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STATE MANDATED BENEFITS AND EMPLOYER PROVIDED HEALTH INSURANCE

Jonathan Gruber

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ABSTRACT

One popular explanation for this low rate of employee coverage is the presence of numerous state regulations which mandate that group health insurance plans must include certain benefits. By raising the minimum costs of providing any health insurance coverage, these mandated benefits make it impossible for firms which would have desired to offer minimal health insurance at a low cost to do so. I use data on insurance coverage among employees in small firms to investigate whether this problem is an important cause of employee non-insurance. I find that mandates have little effect on the rate of insurance coverage; this finding is robust to a variety of specifications of the regulations. I also find that this lack of an effect may be because mandates are not binding, since most firms appear to offer these benefits even in the absence of regulation.

Jonathan Gruber
Department of Economics
MIT
E52-274C
Cambridge, MA 02139
and NBER

Private health insurance in the United States is predominantly provided through the employment relationship. Of the 183 million privately insured individuals in the country, 83% receive health insurance through their employers or the employer of a family member.[1] Employer provided health insurance is primarily regulated at the state level, and, over the past twenty years, there has been a tremendous growth in one variety of state regulation, state mandated health insurance benefits. These are regulations issued by the states which mandate that group health insurance plans in those states must include minimum levels of certain benefits, such as coverage for maternity, mental illness, chiropractic services, or invitro fertilization. In 1965, there were only two state mandates; by 1991, there were almost 1000 (Scandlen, 1987; Scandlen Publishing, 1991).

The efficacy of these state mandates has become an important issue in the debate over reform of the U.S. health care system. Proponents of mandates point to several reasons why insurance markets may fail to provide the appropriate level of these benefits, so that mandating inclusion of the benefit in all plans can be welfare increasing. Opponents of mandates respond that if mandating the inclusion of expensive health insurance benefits increases the premium cost to the employer, then this may lead some employers to not offer health insurance at all. That is, employers who would have desired to offer a "barebones" health insurance policy are stopped from doing so by regulations which impose a minimum of "Cadillac coverage". As Goodman (1991) states: "Standing between an estimated 34 million Americans who lack health insurance and the opportunity to buy an affordable policy are numerous state laws". And, according to the 1991 Economic Report of the President: "These requirements raise the cost of health insurance and make it too expensive for many individuals and firms" (p. 141). Both Goodman and the Economic Report cite studies suggesting that as much as <u>one-quarter</u> of non-insurance in the U.S. is due to state regulation of insurance.

Of the 33 million uninsured persons in the U.S., more than two-thirds are either full time, full year employees or the dependents of such workers.[2] If mandated benefits are such a major factor in firms' decisions not to offer health insurance, then it suggests that this large

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[1]Unpublished data, U.S. Bureau of the Census; figure for 1989.

[2]Chollet (1987).

number of employed uninsured can be substantially lowered in a fairly straightforward manner. Indeed, this presumption has been the impetus behind recent legislation in 22 states which exempts some small firms from including mandated benefits in their health insurance package (Scandlen Publishing, 1991). Thus, a central question in evaluating state mandated benefits is the magnitude of the "displacement effect": the extent to which mandating that all firms include particular health insurance benefits will lead some firms to drop their insurance coverage altogether.

In this paper, I investigate the effect of state mandated benefits on the propensity of small firms to offer health insurance. The empirical work focuses on small firms for two reasons. First, firms which self-insure are exempt from state mandated benefits under the 1974 Employee Retirement Income Security Act (ERISA), and a substantial fraction of large firms (by the late 1980's) self-insure.[3] Second, small firms are much less likely to offer health insurance than large firms: while firms with fewer than 100 employees employ 41% of the full-time workers in the U.S., workers in small firms comprise 67% of the uninsured full time employed.[4] This suggests that attempts to explain the low rate of insurance provision for employees should focus on the experience of small firms.

I proceed by using data from the 1979, 1983, and 1988 May Pension and Employee Benefits supplements to the Current Population Survey (CPS) to measure insurance coverage among employees in small firms. I have also collected comprehensive information on the distribution of mandated benefits across the states, and I match this to the CPS data to estimate the effect of mandates on the propensity to offer insurance. I focus on five high cost mandates: mandated minimum benefits for alcoholism 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 ("continuation of coverage"). These five mandates account for at least one-half of the total cost of mandated benefits, and adding them

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[3]See Congressional Research Service (CRS) (1988a) for an excellent discussion of ERISA preemption. In 1989, 46% of firms with 1000 employees or more self-insured, while only 7% of firms with less than 100 employees did (authors tabulations of Health Insurance Association of America survey data).

to a health insurance plan could increase insurance premiums by as much as 12%. I also examine the response of insurance coverage to state taxation of health insurance premiums, which may be passed onto the consumer and lower the demand for insurance as well.

I find that state regulations have little consistent effect on small firms' decisions to offer health insurance. This finding emerges from specifications where the effect of the separate mandates are evaluated individually, where the sum of their effects is measured, and where the set of mandates are weighted by their "potential premium cost" to calculate a "regulatory cost index" faced by a firm doing business in a given state/year. It is also robust to controls for features of the states which may be correlated with both their propensity to mandate and the level of insurance coverage. And it is confirmed by using the "reverse experiment" represented by the spate of recent legislation which exempts some small firms from mandates. Furthermore, I am able to reconcile my findings with earlier studies which had shown a much larger effect of mandated benefits.

Given the moderately large potential increment to health insurance costs that these mandates represent, there are three reasons why they may not affect firms' propensity to offer health insurance. First, the price elasticity of insurance demand by small firms may be fairly low, so that their reaction to this price increase was too small to be captured in my data. Second, workers may value the increased health insurance benefits at nearly their full cost to firms, so that this cost can be passed onto wages. Finally, mandates may not be binding, in that the mandated minimum level of benefits is exceeded by existing health insurance policies at most firms in the pre-mandate equilibrium. In this case, the price increase to the average firm from these mandates may be very small, and there may be very little coverage response to mandates even in the presence of large price elasticities. While I am unable to fully disentangle these three effects, I present evidence which suggests that these "expensive" mandates are not binding. That is, I find that most small firms offer these benefits, <u>even when they are not mandated to do so</u>. I confirm this result with some evidence which suggests that mandated mental health benefits did not affect the delivery of mental health care in the states. While this evidence is preliminary, it suggests that small firms are either choosing not to insure at all, or to offer generous insurance when they do insure; this non-linearity in demand may offer some insight into the workings of the small business insurance market.

The paper proceeds as follows. In Part I, I discuss the case for and against mandated benefits and the existing evidence on the displacement effect. In Part II, I introduce the data and empirical methodology for estimating the effects of state mandated benefits on insurance coverage. Part III presents those estimates, and assesses their robustness to a variety of specifications. In Part IV, I investigate why mandates do not affect insurance coverage among small firms. Part V concludes by summarizing the implications of these findings for the policy debate over mandated benefits and the small firm insurance market.

PART I: THE DEBATE OVER MANDATED BENEFITS

What are State Mandated Benefits?

Health insurance has traditionally been regulated at the state level, and state mandated benefits have grown to be perhaps the most onerous of these regulations. There are at least 60 different types of mandates, of three different varieties. Provider mandates state that the services of a certain provider type (ie. chiropractors) must be covered by insurance policies. Benefit mandates stipulate that the policy include minimum levels of certain benefits; for example, the mental illness mandate in Montana states that firms must offer inpatient coverage for up to 30 days per year, and outpatient coverage for up to $1000 per benefit period (with a lifetime maximum of $10,000). Coverage mandates provide that insurance plans cover particular classes of individuals; one common example is continuation of coverage mandates.[5]

States can mandate either that all health insurance plans include a given policy, or that health insurance companies offer that policy to groups ("mandated offerings"). I focus only on mandated benefits in this study; it is not clear why a firm would not provide health insurance simply because it was offered a more expensive plan by the insurer.

Of central importance in the debate over mandated benefits is the actual premium increase

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[5]Continuation of coverage mandates provide that (voluntarily or involuntarily) terminated employees and their dependents shall have the right to continue to purchase group health insurance from their former employers, generally at no more than the applicable group rate. These mandates are potentially costly because of the adverse selection inherent in which terminated employees/dependents actually choose to take up health insurance at the full price. See Appendix A for a more detailed discussion.

which these mandates cause. Table A1 reports the results of studies of the cost of mandated benefits done by the Blue Cross/Blue Shields of Massachusetts (1990), Maryland (1988), and Virginia (KPMG Peat Marwick, 1989), and by the Wisconsin Office of the Commissioner of Insurance (1990). In the first row of the Table, I present these studies' estimates of the total costs of all mandated benefits, which averages about 20% of premiums. The remaining rows present the estimates of the costs of five expensive mandates: coverage for treatment of alcoholism, drug abuse, and mental illness, coverage for chiropractic services, and continuation of coverage mandates. These five mandates account for at about 50% of the total mandate cost, on average, across the states, amounting to as much as 12% of premiums in Virginia.[6]

Unfortunately, these do not represent the ideal measure of the cost of the mandates, ie. the marginal premium cost for adding that benefit to a health insurance package. Rather, they are the fraction of total health insurance claims which are paid for each of these benefits. Data on the marginal cost of one of these benefits is in CRS (1988c) for inpatient and outpatient mental health benefits, and it is presented in the column (5) of Table A1; it is somewhat lower than the estimates in the other columns. Also, Huth (1990) presents data on firm's experiences with continuation of coverage claims which allows one to estimate the marginal premium cost of this benefit (as described in Appendix A). This is presented in column (6); it is about double the estimate from Virginia.

Why Should We Mandate?

There may be benefits along several dimensions to mandating a minimum level of coverage for certain illnesses in health insurance packages. Imperfect information between insured and insurer about the prevalence of specific illnesses may lead to adverse selection problems in the health insurance market along the lines described by Rothschild and Stiglitz (1976). As Summers (1989) points out, a particularly cogent example of this adverse selection problem is the employer who is considering offering coverage for AIDS in her health insurance

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[6]The only single other mandate which accounts for a non-trivial fraction of costs is mandated comprehensive coverage for maternity benefits, which has been federal law since 1978, and therefore is not studied here; see Gruber (1992a) for an investigation of the effects of this mandate.

policy. By mandating that all policies include AIDS coverage, regulation can correct this market failure and possibly make both the insured and the insurer (or the employer, who pays the insurer) better off. Adverse selection problems may be worst for particular health benefits such as mental illness; McGuire and Montgomery (1982) present evidence of adverse selection from a case study of mental illness benefits for Federal employees.

There are several other sources of gains from mandates, which are discussed by McGuire and Montgomery, and by Summers. If employees irrationally underestimate the probability of certain illness, then they may not choose to insure against those illnesses in the free market equilibrium, which may lead to suboptimal levels of coverage from the perspective of a "paternalistic" society or one that values insurance as a "merit good". Furthermore, even if individuals optimally insure against their own risk, social costs may exceed private costs due to negative externalities of individual illness. These can arise most obviously in the context of communicable disease. However, they can also arise from the fact that society is unwilling to commit to refuse charity care to those who become ill without insurance. Hospital uncompensated care amounted to over 15 billion dollars in 1989, which is over 6% of hospital gross revenues (Gruber, 1992b). As Coate (1992) shows, government intervention in the insurance market can be justified by the presence of externalities from individuals who underinsure because of the presence of this charity care.[7] Finally, there is the dynamic consideration that increased utilization of low cost medical resources by those insured for particular illnesses may reduce the need for future utilization of more expensive resources which can arise if that illness goes untreated. McGuire and Montgomery discuss evidence that this "offset effect" may be quantitatively important for psychotherapy benefits.[8]

Why Shouldn't We Mandate?

