Identifying the effects of health insurance mandates on small business employment and pay*

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Abstract

The cost of employee health insurance is steadily increasing, due in part to state mandates that require insurance policies to cover additional medical treatments and services. A prevalent empirical finding is that these mandates have uncertain labor market effects, as most results are statistically insignificant. We determine several reasons for the imprecision in this area of research: small sample bias, heterogeneity in the effects of mandates, and collinearity of mandate legislation. Empirically addressing these factors with population-level data on small business employment, we identify significant employment effects for several mandates and illustrate how pervasively these issues plague identification.

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1 Introduction

Empirical research on state health insurance mandates frequently shows a statistically insignificant relationship between mandates and employment outcomes (see, for instance, Gruber [1994b], Kaestner and Simon [2002], Cseh [2008]). Lack of decisive evidence on the effects of mandates becomes especially frustrating in the face of large increases in both health care utilization and health care costs.[1] We determine several reasons for the prevalence of statistical insignificance in this area of research: sampling bias caused by relatively small samples of individuals, heterogeneity in the effects of different mandates, and collinearity of mandate adoption at the state level. Using population-level data on small business employment, we empirically mitigate these complications and assess the employment effects of several mandates. We find that mandates differ substantially in their effects, but that overall they are associated with decreases in small business employment and pay. Although most of the point estimates are non-trivial in magnitude, state-clustered standard errors are large enough that the confidence intervals for many of our estimates overlap zero. Because the impact of a mandate is likely to be dispersed through multiple channels, we view this as exemplifying the immensity of the challenge to pin down labor market effects of a mandate when considering only partial equilibrium outcomes.

According to the Council for Affordable Health Insurance, state laws included 2,156 health insurance mandates in 2010, a figure representing 25-50 new mandates added annually nationwide [Bunce and Wieske, 2010]. Some mandates deal with discrimination or continuation of health insurance coverage, but the vast majority require insurance companies to provide or offer coverage for specific medical treatments or services. State legislatures find mandates attractive because they push the cost of insuring the uninsured (individuals or conditions) off of government budgets [Feldman, 1993]. There is also a paternalistic justification for insurance mandates, as employees may not accurately account for their risk of developing new health conditions [Summers, 1989]. More recently, proponents argue that mandates for additional medical services may reduce overall costs by encouraging preventative care [Andrews, 2009a,b]. For instance, regular chiropractic service may reduce the need for later surgery, or early onset diabetes treatment may prevent necessarily expensive amputations and other surgery (although Huang et al. [2009] provide some clinical evidence discounting this argument).

Although the political motivation for insurance mandates seems straightforward, their economic impact is much less clear. For many conditions (e.g. mental health illnesses), mandating insurance coverage can correct for inefficiencies caused by adverse selection [Summers, 1989]. This correction is not costless, Feldman [1993] cautions, as mandates are distortionary

[1]Health benefits account for 7.6% of all private industry employee compensation as of June 2011, costing employers over a tenth as much as salary and wages [United States Bureau of Labor Statistics, 2011b]. Per the Kaiser Family Foundation, the average annual health insurance premium cost for covered workers with family coverage at small firms has more than doubled in the past decade from $6,959 in 2001 to $14,098 in 2011 [The Kaiser Family Foundation and Health Research and Educational Trust, 2011].

and increase inefficiencies elsewhere. Because medical coverage is a form of non-pecuniary income, Feldman [1993] claims mandates may increase total compensation for low-wage employees, but warns against the belief that employers fully bear the additional cost of a mandate. Furthermore, he notes that employees may not always value mandates at their cost, so prohibiting minimal cost health insurance coverage is likely to have significant labor market consequences. These repercussions could hypothetically take the form of reductions in health insurance coverage [Gruber, 1994b], wages [Kaestner and Simon, 2002], employment [Wolaver et al., 2003], or employment quality [Cseh, 2008].

Cutler and Madrian [1998] maintain that "increases in the cost of providing health insurance must have some effect on labor markets, either in lower wages, changes in the composition of employment, or both." Empirically, they find increased health insurance costs translate into significant increases in reported hours worked by employees in both the March Current Population Survey (CPS) and Survey of Income and Program Participation (SIPP). Employers, they argue, respond to rising insurance costs by increasing the hours demanded of current employees in lieu of hiring new employees (and current employees may consent to this arrangement because of the close link between employment and health insurance [Holtz-Eakin et al., 1996]). This theory of employment shifting is only weakly supported by research examining small business hiring decisions. For instance, Mathur [2010] finds that the probability of a business-owning individual in the SIPP employing another worker changes inversely with the number of state health insurance mandates, but Kapur et al. [2008] analyze the Medical Expenditure Panel Survey (MEPS) and find that—although small businesses are less likely to hire individuals with high expected health costs—hiring decisions appear to be invariant to state small group health insurance reforms.

Empirical estimates are also mixed for the impact of health insurance mandates on health insurance coverage. After computing the additional insurance cost for a set of mandates, Gruber [1994b] uses within-state estimation across several years of the CPS and finds that five of the most "costly" mandates have an insignificant effect on rates of health insurance coverage at firms employing fewer than 100 workers.[2] In contrast, Jensen and Gabel [1992] study survey data for small firms' decisions to offer health insurance and find a large portion of noncoverage is attributable to state mandates.

Broadening the research on non-insurance outcomes of mandates, Kaestner and Simon [2002] study the impact of health insurance mandates on employment and wages in the March CPS. Although they find some evidence of increased hours and decreased wages for employees at small firms, most of their results show no statistically significant effect from mandates. Also using the CPS, Wolaver et al. [2003] examine non-discrimination laws for

[2] Larger firms commonly self-insure, exempting them from following state health insurance mandates. Although there is evidence that self-insured firms often voluntarily meet mandated requirements, most empirical studies focus on how mandates affect small businesses [Jensen et al., 1995; Acs et al., 1996; United States General Accounting Office, 1996].

employer-provided health insurance (EPHI) and find that for every 9.4 low-wage workers who gain insurance coverage, one loses employment. Cseh [2008] studies a broad set of employment and insurance outcomes specifically for mental health parity mandates. Using an extensive set of subsamples of small firm workers in the CPS, he finds no statistically significant effects from state mental health parity laws.

Given the inconclusive evidence on the effects of mandates, Kaestner and Simon [2002] understandably assert that a "consensus has not been reached" on their efficiency.[3] Although a few aforementioned studies find significant results, most empirical research on health insurance mandates shows non-trivial (generally negative) point estimates of their effects, but with large standard errors.[4] In the next section, we discuss several factors that have prevented clean identification of the impact of health insurance mandates. We then empirically assess the employment effects of nine costly mandates. Despite addressing the troublesome factors, we find many of the results to be statistically insignificant at conventional levels, although the point estimates are of sizable magnitude. We view our findings as evidence that health insurance mandates may negatively impact small business employment, but that they differ significantly in their effects. More importantly, we view our results as indicative of the difficulty in quantifying the impact of mandates, even when properly addressing some of the challenges inherent in estimation.

The remainder of this article is organized as follows. In Section 2 we discuss several factors complicating identification of the effects of mandates, and in Section 3 we outline our empirical approach. Section 4 describes the data we use and Section 5 describes our results. We conclude in Section 6.

