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# The Incidence of Mandated Maternity Benefits

By JONATHAN GRUBER\*

*I consider the labor-market effects of mandates which raise the costs of employing a demographically identifiable group. The efficiency of these policies will be largely dependent on the extent to which their costs are shifted to group-specific wages. I study several state and federal mandates which stipulated that childbirth be covered comprehensively in health insurance plans, raising the relative cost of insuring women of childbearing age. I find substantial shifting of the costs of these mandates to the wages of the targeted group. Correspondingly, I find little effect on total labor input for that group. (JEL I18, J32, H51)*

In an era of tight fiscal budget constraints, mandating employer provision of workplace benefits to employees is an attractive means for a government to finance its policy agenda. Consequently, in recent years there has been a growth of interest in mandated benefits as a tool of social policy. For example, the centerpiece of President Bill Clinton's health-care proposal is mandated employer provision of health insurance, and more than 20 states have mandated some form of maternity leave since 1987.

Aside from their political attraction, there may be an efficiency argument for mandates, relative to public expenditure, as a means of financing benefit expansions. As highlighted by Lawrence H. Summers (1989), publicly financed benefits require an increase in government revenue-raising, with the resulting deadweight loss from taxation.

Mandates, however, are financed by a benefits tax; if employees value the benefit that they are receiving, then the deadweight loss from financing that benefit will be lower than from tax financing. In the limit, with full valuation of the benefit by employees, wages will fall to offset the cost of the benefit to the employer, and there will be no efficiency cost. In fact, recent research has suggested that the increased costs of one workplace mandate, workers compensation, were largely shifted to wages with little effect on employment (Jonathan Gruber and Alan B. Krueger, 1991).

This efficiency argument, however, may not apply to a popular type of policy, the "group-specific mandate," which mandates the expansion of benefits for a demographically identifiable group within the workplace.<sup>1</sup> In the case of mandates such as maternity leave, there is likely to be less scope for the free adjustment of wages to

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<sup>1</sup> Given the prevalence of experience rating in insurance markets, any social insurance mandate may be group-specific, since different individuals may cost the employer different amounts. In this paper, I define group-specific mandates as those which affect a demographically identifiable group only. It is unclear whether the results can be extended to cases in which workers are distinguished along more subtle dimensions.

reflect the valuation of the benefit by the targeted group, since there are barriers to *relative* wage adjustment within the workplace (such as antidiscrimination rules or workplace relative-pay norms) which do not affect the adjustment of overall workplace wage levels. Without the ability of relative wages to adjust, there may be substantial deadweight loss from these mandates even if the benefit is valued by that group. Thus, in considering the efficacy of these mandates, a central consideration is whether the cost of the mandate is shifted to the wages of the group that benefits.

This paper uses a set of “natural experiments” to estimate the response of the labor market to a group-specific mandate: state and federal laws that mandated comprehensive coverage for childbirth in health insurance policies. A commonly accepted feature of health insurance benefits before the mid-1970’s was limited coverage for childbirth. Maternity coverage was sometimes excluded from basic health benefits; if included, it was often subject to flat-rate cash amount limits regardless of the cost of delivery. This differential coverage was widely perceived as discriminatory (Geraldine Leshin, 1981; Alan Guttmacher Institute, 1987). Many states responded to this perception in the 1975–1978 period by passing laws which prohibited treating pregnancy differently from “comparable illnesses” in health insurance benefits. Then, in October 1978, the Federal Government passed the Pregnancy Discrimination Act (PDA), which prohibited any differential treatment of pregnancy in the employment relationship.

This set of laws offers two advantages for studying the labor-market impact of a group-specific mandate. First, they affected a readily identifiable group, women of childbearing age and their husbands (under whose insurance these women may have been covered), so that I am able to study their impact based on observable characteristics. Second, they were fairly costly for these individuals, due both to the widespread existence of differential maternity benefits before 1978 and the large fraction

of health insurance costs for women of childbearing age which are accounted for by maternity benefits.

