. Self-insured firms are exempt from state regulations of the

healthcare market under Employee Retirement Income Security Act of 1974 (ERISA). Gabel

and Jensen (1992) note that employers, when faced with higher mandate costs, could choose to

eliminate ESI altogether. Such actions would reduce the probability of access to ESI, but would

mute wage and employment effects.[4] Cutler and Madrian (1996) develop a model in which

employers may increase the labor supply of current employees rather than hire additional

employees.[5] Moreover, employers may opt to rely on part time or temporary workers who are

generally not eligible for employer-sponsored benefits such as health insurance.[6]

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[4] Indeed, if employers drop ESI, and wish to maintain the same level of compensation for employees, we might expect wages (or other forms of compensation) to rise.

[5] Cutler and Madrian (1996) note that, in U.S. labor markets, ESI is generally not paid on a per hour rate (an assumption made by Summers) and instead is a fixed cost per employee.

[6] It is worth noting that if employers are able to pass some, or all of, the mandate costs to employees in the form of health insurance premium increases and/or additional cost sharing (e.g., copayments, deductibles) then the labor

3.2 Persistence

A unique contribution of our study is that we explore the *persistent* effects of mandates for new labor entrants. To the best of our knowledge, there is no specific economic model that considers the persistence of mandates on ESI or labor market outcomes. Thus, we draw on existing theories of career development to establish the possibility of a relationship between high cost mandates at labor market entry and employment outcomes across the life course.

Under the assumptions of perfect competition, the labor market operates as a spot market. In such a market, we would not expect initial conditions, including insurance regulations, to have a persistent impact as only current conditions are important for labor market outcomes. There are numerous reasons, however, to suspect that the U.S. labor market departs from the perfectly competitive ideal (Webber, 2015). More specifically, frictions in the labor market suggest persistent effects stemming from adverse career experiences.

A key source of frictions is the cost of switching from one job to another. Job switching involves search, training, time, financial, reputation, and psychic costs. Incomplete information about job opportunities and finite job offer arrival rates also lead to frictions. Broadly, frictions limit the ability of workers to switch from one job to another and may therefore force workers to remain stuck in poor job matches (Kondo, 2015). While frictions impact all workers, they may be particularly important for new entrants as early career is a critical period of wage growth and skill accumulation (Neumark, 2002, Topel and Ward, 1992).

If mandates induce employers to cut labor costs by extracting additional hours from current workers rather than hiring new workers (Cutler and Madrian, 1996), the quantity of job opportunities for new labor market entrants could be restricted. A job matching model predicts that individuals who leave school in such a market will experience a longer initial job search

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market implications of mandates would be muted. We are unable to study this mechanisms with our data.

9

spell as there are fewer open jobs (Jovanovic, 1979). Relatedly, if mandates impact the distribution of firms in the labor market (Bailey and Webber, 2016), the type of firms that offer jobs may also be altered. Finally, lower wages attributable to mandates may lead employees who do not value the mandate to spend more time searching for a job that offers a wage at or above their reservation wage. Such a demand shock at labor market entrance could lead to new entrants accepting poor job matches, lower earnings, and temporary jobs with fewer hours.

If workers are initially mismatched to their jobs they may acquire the 'wrong' human capital. Such accumulation may be particularly harmful for workers' wage profiles if a job requires firm- or task-specific, rather than general, human capital. If workers are unemployed at labor market entrance, or unemployed for longer spells as they search for a job, they may fail to accumulate human capital and/or experience depreciation in their human capital stock, causing these workers to fall behind. Such limited human capital accumulation opportunities in turn can lead to a persistently flatter wage profile (Genda et al., 2010).

In addition, the labor market may incorrectly interpret the first job placement as a measure of ability, rather than luck (Oyer, 2006). In such a model, a worker who leaves school during a period of reduced labor demand may carry the (negative) first placement signal throughout his career. Moreover, it is possible that worker preferences for job attributes (wage compensation vis-a-vis non-wage compensation, work hours, etc.) may be shaped by their initial job match (Oyer, 2006).

In an internal labor market (ILM) model, workers enter the firm at specific ports of entry. A theoretically important port is labor market entrance (Baker et al., 1994). The ILM is shielded from the external labor market with promotions typically occurring within the firm. Workers in the external labor market usually have limited access to the ILM. If individuals who leave school during periods of weak labor demand are unable to access ILMs, they may be

10

systematically locked out of such segments of the labor market.

Supporting these hypotheses, there is ample evidence that adverse labor market conditions, typically measured by the unemployment rate, at labor market entrance have long-run effects on the earnings, access to ESI, job prestige, and labor supply of workers (Ellwood, 1982, Kahn, 2010, Oreopoulos et al., 2012, Maclean, 2014, Altonji et al., 2016, Genda et al., 2010). For instance, using longitudinal Canadian data, Oreopoulos et al. (2012), find that male college graduates who enter the labor market during a recession experience an initial earnings loss of 9% which persists for 10 years. Moreover, Genda et al. (2010) show that workers who leave school during periods of weak labor demand are persistently more likely to be unemployed. Maclean (2014) demonstrates that workers who leave school during a recession are persistently less likely to have access to ESI and other forms of non-wage compensation. Although these studies explore the implications of a different type of labor demand shock, they do open the door to the possibility that shocks more broadly defined experienced at labor market entrance may have long-lasting effects for workers.

4. Data, variables, and methods

4.1 Data

We draw data on a long panel of workers from the National Longitudinal Survey of Youth 1979 Cohort (NLSY79). We use the geocoded data which allows us to access information on state of residence. The original NLSY79 sample consists of 12,686 youth 14 to 22 years in 1979. The survey was administered annually by the Bureau of Labor Statistics (BLS) between 1979 and 1993, and bi-annually from 1994 to 2012. These data are well suited to our research question as the NLSY79 was specifically designed to track a cohort of workers as they transition into the labor market and throughout their career. We have information on ESI, wages, and labor supply. We are able to follow workers from labor market entrance, which

11

we define as the first job held after leaving school, through mid-career. Specifically, in the most recent round of the NLSY79 (fielded in 2012) workers are in their early- to mid-50s.

We focus on a sample of workers ages 18 to 54 years (Gruber, 1994b, Kaestner and Simon, 2002). These exclusions, and others necessary to construct our analysis sample that are detailed later in the paper, leave us with a sample of 11,013 unique workers and 168,527 worker/year pairs. To preserve sample size, we rely on the unbalanced panel of workers.