2 Identification Complications

In this section we discuss three complications of identification in this area of research: sampling bias caused by relatively small samples of individuals, heterogeneity in the effects of different mandates, and collinearity of mandate adoption at the state level. In addition to showing why these factors inhibit statistically significant findings, we outline tactics that may overcome these complications.

Researchers frequently use the March Current Population Survey to study the effects of insurance mandates. Our assertion that the CPS suffers from small sample bias may be surprising, given that the 1989-2008 survey years include on average more than 170,000

[3]It is important to distinguish a near-zero point estimate with very small standard errors from an insignificant non-zero point estimate with large standard errors. The former case would be informative about the relationship between mandates and employment outcomes, whereas the latter is more indicative of the capability of the data used to identify the relationship.

[4]Estimates for mandates that target an identifiable group are a notable exception, perhaps because they can leverage a triple-differencing identification strategy (e.g. Gruber [1994a], Lahey [2011]).

individuals in each March sample. Taken as a whole, the annual sample size is large, but dividing the sample based on only a few discrete variables often leaves small within-cell samples. For instance, conditioning the March CPS only on state and year reduces the median within-cell sample to 2372 individuals (with a mean of 3363). Further sub-sampling to employees of small firms (fewer than 100 workers) drops this median within-cell sample to 566 individuals (with a mean of 773).[5]

Within-cell samples of these magnitudes appear sufficiently large to draw credible population inference from the weighted CPS surveys, but analysis of within-state annual changes in reported small business employment shows this is not the case.[6] Variations in small business employment levels in the unweighted CPS sample are implausibly large; the distribution has tails reflecting more than 50% annual changes in within-state employment. If the sample is weighted to the population level, the distribution appears much more compressed, with most annual changes being smaller than 20%. Contrasted with data in the Statistics of U.S. Businesses (SUSB), a population-level dataset described in Section 4, this variation is still greatly overstated: the largest annual change in any state's small business employment in the SUSB data is about 9%, less than half of that for the weighted CPS. Figure 1 displays a histogram of the annual changes in small business log-employment by state as reported by surveyed individuals in the March CPS. Figure 2 displays the same for CPS data weighted to the population level using the household weights. Finally, Figure 3 presents the histogram for changes in small business log-employment as measured by the SUSB. All three histograms use the same x-scale and a firm size threshold of 100 employees so as to be directly comparable.

This CPS small sampling problem cannot be resolved by simply dropping a few outlier state-year observations. Nearly 98% of the SUSB state-year employment changes fall within a 10% interval around zero. Achieving the same distributional bounds for the weighted CPS data requires dropping more than 38% of the observations. The effects of these outliers carry into estimations. Using SUSB data, most of our point estimates for mandates are less than 1% in magnitude for small business employment and pay, and the largest is under 3% in magnitude. For the same specifications using weighted CPS data, many point estimates are larger than 10% in magnitude, and some are larger than 20%. It is infeasible to credibly identify the causal impact of a mandate (or other policy shock) if there is so much spurious

[5] Within-household correlation further complicates this issue. The March CPS includes anywhere from one to twenty-six unique personal responses to the survey for a single household, so within-household correlation may not be trivial. Failure to cluster standard errors to account for correlation within households and states can greatly alter the apparent significance of results if the variable of interest is fixed across observations, as in the case of a state health insurance mandate. This problem is intensified for data such as the CPS that is not arithmetically weighted. Angrist and Pischke [2008] provide a useful discussion of this issue (p. 308).

[6] The household weights included in the March CPS are computed as the inverse probability of the household being selected for the survey in that year. The Census Bureau intentionally over-samples households of certain demographic characteristics, creating bias in any inference drawn from estimations using arithmetic weighting of these data [King et al., 2010].

variation in the data due to sampling. This small sampling problem is a statistical issue and can avoided by using data such as the SUSB that is more reflective of true changes in small business employment.

Separating the heterogeneous effects of different mandates, on the other hand, is an identification challenge that is not particular to the data used. Health insurance mandates vary along several dimensions. Primarily, mandates range in compliance cost. LaPierre et al. [2009] study the impact of mandates on premiums and find substantial heterogeneity, with some mandates adding large increases to insurance premiums while others cause negligible adjustments. Besides premium cost, mandates differ in whether they are valued at cost, whether they target an identifiable group, and the scope of cost-offsetting responses for the targeted group [Lahey, 2011]. For example, Gruber [1994a] finds mandated maternity care corrects for real adverse selection, affects an identifiable group, and is fully valued at cost. Infertility mandates, which also target an identifiable group, are not valued at cost and cause labor market distortions [Lahey, 2011].

Variation in the scale and scope of effects from different mandates has important implications for identification. In particular, this precludes use of a linear index of mandates or ordinal sets of mandates—both of which are common approaches in this literature (e.g. Kaestner and Simon [2002], Mathur [2010]). The marginal mandate is usually not the same mandate across states, so these types of functional form restrictions bias results downwards in magnitude, a tendency which is illustrated in results from our alternative specification. The individual effects of differing mandates can be identified by instead including each mandate separately in a regression, an approach we employ in our empirical study.

The political component to insurance regulation often results in states simultaneously enacting several health insurance mandates. Given the relatively short time frames studied in this literature—an artifact both of data limitations and the recent boom in mandate legislation—collinearity of mandate adoption may be especially problematic. With only 51 state clusters (including Washington, D.C.), a short panel is unlikely to provide enough variation in mandate legislation to allow for causal inference.[7] This problem is likely resolvable only by including more years of data.

Small sample bias, mandate heterogeneity, and multicollinearity have prevented precise identification of the effects of health insurance mandates on small business employment outcomes. Our empirical study avoids small sample bias through our choice of data and accounts for mandate heterogeneity via our specifications. Despite using a panel that spans

[7] Business cycles during the period we and most researchers in this literature study are overall very expansionary, including ten years of economic growth from 1991 to 2001 that represents the "longest [expansion] in the NBER's chronology" [Branson et al., 1992; Hall et al., 2001]. If a strong macro-economy enables firms to better absorb increases in insurance costs, then the external validity of results from this period is an important matter.

twenty years, we cannot be certain that our estimates are not biased by the collinearity of mandate legislation, but a large majority of the nine particular mandates we consider were not passed simultaneously within a state, which leads us to believe that multicollinearity is unlikely to affect our results.[8]

3 Empirical Strategy

We assess small business employment outcomes associated with several potentially costly health insurance mandates.[9] To identify the employment effects of health insurance mandates we exploit variation in adoption years for mandates across states. Orthogonal to aggregate year effects and fixed state effects, the coefficient for "treating" a state's employment data with a binary mandate term identifies the impact of the mandate.

Building on the work of Gruber [1994b], Ma [2007] analyzes a large set of mandated benefits to compile a measure of the expected cost each mandate adds to insurance plan premiums. Using state reports of insurance expenses for a selection of states, he estimates the additional cost of each mandate and ranks them by their costliness. From this list, we selected the nine most expensive mandated benefits, which Ma [2007] estimates jointly add about 9% to the cost of insurance in a state, ignoring any economies of scope. Five of these underlying medical services overlap with the mandates Gruber [1994b] studies.[10]

As discussed in Section 2, the effects of mandates are likely to be heterogeneous, so we separately estimate the impact of each mandate. Specifically, we examine mandated benefits for alcoholism treatment, drug abuse treatment, general mental health care, mental health parity, chiropractic services, diabetes treatment, prostate cancer screening, temporomandibular joint disorder treatment (TMD), and well-child care.[11] Because mental health parity laws encompass general mental health care provisions, we treat these two mandates as being mutually exclusive in our specifications in order to separately estimate their effects. Figures 4 and 5 present the cumulative number of states with each mandate for the years 1988-2007.