There are at least three arguments against mandating health insurance benefits.

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[7]This argument may be less compelling for specific benefits than for basic coverage of medical expenses. It seems unlikely, for example, that individuals will forego obtaining insurance against mental illness because of the availability of state funded mental hospitals.

[8]Of course, this is only a market failure to the extent that individuals are not aware of this offset effect, or will not bear the excess costs of foregoing treatment.

Correcting the market failures discussed above can worsen the moral hazard problems of medical resources overutilization which arise from full insurance. Overutilization, in turn, can lead to large welfare losses from a rising spiral of prices for medical care and insurance (Feldstein, 1973). The natural tradeoff between correcting adverse selection and exacerbating moral hazard is exemplified by cases such as that of coverage for outpatient mental illness benefits.

Second, regulatory constraints on the design of health insurance policies may lead firms to self-insure, since such plans are not subject to state regulation. This may be suboptimal for two reasons. First, premium income of insurers is an important tax source for many states, and self-insurance shrinks this tax base, raising the tax rate for a given level of revenues and thus the deadweight loss of revenue raising. Second, in the presence of distortions to the relative costs of insuring and self-funding, firms may choose an inefficiently high level of self-funding given the increased risk inherent in this strategy. Indeed, self-insurance among small and medium-sized firms has grown rapidly in recent years (Foster Higgins, 1990).[9]

Finally, firms who would choose to insure their workers in the absence of mandates may not do so if mandates make insurance more expensive. It is the magnitude of this "displacement effect" which is at the crux of the policy debate over mandated benefits. This effect is modelled in Jensen and Gabel (1990), who analyze the firms decision to offer health insurance in a model where employees are matched to firms based on their tastes for wages and benefits (along the lines of Rosen, 1974). As mandates raise the minimum threshold of insurance costs, the marginal firm will choose to offer higher wages and forgo insurance. The key conclusions of the model are that the displacement effect will be strongest for firms which are at the margin of offering insurance in the pre-mandate equilibrium, and that this effect will be stronger as mandates are more binding relative to the free market outcome.

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[9]Gabel and Jensen (1989) report evidence that firms were more likely to convert to self-insurance from 1981-1984 if they were in states with mandates for psychologists services or continuation of coverage. However, their sample of firms is quite small (280 firms), and it is not clear why mandates which may have been in place since the 1960s should affect firm's decisions to self-insure in the early 1980s; furthermore, as I discuss in appendix B, these authors reliance on Blue Cross data on mandates may lead to misleading results. Using data from the HIAA discussed in Part IV, I find no effect of mandates for alcoholism treatment, drug treatment, and mental illness on the propensity to self-insure in a cross-sectional regression.

The displacement effect is important because losing all insurance coverage could have much larger consequences for the individual, and society, than gaining coverage for a specific benefit. For example, it may be true that the offset effects from primary care are particularly large, relative to that from particular ailments. It is also interesting to note that the mandates for which the perceived benefits may be the largest, such as mental illness or alcoholism abuse, are also potentially the most expensive mandated benefits, so that the displacement effect will be largest. These expensive mandates will thus be the focus of my attempt to estimate the effect of mandates on the insurance decision.

Existing Evidence on the Displacement Effect

A precondition for the displacement effect to be quantitatively important is that there be a sizeable pool of firms that do not offer health insurance. This is examined in Table 1, which examines the rate of insurance coverage by firm size. The first three columns report the results from three employer based studies of health insurance provision. The final column reports tabulations from an employee based survey, the May 1988 Pension and Employee Benefit Supplement to the CPS, which asked workers whether their employer offers health insurance. In fact, insurance offering is virtually universal among large firms in the employer based surveys, and is quite high in the CPS as well. However, the propensity to offer insurance is much lower for smaller firms, with less than half of the smallest firms offering insurance in each of the surveys. In the conclusions, I will review the set of reasons why small employers may be less likely to offer health insurance. One candidate, however, is state mandated benefits, since small firms, with their high turnover and volatile earnings, are likely to be the "marginal" firms in the model of Jensen and Gabel (1990), who will drop insurance coverage in response to the mandates.

Both Jensen and Gabel and Goodman and Musgrave (1988) present empirical evidence that the displacement effect is quantitatively important. Jensen and Gabel used two cross-sections of firm level data from 1985 (small firms only) and 1988 (all sizes), matched to information from the Blue Cross/Blue Shield Association of Washington, D.C. (1990) on the distribution of mandates across the states. They modelled the insurance coverage decision as a function of the presence of a set of expensive mandates. They found that the only mandate

which significantly affected coverage decisions in both samples was continuation of coverage requirements; in the 1988 sample, they also found a significant effect of premium taxation. Their results predict that between 19 and 44% of noncoverage among firms is due to state mandated benefits and premium taxes. Goodman and Musgrave analyzed aggregate data on the percent of the non-elderly population without insurance by state. They found that this fraction was positively related to the number of mandates which had been passed by the state, and that mandates could explain as much as 25% of the level of non-insurance across the states.

There are a number of problems with each of these studies, which are addressed in detail in Appendix B. The Goodman and Musgrave study suffers from the use of inappropriate measures for both the dependent and key independent variable: the percent of the population which is uninsured may be a function of other state policies (ie. Medicaid eligibility) which are correlated with the number of mandated benefits; and mandates differ widely in their potential effects on insurance premium costs, so that the total number of regulations may not contain much useful information about the regulatory pressures on the firm's health insurance coverage decision.[10] Furthermore, their measure of total mandates includes mandated offerings; as mentioned above, it is not clear why these would affect the firms insurance decision. In fact, there is no significant relation between the number of mandated benefits and insurance coverage. Their results are also very sensitive to the regression specification used.

Jensen and Gabel use a regression framework which is more consistent with that employed in this paper, albeit with a slightly different set of expensive mandates. However, the validity of their conclusions suffer from the fact that they rely on tabulations of state laws from Blue Cross/Blue Shield, which are inconsistent with actual state legislation in a number of instances. Furthermore, they do not account for the fact that all firms of more than 20 employees are subject to continuation of coverage mandates after 1986 (described below). When the correctly measured set of mandates are used, there is no net effect on coverage with their data.

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[10]For example, the set of mandated benefits in Virginia range from provider mandates for optometrists, which account for 0.01% of incurred claims, to mandated coverage of mental illness, which accounts for 7.5% of claims (KPMG Peat Marwick, 1989).

Finally, with both studies, there is the possibility that the presence of mandates in a state might be correlated with other factors which determine the propensity of firms to offer health insurance. If states with a traditionally high level of non-insurance respond by mandating comprehensive insurance packages, then this will bias the coefficients on the mandate variables downwards. This possibility is not considered in the cross-sectional regression framework which is used.

PART II: DATA AND EMPIRICAL FRAMEWORK

Data

The primary source of data for the estimation of the displacement effect is the May CPS supplements for 1979, 1983, and 1988. This supplement to the regular survey asked a number of questions about the respondent's job, including firm size, tenure on the job, union status, and details on benefit plans. The sample is comprised of all 16 to 65 year old workers who have non-missing data for tenure and union status. The means for each year are presented in Table 2, for all workers, and for workers in firms with more than 999 employees, less than 100 employees, and less than 25 employees. Workers in small firms are more likely to be younger, female, single, and white. The rate of unionization is dramatically lower in small firms, as is the average tenure on the job. As documented earlier, the rate of insurance coverage falls with size as well. Finally, firms of different sizes are located in states with a similar number and distribution of mandates.

Data on state regulations comes from several sources. Both the Blue Cross/Blue Shield of Washington, D.C. (1990) and the Health Insurance Association of America (unpublished) collect data on state mandates. For the mandates on which the study focuses, I have corroborated the data from these sources using state legislative records, as well as studies by Levin (1988) and Scott et al. (1992). Data on state continuation of coverage laws comes from Hewitt Associates (1985), Thompson Publishing (1992), and Huth (1990), as well as from state legislative records. Starting in July, 1986, all firms with more than 20 employees were mandated to provide continuation of coverage benefits, for up to 18 months to terminated employees and their dependents under Federal law (Thompson Publishing, 1992). I therefore measure all firms with 25 employees or more as being covered by a continuation of coverage

mandate in 1988; for very small firms, the coverage is still a function of state laws.

I also control for the rate at which states tax the premiums earned by insurers which do business in that state, which may be passed forward to the price of insurance. Data on premium tax rates comes from Blue Cross and Blue Shield (1979, 1984, 1989).[11]

Empirical Framework

The regression model is:

$$INSCOV_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 \delta_j + \beta_3 \tau_t + \beta_4 MANDATES_{jt-1}$$

where $INSCOV_{ijt}$ is one if worker i in state j in year t is covered by insurance provided by her employer, and zero if she is not covered by her employer

X_{ijt} is a set of demographic and job characteristics for worker i

δ_j is a set of state dummies

τ_t is a set of year dummies

$MANDATES_{jt-1}$ is either a) a set of dummies for the individual expensive mandates passed in state j by year t-1, or b) the potential cost of these mandates

This regression is run on the 1979, 1983, and 1988 May CPS supplement samples. The dependent variable measures the presence of insurance coverage on the job.[12] The regression controls for: age and its square; education; tenure on this job and its square; a dummy for tenure less than one year; dummies for non-white, female, married, and an interaction of female and married; a dummy for coverage by a union contract; a dummy for part-time workers (less than 35 hours/week); and a dummy for very small firms (less than 25 employees). All regressions also include a set of two year dummies and 21 major industry controls. The regression is run both as a linear probability model and as a probit; the results are not sensitive to the specification used. The displacement effect is measured by the coefficient β_4, the effect of mandates on the level of insurance coverage.

Ideally, the measure of the total effect of mandated benefits would be the set of mandates

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[11]Premium tax rates are an average of those on commercial insurers and those on Blue Cross/Blue Shield, where the weights are the fraction of premiums in the state collected by commercial insurers and the Blues (from HIAA, various). Within the group of commercial insurers, I took an average of the rates on domestic and "foreign" (out-of-state) insurers. The results are not sensitive to the weighting schemes used.

[12]This variable thus confounds both firm insurance offering decisions and worker insurance takeup decisions, as is discussed below.

passed by each state, weighted by the marginal effective premium increment from each type of mandate.[13] Unfortunately, data on the cost of all mandates is unavailable, and entering a full set of 60 individual mandate dummies would cause serious collinearity problems. Thus, I focus on the five high cost mandates discussed above, for which some (average, not marginal) cost data is available; as mentioned earlier, these mandates account for about 50% of the total cost of all mandated benefits.