4.2 ESI and labor market outcomes

We focus on four outcomes using the employment data available across years. NLSY79 respondents can list information, including the ESI and wage variables we study here, on multiple jobs. The number of jobs varies across survey years. For consistency across survey years we assume the first job reported by the respondent is the dominant job (Maclean, 2014). The labor supply measures we examine are cumulative across all jobs.

First, we construct a binary indicator for whether or not a worker has access to ESI. The specific question wording in 1979 is 'Does your employer make health insurance available to you? Medical, surgical, or hospital insurance that covers injuries or major illnesses off the job?'[7] We code respondents as one if they report this offer, and zero otherwise.[8]

Next, we construct three labor market outcomes. First, we consider the hourly wage; we inflate nominal wages to 2012 dollars using the Consumer Price Index – Urban Consumers. We exclude workers with wages less than $1 per hour and those with wages greater than $1,000 per

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[7] The specific question wording has changed across survey years to some extent. Moreover, the ESI question was not asked in the 1981 round of the NSL79, thus we do not have a value for this outcome in 1981. Interested readers can consult the NLSY79 codebook: http://www.bls.gov/nls/nlsy79.htm (accessed April 23rd, 2016).
[8] Unlike previous studies examining the impacts of state health insurance mandates on the provision of employer-sponsored health insurance (Kaestner and Simon, 2002), we are able to isolate offers from take up of ESI. Thus, we are able to avoid confounding offers with endogenous take-up decisions by employees. Although we argue that our ESI offer variable is advantageous, this variable has at least three important caveats. First, this variable captures whether an employee is aware of an offer of ESI. Respondents may decide not to take up this offer for myriad reasons. We are unable to capture such reasons here. Second, this variable does not measure the generosity of any offered ESI, thus we cannot assess whether the respondent's ESI offer covers the mandated benefits. Third, the ESI question described here is only asked to workers and thus our findings may be vulnerable to sample-selection bias.

hour (Kahn, 2010). Second, we construct two measures of past year labor supply: weeks worked and fulltime work (an indicator variable coded one if the respondent usually works 35 or more hours per week, and zero otherwise).[9] We take the logarithm of wages and weeks worked.[10] Thus, estimated regression coefficients have the interpretation of an approximation to the percent change.[11] Due to differences in survey universe and item non-response, our sample sizes vary to some extent across outcomes. Results based on a sample with complete information on all four outcomes are comparable and available on request.

4.3 High cost health insurance mandates at school-leaving

To measure the number of high-cost mandates in place at labor market entrance we use data on five high cost mandates for services, providers, and covered beneficiaries studied by Gruber (1994b). The mandates are: alcohol abuse treatment, illicit drug treatment, mental health treatment, coverage of chiropractic services, and continuing coverage.

To investigate the impact of high cost mandates at the time of labor market entrance, we must locate the year and state in which each individual left school, the period in which most students enter the labor market. A limitation of the NLSY79 data is that we only have state of residence beginning in 1979 (the first year of the survey), but many individuals in our sample entered the labor market before this year (respondents are ages 14 to 22 in 1979). We use state of birth as a proxy for the labor market entrance state for all workers. We exclude respondents with a missing birth state or who were born outside the U.S. Use of the birth state leads to measurement error for those individuals who crossed state lines between birth and labor market entrance. However, for the study period where we observe most respondents enter the labor

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[9] In unreported analyses, we have also estimated regressions in which the outcome variable is the logarithm of the usual number of hours worked per week. Results are comparable and are available on request.
[10] We focus only on those individuals with positive values of these outcomes.
[11] Results using unlogged values of hourly wage and weeks worked are not appreciably different from those reported here and are available on request.

13

market, less than 3% of the U.S. population moved across state lines annually (Bureau of Labor Statistics, 1976). Thus, we suspect that measurement error is not substantial. Moreover, in unreported analyses, we use the state of residence at age 14, and state of residence in 1979 for those who left school in 1979 or an earlier year and interview state for those who left school after 1979 to proxy the labor market entrance state. Results, available on request, are not appreciably different from those reported here.[12] We refer to birth states as 'labor market entrance' states henceforth in the manuscript.

We locate the year of labor market entrance using retrospective information on school-leaving collected between 1979 and 1998. Non-enrolled NLSY79 respondents are asked to report the year in which they left school. If a respondent indicated that they completed no formal education, we exclude them from the analysis sample as we cannot locate a labor market entrance year. We focus on the sample of workers who entered the labor market between 1973 and 1990. These exclusions lead us to drop 133 observations. We exclude those who entered the labor market in earlier (pre-1973) years as cohort sizes are small (less than 20 per year). However, in unreported analyses we relaxed this assumption and results are not appreciably different than those reported here.[13] We exclude respondents who entered the labor market after 1990 (n=81) as our policy data are only available through 1989 (Gruber, 1994b) and in our regressions (detailed later) we lag mandates one year.[14] Appendix Table A reports the number of respondents (both unweighted and weighted) entering the labor market by year. The largest

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[12] We choose not to drop individuals who entered the labor market before 1979 as this would substantially reduce the number of observations in our analysis sample and, more importantly, the number of policy changes that we exploit to identify the effect of high cost mandates on health insurance and labor supply measures. See Table 1.
[13] Specifically, we included all respondents for whom we could locate a labor market entrance year and birth state, regardless of when they entered the labor market (with the exception of those leaving after 1990).
[14] We considered updating the policy data sources to include more recent years. However, this exercise would have required us to combine different sources of data (e.g., National Council of State Legislatures and Blue Cross Blue Shield data). We were concerned that combining data sources could lead to errors in the policy data, indeed, our attempts at combining data sets revealed measurement error problems (details available on request). Moreover, only 81 observations meeting other eligibility criteria for our sample entered the labor market after 1990 suggesting that failure to include these observations might not lead to substantial bias in our estimates.

cohort was 1979 (unweighted n=1,441) and the smallest cohort was 1990 (unweighted n=29).[15]

Table 1 reports the effective date for each of the high cost mandates through 1989. By 1989, all states with the exception of Idaho and Wyoming had implemented at least one of these mandates. Three states (Kansas, Virginia, and Wisconsin) implemented all five mandates by 1989. Only states that implement high cost mandates during our study period contribute variation in our empirical models (differences-in-differences). We use bold text in Table 1 to indicate these changes. During our study period, 25 states implemented an alcohol treatment mandate, 15 states implemented an illicit drug treatment mandate, 11 states implemented a mental health treatment mandate, 27 states implemented a chiropractor mandate, and 30 states implemented a continuing coverage mandate.[16] We take the unweighted count of the number of high cost mandates. This variable ranges from 0 to 5.