[8]Specifically, states legislated 194 new mandates during 1988-2007 for the nine medical treatments and services we consider. Of these, 126 were passed uniquely within a state-year.

[9]State mandates are categorized either as coverage, provider, offer, or benefit. Coverage mandates determine who must be offered health insurance coverage and when. Provider mandates determine which types of medical practitioners must be included in an insurance plan (e.g. chiropractors). Offer mandates require insurance companies to offer firms a policy covering a particular medical service, whereas benefit mandates require a policy covering the medical service to be made available to employees.

[10]Strictly speaking, mental health parity mandates did not exist in the U.S. for the years Gruber [1994b] examines, and he additionally considers mandates for continuation of insurance coverage, which more recently were supplanted by the Federal Consolidated Omnibus Budget Reconciliation Act (COBRA).

[11]See Bunce and Wieske [2010] for a detailed description of each mandate we consider. We focus exclusively on mandated benefits, ignoring mandated offers. As Gruber [1994b] notes, it is not clear why there would be labor market effects merely from an insurance company *offering* a firm a more expensive plan.

For this set of mandates, we matched small business employment statistics at the state-year level using the legislation date for each mandate.[12] We restrict our analysis to small firms for several reasons. First, small businesses deserve analytical attention in their own right: in 2008, businesses with fewer than 500 employees accounted for 99.7% of all firms (of 5.9 million total firms) and 49.4% of all non-farm employment (of 121 million total paid employees) [United States Census Bureau, 2008]. Second, under the Employee Retirement Income Security Act (ERISA), larger firms can—and often do—self-insure, which exempts them from following state insurance mandates [United States Department of Labor, 2011]. As the decision to self-insure is made directly by firms, there is no clear firm size threshold at which we expect mandates to no longer bind. Kapur et al. [2011] study firm survey data by KPMG Consulting and find evidence that firms close to reform thresholds grow slightly in order to avoid being subject to small group insurance reforms. Taking this as evidence that firms manipulate their size in response to health insurance regulation, we follow existing work in the literature (e.g. Gruber [1994b], Kaestner and Simon [2002], Cseh [2008]) and use a firm size cutoff of 100 employees, which should be well below the size at which most firms would self-insure.[13] Third, we focus on smaller firms because of the attention they receive in the political discourse.[14]

For these firms, separated by size into bins of fewer than twenty employees and between twenty and ninety-nine employees, we use linear regression analysis to identify the effect of health insurance mandates on total employment and employee pay at the state level. Our primary specifications, which we estimate separately by firm size bin, are in the form of Equation 1:

$$\begin{aligned}\ln(Y_{st}) &= \alpha + \textstyle\sum_{k=1}^{9} \beta_k \cdot \text{mandate}_{kst} + \sum_{k=1}^{9} \gamma_k \cdot \text{exemption}_{kst} \cdot \text{mandate}_{kst} \\ &+\delta \cdot \text{minimum wage}_{st} + \mu_s + \tau_t + \theta_s \cdot t + u_{st}\end{aligned} \tag{1}$$

Here, Y_{st} is an employment outcome (total employment, total pay, or average pay) for a state-year bin, the mandate terms are binary indicators for whether state s had mandate k in year t, and the exemption terms are binary indicators for whether mandate k in state s in year t included an exemption based on firm size for all firms in the bin. Pay and the state minimum wage are measured in constant dollars (indexed to 2007 using the CPI-Urban

[12] The date of legislation, rather than the date a law actually takes effect, is conventionally used in this literature under an assumption that people begin adapting their behavior in advance of the new law. Often, there is little lag between the legislation date and effective date.

[13] Under the Americans with Disabilities Act (ADA), very small firms are more likely—and even legally allowed—to screen new employees on the basis of pre-existing health conditions [United States Equal Employment Opportunity Commission, 2011; Kapur, 2004; Kapur et al., 2008]. If the employee composition at very small firms tends to be healthier relative to larger firms, this selection issue will provide bias towards zero for the impact of mandates on small business employment.

[14] For example, a recent *Forbes* article quips: "And the painful cost of health care and its growth vector are starkly clear: many employers, especially smaller ones, find a commitment to paying most of employee health costs unbearable in both amount and uncertainty" [Hixon, 2011].

[United States Bureau of Labor Statistics, 2011a]). Additionally, we allow for state (μ_s) and year (τ_t) fixed effects, state-specific linear trends ($\theta_s \cdot t$), and we allow the stochastic disturbance term u_{st} to be correlated by state across years.

We implement a fixed effects (within-state) approach to estimate the parameters for Equation 1. The interpretation of a coefficient for mandate k is thus the percent change in the employment outcome that is attributable to the mandate. The flexible form we specify allows each mandate to have a differing effect on employment outcomes. We include firm size exemption terms in some of our reported results because these exemptions prevent a mandate from being legally binding in a state, although the mandate may still bind de facto. Acs et al. [1996] find that (large) self-insured firms frequently provide mandated health insurance benefits despite being legally exempt, and small firms may behave likewise. As an alternate method of including firm size exemption information (results not reported in this paper), we set mandate k equal to zero if an exemption is present that affects all firms in the bin. Point estimates and standard errors are very similar across both methods. We prefer the approach in Equation 1 because it is less restrictive than setting mandates equal to zero, but in either case firm size exemptions appear to affect employment outcomes. As Cseh [2008] notes, results from estimations that ignore exemption terms "might be confounded."

We include each state's legal minimum wage because it may prevent firms from adjusting the pay for low-wage workers in response to a mandate [Kapur, 2003; Baicker and Levy, 2008]. Although the year fixed effects account for changes in the Federal minimum wage, many states require firms to pay a wage greater than the Federal standard, and there is significant across- and within-state variation [United States Department of Labor, 2010].

Our use of aggregate-level state data is unconventional in this literature, but we believe it is acceptable for several reasons. Variation in mandates occurs at the state level, so using aggregate data circumvents the need to adjust standard errors in order to account for correlated shocks across individuals within a state. Furthermore, using aggregate data still enables us to recover estimates of the local average treatment effects (LATE) of mandates that are identical to those that would be obtained from direct use of the micro data [Angrist and Pischke, 2008, p. 34-41, 227-243]. Finally, as discussed in Section 2, using population-level data avoids the small sample bias present in the CPS and other surveys.

In addition to estimating how mandates affect employment and average pay, we examine effects on total pay in order to simultaneously assess both the extensive and intensive margins of employee pay. If an insurance mandate increases the value—and cost—of non-pecuniary income awarded to employees, then firms may react either by laying off (or hiring fewer) workers, or by maintaining the same workforce but lowering pay, or both [Feldman, 1993; Lahey, 2011]. If firms only reduce employee pay, then a mandate will cause average pay to decline. In contrast, if firms react to a mandate by laying off (or simply hiring fewer) lower-paid workers, whose optimal compensations are effectively corner solutions, then a mandate will actually increase average pay despite its cost to firms. We see no good reason to think that a mandate should affect average pay through only one of these channels, so we examine both possibilities using total pay and total employment.[15]

4 Data

4.1 Statistics of U.S. Businesses

As discussed in Section 2, the March CPS and most other employment surveys provide inadequate sampling to credibly assess the effects of state insurance mandates on small business employment. In light of this, we analyze the Statistics of U.S. Businesses (SUSB) employment data provided by the U.S. Small Business Administration (SBA).[16] To our knowledge, no health insurance mandates research to date uses the SUSB for outcome data.