I assess the effect of these mandates in three ways in regression framework. First, I simply enter a set of five dummies which are equal to one if the state has passed a mandate before year t, and zero otherwise (as well as the premium tax rate as a percentage of premiums), and examine the individual coefficients. However, this reduced set of mandates remains somewhat collinear; this is particularly true once the state effects are included in the model. I therefore also examine the overall significance of the set of mandates, rather than focusing solely on individual mandate coefficients. That is, in each regression below, I report the coefficient and standard error on the sum of the individual mandate dummies. To the extent that collinearity is a problem, this will be the relevant summary statistic for the effects of the mandates.

Finally, I use the cost data from Table A1 to approximate the premium increment due to mandated benefits, by creating a "potential mandate cost index". To create this cost index, I have weighted each of the set of expensive mandates by estimates of the potential increment which they add to group health insurance rates. The weighting which is used is:

$$\text{MANCOST} = \text{ALC} + \text{DRUG} + 5*\text{MENTAL} + 1.5*\text{CHIRO} + 3*\text{CONTCOV} + \text{PREMTAX}$$

where ALC, DRUG, MENTAL, CHIRO, and CONTCOV are the mandate dummies and PREMTAX is the premium tax rate as a fraction of premiums

This weighting is primarily based on the fraction of claims dollars paid out under each of these benefits, as estimated from the studies cited earlier; the exact weighting is justified in Appendix A. The mean of this potential cost, reported in Table 2, is about 5% of premiums;

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[13]This measure would also adjust the effect for the extent of employee valuation of the benefit, since the cost to the firm is only the premium increment net of that valuation.

the maximum is over 13%. This is only the potential premium increase for the average firm, because, to the extent that these mandates are not binding, this index will overstate the effect of these laws on that firm; evidence on this point is presented in Part IV. Relative to the other two approaches, this linear combination is potentially more efficient (if the relative weights are correct), but also potentially biased (if they are not). Given the moderate size of the effects which one would expect in response to these mandates, this extra efficiency could be important.

The question of whether to include state effects in this regression is an important one. The displacement effect hypothesis is that the passage of mandates led to a reduction in the propensity of small firms to offer health insurance. This is ideally tested by a specification which includes state effects, so that changes in legislation are compared to changes in insurance coverage. However, as Table A2 highlights, this "within" estimation throws away much of the variation in the data which may be used to identify the effect of mandates. This table reports the effective dates of passage of the five mandates which are studied in this paper, as well as the number of states passing each mandate before 1988, and the number passing it between 1979 and 1987.[14] Without state effects, the impact of the mandates will be identified by both the mandates passed before the sample period and those passed during those years (both "between" and "within" variation). With state effects, only the law changes will be used. Furthermore, many of these laws were passed simultaneously, so that there is even less information available to identify the impact of the individual mandates in the state effects specification than is indicated by the number of law changes (although this consideration does reduce the "between" variation as well). Finally, if firms react slowly to increases in the price of their insurance coverage, then the short run responses which are estimated in the model with state effects will understate the long run effects of these laws.

On the other hand, it is not clear whether the "between" variation is a legitimate source of identification of the effect of mandates. The passage of these regulations may be an endogenous response to longstanding differences in the level of insurance coverage across the states, so that estimates of the effects of this legislation on insurance coverage in a cross-section

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[14]For continuation of coverage laws, the dates of state laws are reported; as discussed above, for firms of more than 20 employees there was a federal law put in place in 1986.

will be biased.[15] It seems less likely that changes in legislation were endogenous responses to changes in the level of insurance coverage over this nine year period. That is, while the inclusion of state effects does not establish causality, it does lower the probability of spurious identification. Thus, there is a tradeoff: including the state effects removes much of variation in the data, but that variation may not be appropriate for identifying the true effect of mandates. I will therefore present the results below both with and without state effects; in general, they yield similar inferences.

PART IV: ESTIMATING THE DISPLACEMENT EFFECT OF STATE MANDATES

Table 3 presents the basic results from insurance coverage regressions for employees in small firms (less than 100 employees). The marginal probabilities from the probit model are interpreted in the third and sixth columns; the results from the probit and the linear probability model are very similar.[16] In the specification without state effects (columns (1)-(3)), only two of the six mandate dummies (in the probit model) have negative coefficients, and only one (mental illness) has a coefficient which is greater than its standard error (although it is insignificant at the 10% level). As a result, the sum of the mandates has an insignificant negative coefficient, which implies that imposing this entire set of mandates would only lower the probability of insurance coverage by 0.3%. Given that these regulations account for about one-half of the total cost of mandated benefits, this does not suggest a very large role for mandates in reducing coverage.

The "mandate cost" row presents the coefficient from a regression which included the

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[15]The only study of the determinants of mandates is that of Lambert and McGuire (1990), who find that mandates respond to lobbying pressure from provider groups, but who do not analyze the role of insurance coverage in the state.

[16]The marginal probabilities are calculated by: (for dummy variables) predicting the probability of coverage if the dummy were equal to one for the entire sample, predicting the probability with the dummy set to zero for the sample, and taking the average of the differences in these predictions across individuals; (for continuous variables) predicting the probability at the current level of the variable, predicting the probability adding one to the value, and taking the average of the differences in the predictions across individuals.

potential cost index in place of the individual dummies.[17] This coefficient is insignificant as well, and implies that a one percent increase in potential cost led to a 0.06% fall in insurance coverage.

The set of covariates have their expected effects. Insurance coverage is found to rise with age, but at a decreasing rate; the pattern with respect to job tenure is similar, and there is a much lower rate of coverage among first year workers. There is more coverage for educated workers, white workers, single females, and married males, and much less coverage for married females. Union coverage substantially increases the probability of health insurance coverage, and the probability is much lower for part-time workers and those in very small firms.

In columns (4)-(6) I add a set of 48 state effects to these regressions.[18] The mental illness mandate once again has a sizeable negative coefficient which is larger than its standard error (although insignificant); it implies that mental illness mandates lowered the probability of insurance coverage by 3.1%. However, none of the other mandates has much of an effect, and the sum is once again quite insignificant; its coefficient suggests that passing the full set of "expensive" mandates would lower insurance coverage in small firms by only 1.2%. The coefficient on the potential mandate cost is also insignificant, with a one percent increase in potential cost leading to a 0.17% fall in coverage. The state effects do not have much effect on the covariates. The robustness of these findings to the inclusion of state effects is supportive of the contention that the displacement effect is not quantitatively important. However, while the point estimates are similar to the cross-sectional regressions of columns (1) and (2), the confidence intervals for these estimates have risen substantially.

Specification Checks: Very Small Firms and Tests of Endogenous Legislation

Since even some firms of less than 100 employees self-insure, and since the probability

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[17]Note that, although the mandate cost coefficient is reported in the same column as the mandate dummies in the remainder of the tables, it is obtained from a distinct regression.

[18]Washington, D.C. is excluded because of incomplete data on mandates. Hawaii is excluded because there is a general mandate that most employers must provided insurance to their employees in that state (since 1974). West Virginia is excluded because there is no information on the date on which they passed their continuation of coverage mandate.

of offering insurance coverage is lowest for the very smallest firms, it may be that mandated benefits have a noticeable impact on very small firms only. I therefore rerun the regressions for firms with less than 25 employees only in columns (1) and (2) of Table 4. In fact, the results are even weaker for these firms; both the sum of the mandates and the mandate cost are positive when state effects are excluded, and they are very close to zero when state effects are included. This provides further confirmation that mandates do not matter, since, if they did, this would be the sample in which they would be expected to have the largest effect.

One potential problem with this specification may be the endogeneity of the passage of these mandates to the extent of non-insurance in the state. By including state effects, I control for time invariant correlations between the level of insurance coverage and the propensity to legislate in a given state. However, it may be that states responded to changes in insurance status by passing mandates. I have attempted two checks of the specification in order to address this spurious causality supposition.

In columns (3) and (4) of Table 4, I include a control for the number of mandated benefits which were passed by the state by year t-1, to capture any correlation between insurance coverage variation and the propensity to mandate in general. In both the specifications with and without state effects, this variable lowers the coefficients on all of the individual mandates, thereby increasing (in absolute value) the sum. However, the sum remains below its standard error in both cases; while the potential mandate cost index coefficient rises, it is still insignificant at even the 15% level. The coefficient on the number of mandates is positive (but insignificant) in both specifications, indicating that states tend to mandate when the level of insurance coverage is high and rising.

If legislators are responding to changes in insurance status in passing mandates, they may not be responding to contemporaneous changes, but rather to longer-run trends in the extent of coverage in the state. This suggests that including a fixed trend for each state, along with the fixed effect, can control for this response.[19] Column (5) of Table 4 reports the results from a

[19]See Jacobson, Lalonde, and Sullivan (1991) for an application of this type of model. The trend term, which takes the values 1, 2, and 3 in 1979, 1983, and 1988 respectively, is entered linearly in the regression and is interacted with each fixed state effect as well.

model which includes both state effects and state trends. The standard errors rise even further than when state effects alone are included, but the point estimates are similar, indicating no effect of the mandate sum and a small effect of the mandate cost index. This suggests that, if endogeneity is a problem, it is only due to laws which are passed fairly quickly in response to short-run changes in insurance status.

Mandate Waiver Laws

As was mentioned in the introduction, 22 states have responded to the perception that the displacement effect is important by waiving mandates for some small firms.[20] The availability of these "barebones" insurance policies offers a further test of the importance of mandates: if mandates are causing firms not to offer insurance, than the waiver laws should cause a large increase in insurance coverage among small firms. In Table 5, I examine the effect of waiver laws in 12 states on insurance coverage among firms of less than 25 employees. I do so by using the March 1990 and March 1992 CPS, which ask both about coverage by employer provided insurance in the previous year (1989 and 1991) and firm size. The 12 states are those which had passed waivers by July 1, 1991, so that we can observe their effect with the 1992 CPS.[21]

The advantage of the March data is that the sample is somewhat larger, so that I can reduce the confidence intervals in the result with state effects.[22] The disadvantage is that the firm size question changed between the 1990 and 1992 surveys: in 1990, it is establishment size,

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[20]Restrictions are generally placed on firm size (typically less than 25 employees), and the fact that the firm cannot have offered insurance for some period prior to its taking the "barebones" plan (usually one year).

[21]The retrospective insurance question in the March CPS asks whether you were covered by insurance provided by your employer at any point during the previous year, so that this selection rule leaves at least six months for firm coverage decisions to respond to the law. Below I will restrict the sample to look at laws which were in effect for a longer period. The twelve states are: Alaska, Illinois, Iowa, Kentucky, Maryland, Missouri, Montana, New Mexico, North Dakota, Rhode Island, Virginia, and Washington. The results exclude the self-employed for comparability to the Pension supplements, which are surveys of employed workers.

[22]Note that I could not use the March surveys for the earlier part of the analysis because the firm size question was not added until 1988.

and in 1992 it is total firm size (including all locations where the employer operates). However, there is exactly the same fraction of workers who respond that they are in firms/establishments of less than 25 employees in both years (30.9%), so comparability may not be an important problem.