4.4 Control variables

We include a set of pre-determined individual-level variables that are expected to predict the probability of an ESI offer, hourly wages, and labor supply in all regression models: race/ethnicity (African American and Hispanic, with White as the omitted group), age at labor market entrance, level of education at labor market entrance (high school, some college, and a college degree, with less than high school as the omitted category), a proxy for ability (age-standardized Armed Forces Qualification Test [AFQT]), parental education as measured by mother's and father's years of education entered linearly and separately, rural residence at age

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[15] We have estimated regressions dropping the school-leaving year 1973 from the analysis sample as ERISA became effective in 1974. Results, available on request, are not appreciably different that those reported here.

[16] In 1985 the Federal government passed the Consolidated Omnibus Budget Reconciliation Act (COBRA). This Act became effective in April 1986 and mandated that a private insurance program which gives some employees the ability to continue health insurance coverage after leaving employment for a period up to 18 months. Therefore, this Act superseded the state continuing coverage laws studied here and these state laws may have little bite post-1986. To address this issue, we re-estimated our regression models excluding the continuing coverage law from our high cost mandate variable. More specifically, we construct our high cost mandate variable as the count of alcohol abuse treatment, illicit drug treatment, mental health treatment, and coverage of chiropractic services only (this variable ranges from 0 to 4). Results, available on request, are not appreciably different from those reported here.

14, and indicators for access to cultural materials within the household at age 14 (library card, newspapers, and magazines). In addition, we include the number of years (entered linearly) between labor market entrance and the periods in which our outcome variable is measured. This variable is our proxy for potential experience in the labor market (Maclean, 2013).

We include indicators for missing covariates and assign missing observations the sample mean (continuous variable) or mode (binary variable) in our regression models. However, results are robust if we instead drop all observations with missing information on covariates.

We also include labor market entrance state characteristics. Ideally, we would like to control for variables that are likely to influence both our outcomes and a state's propensity to pass the high cost mandates we study. Because our study period extends back to 1973, there is limited state-level information available. Thus, we leverage information contained in the Annual Social and Economic Supplement to the Current Population Survey (CPS): unemployment rate, poverty rate, share of the population with some college education, average age, and the share of the population working for pay with a private employer.[17]

4.5 Empirical model

Equation (1) presents the regression model we use the estimate the effects of high cost mandates at labor market entrance on ESI and labor market outcomes across the lifecycle:

(1) $L_{istg} = \alpha_0 + \alpha_1 M_{st} + \alpha_2 E_{ig-t} + \alpha_3 M_{st} * E_{ig-t} + \alpha_4' X_i + \alpha_5' C_{st} + G_g + S_s + D_t + \Omega_{st} + \varepsilon_{istg}$

L_{istg} is an outcome measured for individual *i* in labor market entrance state *s* and labor market entrance year *t* measured in survey year *g*. M_{st} is the lagged number of high cost mandates in labor market entrance state *s* and labor market entrance year *t*.[18] E_{ig-t} is potential

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labor market experience. $M_{st} * E_{ig-t}$ is the interaction between the number of high cost

mandates at labor market entrance and potential experience. Including this interaction term

allows the effect of high costs mandates at labor market entrance to vary across time. For

example, the effects (if present) may increase or decrease with time spent in the labor market.

X_i is a vector of personal characteristics and C_{st} is a vector of labor market entrance state

characteristics.[19] G_g includes survey year fixed effects. S_s and D_t are vectors of labor market

entrance state and year fixed effects. Inclusion of the labor market entrance state fixed effects

implies that we use within labor market entrance state variation in high cost mandates to identify

effects. These fixed effects control for time invariant and difficult-to-observe (to the

econometrician) between labor market entrance state differences that may be correlated with

both the number of high cost mandates in the labor market entrance state and our outcomes.

Finally, we include a separate linear time trend for each labor market entrance state (Ω_{st}). These

variables allow us to control for unobservable labor market entrance state variables.[20]

We utilize linear probability models (LPMs) for binary outcomes[21] and OLS for

continuous outcomes. We estimate separate models for men and women given established

differences across sex in terms of labor market outcomes (Blau and Kahn, 2007). We apply

NLSY79 sample weights, but unweighted results are similar and available on request (Solon et

al., 2015). Standard errors are clustered around the labor market entrance state.[22]

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[19] These characteristics are time invariant. One may be concerned that including age at school-leaving, school-leaving year, and potential experience may lead to collinearity issues. In unreported analyses, we re-estimated Equation (1) removing one or two of these variables (more details are available from the corresponding author). Moreover, we have estimated regression models with no individual characteristics. Results are not appreciably different and are available on request.

[20] Results are not appreciably different if we instead exclude the labor market entrance state-specific linear time trends (and therefore only control for time invariant unobservables with our labor market entrance state fixed effects) and if we use labor market entrance state-specific quadratic time trends. Results are also robust to including state-of-residence at the time outcome variables are measured.

[21] We choose the LPM over a probit or logit model as the LPM is not vulnerable to the incidental parameters problem (Greene, 2004).

[22] We have 51 clusters in our data. Thus, we believe that we have a sufficient number of clusters to consistently estimate standard errors (Cameron and Miller, 2015).

17

5. Results

5.1 Do high cost mandates have a contemporaneous effect on ESI and labor market outcomes?

We first document that the high cost mandates we study have a contemporaneous effect on the labor market outcomes we study. If we do not observe an immediate effect of these mandates, it is less clear as to why we should expect persistent effects.[23] To this end, we turn to the CPS from the Integrated Public Use Microdata Series (IPUMS) project (Flood et al., 2015), a common data set used to study state mandates in the U.S.[24]

We draw data from the 1976 to 1990 CPS surveys and focus on workers ages 18 to 54 years, thus the same age range we examine in our study and in previous investigations of mandate effects (Kaestner and Simon, 2002).[25] Ideally we would like to use the same study period in the CPS as we do in the NLSY79 (1973 to 1990), but prior to the 1976 CPS survey many of our outcome variables are not available.[26] We estimate the following regression model:

(2) $LM_{ist} = \beta_0 + \beta_1 M_{st} + \beta_2' X_i + C_s + Y_t + \Delta_{st} + \mu_{ist}$

Thus, this regression model is similar to Equation (1),[27] although we cannot determine potential experience and instead simply control for age. Moreover, we use current state and year fixed effects (C_s and Y_t), and current state-specific linear time trends Δ_{st}.[28] Our outcome

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[23] However, a failure to identify initial effects does not necessarily preclude an analysis of persistence effects if effects do not emerge immediately and instead take time to evolve (Maclean and Hill, 2015).