I use the Current Population Survey (CPS) to study the extent to which the cost of these group-specific mandates was shifted to the targeted group’s wages and the effect on net labor input. I begin by examining changes in wages, hours worked, and employment for married women of childbearing age in states which passed these “maternity mandates,” relative to a set of control individuals within the state and relative to similar states which did not pass this legislation. I then assign each worker an individual-specific cost of the mandate, which is a function of the age-specific cost of maternity coverage, the probability that the worker receives insurance on the job, and the predicted type of insurance coverage that she receives. This allows me to use individual variation in identifying the impact of the mandate and to estimate more precisely the extent of shifting. Finally, I note that, with the passage of the 1978 federal law, the existing state laws provide a natural set of controls; I exploit this “reverse experiment” to confirm my earlier findings. The findings consistently suggest shifting of the costs of the mandates on the order of 100 percent, with little effect on net labor input.

The paper proceeds as follows. Section I presents some background on health insurance benefits for maternity in the 1970’s and discusses the economics of a group-specific mandated benefit. After describing the data and my estimation strategy in Section II, I estimate the impact of the state mandates on the labor-market outcomes of women of childbearing age (and their husbands) in Section III. In Section IV, I study the federal mandate. Section V concludes by discussing the welfare implications of these findings and suggesting directions for future research.

## I. Background

Before 1978, health insurance benefits for maternity were generally limited along two

dimensions: either there was no coverage for pregnancy, or benefits were paid as a flat lump-sum cash amount, regardless of the ultimate costs of childbirth. This stood in contrast to coverage for common illnesses in this era, which was fairly complete.<sup>2</sup> During the 1975–1979 period, 23 states passed laws that outlawed treating pregnancy differently from comparable illnesses. This was also an important feature of the 1978 federal legislation, the PDA, which prohibited discrimination against pregnant women more broadly. The employer cost of the state (and later federal) mandates depends on two factors: the extent of differential coverage before these laws and the cost of its removal.

#### A. *The Extent of Differential Coverage*

There are two previous estimates of the extent of differential coverage for maternity benefits in this era. Dorothy R. Kittner (1978) used a 1976 Labor Department survey of health insurance plans to show that, while over 90 percent of plans included maternity benefits, nearly 60 percent of the plans provided less generous benefits for childbirth than for other disabilities. However, the Health Insurance Association of America (1978) used data from a survey of new group health insurance policies written in early 1978 to estimate that only 52 percent of employees had any coverage for maternity. Both of these estimates are problematic: Kittner's only includes firms with more than 26 employees and does not include information on dependent coverage; the Health Insurance Association of America looks only at new policies, which may have been supplementary to existing policies (and therefore less generous), and does not focus on women of childbearing age.

To obtain more accurate estimates, I use the 1977 National Medical Care Expendi-

<sup>2</sup>This differential coverage may have been a natural response to problems of adverse selection in the timing of pregnancy. Arleen Leibowitz (1990) finds that fertility rates of women with first-dollar coverage were 29-percent higher than those with some coinsurance in the RAND Health Insurance Experiment.

ture Survey (NMCES), which collected data on demographics and health insurance coverage for a nationally representative sample of more than 40,000 individuals. While this survey was completed before the PDA was put in place, many states had passed their own maternity mandates by 1977, so that my calculations will represent underestimates of the extent of discrimination in the early 1970's.<sup>3</sup>

The NMCES contains data on approximately 2,900 females between ages 20 and 40 who were covered through employment-based group health insurance, either in their own name or through a family member. I use "hospital room and board" and "other inpatient services" as comparable illnesses in order to define differential coverage. I find that about 20 percent of women did not have coverage for maternity benefits when they had coverage for either of these comparable illnesses. There were an additional 30 percent of women who received less coverage of the physician's "usual, customary, and reasonable charges" for delivery than for other services, or received only a flat lump-sum provision (less than \$250) for a delivery fee. Thus, at least 50 percent of women faced either differential coverage or benefits.<sup>4</sup>

#### B. *The Cost of Expanding Maternity Benefits*

Estimating the cost of the maternity mandates would require information on the increase in premiums for adding maternity

<sup>3</sup>The data do not contain state identifiers, so I was unable to control for the effects of state laws. Regional controls were not sufficient, due to the widespread passage of state laws in 1976 and 1977. By January 1, 1977, 28 percent of the U.S. population lived in states that had passed mandates; if all firms in these states had completely eradicated differential benefits by the time of the survey, then the discrimination figures should be multiplied by a factor of 1.39.