The regression framework is the same as that used above, although there is no data available on union status or tenure with the firm. The dependent variable is once again a dummy for receiving coverage through ones employer. The model therefore is:

$$INSCOV_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 \delta_j + \beta_3 AFTER + \beta_4 WAIVER*AFTER$$

where WAIVER is a dummy for being in a waiver states
AFTER is a dummy which is zero in 1989 and one in 1991

The effect of the waiver laws is identified by the coefficient on the interaction term, β_4. As above, the effect of the waivers can be identified by either a cross-sectional regression for 1991 (where there is no state effects or AFTER control, and the final interaction is simply replaced with a WAIVER dummy), or from a regression for both years which controls for fixed state effects. The latter specification identifies the effect of the waivers from the change in insurance coverage in states which passed the waivers versus those which did not; both results are presented below. All regression are run as probits.

The first column of Table 5 shows the cross-sectional regression for 1991. The coefficient on the waiver dummy is wrong signed (having a waiver is found to lead to less coverage) and insignificant. The second column runs the two year regression with state fixed effects. The coefficient on the interaction is once again negative, but is completely insignificant. Importantly, this estimate is fairly precise; the confidence interval is approximately 2.4% on either side, which is slightly more than one-third of the confidence interval on the sum of the mandates in the state effects specification of Table 3. This suggests that these waiver laws, at most, could have increased insurance coverage among small firms by less than 2%.

One potential problem with these regressions is that six months may not be sufficient time for the laws to have an effect. I address this in column (3) of Table 5, where I drop the states whose laws went into effect after January 1, 1991, so that I only consider states where the waivers were in place for one full year. The result is fairly similar (negative and insignificant).

It is also similar if I replace the waiver dummy with a measure of the number of months that the waiver had been in place by the end of 1991.

A further issue with these regressions is that the waiver laws are not homogenous; they should be expected to have more of an effect in states where more mandates were in place. In column (4), I therefore interact the WAIVER*AFTER interaction with the mandate cost index used above, to capture the extent to which waivers are reducing regulatory burdens. The coefficient is wrong-signed, and is even larger than its standard error; once again, the confidence interval on the estimate is fairly tight. The result is similar if I interact the waiver dummy with a dummy for the presence of a mental illness mandate, or the sum of the individual mandate dummies.

Thus, even this "reverse experiment" rejects an important role for mandates in causing non-insurance among small firms; furthermore, we can now state that the displacement effect is small with more precision once state effects are included.

Firm Offering vs. Worker Coverage

It is important to note that the dependent variable in this analysis is whether a given worker is covered by insurance on their current job. This variable will take a value of zero if either a firm doesn't offer health insurance, or if an individual working for a firm that doesn't offer health insurance turns down coverage. Therefore, if worker takeup rates are responding positively to these mandates, then there may in fact be a negative response among firm coverage decisions which is masked by worker takeup decisions. Unfortunately, only the 1988 CPS supplement asked about both firm offering and worker coverage, so that state effects regressions for firm offering are not possible with this data. Cross-sectional regressions for 1988 reveal that firms are more likely to offer insurance when mandates are present, and that the earlier finding of no effect on worker coverage is due to a negative relation between worker takeup and mandates. However, this may be due to problems in the specification without state effects. When the insurance coverage model with state effects is run over all years for a sample among which takeup is over 90%, married men who are full time workers and who have tenure of at least one year (so that the coverage regressions should approximate an offering regression), the results are similar to those in Table 3.

There is some firm level data available with which this issue can be more directly addressed. The National Federation of Independent Business, a trade organization for small employers, conducted two surveys of small employers, in late 1985 and early 1989. The surveys were roughly similar, although the second had a much larger sample, and a slightly higher response rate.[23] They both collected information on firm size, form of business (ie. corporation versus partnership), insurance coverage, part-time and full-time employment, and gross receipts.

For this analysis, I use all observations in each year which have information on the form of business, industry, and employment, and which have less than 100 employees. The firm is defined as offering insurance if it offers that insurance to any of its employees. The regression model is similar to that used above, but here the controls consist of four dummies for ownership type, nine dummies for major industry, the fraction of the workforce that is part-time, a dummy for having less than 25 employees, and three dummies for the level of gross receipts.

Table 6 reports the results using the NFIB data. Without state effects, there are sizeable individual negative coefficients on drug abuse and continuation of coverage mandates, and on the premium tax rate. However, there is a more sizeable and positive coefficient on mental illness mandates, which is very inconsistent with the CPS findings. As a result, the sum of the mandates is insignificant and fairly small; it implies that imposing this set of mandates would lower coverage by 1%. Furthermore, the mandate cost coefficient is positive and significant.

Once state effects are included, only the alcoholism mandate coefficient is larger than its standard error; it is significant at the 9% level. The sum of the mandates increases, but remains insignificant; mandate cost is now negative, implying an elasticity with respect to potential cost of -0.66, but it is still smaller than its standard error. Unfortunately, the confidence intervals for this data set, once the state effects are included, are simply too large to permit strong conclusions. Nevertheless, the overall results from both columns (1) and (2) are supportive of the CPS findings that mandates do not significantly affect the propensity of firms to offer health

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[23]There are 1439 observations in the 1985 survey, which had a response rate of 19%; there are 5368 observations in 1989, when the response rate was 29%. Dennis (1985) and Hall and Kuder (1989) conclude that the respondents are representative of the small business universe in both surveys. I am grateful to Jeff VanHulle of the NFIB for providing me with these surveys.

insurance.

PART IV: WHY DON'T MANDATES MATTER?

There are three classes of explanations for the finding that mandates do not affect the propensity of small firms to offer insurance. First, it may be that small firms have a low price elasticity of demand. The existing evidence on this key parameter is mixed. Many studies have found very large responses of health insurance coverage to variation in the price of insurance and the tax subsidy to insurance purchase.[24] However, recent evidence from evaluations of subsidies to the purchase of health insurance by small firms in a variety of cities suggests that the effect may be much smaller, with elasticities on the order of -0.2 (Thorpe et al., 1992; Helms et al., 1992). Within this latter range of elasticities, the response of firms to a price increase on the order of 10% of premiums may have been too small to observe in the data.

Second, it may be that employees at insured firms valued these mandates at (close to) their cost to the employer, so that they were willing to pay for that mandate through lower wages.[25] That is, mandates do not represent a simple price rise, but rather a price that is paid for increased quality of health insurance; if that quality is valued at close to its price, then the mandates will have little effect. Employee valuation will be a function of the prevalence of the market failures analyzed above; if there are no market failures, then the fact that the benefit isn't provided ex ante suggests that it isn't valued. The evidence in Gruber (1992a), which evaluated mandated comprehensive coverage for maternity benefits, suggests that, for mandates which benefit demographically identifiable groups, there will be substantial shifting to the wages of that group. It is not clear, however, whether the results extend to mandates, such as mental illness or chiropractic services, which benefit a less readily identifiable portion of the workforce. When the cost of a mandate must be spread out over workers which do not benefit from that mandate, valuation will be less than full, so the majority of workers may in fact prefer to forego insurance

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[24]See Woodbury and Hamermesh (1992) for some recent estimates and references to the past literature; Leibowitz and Chernow (1992) also find large price elasticities.

[25]This argument presumes that there is no impediment to the adjustment of wages to reflect employee valuation, as discussed in Gruber (1992a).

coverage altogether rather than to accept lower wages to pay for the benefit. Unfortunately, the cost of these mandates are so small, as a fraction of wages, that this presumption cannot be investigated empirically.[26]

Finally, it may be that mandates are simply not "binding" on the firm's insurance decision. If most firms, due to employee valuation or other considerations, choose to offer the mandated benefits ex-ante, then the resulting insurance price increase to the average firm from these laws is much lower than the figures reported in Table A1. This supposition is addressed in Table 7, which summarizes data from an employer survey by the Health Insurance Association of America which asked about the provision of several specific benefits to employees.[27] The first panel looks at all firms with 1000 employees or more versus those with less than 100 employees. The figures in square brackets in this panel are t-tests for the difference in the propensity to offer benefits across the firm sizes.

Interestingly, it appears that small firms which do offer insurance include benefits which are nearly as generous as that offered in large firms. For example, while small firms are about 50% less likely to offer insurance (last row), they are only 16% less likely to include alcoholism benefits if they do offer insurance. This evidence is confirmed by data from surveys of small and large firms done by the Bureau of Labor Statistics (BLS), as reported in Employee Benefits Research Institute (EBRI, 1992). They find that small firms are just as likely as larger ones to offer coverage for substance abuse, mental illness, and other benefits such as home health care, routine physicals, well baby care, and even organ transplants.

In panel (2), I show that small firms are relatively generous <u>even in states without mandates for these benefits</u>, so that the finding is not an artifact of firm location. The "Barebones" row presents the mean of a variable which is equal to one if the firm does not offer any of these benefits, and is equal to zero if it offers any of them. This figure is quite low for

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[26]Health insurance costs accounted for approximately 10% of compensation in 1988 (U.S. Chamber Research Center, 1989), so that the wage offset for a 10% rise in costs would be too small to be estimated.

[27]I am grateful to Cynthia Sullivan for providing me with the HIAA data. I focus on the three high cost mandates for which data are available.

small firms, even in states without any of these mandates. With the rate of coverage for these expensive benefits in small firms in the unregulated equilibrium being so high, mandates should not be expected to have much of an influence on the average price of insurance. Furthermore, the "barebones" policies offered in the waiver states simply may not have much of a market, which would account for the lack of a response to these waivers uncovered above.

In fact, as the next panel shows, firms in states with mandates do not seem to offer these benefits much more frequently that those in states without mandates. The figures in square brackets in panel (3) are t-tests for whether a given benefit is offered more frequently in states with a mandate for that benefit, for a given firm size. While coverage does generally go up when mandates are in place, the difference for small firms is only significant for mental illness. Furthermore, the presence of all of these mandates does not significantly reduce the likelihood of barebones policies being offered by firms of either size.

One problem with interpreting these results, however, is that the benefits do not appear to be universally offered in states where they are mandated. There are several possible reasons for this finding. First, firm location is assigned by the firm's headquarters, and the reported benefits may be for a plan which is in place in another state; this should not be a major issue for small employers. Second, mandate legislation is often unclear on whether the regulations are limited to policies purchased from in-state insurers, so that firms which purchase their insurance from out-of-state insurers may not be subject to the mandates. Third, this result could be due to firms which self-insure, since these firms are not subject to mandates; as I will show in a moment, this is not likely to be the cause.[28] Finally, it could be due to reporting error in this data on benefit coverage, or to noncompliance with the mandates by small firms (and their insurers).

To the extent that this problem is due to issues of locational definitions, an alternative means of identifying whether mandates are "binding" is to compare firms which self insure to

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[28]Furthermore, the results are very similar if restricted to firms which don't self insure. However, while most small firms do not self-insure directly, they may belong to voluntary pools which do self-insure. Leibowitz et al. (1992) reports that as many as 10% of small firms may belong to multi-employer associations; whether such associations are subject to state regulation is unclear.

those which do not. As panels (4) and (5) of Table 7 show, firms which self-insure are in fact no less likely to offer benefits than those which do, and may be more likely in some cases. This provides further evidence that mandates are not binding, since self insured firms are not subject to the mandates, but seem to offer generous benefits regardless. However, the disadvantage of this approach is that not many small firms self insure, so that the statistical reliability of the conclusions for this group are limited.[29]

The last row of Table 7 examines the effect of mandates on firm insurance decisions. Insurance coverage seems to be higher for small firms in states with all of these mandates than in states with none of the mandates; however, the difference is not statistically significant. This is roughly consistent with the regression findings using the CPS data reported in Part III.