[24] We could have conducted the contemporaneous analysis of mandates in the NLSY79 sample. However, the NLSY79 commences in 1979 and thus we miss law changes that occur prior to this year. In unreported analyses, we re-estimated our contemporaneous models in the NLSY79 between 1979 and 1990. Results are comparable in sign, but are less precisely estimated. We suspect that the drop in precision is driven by the reduction in the number of law changes that we use for identification (see Table 1).

[25] We do not condition on working for a private employer as have previous investigations on contemporaneous mandate effects (Kaestner and Simon, 2002) as this variable is not available in all years of our CPS study period.

[26] Specifically, the CPS underwent a major redesign between 1975 and 1976. Based on our analysis of the CPS data, our measures of wages and labor supply cannot be easily compared before and after this change. Our measure of ESI is only available in the CPS from 1980 onward as this is the year in which the question was first asked to CPS respondents. However, if we restrict our NLSY79 sample to those individuals who left school in 1976 and onward, and therefore match the CPS sample more accurately, results generated in our main model of persistent effects are not appreciably different from those reported here.

[27] This regression model is also similar to studies that explore the contemporaneous effects of health insurance mandates on ESI and labor market outcomes. For example, Gruber (1994b) and Kaestner and Simon (2002).

[28] The CPS has less detailed information on respondent personal characteristics than the NLSY79. Thus, we cannot

variables closely match our NLSY79 outcomes and pertain to the past year: holding ESI (0/1), the logarithm of the hourly wage,[29] the logarithm of weeks worked, and fulltime work (working 35 hours per week or more; 0/1). We apply CPS sample weights and cluster standard errors around the state. Results are reported in Table 2.

Findings from this analysis of the CPS document that, during our study period, for both men and women, the passage of these mandates did not have a discernable effect on the probability of holding ESI or number of weeks worked but lead to reductions in wages and the probability of working fulltime. The passage of an additional high cost mandate leads to a 0.7% and 0.6% reduction in wages for men and women respectively. Both effects are statistically significant at the 10% level. An additional high cost mandate also reduces the probability of holding a fulltime job by a statistically significant 0.3 percentage points (0.3%) for men and 0.4 percentage points (0.6%) for women.

Collectively, these findings suggest that the high cost mandates that we study here have a contemporaneous impact labor market outcomes and, therefore, it is reasonable to explore persistent effects. To the best of our knowledge, this contemporaneous relationship has not been documented. It may be worthwhile to consider why we identify mandate effects while some other studies (see Section 2.1), using similar data and research designs, have reached different (often null) conclusions. We suspect that our focus on an earlier time period (1976 to 1990; with health insurance mandate data covering 1975 to 1989) is a potential explanation (most studies that leverage the CPS consider data from the 1980s and onward). State mandates may have had more bite in the 1970s than they have in more recent periods (National Council of State Legislatures, 2015), that is employers did not voluntarily choose to cover such benefits prior to

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include all controls that we include in Equation (1). Instead, we control for current age, race/ethnicity, and education. However, as noted earlier in the manuscript removing individual-level controls from Equation (1) does not lead to appreciably different findings.

[29] As we do in the NLSY79 analyses, we drop outlier wages: hourly wages less than $1 and greater than $1000.

state regulations (Gruber, 1994b). Moreover, we focus on high cost mandates rather than overall

counts of mandates, and the latter parametrization may dilute mandate effects.[30] Finally, self-

insurance among firms was less common in the earlier years we study here.

5.2 Summary statistics: NLSY79

We now return to our analyses of persistent mandate effects in the NLSY79. Summary

statistics are reported in Table 3. Male workers have slightly higher values for all our outcome

variables than female workers, which is in line with higher labor market attachment among men

than women. For instance, 79% of male workers report employer sponsored health insurance

offer compared to 76% of female workers. In addition, men report higher hourly wages ($22.08

vs $16.68) and likelihood of working fulltime (89.7% vs 72.4%). The lagged mean number of

high cost mandates in place at school-leaving is similar for both sexes. Demographics are also

broadly similar for both men and women, and are comparable to an older sample such as the

NLSY79. For example, the sample is less racially and ethnically diverse, and has lower

educational attainment, relative to the current U.S. population.

5.3 Life course effects of high costs mandates on ESI and labor market outcomes

Table 4 reports estimates of the effect of high cost mandates at labor market entrance on

employer-sponsored health insurance, wages, and labor supply. A full set of coefficient

estimates is available on request.

We find no statistically significant effect of high cost mandates on the probability that an

employer offers health insurance to either men or women. However, male and female workers

incur an initial wage penalty of 4.3% and 3.9% respectively for each additional high cost

mandate. While these effects dissipate with time in the labor market, they are observable for 13

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[30] An overall count of the number of mandates in effect treats high cost mandates (such as those we study here) and low cost mandates equally. If high cost mandates do indeed, as we document here, have an impact on outcomes then averaging these high cost mandates with low cost mandates can attenuate effects.

years among men and 18 years among women after entering the job market.[31] Passage of a high

cost mandate leads to a reduction in the number of weeks worked among men, but not women.

More specifically, men who enter a labor market with an additional high cost mandate work

1.6% fewer weeks per year than otherwise comparable men and this disparity is observable for

roughly 13 years. In terms of fulltime employment, we find that entering a labor market with

high cost mandates reduces the propensity of working fulltime among both men and women, but

these effects are not permanent. Indeed, an additional high cost mandate at labor market

entrance reduces the probability of fulltime work by 2.4 percentage points (2.6%) among men

and 2.7 percentage points (3.8%) among women. These effects are observable approximately 13

years for men and 34 years for women after labor market entrance.

5.4 The importance of employer size at labor market entrance

      We next separately estimate Equation (1) for those workers who started their careers

(i.e., their first job after school-leaving) with a small employer and those workers who started

their careers with a large employer. We expect that the mandate effects will be stronger for

those individuals who began their careers with small employers (Kaestner and Simon, 2002).

Although the previous literature has used contemporaneous employer size in analyses of

mandate effects, because we are examining the importance of mandates at school-leaving/labor

market entry we argue that it is the employer size at labor market entrance that is relevant here.