Unlike the CPS and other household- or individual-level surveys, the SUSB data are not a sample. In conjunction with the Census Bureau, the SBA uses firms' Internal Revenue Service (IRS) tax filings to total annual firm counts, employment, and payrolls for a selection of firm size bins at the state level each March.[17] We use non-farm employment data from 1988-2007 for all fifty U.S. states as well as the District of Columbia, giving us a total of 1020 state-year observations.

The SUSB data offer two strengths. First, employment and payroll data directly reported by firms to the IRS for tax purposes are likely to be less noisy than responses reported from individual-level surveys. Second, our use of population-level data provides for the cleanest assessment of the overall policy impact of health insurance mandates by avoiding small-sampling bias and enabling us to obtain more accurate estimates of the treatment effects of mandates on both the extensive and intensive employment margins.

These gains do not come without cost. In particular, we are unable to assess any heterogeneity of labor market effects across demographic groups. Nor can we analyze employment dynamics at the individual level, as a micro-data panel enables. We view these costs as unfortunate but worthwhile, justified by the advantages of the SUSB data.

[15]This argument for the indeterminate direction in which a mandate affects average pay applies equally to health insurance coverage. Firms may respond to a mandate either by offering insurance to fewer employees (for example, by switching to more independent contractor employment [Right Management, 2011]), which reduces coverage rates, or by laying off (or hiring fewer) uninsured workers, which increases coverage rates.

[16]The SBA provides these and other historical employment data at `http://archive.sba.gov/advo/`.

[17]In particular, the SUSB data is compiled using the IRS Business Master File, a record of the names and addresses for all business tax filers, and quarterly IRS Form 941 filings, which contain payroll and employment reported with Social Security tax payments. The SBA does impute missing payroll data using prior year data and first quarter payroll data, which are more frequently reported. See Armington [1998] for additional details on the contents and construction of the SUSB and other SBA data sets.

4.2 State Health Insurance Mandates

Building on extensive state mandate data collected by Lahey [2011], we used the website of the National Conference of State Legislatures (NCSL) to gather mandate information directly from state legal codes.[18] For each of the nine mandated benefits discussed in Section 3, we determined the legislation date and any exemptions based on firm size. None of the mandates we analyze were repealed during the years of our study. Because we analyze only private sector employment outcomes, we ignored mandates exclusively targeting public sector employees.

4.3 State Minimum Wages

In addition to SUSB employment data and state mandate laws, we use state minimum wage data from the U.S. Department of Labor.[19] States frequently legislate a menu of wages that differ within a year across dates or firms of different sizes or industries. In constructing our variable we use the maximum of the Federal minimum wage and the set of possible state minimum wages for the year. Because there is substantial firm-level heterogeneity in the applicable wage level, our definition allows the minimum wage term to serve as an upper bound for the minimum wage a firm would actually face. We transform pay and minimum wages into constant 2007 dollars using the CPI-Urban [United States Bureau of Labor Statistics, 2011a].

We present summary statistics for state mandates, minimum wages, and small business employment and pay in Table 1. Table 2 includes information on mandate exemptions.

5 Results

We estimate Equation 1 for several outcome variables. We begin with the natural log of employment for each firm size bin, reported in Table 3. Columns (1) and (3) report estimates excluding the firm size exemption terms for firms with fewer than 20 and between 20 and 99 employees, respectively, while Columns (2) and (4) include those terms. State laws do not allow for firm size exemptions for four of the nine mandates we consider, so there are at most five exemption terms. As indicated in Table 2, many mandates have sparse firm size exemptions, which prevents identification of exemption effects. In light of this, we do not report estimates for the exemption terms although we view controlling for size exemptions as important and F-tests confirm that exemptions are jointly significant in our specifications. Full results are available upon request.

[18] We thank Joanna Lahey for providing us with her detailed information on state mandates, combed directly from state laws. The NCSL website (http://www.ncsl.org) provides both accurate summaries and direct links to the relevant sections of many mandate laws as published on the websites of state governments.

[19] The minimum wage data are available at http://www.dol.gov/whd/state/stateMinWageHis.htm.

Several mandates have effects on total employment. Drug abuse treatment has a large effect of approximately -2.5 percent across both specifications and firm size bins, though the coefficient is imprecisely estimated for firms between 20 and 99 employees. General mental health mandates are similarly large and significant for firms between 20 and 99 employees, about -1.9 percent, but the coefficients are small and imprecisely estimated for smaller firms. The other mandates tend to be both small and statistically insignificant.

Turning to the log of total pay, reported in Table 4, we again see large negative effects of about -2 percent for drug abuse treatment mandates for both firm size bins, though they are imprecisely estimated, and large negative effects for general mental health and mental health parity mandates for firms with between 20 and 99 employees.

For both employment and total pay, we find that mandates which were enacted earlier tend to have larger negative point estimates, whereas those enacted later have smaller positive point estimates (see Figures 4 and 5 for a graphical depiction of cumulative mandate adoption). Several mandates appear to have little effect on employment and total pay. For instance, the coefficients for diabetes treatment are essentially zero (especially for very small firms), with fairly tight confidence intervals.

Table 5 shows results for the log of average pay. For most mandates, the coefficients are small and statistically insignificant. However, alcoholism treatment mandates are associated with a statistically significant and relatively large effect of about -2.2 percent on average pay for the small firms in Column (2), and a similarly large but statistically insignificant effect of -1.8 percent for the larger firms in Column (4). Well-child care is also associated with a statistically significant decline in average pay of about 1 percent for both firm size bins. These results are consistent with the literature on adverse selection and health insurance mandates, discussed in Section 1. For some mandates, although total pay decreases, estimates for average pay are close to zero or even positive (e.g. drug abuse treatment, chiropractic services). This finding is consistent with contemporary opinion in the "sticky wage" literature on aggregate economic shocks: in response to a negative shock, firms maintain current employee wage levels but refrain from opening entry-level positions [Hall, 2005]. There are some notable exceptions to this finding. Alcoholism treatment and well-child care mandates significantly reduce average pay at small firms, but have a positive (statistically insignificant) effect on employment, suggesting a change in small firm workforce composition. We can only speculate as to the underlying cause of these results: it is possible that small firms hire younger, lower-paid employees to replace somewhat older workers who are more likely to have children or seek treatment for alcoholism.

In contrast, prostate cancer screening mandates are are associated with increases in average pay for small businesses and are similarly positively associated with total pay for small business in Table 4. While statistically insignificant, prostate cancer screenings had the largest positive effect (about 0.8 percent) on total employment of any mandate for small businesses in Table 3. Taken together, this combination of results could indicate that smaller firms respond to this particular mandate by transitioning to a larger number of part-time workers who are paid lower average wages and generally do not receive health benefits, an outcome consistent with evidence from Buchmueller et al. [2011].