Table 7 therefore presents some tentative evidence that state mandates are not binding, which would explain why insurance coverage does not appear to be any lower in the presence of mandates. It is not obvious why small firms would choose to either not offer health insurance, or to offer policies which are quite generous; one possible explanation is offered in the conclusions.

However, these data have some important weaknesses. First, there is the problem that the reported benefit coverage is not universal in states where it is mandated, which suggests the potential for important reporting error in the survey itself. Second, even if unregulated firms offer benefits which parallel these mandates, the levels of their benefits may be less generous than the mandated minimums. That is, even if mandates are not binding in terms of benefits offered, they may bind in terms of the level of these benefits, so that the mandates will cause some increase in the average price of insurance. The BLS survey, which asks about the details of health insurance plans, offers some evidence for this interpretation, by showing that smaller firms are less generous in the details of their health benefits. While only 66% of larger firms limit the amount of coverage for outpatient mental illness benefits, 77% of small firms do; and while 43% of large firms cover alcohol treatment as generously as other illnesses, only 28% of small firms do (EBRI, 1992).

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[29]There are only about 20 firms with less than 100 employees that self insure, which is 7% of the small firm sample.

Finally, these results are based on a single cross-section, and do not control for differences in the composition of firms in states with and without mandates. As a result, they may be confounded by other factors in the states without mandates which led firms in those states to both offer generous benefits and more frequent health insurance coverage.[30] In the next section, I therefore attempt to corroborate the conclusion that mandates do not affect insurance coverage by focusing on utilization of mental health resources.

Mental Health

As demonstrated by the RAND Health Insurance Experiment (Manning et. al, 1987), demand for medical services is sensitive to the generosity of insurance coverage of those services. Given this non-zero elasticity, if mandates do substantially increase the level of insurance coverage for particular illnesses, then their passage should be reflected in the utilization of medical resources which are typically used in treating that illness. Thus, a further test of whether mandates are binding is to look directly at medical utilization; I do so in Table 8 for mental illness mandates, which were found to have the largest effect on insurance coverage in the earlier regressions.

In column (1), I use state level data on total expenditures on "specialty mental health organizations", for the years 1983 and 1986, from the National Institute of Mental Health (1990). This includes spending on outpatient mental health clinics, psychiatric hospitals, residential treatment centers for emotionally disturbed children, multiservice mental health organizations, and psychiatric units of general hospitals; importantly, it excludes spending on office based psychiatric care. Over this period, four states passed mental health mandates: Maine, North Dakota, Montana, and Oregon. I have selected several states from the same region as each of these states as controls (for a total of 17 states), and I regress log mental health expenditures on log population, a dummy for a mental health mandate, a year dummy for 1986, and a set of state dummies. There is no effect of the mental health mandate on expenditures; the result is the same if the state dummies are excluded.

Of course, the major effect of these mandates may have been on office-based mental

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[30]Similarly, those small firms which self-insure may be "better" along unobserved dimensions which lead them to offer generous health insurance.

health care, for which the NIMH did not collect data.[31] While there is no data available over time on expenditures on office-based care by state, I have collected data from 1970 to the present on the number of members of the American Psychological Association (APA) by state (APA, various). The APA is the major national trade organization of psychologists, and 81% of licensed or certified psychologists in the United States were members in 1983 (Stapp et al., 1985). This data includes APA members who are both providers and non-providers, but 72% of members were providers in 1983, and there was little variation across the states in the fraction of psychologists providing services (Stapp et al., 1985; Dial et al., 1990).

In column (2), I use the same set of states as in column (1), but with the expanded set of years; once again, the regression includes state effects. There is a striking increase in the number of APA members in the states which passed mental illness mandates; a mandate is associated with a 9% rise in the number of psychologists. In column (3), I expand the sample to include all of the states over this period; the result is somewhat weakened, but remains highly significant. This suggests, in contrast to the findings of Table 7 and column (1) of Table 8, that mandates are having an effect.

One problem with this result, however, is that the passage of mandates may be endogenous to the change in the number of psychologists; states where the psychologist population was growing exogenously may have had the greatest propensity to mandate. For example, Lambert and McGuire (1990) find that increased lobbying pressure from psychologists was associated with the passage of mental illnesss mandates in the mid-1970s. To the extent that an increase in the psychologist population is associated with an increase in lobbying pressure in a given state, this could yield a spurious positive correlation in regressions such as that in column (3).[32] As in Table 4, I test for this by introducing state-specific trend terms, as well as

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[31]As McGuire and Montgomery (1982) point out, an increase in the use of office-based psychotherapy would be a natural result of the "offset" effects of expanded mental illness coverage. However, they use a cross-section of data for 1978 to show that mental illness mandates are not significantly related to the number of fee-for-service hours delivered by psychologists in the state.

[32]Note that this is <u>not</u> inconsistent with my assumption of exogenous changes in laws in the insurance coverage section: the legislation can be endogenous with respect to provider pressure

state fixed effects, in the regression, to control for the within-state trend growth in the number of providers. If mandates are causing an increase in the number of providers (rather than vice-versa), then this increase should emerge relative to the state-specific trend. Instead, however, the mental illness mandate coefficient becomes negative and insignificant in this specification. This suggests that the findings of columns (2) and (3) may have been an artifact of the endogeneity of mental illness mandates to the pressure of a growing number of providers, confirming the conclusions of Lambert and McGuire.

PART V: CONCLUSIONS

The results in this paper provide fairly strong evidence that state mandated health insurance benefits are not an important cause of the low rate of health insurance coverage among small firms. This conclusion emerged from a variety of specifications of the effects of the mandates, and was robust to the inclusion of state effects and other controls for potential legislative endogeneity in the insurance coverage regressions. It was also shown to be more robust than other findings in the literature which implied a large role for mandated benefits. Furthermore, there is some evidence that this lack of an effect of mandates on firm insurance decisions is due to the fact that mandates are not binding; however, these findings are of a more tenuous nature, and I am unable to rule out alternative explanations for the lack of a displacement effect.

An important caveat to my findings is that the standard errors on my estimates are large (especially when state effects are included) so that fairly large displacement effects of mandates are within the estimated confidence intervals. I attempted to address this problem by looking at the "reverse experiment" represented by the recent mandate waiver laws. These results confirmed the lack of an effect of mandates on coverage, and increased the precision with which this conclusion can be drawn. At a minimum, these results, together with the specification problems and fragility of previous findings, suggest that the burden of proof remains with those who claim that mandates do affect the firm's decision to offer insurance.

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but still not be a function of changes in insurance coverage.

This analysis raises three important questions. First, if mandates do not matter, why is there so much legislative interest in them, both in terms of provider lobbying for their passage, and insurer lobbying for their repeal? A study of the legislative process behind the fight over mandated benefits would be useful in understanding why there is so much interest in such an apparently (from the evidence presented here) unimportant issue.

Second, why do small employers seem to either not offer health insurance coverage, or to offer coverage which is nearly as generous as that offered by larger firms (Table 7)? This equilibrium may be a function of the difficulty of distinguishing episodes of certain types of care from others; for example, it may not be possible for insurers to separate hospitalization for treatment of alcoholism from hospitalization for other reasons. Small firms which do not cover alcoholism in their insurance policies may thus try to "relabel" such treatment into some other category which is covered in their plan. In this case, it is optimal for insurers to insist that, if small firms insure at all, they include these benefits. Evidence on the availability of "barebones" policies in the small firm market would help to resolve this question.

Third, if state mandates are not the culprit, why is there such a low rate of insurance coverage among small firms? One obvious reason is the higher price of insurance for small firms; loading factors (the difference between what is collected in premiums and what is paid out in claims) are eight times as large for a firm of five employees as for a firm of 10,000 employees (CRS, 1988c). However, unless there is a very high price elasticity of demand for insurance among small firms, this price differential cannot explain the coverage differential observed in Table 1; at the maximum price elasticity obtained by Thorpe et al. (1992), the loading factor differential can only explain about 20% of the coverage differential between the very largest and very smallest firms. Sorting out the difference between the recent very small "experimental" price elasticities and the higher elasticities found in the earlier literature should be a high research priority.

A further reason for low rates of coverage may be that the workers in small firms do not desire coverage, even at lower prices. Long and Marquis (1992) find that workers in firms that do not offer insurance are very similar, in terms of demographic characteristics, to workers who do not takeup insurance coverage when it is offered by their firm. This "worker demand" problem is exacerbated by insurance industry practices which are designed to mitigate adverse

selection within the small firm. Insurance companies will often insist that between 85 and 100% of employees in a small firm participate if the firm chooses to insure (CRS, 1988b). Since the likelihood of an employee turning down insurance coverage is almost twice as high in a firm of less than 100 employees as in a firm of more than 500 employees, this restriction may lead to many small firms not offering health insurance at all.[33]

There are at least two other reasons why small firms may be less inclined to offer insurance. First, the health insurance tax subsidy is of less value to small firms since unincorporated businesses and subchapter S corporations cannot deduct the full cost of health insurance premiums on their taxes, but rather only 25% of those costs; and because small firms may be less likely to have any taxable income against which these costs can be deducted. Second, small businesses are simply more volatile than large firms. The birth rate (number of new business started over average number of existing businesses) for single establishment firms of 1-4 employees was 36% between 1976 and 1982, and the death rate was 25%. For firms of 500 employees or more, the birth rate was 5% and the death rate was 7% (SBA, 1987, p.125). If start up costs are the bulk of the administrative burden of health insurance, or if new firms have less resources available with which to buy health insurance, then higher volatility will be tied to lower rates of health insurance coverage.[34]

Clearly, further research is needed on the important question of why insurance coverage rates are so low among small firms. This study suggests that such research must look beyond the role of state regulation of insurance in explaining this phenomenon.

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[33]Based on authors tabulations of May 1988 CPS supplement. The major reason for turning down health insurance coverage offered by the employer is other coverage from elsewhere, such as a spouse.

[34]The 1989 NFIB survey of owners of small firms included a question about the length of ownership, which can be taken as a rough proxy for firm age. Controlling for the size and composition of the workforce, gross receipts, ownership, and industry (see regression model in Table 6), there is a strong positive relationship between years of ownership and the probability of offering insurance: one more year of ownership is associated with a 0.2% increase in the probability of offering insurance.

Table 1: Insurance Coverage and Firm Size - Survey Results				
Column	(1)	(3)	(4)	(5)
Survey	SBA (1986)	NFIB (1989)	HIAA (1989)	CPS (1988)
1-9 employees	46%	49%	32%	41%
10-24 employees	78%	77%	74%	67%
25-99 employees	92%	88%	93%	88%
100-499 employees	98%	N/A	97%	93%
500+ employees	100%	N/A	100%	93%

Notes:

1) Figures in columns (1)-(4) are percentage of firms offering health insurance to at least some of their employees; figure in column (5) is percentage of employees reporting that their firm offers health insurance.
2) Data are from ICF (1987), tabulations from National Federation of Independen Business data for 1989, and Health Insurance Accociation of America data for 1989 (see text for a discussion of these surveys); and tabulations from May 1988 CPS Pension Supplement for 1988.
3) NFIB survey data is not reliable for firms with more than 100 employees.