      A limitation of the NLSY79 data is that employer size is not available between 1981 and

1985. These years are important as many workers in our sample entered the labor market in the

early 1980s (see Appendix Table A). Moreover, we lack data on employer size before the

NLSY79 survey commences, and many workers in our sample also entered the labor market in

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[31]We calculate the number of years at which the effects become zero by taking the derivative of Equation (1) with respect to M_{st}, setting the derivative to zero, and solving for PE_{ig-t}.

the 1970s (see Appendix Table A). Thus, this limitation of the NLSY79 prevents us from accurately identifying the sample of firms for whom the mandates bind.

We use information on employer size available in 1979 to impute employer size in the first job for individuals who entered the labor market between 1973 and 1978. Thus, we implicitly assume that employer size remains constant across these years. Next, we use information on employer size in 1980 to impute employer size for the workers who entered the labor market between 1981 and 1985.[32] For workers who entered the labor market in other years (i.e., 1979, 1980, and 1986-1990) we use the employer size information from the labor market entrance year. We stratify workers in the following manner: employer size at labor market entrance less than or equal to 100 workers vs. more than 100 workers.

This exercise potentially leads to a substantial degree of measurement error. Moreover, if employer size at labor market entrance is endogenous to the number of high cost mandates in place —for example, if mandates impact the propensity that a worker's first job is with a small employer (Bailey and Webber, 2016) —then we may be stratifying our sample on an endogenous variable which can lead to bias. For these reasons, we interpret findings from this analysis cautiously and encourage readers to do the same.

Table 5A reports results for men and Table 5B reports results for women. The findings for men are in line with the hypothesis that mandates should have larger effects among workers who start their careers working for smaller employers. In fact there are wrong-signed (positive) coefficient estimates for the sample of workers who start careers with large employers. For the female sample, we find evidence that high cost mandates reduce the probability of working fulltime in the small employer samples. The coefficient estimates in the large employer sample are not statistically different from zero. Overall, the results for women are also in line with the

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[32] If employer size is missing in 1979, we use the 1980 value (if non-missing) and vice-versa.

hypothesis that mandates are more binding for small employers than for large employers.

6. Robustness checks and extensions

We next report results from several robustness checks to examine the stability of our findings to different modeling approaches. We also explore extensions to the main model.

6.1 Accounting for differences in mandate costs

In the main analyses we use an unweighted count of the number of high cost mandates in the state/year of labor market entrance. However, it is plausible that the five mandates we study impose different costs on employers and/or are differently valued by workers. To explore this possibility to some extent, we follow a weighting scheme developed by Gruber (1994b) to account for differences in mandate cost (Gruber's weighting scheme, based on his analysis of the specific mandate costs, upweights more costly mandates and downweights less costly mandates). We construct the weighted number of mandates in the state/year of labor market entrance with the following equation:

(3) $\quad WM_{st} = Alcohol_{st} + Drug_{st} + 5 * Mental_{st} + 1.5 * Chiropractor_{st} + 3 * Con\ Cov_{st}$

where $Alcohol_{st}$, $Drug_{st}$, $Mental_{st}$, $Chiropractor_{st}$, and $Con\ Cov_{st}$ are alcohol treatment, illicit drug treatment, mental health treatment, access to a chiropractor, and continuing coverage mandates respectively. Thus, this variable up-weights mental health treatment, chiropractor, and continuing coverage mandates relative to alcohol treatment and illicit drug treatment mandates to account for differences in costs. Results are reported in Appendix Table B and are broadly similar to those generated in Equation (1), which equally weights the mandates we study.

6.2 The importance of worker skill

We next estimate separate regressions for workers of different skill levels. More specifically, we focus on workers who left school with a high school diploma or less ('lesser

skill workers') and some college education ('higher skill workers').[33, 34] *Ex ante*, it is not clear

whether we should expect stronger mandate effects for those lesser or higher skill workers. On

the one hand, higher skill workers have higher attachment to the labor market and thus adverse

labor demand shocks may have larger implications for their life course employment outcomes.

Alternatively, lesser skill workers may work for employers who are more likely to pass on labor

costs to employees. In general lesser skill workers are most adversely affected during recessions

than higher skill workers suggesting that they are more vulnerable to negative contemporaneous

labor demand shocks than higher skill workers (Hoynes et al., 2012). Finally, Buchmueller et al.

(2011) note that theory suggests that the effects of a mandate should be largest for workers who

place a low value on health insurance and thus have lower rates of ESI in the absence of a

mandate, in particular such workers are likely to be of lower skill.

Appendix Tables C1 and C2 present estimates by skill level at school-leaving for the

male and female sample. Our findings suggest that lesser skill workers, especially those who

are female, disproportionately bear the incidence of mandate costs: coefficient estimates are

generally larger and more precisely estimated in the sample of lesser skill workers.

6.3 Heterogeneity across mandates

We also estimate Equation (1) entering one mandate at a time into the regression model.

The purpose of this exercise is to assess whether there are differences in the relationship

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[33] We classify those workers who left school with 12 years or less as 'lesser skill workers' and those workers who left school with some college (but less than a college degree), a college degree, or a graduate degree as 'high skill workers'.

[34] In unreported analyses, we also explore heterogeneity across union status at school-leaving. We hypothesize that unionized jobs may offer some protections against the negative impacts of private health insurance mandates on labor market outcomes. Information on union status is collected in all years of the NLSY79, but we do not have information on union status in years before 1979. Thus, similar to our firm size variable, we assign the 1979 value to school-leaving years 1973 to 1978. We find, although not fully consistent across samples, that workers whose first job is a unionized job are less impacted by high cost mandates than those workers whose first job is non-unionized. These findings suggest that unionized workers are protected from market factors such as mandates. However, we cannot discriminate between this hypothesis and the hypothesis that unionized workers are more likely to work for self-insured employers who are not bound by ERISA (Acs et al., 1996).

between the mandates and our outcomes. Results are available on request. We chose not to

include all mandates in the same regression as collinearity between the mandates may impede

our ability to precisely estimate treatment effects (Gruber, 1994b, Meer and West, 2011).