Although self-insured firms are not legally subject to the same insurance regulation (discussed in Section 3), mandates may still bind de facto for larger firms, so we present results in Table 6 for firm size bins of 100-499 employees and more than 500 employees. Results for employment and pay in the larger firm size bins are similar in magnitude to those for smaller firms with the exemption terms included, with some exceptions. The apparent similarity of the results for large firms is consistent with existing work that shows self-insured firms tend to follow state insurance regulation (e.g. Acs et al. [1996]).

Finally, we present binned results for the natural log of employment regressed on several indices specifications in Table 7. Because mandates differ in the employment outcomes they affect, grouping mandates together is inappropriate, as discussed in Section 2. These alternative specifications serve as a good illustration of the problems associated with using indices of mandates in analysis: columns (1) and (2), for instance, have point estimates of less than two-hundredths of a percent (and small standard errors), showing evidence of the bias towards zero that is inherent in such an approach.

The standard errors for most of our estimates are relatively large, making many coefficients statistically insignificant at conventional levels.[20] Because we use population-level data, the standard errors for the estimates do not represent uncertainty due to sampling; rather, they represent the limitation of the predictive power for the point estimates. The confidence intervals around our estimates can be interpreted as bounds on the conditional expectation for the effect if another state was to enact a mandate.

We believe that using total pay, employment, and average pay together to analyze intensive and extensive margin effects on the labor market has merit. For instance, the point estimates are negative for both employment and total pay for drug abuse treatment and general mental health care mandates in the 20-99 employee firm size bin. For drug abuse treatment, the estimated effect on employment is larger in magnitude than that for total pay, and the point estimate for average pay is positive. In contrast, point estimates show that total pay falls by more than employment in response to a general mental health care mandate, and average pay decreases. The effect for the drug abuse treatment mandate reflects a "sticky wages" type response, whereas for mental health care the estimates suggest that firms may either be substituting non-pecuniary benefits for pay or hiring lower paid employees to replace more costly workers. Qualified by the lack of significance for many coefficients, we view the results

[20]Without clustering standard errors by state, many coefficients in all of the specifications are quite significant. This illustrates both the importance of accounting for correlated shocks across observations and the challenge in identifying the effects of mandates with so few clusters.

as providing evidence that mandates have some effect on employment outcomes and that these effects differ across mandates.

6 Conclusion

In this article we discuss several factors that have complicated identification of the effects of state health insurance mandates on small business employment. Additionally, we present empirical evidence underscoring the importance of considering both the intensive and extensive margins for employment effects, and of accounting for legal exemptions to mandates based on firm size.

A prevalent finding in the literature is that health insurance mandates have no statistically significant effect on employment outcomes. With U.S. annual health care expenditures reaching $2.5 trillion in 2009 [United States Department of Health and Human Services Centers, 2009], a better understanding of the labor market effects of health insurance regulation seems paramount. We view several factors as being responsible for the imprecision in this area of research. First, many conventional data sources such as the March CPS survey so few individuals employed in small businesses that estimates using these data suffer from small sample bias. Second, grouping mandates together into indices biases estimations because the marginal mandate is not uniform across states. This becomes a serious issue if mandates differ in their effects and as a result this type of estimation is biased towards zero. Finally, legislation of new insurance mandates tends to be collinear, making causal inference challenging with a short panel of state clusters.

Our empirical study examines the effect of nine costly mandates on small business employment and pay. We overcome the first concern by using a state-level data set compiled from the population of small businesses. This approach provides identical estimates of the local average treatment effects of mandates to those using population micro-data (which are not publicly available), but avoids the limitations of available samples. By specifying each mandate as a separate binary variable, we allay the second concern. We mitigate concerns regarding the collinearity of new mandates by studying a panel that spans twenty years during which state legislatures passed a large number of insurance mandates, indicating that a priori we should be able to identify the effects of mandates.

Overall, we find mainly statistically insignificant evidence that mandates are associated with decreases in small business employment and total pay—and to a lesser extent, average pay—but that mandates differ substantially in their effects. The clustered standard errors are large enough that the confidence intervals for most of our estimates overlap zero. However, the point estimates are non-trivial in magnitude, making us hesitant to assert that insurance mandates do not affect small business employment. Ma [2007] estimates about a 1% average additional premium cost for the health insurance mandates we study. Small businesses are likely to respond to a 1% shock to costs using many mechanisms: they may reduce employment, reduce employee pay, reduce employee health insurance coverage, reduce the quality of working conditions, change the composition of the workforce, or simply reduce profits. To a varying extent, all of these will probably occur in response to a mandate. With so many channels potentially affected by a mandate, it is immensely challenging to pin down the overall labor market effect. Our analysis of total employment along with total pay is useful, but ultimately this approach still considers only partial equilibrium effects.

Quantifying the employment effects of health insurance mandates—and, more broadly, health insurance regulation—remains very much an open question. In this study we determined several of the difficulties in identifying this relationship. Hopefully, future researchers will be better positioned to deliver decisive evidence on the subject. In the meantime, the appropriate policy interpretation is not to treat the inability of researchers to precisely estimate the multi-faceted effects of health insurance regulation as a sign that mandates do not affect labor markets.

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7 Tables and Figures

Table 1: Summary statistics for small businesses

	Mean	Std. Dev.	Minimum	Maximum
State Minimum Wage ($nominal)	4.77	0.928	3.35	7.93
State Minimum Wage ($2007)	6.08	0.569	5.15	8.06
Mandated Benefit:				
Alcoholism Treatment	0.541	0.499	0	1
Drug Abuse Treatment	0.365	0.482	0	1
General Mental Health	0.296	0.457	0	1
Mental Health Parity	0.294	0.456	0	1
Chiropractic Services	0.833	0.373	0	1
Diabetes Treatment	0.422	0.494	0	1
Prostate Cancer Screening	0.278	0.448	0	1
TMD Treatment	0.252	0.434	0	1
Well-child Care	0.425	0.495	0	1
Firms with fewer than 20 employees:				
Employment (thousands)	392.5	419.2	41.60	2570.8
Total Pay ($2007, millions)	12,989	15,758	1053.3	102,003
Average Pay ($2007, thousands)	31.05	5.959	21.93	59.99
Firms with 20-99 employees:				
Employment (thousands)	372.9	409.0	26.59	2586.1
Total Pay ($2007, millions)	12,755	15,462	749.4	103,469
Average Pay ($2007, thousands)	32.19	5.537	23.04	56.74
Observations (state-years)	1020			

Notes: Minimum wage and mandate statistics are computed by state-year for 1988-2007 for the fifty U.S. states and Washington, D.C. The employment outcome statistics are computed for the SUSB population of workers by state-year for each firm size bin. State minimum wage is defined annually as the maximum of the Federal minimum wage and the state's set of minimum wages. All dollar amounts are indexed to $2007 using the CPI-Urban. The binary mandate terms are set equal to one for a state that has legislated the mandate.

Table 2: Mandated benefit exemptions based on firm size

	Fewer than 20 employees		From 20-99 employees	
	States	Observations	States	Observations
Alcoholism Treatment	4	64	2	39
Drug Abuse Treatment	3	49		
General Mental Health	5	68		
Mental Health Parity	9	68	1	7
Prostate Cancer Screening	1	11		

Notes: Values are computed by state-year for 1988-2007 for the fifty U.S. states and Washington, D.C. An exemption is counted only if it covers all firms within the bin for a state. General Mental Health is treated as mutually exclusive to Mental Health Parity. There are 1020 total observations.