<sup>4</sup>Another 33 percent of women did not receive any major-medical coverage of normal pregnancies in the presence of major-medical coverage of comparable illnesses. However, it is unclear whether these laws should be construed to require major-medical coverage of normal childbirth.

benefits to a group health package, as well as the cost of increasing the generosity of benefits to the level of comparable illnesses. This sort of data is difficult to gather because nondifferential maternity benefits are now mandated nationally. However, as with all Equal Employment Opportunity Commission legislation, this mandate does not apply to firms with fewer than 15 employees. I have thus been able to gather information on the cost of adding maternity benefits to a small-group plan by using a premium-calculation package from a national insurer. This program is typical of that used by a group-health-insurance salesperson for calculating premiums for a small firm: it inputs the details of the plan and the demographic composition of the workforce and returns the premium cost.<sup>5</sup> For each of several demographic classifications, I use this program to observe the increase in premium cost with the addition of maternity benefits to a typical health insurance plan.

Table 1 presents the cost of adding maternity benefits to a group package for six demographic classifications, in 1990 dollars, in 1978 dollars, and as a percentage of average earnings for each group in 1978. The 1990 cost was deflated to 1978 by using a weighted average of the detailed CPI for hospital services and physician services, where the weights correspond to the fraction of costs in a typical delivery attributable to each.<sup>6</sup> The cost for each group varies widely as a percentage of wages, from less than 1 percent to almost 5 percent. To the extent that there was coverage for childbirth in health insurance plans before the

mandates but differential benefits, the figures in Table 1 will be overestimates of the mandates' costs.<sup>7</sup>

A check on these costs is provided by comparing them to the expected cost of childbirth for an employee in these categories. In 1989, the average cost of a normal delivery was \$4,334 (Health Insurance Association of America, 1989); for married women 20–30 years old, the average probability of having a child in a year was 17.7 percent (U.S. Department of Health and Human Services, 1987). The annual expected cost of childbirth for this group is thus \$767. Compared to the additional cost of \$984 for maternity benefits in family coverage for this age group, this implies an insurance loading factor of 28 percent, which appears reasonable.<sup>8</sup> The high cost of childbirth meant that this mandate was an expensive one for many insured persons.

### C. *The Economics of Group-Specific Mandated Benefits*

The advantages and disadvantages of mandates as tools of social policy are discussed by Thomas G. McGuire and John T. Montgomery (1982), Summers (1989), and Gruber (1992b). Summers presents the efficiency argument for mandates (relative to public provision), noting that when a benefit is provided through the workplace only, individuals will increase their labor supply in order to take advantage of it. Thus, mandates are benefit taxes; if employees value the benefit they are receiving, the increase in labor supply will reduce the deadweight

<sup>5</sup>Since much of the insurance price differential across firm sizes arises from fixed administrative costs, the incremental cost of maternity benefits should not be very sensitive to firm size. The fact that maternity benefits are now mandated nationally for large firms makes this contention difficult to confirm, although, within the range of this program, there is no effect of size differences. My source for this program requested anonymity.

<sup>6</sup>These were two-thirds and one-third, respectively (Health Insurance Association of America, 1989). Unfortunately, the detailed CPI for obstetrics was discontinued in 1978.

<sup>7</sup>These mandates raised the cost of individual workers due to the widespread existence of experience rating in insurance policies in this era (see the appendix to Gruber [1992a]).