Although one may be concerned that excluding mandates may lead to omitted variable bias, our

inclusion of labor market entrance state-specific linear time trends (which, in a specific and

parametric manner, account for unobservable/excluded variables at the state level) should

minimize such concerns. We find that there is heterogeneity in the relationship between the

mandates that we study here and our outcomes. For example, when we use the count of high

cost mandates in place in our key specification, we find no statistically significant evidence that

mandates impact the probability that a worker receives an offer of health insurance from his

employer or the number of weeks worked in the past year, but when studying the mandates

separately we find that chiropractor mandates reduces the probability that a worker is offered

ESI. Moreover, the wage effects we identify in our main findings appear to be drive by alcohol

treatment and continuing coverage mandates. We find some evidence that a continuing

coverage mandate in place at labor market entrance increases the number of weeks worked.[35]

7. Discussion

The debate over the relative merits and demerits of mandated benefits on access to

equitable and affordable health insurance and labor market outcomes is both long-standing and

contentious. While we do not propose that our study can provide consensus on the broader

welfare impacts of mandated benefits, we are able to offer new insight on mandate effects.

Specifically, we are the first study, to the best of our knowledge, to explore the persistent effects

of high cost mandates on new labor market entrants' access to employer-sponsored health

insurance (ESI), wages, and labor supply. Four central findings emerge from our analysis. First,

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[35] The predominately null findings for continuing coverage may be due to COBRA as noted earlier in the
manuscript.

25

we find that high cost mandates have no discernable effects on access to ESI. Second, mandates at labor market entrance reduce wages and labor supply as measured by weeks worked and the probability of holding a fulltime job. Third, we find that these effects are persistent, but not permanent as they dissipate with time spent in the labor market. Fourth, the findings are concentrated among workers who began their career with small (<100) employers and lesser skill workers. Thus, while the previous literature on health insurance mandates, and mandated benefits more broadly, has focused only on contemporaneous effects, we document that these mandates lead to persistent distortions in labor market outcomes for some workers.

Our study, while novel in many ways, is not without limitations. First, we focus on older cohorts of workers and five specific health insurance mandates. Therefore, the generalizability of our findings to different cohorts and mandates is not clear. Second, we lack information on the generosity of ESI held by respondents. For instance, employers can pass on the cost of mandates by increasing premiums and or copays, or by offering insurance coverage that is less generous along other dimensions. Our research is silent on this possible channel of adjustment.

Keeping the above-noted limitations in mind, our findings may be useful for thinking through health policy recommendations that extends employers' responsibility for health insurance. Our estimates suggests that that any law that increases the cost of health insurance for employers may lead to distortions of wages and labor supply, and that these distortions will be experienced by current workers as well as those entering the labor market. Whether the trade-offs between generous health insurance, wages, and labor supply these workers may experience is welfare-enhancing is not clear. At minimum, policy makers should consider the downstream and perhaps unintended consequences of these regulations.

Table 1. State private health insurance high cost mandate effective dates

State	Alcohol treatment mandate	Illicit drug treatment mandate	Mental health treatment mandate	Chiropractor mandate	Continuing coverage mandate
AK	**1989**	**1989**			
AL				**1984**	
AR				**1975**	
AZ				1971	**1979**
CA			**1976**	**1983**	
CO			1971	1969	**1985**
CT	**1977**			**1975**	**1986**
DE				**1989**	1975
FL				1963	
GA				**1974**	
IA				**1980**	**1986**
ID					**1984**
IL	1972				
IN			**1978**	1969	**1984**
KS	**1978**	**1978**		**1974**	
KY				**1973**	**1978**
LA			**1976**	**1986**	**1980**
MA	**1976**		**1973**	**1975**	
MD	**1981**	**1979**	**1984**	**1985**	**1977**
ME	**1984**	**1984**		**1974**	**1983**
MI	**1982**	**1982**		**1981**	
MN	**1978**	**1978**		1968	
MO	**1977**			**1973**	**1974**
MS	**1975**		**1984**	**1976**	**1985**
MT	**1984**	**1984**		**1980**	
NC			**1985**	1967	
ND	**1985**	**1985**		**1977**	**1982**
NE			**1976**		**1983**
NH				**1975**	**1981**
NJ	**1977**			1969	**1981**
NM					
NV	**1985**	**1985**		**1973**	**1983**
NY	**1981**	**1988**	**1979**	**1975**	**1988**
OH	**1979**			**1973**	
OK			**1984**	1969	**1984**
OR	**1984**	**1984**		1971	**1976**
PA	**1986**				**1981**
RI	**1980**	**1988**		1971	
SC				1968	**1977**
SD					**1979**
TN				1970	**1984**
TX	**1986**	1990		**1981**	**1981**
UT			**1977**	**1977**	**1986**
VA	**1978**	**1978**		**1975**	**1986**
VT	**1986**			**1973**	**1986**
WA	**1975**	**1975**	1971		
WI	**1975**	**1975**		1971	
WY				**1988**	**1980**

Notes: Bold text indicates changes that occur during study period 1972 to 1989 (mandates are lagged one year in regression models). Data source: Gruber (1994b).

Table 2. Contemporaneous effect of high cost mandates on ESI access and labor market outcomes: Current Population Survey 1976-1990

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
Men				
Baseline proportion/mean	0.724	21.63	45.73	0.902
High cost mandates	-0.001	-0.007*	-0.003	-0.003**
	(0.003)	(0.004)	(0.002)	(0.001)
Unweighted observations	361,684	468,965	511,489	511,489
Women				
Baseline proportion/mean	0.589	14.75494	41.84	0.709
High cost mandates	-0.002	-0.006*	-0.004	-0.004**
	(0.004)	(0.003)	(0.002)	(0.002)
Unweighted observations	299,869	411,717	437,220	437,220

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, state fixed effects, year fixed effects, and state-specific linear time trends. CPS sample weights applied. Standard errors are clustered around the state and reported in parentheses.
[1]ESI=Hold employer-sponsored health insurance. This variable is only available 1980-1990.
***;**;*=statistically different from zero at the 1%; 5%;10% level.