Table 3: Regressions of ln(employment) on mandates by firm size bin

	Fewer than 20 employees		From 20-99 employees	
	(1)	(2)	(3)	(4)
Alcoholism Treatment	0.0182	0.0023	0.0154	0.0183
	(0.015)	(0.017)	(0.025)	(0.028)
Drug Abuse Treatment	-0.0252**	-0.0242*	-0.0237	-0.0261
	(0.012)	(0.013)	(0.021)	(0.023)
General Mental Health	-0.0044	0.0005	-0.0198*	-0.0189*
	(0.006)	(0.007)	(0.010)	(0.011)
Mental Health Parity	-0.0100	-0.0028	-0.0190	-0.0183
	(0.008)	(0.010)	(0.013)	(0.013)
Chiropractic Services	-0.0073	-0.0074	-0.0142	-0.0142
	(0.012)	(0.012)	(0.022)	(0.022)
Diabetes Treatment	0.0006	0.0001	0.0027	0.0025
	(0.007)	(0.007)	(0.011)	(0.011)
Prostate Cancer Screening	0.0079	0.0084	0.0102	0.0102
	(0.008)	(0.007)	(0.013)	(0.013)
TMD Treatment	0.0070	0.0057	0.0125	0.0130
	(0.007)	(0.007)	(0.012)	(0.012)
Well-child Care	0.0054	0.0058	0.0153	0.0154
	(0.006)	(0.007)	(0.012)	(0.012)
Exemption Terms	No	Yes	No	Yes
Observations	1020	1020	1020	1020

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors are clustered by state and reported in parentheses. All estimations include state and year fixed effects, state-specific linear trends, and state minimum wage, which is defined annually as the maximum of the Federal minimum wage and the state's set of minimum wages. All dollar amounts are indexed to $2007 using the CPI-Urban. Each binary mandate term is set equal to one for a state that has legislated the mandate. Where included, binary exemption terms for each mandate are set equal to one if a state's mandate exempts all firms within the firm size bin. The data used are for the aggregate employed populations within each firm size bin at the state-year level, as reported by the U.S. Small Business Administration.

Table 4: Regressions of ln(total pay) on mandates by firm size bin

	Fewer than 20 employees		From 20-99 employees	
	(1)	(2)	(3)	(4)
Alcoholism Treatment	-0.0058	-0.0195	-0.0004	0.0002
	(0.014)	(0.018)	(0.027)	(0.030)
Drug Abuse Treatment	-0.0193	-0.0256	-0.0213	-0.0232
	(0.016)	(0.018)	(0.027)	(0.028)
General Mental Health	-0.0088	-0.0014	-0.0276**	-0.0261**
	(0.007)	(0.009)	(0.011)	(0.011)
Mental Health Parity	-0.0132	-0.0048	-0.0258*	-0.0234
	(0.009)	(0.011)	(0.014)	(0.014)
Chiropractic Services	-0.0107	-0.0109	-0.0134	-0.0130
	(0.015)	(0.014)	(0.019)	(0.019)
Diabetes Treatment	0.0003	-0.0006	0.0039	0.0035
	(0.009)	(0.009)	(0.013)	(0.013)
Prostate Cancer Screening	0.0205**	0.0202**	0.0166	0.0166
	(0.009)	(0.009)	(0.013)	(0.013)
TMD Treatment	0.0149	0.0143	0.0145	0.0133
	(0.011)	(0.011)	(0.012)	(0.012)
Well-child Care	-0.0055	-0.0040	0.0035	0.0038
	(0.009)	(0.009)	(0.012)	(0.012)
Exemption Terms	No	Yes	No	Yes
Observations	1020	1020	1020	1020

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$ Notes: See the notes for table 3.

Table 5: Regressions of ln(average pay) on mandates by firm size bin

	Fewer than 20 employees		From 20-99 employees	
	(1)	(2)	(3)	(4)
Alcoholism Treatment	-0.0240**	-0.0218***	-0.0158	-0.0181
	(0.010)	(0.007)	(0.013)	(0.014)
Drug Abuse Treatment	0.0059	-0.0014	0.0024	0.0030
	(0.011)	(0.011)	(0.017)	(0.018)
General Mental Health	-0.0043	-0.0019	-0.0079*	-0.0072
	(0.004)	(0.005)	(0.005)	(0.004)
Mental Health Parity	-0.0032	-0.0019	-0.0068	-0.0051
	(0.004)	(0.005)	(0.005)	(0.006)
Chiropractic Services	-0.0034	-0.0036	0.0008	0.0012
	(0.006)	(0.006)	(0.006)	(0.006)
Diabetes Treatment	-0.0003	-0.0006	0.0012	0.0009
	(0.006)	(0.005)	(0.005)	(0.005)
Prostate Cancer Screening	0.0126**	0.0119*	0.0064	0.0064
	(0.006)	(0.006)	(0.005)	(0.005)
TMD Treatment	0.0079	0.0086	0.0020	0.0003
	(0.007)	(0.007)	(0.007)	(0.007)
Well-child Care	-0.0109**	-0.0099**	-0.0118*	-0.0116*
	(0.005)	(0.005)	(0.007)	(0.006)
Exemption Terms	No	Yes	No	Yes
Observations	1020	1020	1020	1020

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$ Notes: See the notes for table 3.

Table 6: Regressions of employment outcomes on mandates for larger firms

	From 100-499 employees			More than 500 employees		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(employment)	ln(total pay)	ln(average pay)	ln(employment)	ln(total pay)	ln(average pay)
Alcoholism Treatment	0.0030	0.0070	0.0040	0.0079	0.0087	0.0008
	(0.029)	(0.036)	(0.011)	(0.019)	(0.026)	(0.010)
Drug Abuse Treatment	-0.0036	-0.0134	-0.0098	-0.0187	-0.0116	0.0071
	(0.025)	(0.035)	(0.016)	(0.022)	(0.030)	(0.011)
General Mental Health	-0.0247**	-0.0258*	-0.0011	-0.0178*	-0.0206*	-0.0027
	(0.012)	(0.014)	(0.007)	(0.010)	(0.011)	(0.005)
Mental Health Parity	-0.0158	-0.0230	-0.0072	-0.0259*	-0.0233	0.0026
	(0.014)	(0.018)	(0.007)	(0.013)	(0.014)	(0.006)
Chiropractic Services	-0.0291	-0.0279	0.0012	-0.0136	0.0084	0.0219*
	(0.019)	(0.023)	(0.014)	(0.017)	(0.025)	(0.012)
Diabetes Treatment	-0.0063	-0.0082	-0.0019	0.0001	-0.0006	-0.0007
	(0.013)	(0.014)	(0.007)	(0.012)	(0.014)	(0.006)
Prostate Cancer Screening	0.0073	0.0202	0.0129*	0.0070	0.0069	-0.0001
	(0.013)	(0.014)	(0.007)	(0.012)	(0.017)	(0.008)
TMD Treatment	0.0251*	0.0252**	0.0002	0.0281**	0.0237*	-0.0044
	(0.014)	(0.012)	(0.009)	(0.013)	(0.013)	(0.006)
Well-child Care	0.0071	0.0006	-0.0065	0.0042	-0.0018	-0.0061
	(0.013)	(0.014)	(0.007)	(0.012)	(0.013)	(0.004)
Exemption Terms:	N/A	N/A	N/A	N/A	N/A	N/A
Observations	1020	1020	1020	1020	1020	1020

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$ Notes: See the notes for table 3. Exemptions do not apply to firms this large.