Table 2: Means of CPS Data				
Sample	All	>999 workers	<100 workers	<25 workers
Age	36.4	37.4	35.1	34.7
% Female	45.9	42.1	46.4	47.1
% Married	63.3	66.6	60.0	58.3
% Non-white	9.9	10.3	7.8	7.6
% Union	23.8	35.0	9.3	5.2
Tenure	6.74	8.97	4.78	4.43
% Tenure < 1 year	21.1	13.5	28.1	30.8
Yrs of Education	12.8	13.3	12.4	12.3
% Covered by Insurance	67.6	84.6	46.5	36.1
% Part-time (<35 hrs/wk)	15.7	10.1	21.9	25.9
% w/ alcohol mandate	39.2	41.9	36.1	34.5
% w/ drug abuse mandate	14.4	14.9	14.7	14.4
% w/ mental illness man.	16.9	17.7	16.2	15.4
% w/ chiropractor mandate	78.0	79.8	76.6	75.9
% w/ cont of cov mandate	40.1	45.7	34.1	28.8
Premium Tax Rate (%)	1.16	1.14	1.18	1.19
Mandate Cost (% of premiums)	4.71	4.57	4.47	4.26
Number of mandates in state	11.7	12.1	11.3	11.2
Number of Obs	65912	22801	22910	14986

Notes:

1) Based on tabulations from 1979, 1983, and 1988 May CPS Supplements. See text for details.
2) Mandate cost is calculated as described in text.
3) Number of mandates in state is from Scandlen Publishing (1991).

Table 3: Basic Results - Firms with less than 100 employees
Dependent Variable is dummy for insurance coverage on the job

Column	(1)	(2)	(3)	(4)	(5)	(6)
Model	Linear Prob	Probit	Marg Prob	Linear Prob	Probit	Marg Prob
Alcoholism Mandate	0.0109 (0.0078)	0.0379 (0.0273)	0.0107	0.0056 (0.0186)	0.0264 (0.0654)	0.0074
Drug Abuse Mandate	-0.0056 (0.0096)	-0.0194 (0.0338)	-0.0055	0.0106 (0.0242)	0.0469 (0.0854)	0.0132
Mental Illness Mandate	-0.0130 (0.0083)	-0.0452 (0.0291)	-0.0127	-0.0294 (0.0246)	-0.1102 (0.0868)	-0.0309
Chiropractor Mandate	-0.0003 (0.0066)	0.0001 (0.0232)	0.00003	-0.0103 (0.0175)	-0.0126 (0.0610)	-0.0036
Continuation of Coverage Mandate	0.0055 (0.0074)	0.0156 (0.0260)	0.0044	-0.0079 (0.0106)	-0.0301 (0.0371)	-0.0085
Premium Tax Rate	0.0010 (0.0048)	0.0014 (0.0167)	0.0004	0.0087 (0.0129)	0.0387 (0.0448)	0.0109
Sum of Mandates	-0.0013 (0.0143)	-0.0095 (0.0501)	-0.0027	-0.0225 (0.0323)	-0.0411 (0.1135)	-0.0116
Mandate Cost	-0.0005 (0.0010)	-0.0023 (0.0034)	-0.0006	-0.0020 (0.0022)	-0.0062 (0.0076)	-0.0017
Age	0.0108 (0.0015)	0.0387 (0.0054)	0.0109	0.0108 (0.0015)	0.0390 (0.0054)	0.0110
Age squared (times 100)	-0.0142 (0.0019)	-0.0506 (0.0067)	-0.0143	-0.0143 (0.0019)	-0.0511 (0.0068)	-0.0144
Education	0.0207 (0.0011)	0.0702 (0.0039)	0.0199	0.0208 (0.0011)	0.0709 (0.0040)	0.0200
Tenure	0.0207 (0.0013)	0.0674 (0.0047)	0.0191	0.0207 (0.0013)	0.0679 (0.0047)	0.0191
Tenure squared (times 100)	-0.0405 (0.0044)	-0.1275 (0.0158)	-0.0360	-0.0408 (0.0044)	-0.1287 (0.0159)	-0.0362
Tenure less than one year	-0.0990 (0.0075)	-0.3430 (0.0264)	-0.0993	-0.0984 (0.0075)	-0.3429 (0.0266)	-0.0988
Non-White	-0.0498 (0.0105)	-0.1746 (0.0373)	-0.0492	-0.0534 (0.0107)	-0.1854 (0.0381)	-0.0519
Female	0.0214 (0.0091)	0.0822 (0.0323)	0.0231	0.0206 (0.0091)	0.0802 (0.0324)	0.0224

Table 3, Continued						
Column	(1)	(2)	(3)	(4)	(5)	(6)
Married	0.0556 (0.0085)	0.1622 (0.0290)	0.0461	0.0549 (0.0085)	0.1632 (0.0292)	0.0461
Married*Female	-0.1715 (0.0114)	-0.5367 (0.0399)	-0.1522	-0.1706 (0.0114)	-0.5361 (0.0400)	-0.1514
Union	0.1953 (0.0101)	0.6879 (0.0375)	0.1961	0.1970 (0.0102)	0.6966 (0.0379)	0.1976
Part-Time	-0.2498 (0.0074)	-0.9270 (0.0286)	-0.2682	-0.2505 (0.0074)	-0.9337 (0.0287)	-0.2689
Less than 25 employees	-0.1743 (0.0062)	-0.5620 (0.0212)	-0.1662	-0.1740 (0.0063)	-0.5629 (0.0215)	-0.1657
State Dummies	No	No		Yes	Yes	
Number of Obs	22879	22879		22879	22879	

Notes:

1) Numbers in parentheses are standard errors.
2) Dependent variable is a dummy which is equal to one if the individual is covered by insurance on the job and zero if he/she is not covered.
3) Regressions are run as unweighted linear probability models in columns (1) and (4), and as probits in columns (2) and (5); columns (3) and (6) interpret the probit coefficients as changes in probabilities (see text for description).
4) Coefficients on the demographic covariates are from the regression which is run with the set of mandates dummies.
5) Sum of Mandates is the t-statistic for the test that the sum of the mandate dummy coefficients is greater than zero.
6) Mandate Cost is a linear combination of the presence of the set of "expensive" state mandates, where the weights are the potential premium cost of the mandates. This coefficient is from a distinct regression. See text for discussion.
7) All regressions include a set of 21 major industry dummies and 2 year dummies.

Table 4: Specification Checks

Specification	<25 ees	<25 ees	# of Mandates	# of Mandates	Fixed Trend
Alcohol	0.0343	0.0892	0.0252	0.0260	0.0086
	(0.0339)	(0.0830)	(0.0290)	(0.0654)	(0.1181)
	[0.0100]	[0.0260]	[0.0071]	[0.0073]	[0.0024]
Drugs	-0.0050	-0.0131	-0.0172	0.0371	-0.2245
	(0.0422)	(0.1065)	(0.0339)	(0.0869)	(0.1640)
	[-0.0014]	[-0.0038]	[-0.0048]	[0.0104]	[-0.0626]
Mental Illness	-0.0142	-0.1160	-0.0536	-0.1228	0.1687
	(0.0366)	(0.1085)	(0.0298)	(0.0893)	(0.1692)
	[-0.0042]	[-0.0334]	[-0.0151]	[-0.0345]	[0.0474]
Chiropractor	0.0124	-0.0253	-0.0122	-0.0213	0.0247
	(0.0283)	(0.0757)	(0.0250)	(0.0627)	(0.1143)
	[0.0036]	[-0.0074]	[-0.0034]	[-0.0060]	[0.0069]
Continuation of Coverage	0.0381	0.0095	0.0092	-0.0325	-0.0490
	(0.0306)	(0.0484)	(0.0265)	(0.0373)	(0.0446)
	[0.0111]	[0.0028]	[0.0026]	[-0.0092]	[-0.0137]
Premium Tax	-0.0150	0.0428	-0.0006	0.0386	0.0760
	(0.0206)	(0.0555)	(0.0168)	(0.0448)	(0.0854)
	[-0.0044]	[0.0125]	[-0.0002]	[0.0109]	[0.0214]
Sum of Mandates	0.0507	-0.0129	-0.0492	-0.0750	0.0046
	(0.0607)	(0.1415)	(0.0586)	(0.1266)	(0.2184)
	[0.0148]	[-0.0038]	[-0.0139]	[-0.0211]	[0.0013]
Mandate Cost	0.0037	-0.0009	-0.0052	-0.0084	-0.0100
	(0.0042)	(0.0097)	(0.0038)	(0.0083)	(0.0121)
	[0.0011]	[-0.0003]	[-0.0015]	[-0.0024]	[-0.0028]
# of Mandates			0.0034	0.0040	
			(0.0026)	(0.0066)	
			[0.0010]	[0.0011]	
State Effects	No	Yes	No	Yes	Yes

Notes:

1) Standard errors in parentheses; marginal probabilities in square brackets.
2) Columns (1)-(3) run for employees in firms with less than 100 workers. Columns (1) and (2) include the total number of mandates in the state/year.
3) Column (3) controls for a trend for each state, as well as a fixed state effect.

Table 5: "Reverse Experiment" Using the Waiver Laws
Dependent Variable is a dummy for insurance coverage on the job

Column	(1)	(2)	(3)	(4)
Specification	Cross-Section	Both Years	Waiver > 1 Year	Waiver*Cost of Mandates
After Dummy		-2.3030	-2.3348	-2.2983
		(0.0960)	(0.0989)	(0.0959)
Waiver State	-0.0353			
	(0.0314)			
	[-0.0052]			
Waiver*After		-0.0312	-0.0138	
		(0.0435)	(0.0561)	
		[-0.0090]	[-0.0090]	
Waiver*After* Cost of Mandates				-0.0084
				(0.0061)
				[-0.0024]
Age	0.0453	0.0494	0.0481	0.0494
	(0.0065)	(0.0045)	(0.0047)	(0.0045)
Age squared (times 100)	-0.0358	-0.0432	-0.0415	-0.0433
	(0.0080)	(0.0056)	(0.0058)	(0.0055)
Education	0.0915	0.0837	0.0849	0.0837
	(0.0051)	(0.0034)	(0.0036)	(0.0035)
Non-White	-0.2468	-0.2364	-0.2169	-0.2362
	(0.0421)	(0.0300)	(0.0306)	(0.0300)
Female	0.0600	0.0208	0.0359	0.0207
	(0.0384)	(0.0270)	(0.0280)	(0.0270)
Married	0.1300	0.1150	0.1224	0.1151
	(0.0341)	(0.0240)	(0.0250)	(0.0240)
Married*Female	-0.5712	-0.5126	-0.5190	-0.5125
	(0.0481)	(0.0338)	(0.0351)	(0.0338)
Part-Time	-0.9005	-0.9162	-0.9172	-0.9163
	(0.0326)	(0.0228)	(0.0238)	(0.0228)
State Effects	No	Yes	Yes	Yes
Number of Obs	15847	32060	29321	32060

Notes:

1) Standard errors in parentheses; marginal probabilities in square brackets.
2) All regressions run as probits.
3) First column uses data for 1991 only; second column uses 1989 and 1991.
4) Sample is all firms with less than 25 employees in columns (1),(2), and (4); in column (3), the sample excludes those states with waivers passed during 1991.