Table 3. Summary statistics: NLSY79 1979-2012 sample

Sample:	Men	Women
Outcome Variables		
ESI†	0.790	0.761
Hourly wage	22.08	16.68
Weeks worked, past year	46.11	43.95
Work fulltime, past year	0.897	0.724
Private health insurance mandates		
Lagged high cost mandates at labor market entrance	1.439	1.414
Demographics		
Age at school-leaving	18.78	18.60
White	0.818	0.817
African American	0.129	0.132
Hispanic	0.0524	0.0511
Less than high school at labor market entrance	0.177	0.132
High school at labor market entrance	0.493	0.493
Some college at labor market entrance	0.149	0.192
College degree at labor market entrance	0.181	0.183
Age-adjusted AFQT score	-0.0383	-0.0366
Mother's education	11.72	11.63
Father's education	11.90	11.76
Rural residence at age 14	0.232	0.226
Live with both biological parents at age 14	0.768	0.757
Library card in the home at age 14	0.739	0.784
Magazines in the home at age 14	0.679	0.670
Newspapers in the home at age 14	0.854	0.840
Labor market entrance year	1979.9	1979.7
Survey year	1992.7	1992.8
School-leaving state level characteristics		
Unemployment rate	0.0479	0.0477
Poverty rate	0.124	0.125
Some college education rate	0.282	0.280
Age	33.17	33.14
Private wage-earning worker rate	0.751	0.750
Unweighted observations	85,616	82,911

Notes: Sample includes observations that provide a valid response to one of the four outcome variables, thus this sample departs from the sample sizes in the regression tables. NLSY79 weights applied.
†ESI=Employer-sponsored health insurance offer.

Table 4. The persistent effect of high cost mandates at school-leaving on ESI access and labor market outcomes: NLSY79 1979-2012

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
Male sample				
Baseline proportion/mean	0.790	22.08	46.11	0.897
High cost mandates	-0.0058	-0.0428**	-0.0161*	-0.0235***
	(0.0105)	(0.0207)	(0.0094)	(0.0034)
High cost mandates*	0.0004	0.0033***	0.0012***	0.0018***
potential experience	(0.0003)	(0.0008)	(0.0004)	(0.0003)
Unweighted observations	68,464	79,806	83,489	82,179
Female sample				
Baseline proportion/mean	0.761	16.68	43.95	0.724
High cost mandates	-0.0126	-0.0391***	0.0026	-0.0272***
	(0.0092)	(0.0126)	(0.0112)	(0.0078)
High cost mandates*	0.0005	0.0022***	-0.0003	0.0008*
potential experience	(0.0004)	(0.0005)	(0.0003)	(0.0004)
Unweighted observations	64,163	76,596	80,012	78,855

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, labor market entrance state fixed effects, labor market entrance year fixed effects, and labor market entrance state-specific linear time trends. NLSY79 sample weights applied. Standard errors are clustered around the labor market entrance state and reported in parentheses.
[1]ESI=Employer-sponsored health insurance offer.
***;**;*=statistically different from zero at the 1%; 5%;10% level.

Table 5A. The persistent effect of high cost mandates at school-leaving on ESI access and labor market outcomes among men in the NLSY79 1979-2012 sample: Employer size at school-leaving

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
≤100 employees				
Baseline proportion/mean	0.798	22.92	47.43	0.905
High cost mandates	-0.0185	-0.0881***	-0.0246**	-0.0373***
	(0.0130)	(0.0294)	(0.0110)	(0.0080)
High cost mandates*	0.0007	0.0040***	0.0012***	0.0023***
potential experience	(0.0005)	(0.0009)	(0.0003)	(0.0004)
Unweighted observations	25,192	29,116	30,274	29,800
>100 employees				
Baseline proportion/mean	0.864	23.49	47.54	0.940
High cost mandates	0.0143	-0.0379	0.0614***	0.0037
	(0.0233)	(0.0463)	(0.0114)	(0.0161)
High cost mandates*	0.0012	0.0035**	-0.0000	0.0011*
potential experience	(0.0008)	(0.0017)	(0.0004)	(0.0006)
Unweighted observations	7,124	7,867	8,086	7,988

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, labor market entrance state fixed effects, labor market entrance year fixed effects, and labor market entrance state-specific linear time trends. NLSY79 sample weights applied. Standard errors are clustered around the labor market entrance state and reported in parentheses. ***;**;*=statistically different from zero at the 1%;5%;10% level.

[1]ESI=Employer-sponsored health insurance offer.

Table 5B. The persistent effect of high cost mandates at school-leaving on ESI access and labor market outcomes among women in the NLSY79 1979-2012 sample: Employer size at school-leaving

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
≤100 employees				
Baseline proportion/mean	0.759	17.07	45.05	0.718
High cost mandates	0.0010	-0.0455	0.0052	-0.0359***
	(0.0183)	(0.0322)	(0.0229)	(0.0130)
High cost mandates*	-0.0001	0.0025***	-0.0003	0.0010
potential experience	(0.0006)	(0.0008)	(0.0005)	(0.0006)
Unweighted observations	20,006	23,880	24,773	24,408
>100 employees				
Baseline proportion/mean	0.835	19.06	46.62	0.765
High cost mandates	-0.0234	-0.0517	-0.0108	-0.0601
	(0.0305)	(0.0491)	(0.0258)	(0.0366)
High cost mandates*	0.0009	0.0050***	0.0002	0.0011
potential experience	(0.0009)	(0.0013)	(0.0006)	(0.0010)
Unweighted observations	7,220	8,293	8,583	8,455

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, labor market entrance state fixed effects, labor market entrance year fixed effects, and labor market entrance state-specific linear time trends. NLSY79 sample weights applied. Standard errors are clustered around the labor market entrance state and reported in parentheses.
***;**;*=statistically different from zero at the 1%;5%;10% level.
[1]ESI=Employer-sponsored health insurance offer.

Appendix Table A. Labor market entrance cohort size: NLSY79 1979-2012 sample

School-leaving year	Number of labor market entrants (unweighted)	Total number of labor market entrants (weighted)
1973	88	181,518
1974	165	403,624
1975	601	1,519,011
1976	988	2,410,380
1977	1,164	2,817,389
1978	1,423	3,572,607
1979	1,441	3,809,763
1980	1,189	3,134,488
1981	1,136	3,209,784
1982	1,023	3,077,539
1983	632	2,009,049
1984	404	1,309,966
1985	270	922,537
1986	203	802,908
1987	123	405,266
1988	76	250,557
1989	58	174,093
1990	29	9,5623
Total	11,013	30,106,099

Notes: One observation per respondent that have a valid response to at least one of the outcome variables.