Table 7: Regressions of ln(employment) on mandate indices by firm size bin

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Under 20	20-99	100-499	500+	Under 20	20-99	100-499	500+
Mandate Count	0.0002 (0.002)	-0.0001 (0.003)	-0.0030 (0.004)	-0.0025 (0.003)				
First Mandate					-0.0087 (0.017)	-0.0157 (0.034)	-0.0381 (0.034)	-0.0030 (0.026)
Second Mandate					-0.0073 (0.007)	-0.0226* (0.013)	-0.0209 (0.016)	-0.0066 (0.011)
Third Mandate					-0.0047 (0.006)	-0.0081 (0.011)	-0.0048 (0.013)	-0.0089 (0.010)
Fourth Mandate					0.0018 (0.005)	-0.0004 (0.008)	-0.0117 (0.009)	-0.0089 (0.008)
Fifth Mandate					0.0030 (0.006)	0.0099 (0.012)	0.0080 (0.011)	0.0045 (0.011)
Sixth Mandate					0.0040 (0.007)	0.0130 (0.011)	0.0074 (0.012)	-0.0010 (0.013)
Seventh Mandate					0.0121 (0.013)	0.0080 (0.017)	0.0197 (0.018)	0.0207* (0.011)
Eighth Mandate					0.0073 (0.013)	0.0130 (0.020)	0.0145 (0.024)	0.0121 (0.020)
Exemptions:	Yes	Yes	N/A	N/A	Yes	Yes	N/A	N/A
Observations	1020	1020	1020	1020	1020	1020	1020	1020

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Notes: Robust standard errors are clustered by state and reported in parentheses. All estimations include state and year fixed effects, state-specific linear trends, and state minimum wage, which is defined annually as the maximum of the Federal minimum wage and the state's set of minimum wages. All dollar amounts are indexed to $2007 using the CPI-Urban. Columns (1) through (4) use a linear index for the number of state mandates enacted of the nine we consider. Columns (5) through (8) use a (nested) separate regressor for each quantity of mandates. Because we treat a general mental health mandated benefit as mutually exclusive to a mental health parity mandate, the maximum number of simultaneously enacted mandates is eight. Where included, binary exemption terms for each mandate are set equal to one if a state's mandate exempts all firms within the firm size bin.