Table 6: Insurance Coverage Regressions - NFIB Data		
Column	(1)	(2)
Percent Part-Time	-0.7124 (0.0644)	-0.7250 (0.0653)
Gross Receipts < 100,000	-1.4072 (0.0874)	-1.3954 (0.0883)
Gross Receipts < 750,000	-0.8082 (0.0680)	-0.8012 (0.0687)
Gross Receipts < 1.5 million	-0.3942 (0.0756)	-0.3819 (0.0764)
< 25 Employees	-0.3261 (0.0628)	-0.3457 (0.0665)
Alcoholism Mandate	0.0292 (0.0560) [0.0084]	-0.4346 (0.2519) [-0.1212]
Drug Abuse Mandate	-0.0869 (0.0536) [-0.0251]	0.1545 (0.2196) [0.0435]
Mental Illness Mandate	0.1657 (0.0507) [0.0473]	0.1928 (0.5498) [0.0542]
Chiropractor Mandate	0.0381 (0.0542) [0.0110]	0.0297 (0.2313) [0.0085]
Continuation of Coverage Mandate	-0.0600 (0.0438) [-0.0172]	-0.0805 (0.0821) [-0.0228]
Premium Tax Rate	-0.1351 (0.0330) [-0.0395]	-0.1382 (0.1161) [-0.0399]
Sum of Mandates	-0.0490 (0.1016) [-0.0141]	-0.2764 (0.5208) [-0.0779]
Mandate Cost	0.0111 (0.0059) [0.0032]	-0.0230 (0.0246) [-0.0066]
State Effects	No	Yes

Notes:
1) Standard errors in parentheses.
2) Data are from 1985 and 1989 NFIB surveys; see text for discussion.
3) Dependent variable is a dummy which is equal to one if the firm offers insurance to some of its employees, and zero otherwise.
4) Model also includes 9 industry dummies, 4 form of business dummies, and a year dummy.
5) Both regressions have 6187 observations.

Table 7: Details of Firm Health Insurance Benefits - HIAA Data

	(1) All States		(2) States Without Mandates		(3) States With Mandates		(4) Self Insured Firms		(5) Non-Self Insured Firms	
Firm Size	>999	<100	>999	<100	>999	<100	>999	<100	>999	<100
Alcoholism	0.962	0.799	0.943	0.800	0.975	0.799	0.957	0.800	0.964	0.808
	(0.007)	(0.022)	(0.008)	(0.034)	(0.008)	(0.030)	(0.011)	(0.092)	(0.010)	(0.024)
		[-6.922]		[-4.119]	[3.002]	[-0.022]		[-1.700]	[0.472]	[0.084]
Drug Abuse	0.945	0.785	0.938	0.771	0.967	0.825	0.950	0.773	0.943	0.790
	(0.008)	(0.023)	(0.010)	(0.027)	(0.013)	(0.043)	(0.012)	(0.091)	(0.012)	(0.025)
		[-6.567]		[-5.794]	[1.726]	[1.070]		[-1.918]	[-0.412]	[0.179]
Outpatient Mental Illness	0.965	0.818	0.960	0.798	0.982	0.886	0.960	0.909	0.969	0.807
	(0.007)	(0.021)	(0.008)	(0.025)	(0.010)	(0.036)	(0.011)	(0.063)	(0.009)	(0.024)
		[-6.728]		[-6.242]	[1.650]	[2.018]		[-0.802]	[0.639]	[-1.523]
Inpatient Mental Illness	0.990	0.913	0.988	0.899	1.000	0.961	0.994	1.000	0.987	0.901
	(0.004)	(0.015)	(0.005)	(0.018)	(0.000)	(0.022)	(0.004)	(0.000)	(0.006)	(0.018)
		[-4.909]		[-4.672]	[2.602]	[2.130]		[1.365]	[-0.966]	[-5.542]
Barebones	0.003	0.039	0.007	0.039	0.000	0.043	0.000	0.000	0.005	0.041
	(0.000)	(0.010)	(0.005)	(0.016)	(0.000)	(0.030)	(0.000)	(0.000)	(0.004)	(0.012)
		[3.477]		[1.952]	[-1.419]	[0.119]		[0.000]	[1.366]	[3.557]
Offer Insurance	0.999	0.481	1.000	0.470	1.000	0.535				
	(0.001)	(0.015)	(0.000)	(0.023)	(0.000)	(0.047)				
		[-33.77]		[-23.08]	[0.000]	[1.244]				

Notes:

1) Based on tabulations for 1989 HIAA survey of firms.
2) Figures on top of each cell for benefits (ie. alcoholism) are percentage of firms in that cell that offer that benefit (given that they offer health insurance at all).
3) "Barebones" is the fraction of firms in that cell that offer none of the above benefits (compared to firms that offer at least one).
4) "Offer insurance" is the fraction of firms in that cell that offer insurance.
5) For both "barebones" and "offer insurance", states with mandates are states with all of the mandates, and states without mandates are states without any of the mandates.
6) Figures in parentheses are standard errors; figures in brackets in panels (1), (2), and (4) are t-tests for difference in means relative to large firms; figures in brackets in panel (3) are t-tests for difference in means relative to same size firms in states without mandates; in panel (5), they are for difference from firms which self insure.

Table 8: Effects of Mental Health Mandates				
	Log Total Expenditures '84-'85 Mandates	Log APA Psychologists '84-'85 Mandates	Log APA Psychologists All Mandates	Log APA Psychologists All Mandates
Mental Illness Mandate	-0.0056 (0.0818)	0.0920 (0.0317)	0.0554 (0.0203)	-0.0125 (0.0222)
Log Population	1.1793 (1.1781)	0.8196 (0.1116)	1.1286 (0.0656)	1.2524 (0.1579)
Controls	State Effects	State Effects	State Effects	State Effects State Trends

Notes:

1) Standard errors in parentheses.
2) Dependent variable in column (1) is log total expenditures on non-office based mental health for 1983 and 1986. Data are for four regions, with one mental health mandate per region: Atlantic (Maine had mandate in '84; Vermont, New Hampshire, and Massachusetts as controls); Midwest (North Dakota had mandate in '85; South Dakota, Minnesota, Iowa, and Nebraska as controls); Mountain (Montana had mandate in '84; Colorado, Idaho, Nevada, and Wyoming as controls); and West (Oregon had mandate in '84; Washington and California as controls).
3) Dependent variable in columns (2)-(4) is log APA registered psychologists, for the years 1970, 1973, 1975, 1978, 1980, 1982, 1985, 1988, and 1991.
4) In column (2), the set of experimental and control states used are the same as in column (1); in columns (3)-(4), all states are used.
5) Columns (1)-(3) include fixed state effects. Column (4) includes state-specific trends as well. All columns include a full set of year effects.

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APPENDIX A

Table A1 below presents cost estimates for the set of five expensive mandates which will be the focus of this study; these costs will be used to form the weights on the "potential mandate cost" index. Unfortunately, these states do not each have the full set of mandates, nor is the mandate cost for each benefit presented distinctly in each case. However, estimates of the costs of these mandates can be based on comparisons across the states. For alcohol and drug abuse, the Virginia estimate is that the sum of the two is 2% of costs, while Maryland finds that alcohol alone adds 1% to costs, so that I give each of these mandates a weight of 1%.

Mental illness is the most common mandate. It amounts to about 7.5% of costs in Virginia and Maryland, and at least that in Massachusetts (given the estimated cost of alcohol above). However, the total for mental, drugs, and alcohol in Wisconsin is only 5%, and the CRS study, which is the only one to marginally price the premiums, finds that the costs of adding mental illness coverage are less than 5%. Thus, as a compromise, I use a weight of 5% on mental illness. The weighting on chiropractor mandates represents a simple average of the estimates from the Virginia, Massachusetts, and Wisconsin studies.

The weighting on continuation of coverage mandates comes from data in Employee Benefit Plan Review (1990). They report that the average continuation of coverage claim costs 152% of the average claim for active employees, that 8.1% of employees had events during the year which qualified them for coverage continuation, and that 24.6% of qualified individuals chose to take up the continuation. At these costs and frequencies, a continuation of coverage option raises the average claims costs of a firm by about 3%. Finally, premium tax rates are simply added to the cost, since they are expressed as a fraction of premiums.

Table A1: The Cost of the Mandates

	Virginia	Massachusetts	Maryland	Wisconsin	CRS	Huth
Total Mandate Cost	22%	18%	22%			
Alcohol			0.9			
Drugs						
Alcohol + Drugs	2.30					
Mental	7.51		7.8		4.2	
Alcohol + Mental		10.30				
Alcohol + Mental + Drugs				4.82		
Chiropractor	0.74	2.1		1.67		
Continuation of Coverage	1.51					3.0

Notes:
1) Data sources are described above and in the text.
2) Figures are percent of claims dollars in each of the categories for columns (1)-(4).

Table A2: Dates of Passage of the Mandates

State	Alc	Drug	Mental	Chiro	CCov	State	Alc	Drug	Mental	Chiro	CCov
AK	89	89		84		MT	84	84	84	67	
AL				75		NC				77	82
AR				71	79	ND	85	85	85		83
AZ				83		NE				75	81
CA				69	85	NH			76	69	81
CO			76	75	86	NJ	77				
CT	77		71	89	75	NM				73	83
DE				63		NV	85	85		75	88
FL				74		NY	81	88		73	
GA				80	86	OH	79		79	69	84
IA					84	OK				71	76
ID						OR	84	84	84		81
IL	72			69	84	PA	86			71	
IN				74		RI	80	88		68	77
KS	78	78	78	73	78	SC					79
KY				86	80	SD				70	84
LA				75		TN				81	81
MA	76		76	85	77	TX	86	90		77	86
MD	81	79	73	74	83	UT				75	86
ME	84	84	84	81		VA	78	78	77	73	86
MI	82	82		68		VT	86				
MN	78	78		73	74	WA	75	75		71	
MO	77			76	85	WI	75	75	71	88	80
MS	75			80		WY					
						Total	24	12	13	38	29
						Δ 79-87	13	7	5	8	22

Notes:

1) Numbers for each state are dates of passage of mandates.
2) Total is total number of states which passed the mandate for that column before 1988.
3) Δ 79-87 is total number of states which passed the mandate between 1979 and 1987.

APPENDIX B: COMPARISON WITH PREVIOUS STUDIES

Both Goodman and Musgrave (1988) and Jensen and Gabel (1990) have also studied the effects of state mandates on insurance coverage, and they reach different conclusions than those drawn here. In this appendix, I attempt to reconcile the estimates.

Goodman and Musgrave

The dependent variable for this study is the fraction of the non-elderly population which is uninsured in each state, as reported by Chollet (1988). The independent variable of interest is the total number of mandates (mandated benefits and mandated offerings) passed by the state through 1986, from tabulations by the Blue Cross/Blue Shield Association.[1] The authors' model also controls for: the state unemployment rate; several dummies for state regulation of insurance; per capita gross state product; per capita state indirect business taxes; and the distribution of state product across 13 major industries.[2] Their regressions are weighted by total state population. As column (1) shows, they find a significant positive effect of mandates; at mean values, this finding implies that mandates can explain about 25% of non-insurance.