Appendix B. The persistent effect of high cost mandates at school-leaving on ESI access and labor market outcomes, NLSY79 1979-2012 sample: Weighted count of high cost mandates

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
Male sample				
Baseline proportion/mean	0.790	22.08	46.11	0.897
High cost mandates	0.0007	-0.0073	-0.0024	-0.0069***
	(0.0046)	(0.0087)	(0.0031)	(0.0018)
High cost mandates*	-0.0000	0.0010***	0.0004**	0.0006***
potential experience	(0.0001)	(0.0003)	(0.0002)	(0.0001)
Unweighted observations	68,464	79,806	83,489	82,179
Female sample				
Baseline proportion/mean	0.761	16.68	43.95	0.724
High cost mandates	-0.0023	-0.0152***	0.0029	-0.0082**
	(0.0042)	(0.0050)	(0.0050)	(0.0041)
High cost mandates*	0.0002	0.0009***	-0.0002	0.0003
potential experience	(0.0002)	(0.0002)	(0.0001)	(0.0002)
Unweighted observations	64,163	76,596	80,012	78,855

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, labor market entrance state fixed effects, labor market entrance year fixed effects, and labor market entrance state-specific linear time trends. NLSY79 sample weights applied. Standard errors are clustered around the labor market entrance state and reported in parentheses. The weighted mandate count is calculated following Gruber (1994): alcohol mandate + illicit drug mandate + 5 * mental health mandate + 1.5 *chiropractor mandate + 3 * continuing coverage mandate.
***;**;*=statistically different from zero at the 1%;5%;10% level.
[1]ESI=Employer-sponsored health insurance offer.

Appendix Table C1. Effect of high cost mandates at school-leaving on labor market outcomes among men: Worker skill at school-leaving

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
Lesser skill[2]				
Baseline proportion/mean	0.752	18.50	45.39	0.899
High cost mandates	-0.0138	-0.0378*	-0.0160	-0.0182***
	(0.0157)	(0.0218)	(0.0111)	(0.0056)
High cost mandates*	0.0006	0.0021***	0.0009*	0.0017***
potential experience	(0.0005)	(0.0006)	(0.0005)	(0.0003)
Unweighted observations	48,413	57,532	60,000	59,036
Higher skill[3]				
Baseline proportion/mean	0.841	25.00	46.64	0.805
High cost mandates	0.0232	0.0275	-0.0157	-0.0221**
	(0.0188)	(0.0462)	(0.0135)	(0.0097)
High cost mandates*	0.0006	0.0020*	0.0018***	0.0014***
potential experience	(0.0005)	(0.0011)	(0.0006)	(0.0004)
Unweighted observations	20,051	22,274	23,489	23,143

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, birth state fixed effects, school-leaving year fixed effects, and birth state-specific linear time trends. NLSY79 sample weights applied. Standard errors are clustered around the birth state and reported in parentheses.

***;**;*=statistically different from zero at the 1%;5%;10% level.

[1]ESI=Employer-sponsored health insurance offer.

[2]Lesser skill =A high school diploma or less at school-leaving.

[3]High skill =Some college at school-leaving.

Appendix Table C2. Effect of high cost mandates at school-leaving on labor market outcomes among women: Worker skill at school-leaving

Outcome:	ESI[1]	Log(wages)	Log(weeks)	Fulltime
Lesser skill[2]				
Baseline proportion/mean	0.725	14.07	42.81	0.723
High cost mandates	-0.0306**	-0.0544***	-0.0098	-0.0356***
	(0.0129)	(0.0129)	(0.0163)	(0.0091)
High cost mandates*	0.0005	0.0022***	-0.0002	0.0009**
potential experience	(0.0005)	(0.0005)	(0.0005)	(0.0004)
Unweighted observations	40,760	49,647	51,634	50,892
Higher skill[3]				
Baseline proportion/mean	0.863	29.36	47.55	0.891
High cost mandates	-0.0082	-0.0227	0.0043	-0.0242
	(0.0188)	(0.0298)	(0.0155)	(0.0249)
High cost mandates*	0.0009*	0.0017	0.0001	0.0010
potential experience	(0.0005)	(0.0011)	(0.0005)	(0.0007)
Unweighted observations	23,403	26,949	28,378	27,963

Notes: All models estimated with least squares (continuous variables) or a linear probability model (binary variables), and control for demographics, birth state fixed effects, school-leaving year fixed effects, and birth state-specific linear time trends. NLSY79 sample weights applied. Standard errors are clustered around the birth state and reported in parentheses.
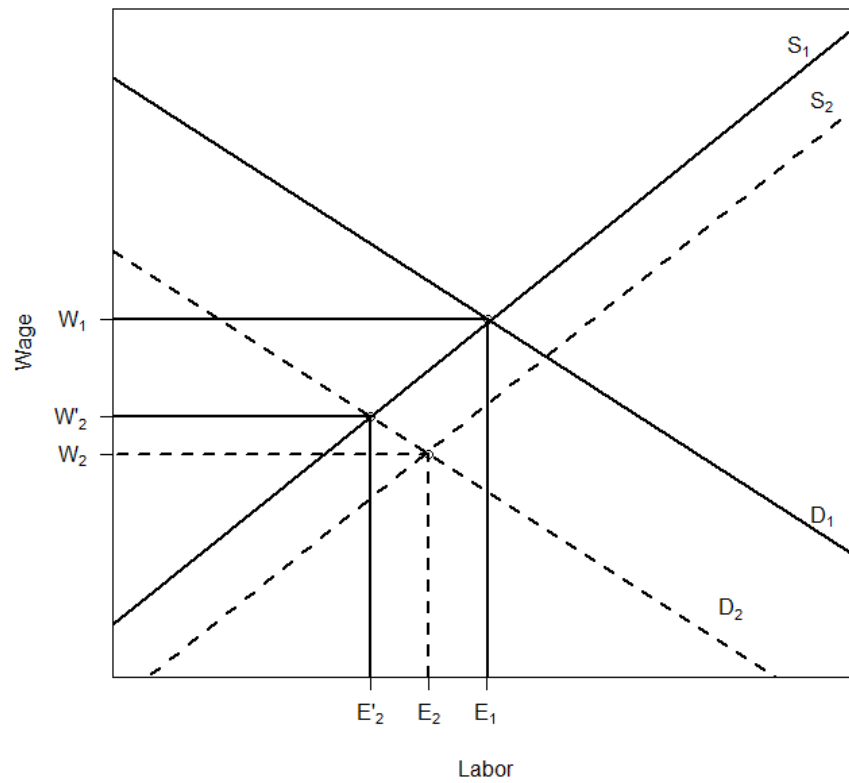
***;**;*=statistically different from zero at the 1%;5%;10% level.

[1]ESI=Employer-sponsored health insurance offer.

[2]Lesser skill =A high school diploma or less at school-leaving.

[3]High skill =Some college at school-leaving.

Figure 1. The effect of mandated benefits on wages and employment

Notes: Figure based on Summers (1989). The magnitude of the shifts in the demand and supply curves are arbitrarily chosen and are for illustrative purposes only.

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