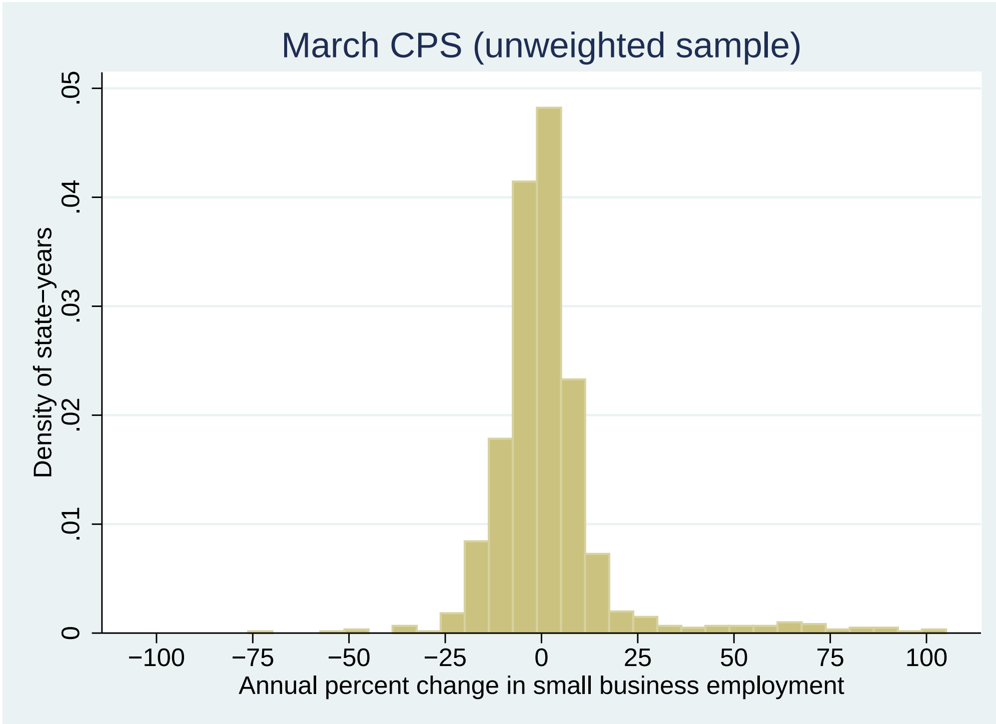

Figure 1: Small sample bias in the unweighted CPS data

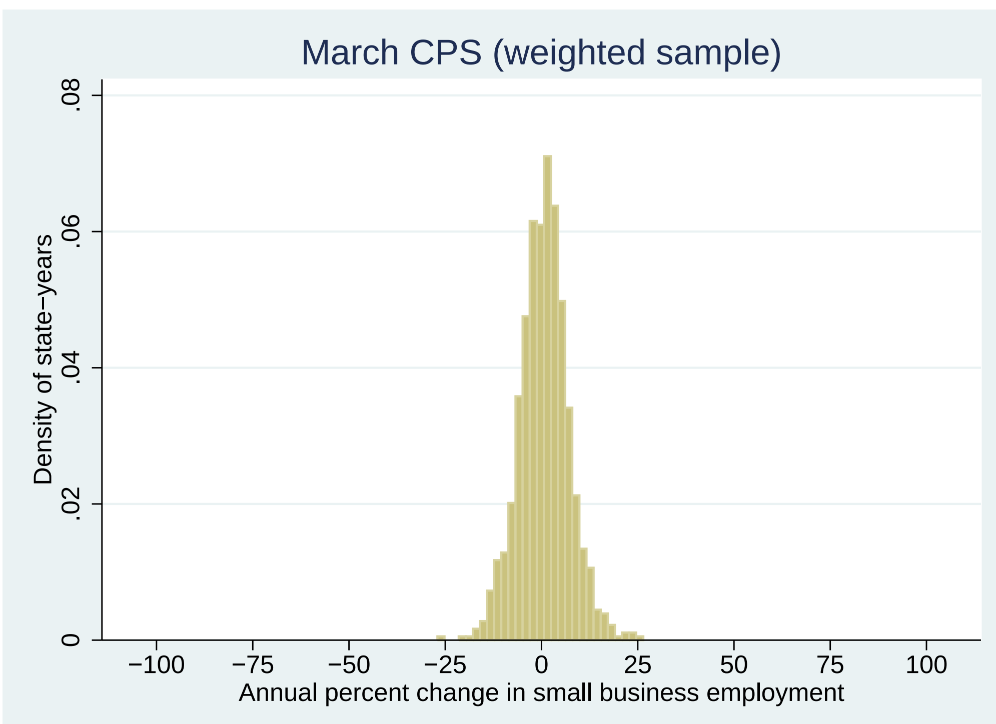

Figure 2: Small sample bias in the household-weighted CPS data

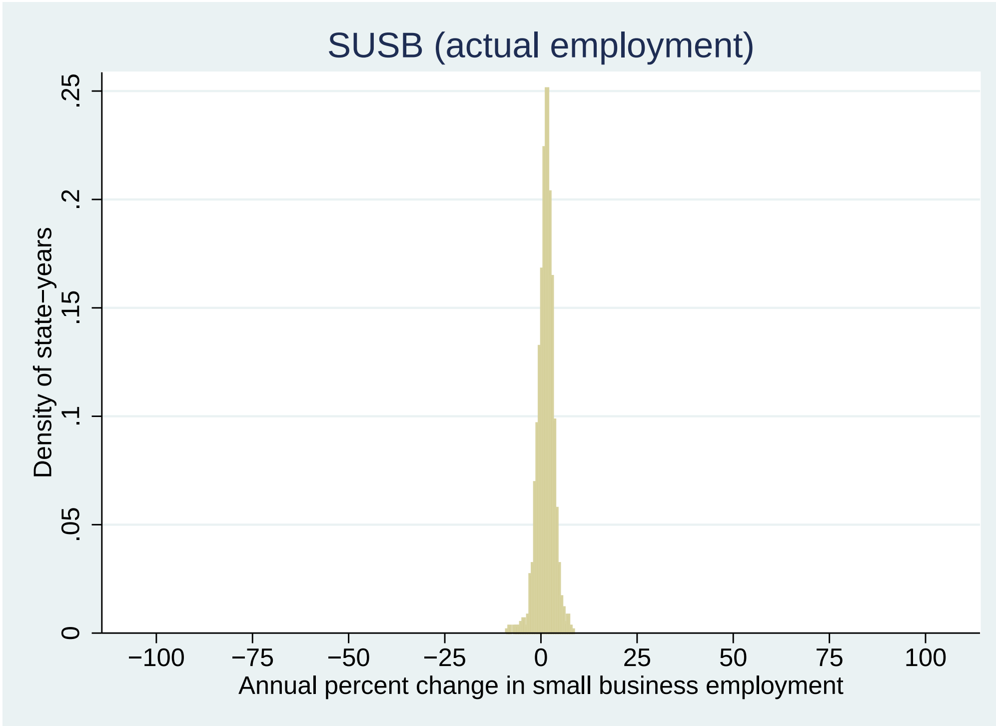

Figure 3: Actual changes in small business employment

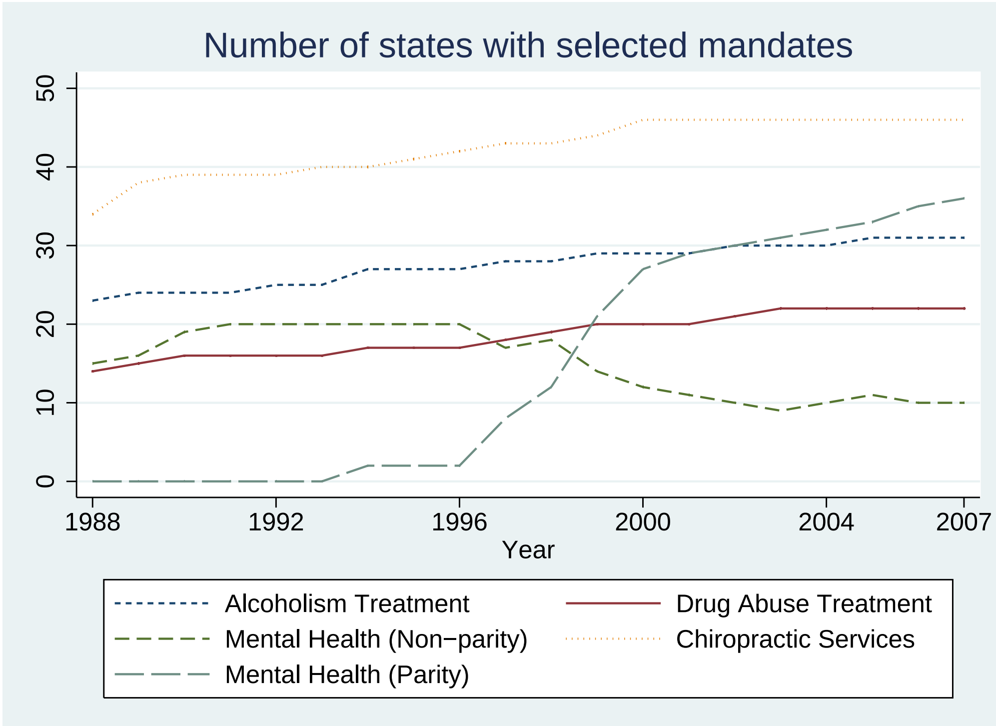

Figure 4: Mandates considered in Gruber [1994b] (except continuation of coverage)

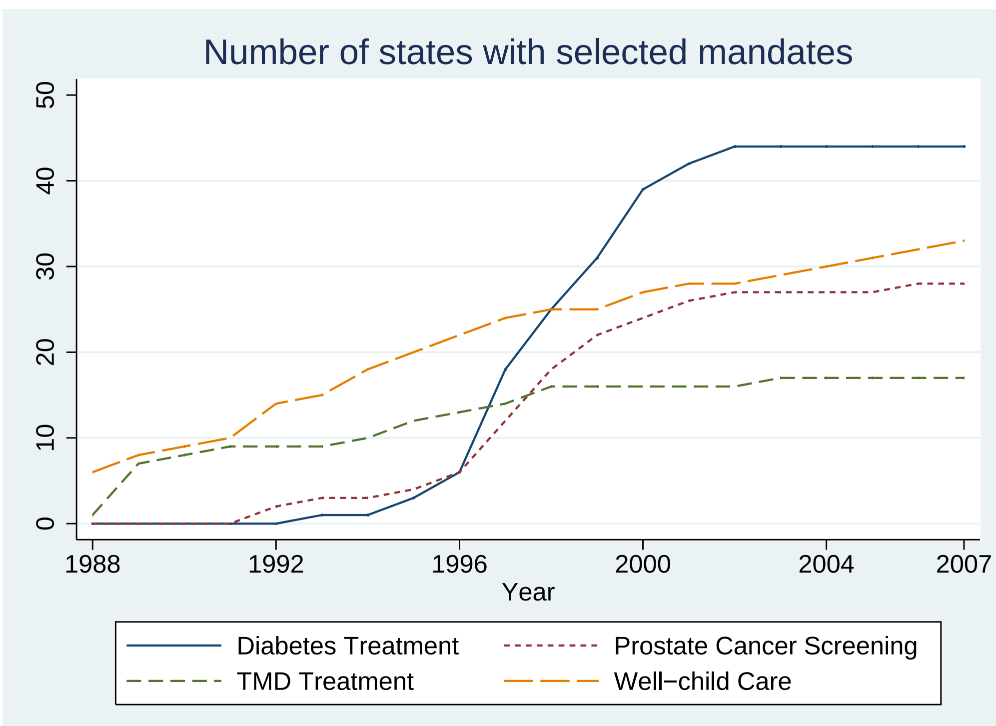

Figure 5: Additional mandates considered in Ma [2007]