There are three important problems with these results. First, the dependent variable, the percent uninsured, is not the appropriate one for studying laws which should only affect the propensity of employers to provide insurance. Interaction between mandate policy and other policies which affect the uninsured, such as Medicaid eligibility, may bias the results; this problem could be especially important in a cross-sectional framework, which does not control for time-invariant state characteristics. Second, the independent variable of interest, the number of mandates, has little meaning in terms of regulatory influence on insurance prices. Most mandated benefits have a trivial potential cost: of the 17 mandates studied in Virginia by KPMG Peat Marwick (1989), 6 account for less than 0.1% of claims costs each, and only 5 account for more than 1% of claims costs.

Finally, their results seem to be very sensitive to the details of their specification. In column (2) of Table B1, I replicate their regression, using my own tabulation of the total number of mandates in the state.[3] The top row reveals a coefficient which is slightly smaller than that reported by the authors, but which is also significant (at the 7% level). In the second row, I replace the total number of mandates with the number of mandated benefits; as described in the

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[1]Besides the cross-sectional 1986 regression, the authors also report two other regressions; since the 1986 regression yields the strongest results, as well as the conclusion that seems to be most often quoted, I focus on it in this comparison.

[2]Table B1 does not report these other controls, but they are included in all of the models.

[3]I am grateful to Mr. Musgrave for providing me with the data used in their study. My tabulations of the total number of mandates come from the same source used by the authors, although I obtain somewhat different numbers. I also exclude Hawaii from my regressions, since that state has mandated health insurance for all employers, so there should be no displacement effect of mandates; this does not much affect the results.

text, it does not seem likely that mandated offerings affect the firms decision to insure.[4] The coefficient is halved when the number of benefits is used, and it is smaller than its standard error. In column (3), I return to their definition of total mandates, but I do not weight the regression by total state population. The result completely disappears in this case; using the number of mandated benefits, the coefficient is also close to zero.

This sensitivity to the weights used is important. Ideally, the weights should reflect the variance of the estimate of the percent uninsured in the state; while population is potentially appropriate for this task, better weights are available. An improvement over the author's procedure would be a two step estimator: the first step is an individual level regression of insurance status on demographic controls, occupation dummies, industry dummies, and a set of state dummies; the second step regresses the 50 state dummy coefficients from the micro-data regression on the number of mandates, weighted by the inverse of the variance of the state dummy estimates from the first step. The results from this two step procedure are presented in column (4): they once again reject any significant role for mandates, using either regulatory definition.[5] Finally, in column (5), I use a more appropriate measure of regulatory pressure, the set of expensive mandate dummies used in the analysis of Part III of the paper, within the two step regression. None of the dummies are significant, and their sum is approximately zero.

Jensen and Gabel

Jensen and Gabel report two cross-sectional regressions, using 1985 NFIB data (small firms only) and 1988 HIAA data (all firms). The dependent variable in each case is a dummy for offering insurance, and the regressions are run as logits. Their independent variables include a set of mandate dummies which are similar to those used in my work, although they exclude the chiropractic dummy, and include a dummy for freedom to choose psychologist services for the delivery of mental health benefits. They also include dummies for the presence of a state high risk insurance pool, the premium tax rate, the total number of mandates in the state, and the number of mandates passed within the previous two years.[6] Their regressions also controls for: the percentage of the firm's workforce that is teenaged, the fraction that is part-time, and the ownership of the firm (in 1985); the log of the firm's age (1988); the log of firm employment (both); the average earnings in that firm's state/industry cell, the percentage of the workforce in that cell that is female, and the percentage which is black (1985 only); the state

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[4]The second row is from a distinct regression in columns (2)-(4).

[5]The first stage regression is run using the March 1987 CPS, for all non-elderly persons; the result is available upon request. The optimal weights would also account for the covariance of each estimated dummy with the other dummies and the covariates, as discussed in Borjas and Sueyoshi (1991).

[6]Their measure of premium tax rates is slightly different than my own, but is highly correlated with mine. State high risk insurance pools are mechanisms whereby otherwise "uninsurable" individuals can purchase insurance from a state pool at some multiple of the average insurance price in the state; these pools are often funded by assessments on insurers, so they may have similar impacts to premium taxes.

unemployment rate and average cost of hospital room and board; and the state's tax rate (to proxy for the tax subsidy to health insurance). In the model below, I have measured all of these variables except for the tax rate; my measures of percent black, female, and earnings are at the state level rather than the state by industry level.[7]

In Column (1) of Table B2, I present the Jensen and Gabel results for 1985. Only the continuation of coverage dummy is significant and negative; the drug abuse dummy is positive and significant at the 10% level. In column (2), I present my attempt to replicate this regression. I also obtain a negative coefficient on continuation of coverage laws, but it is insignificant; I get a positive and significant effect of psychologist mandates. The coefficient on the number of mandates passed recently is also negative and significant; while suggestive, the possibility of legislative endogeneity makes a causal inference difficult. The coefficient on the sum of the mandate dummies is positive and larger than its standard error.

One potential problem with this regression is that the mandate variables are measured using data from Blue Cross/Blue Shield, which is inconsistent with actual legislation in a number of instances. In the next column, I present the regression with the set of mandate dummies used in my regressions, so that the dummies for alcoholism, drugs, and mental health are appropriately measured. The continuation of coverage dummy is now negative and significant, but none of the others are; the sum of the mandates is negative and insignificant.

In column (4) of the table, I present the authors' regression for 1988, and I replicate this regression in column (5).[8] The authors find a significant negative effect of continuation of coverage mandates and premium taxes; I obtain large negative coefficients on these variables as well. However, a major problem with this specification is that the authors do not seem to consider the fact that continuation of coverage was a national mandate for large firms by 1988. I account for this in column (6), where I change this dummy to one for all firms with more than 20 employees. This wipes out the coefficient on continuation of coverage mandates, so that the sum of the mandates is now insignificant.

In column (7), as before, I replace Jensen and Gabel's mandate dummies with the set of dummies used in my regression. None of the negative coefficients are now significant, and the sum is very close to zero. Finally, I rerun this regression only for small firms (less than 100 employees). For reasons discussed in the text, this is the most appropriate sample for this study, and this regression is most comparable to that run with the 1985 data. Once again, none of the individual mandates are negative and significant, and the sum is approximately zero; the largest effects is found for mental illness mandates, which is consistent with the results from the regressions in Table 3.

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[7]Although my results are insensitive to the inclusion of major industry dummies. Once again, the controls are included but not reported in Table B2.

[8]My 1988 HIAA data is a smaller sample than that used by the authors; my sample is approximately 2/3 as large as theirs. The difference is due to the fact that I only have firms that were resampled by the HIAA in 1989; this sample selection criterion should be orthogonal to the independent variables of interest. If I use the full 1989 sample, which is three times as large as that for 1988, the results are fairly similar.

Summary

The findings in this appendix suggest that the previous consensus of a strong effect of mandated benefits was based on very fragile results. The regression of Goodman and Musgrave uses inappropriate dependent and independent variables, and the relation between these variables is not apparent in the data; the partial correlation between the percent uninsured and the number of mandates is negative. Jensen and Gabel use a more appropriate regression framework, but their findings appear to be very sensitive to the measurement of the mandate variables.

Table B1: Replication of Goodman and Musgrave

	Result	Replication	Unweighted	State Dums	State Dums Expens Mand
Total Number of Mandates	0.301 (0.122)	0.218 (0.116)	0.059 (0.125)	0.0008 (0.0011)	
Number of Mandated Ben		0.126 (0.147)	0.019 (0.117)	0.0003 (0.0011)	
Alcohol					0.0068 (0.0158)
Drugs					-0.0144 (0.0187)
Mental					-0.0126 (0.0155)
Chiropractor					0.0124 (0.0126)
Premium Taxes					0.0125 (0.0101)
Sum of Mandates					0.0046 (0.0256)

Notes:

1) Standard errors in parentheses.
2) All regressions also include controls for the state unemployment rate, per capita gross state product, per capita state indirect business taxes, three dummies for the state regulatory regime, and the distribution of state product across 13 major industries. The regressions in columns (1) and (2) are weighted by state population.
3) Column (1) presents the Goodman and Musgrave result.
4) Column (2) presents the same regression, using my mandate count
5) The first and second row are from distinct regressions in columns (2)-(4).
6) Column (3) presents the unweighted regression results.
7) Columns (4) and (5) present the results from the two step procedure described above.

Table B2: Replication of Jensen and Gabel

Specification	(1) J & G 1985	(2) Repl 1985	(3) Correct Laws	(4) J & G 1988	(5) Repl 1988	(6) Correct CCov	(7) Correct Laws	(8) (7) & < 100 ees
Alcoholism	0.083	0.317	0.140	-0.181	0.247	0.497	0.848	1.230
	(0.233)	(0.229)	(0.250)	(0.323)	(0.493)	(0.507)	(0.448)	(0.537)
Drug Abuse	0.420	0.353	-0.028	-0.012	0.097	0.063	0.331	0.343
	(0.252)	(0.261)	(0.240)	(0.368)	(0.527)	(0.518)	(0.472)	(0.543)
Mental Illness	0.009	-0.228	0.089	-0.285	-0.449	-0.393	-0.550	-0.928
	(0.231)	(0.215)	(0.206)	(0.325)	(0.447)	(0.453)	(0.470)	(0.564)
Psychologists	-0.076	0.498		-0.067	-0.973	-0.584		
	(0.225)	(0.219)		(0.375)	(0.538)	(0.502)		
Continuation of Coverage	-0.428	-0.211	-0.439	-1.170	-0.808	0.069	-0.026	0.103
	(0.213)	(0.191)	(0.158)	(0.329)	(0.393)	(0.416)	(0.395)	(0.462)
Chiropractors			0.002				0.078	0.076
			(0.228)				(0.609)	(0.691)
Premium Tax Rate	-0.169	-0.189	-0.123	-0.613	-0.250	0.060	-0.156	-0.131
	(0.149)	(0.128)	(0.128)	(0.253)	(0.336)	(0.319)	(0.308)	(0.356)
Risk Pool	-0.320	0.079	-0.072	-0.235	-0.720	-0.584	-0.550	-0.530
	(0.259)	(0.262)	(0.237)	(0.303)	(0.504)	(0.502)	(0.470)	(0.551)
Sum of Mandates		0.617	-0.431		-2.856	-1.303	-0.026	0.164
		(0.550)	(0.506)		(1.302)	(1.122)	(1.183)	(1.354)
Total Mandates	-0.019	-0.009	0.022	-0.006	0.041	-0.001	-0.048	-0.075
	(0.019)	(0.024)	(0.021)	(0.026)	(0.040)	(0.035)	(0.041)	(0.049)
New Mandates	-0.058	-0.131	-0.127	0.039	0.021	0.047	0.051	0.078
	(0.057)	(0.055)	(0.056)	(0.071)	(0.085)	(0.084)	(0.082)	(0.097)

Notes:

1) Columns (1) and (4) report the regression results of Jensen and Gabel; columns (2) and (5) replicate these regressions.

2) Columns (3), (7), and (8) use the set of mandate dummies used in the paper.

3) All regressions also include the controls described in the text.