<sup>8</sup>Furthermore, the costs in Table 1 will account for the possibility of a nonnormal delivery; in 1989, cesarean sections cost 66-percent more than normal births on average (Health Insurance Association of America, 1989). On the other hand, the fertility rate for working women may be somewhat lower than the average rate overall. A 28-percent loading factor is approximately the average for a firm of 50 employees, according to Congressional Research Service (1988).

TABLE 1—THE COST OF ADDING MATERNITY BENEFITS TO A HEALTH INSURANCE PACKAGE

Coverage	Demographic group	Annual cost (1990 dollars)	Annual cost (1978 dollars)	Cost as percentage of 1978 weekly earnings
Family	20–29-year-old females	\$984	\$360	4.6
Family	30–39-year-old females	\$756	\$277	3.5
Individual	20–29-year-old females	\$324	\$119	1.5
Individual	30–39-year-old females	\$252	\$92	0.9
Family	20–29-year-old males	\$984	\$360	2.9
Family	30–39-year-old males	\$756	\$277	1.7

*Notes:* The source of the data is a premium-calculation program from an anonymous insurance carrier. The cost was calculated for a two-person firm in Maryland. Maryland was a location which was approximately at the midpoint of the locational cost distribution. The results are not sensitive to variations in firm size. Costs are for 1990; they are deflated to 1978 using a weighted average of the detailed CPI for hospital services and physician services, where the weights are  $\frac{2}{3}$  and  $\frac{1}{3}$ , respectively. Costs are normalized by 1978 weekly wages from the May 1978 CPS. For single coverage, wages of unmarried persons are used; for family coverage, wages of married persons are used.

loss of finance. If valuation is full, then there is no deadweight loss from the mandate.

As I showed in an earlier version of this paper (Gruber, 1992a), this analysis is readily extended to the case of a group-specific mandate.<sup>9</sup> However, there may be a number of barriers to full group-specific shifting which are less important when the benefit is extended to everyone in the workplace. Most obviously, there are antidiscrimination regulations which prohibit differential pay for the same job across groups, or which prevent differential promotion decisions by demographic characteristic.<sup>10</sup> Furthermore,

<sup>9</sup>Of course, if there is full valuation, the fact that the benefit is not part of the existing compensation package implies that there must be a market failure in its provision. In fact, there is a strong a priori argument for market failures in many cases of group-specific mandates, such as maternity leave, maternity insurance, or coverage for AIDS, due to problems of adverse selection in insurance markets.

<sup>10</sup>See Ronald G. Ehrenberg and Robert S. Smith (1991) for a discussion of U.S. antidiscrimination legislation, which was in place well before the mid-1970's. In this discussion, I focus only on laws prohibiting discrimination in rates of pay or promotion. In fact, if there are *also* binding restrictions on relative hiring

workplace norms that prohibit different pay across groups or union rules about equality of relative pay may have similar effects to antidiscrimination rules. Finally, if the group that benefits is disproportionately composed of workers earning at or near the minimum wage, there may not be scope for shifting to wages.

As Gruber (1992a) shows, these rigidities can cause mandates to have efficiency consequences even in the presence of full valuation, with resulting group-specific unemployment. Furthermore, the distortion will be higher than that which would arise if the group-specific benefit were financed by a payroll tax assessed on all workers. This is because the smaller tax base for a group-specific mandate will lead to a higher tax rate for a given level of expenditures, and the deadweight loss from taxation rises with the square of the tax rate. As Summers (1989 p. 182) states, referring to the effects of these types of impediments, "It is thus possible that mandated benefit programs can work against the interests of those who most

practices, then employers may be forced to bear the cost of the mandate. If discrimination rules are *only* binding on the hiring side, then they will not impede group-specific shifting in the case of full valuation.

require the benefit being offered. Publicly provided benefits do not drive a wedge between the marginal costs of hiring different workers and so do not give rise to a distortion of this kind.”

Since the efficiency case for mandates rests largely on employee valuation which is reflected in wage adjustments, the empirical work below will estimate the extent of group-specific shifting to wages of the cost of mandated health insurance for maternity. If there is *not* shifting to wages, then either the group that benefits does not value the mandate, or there are impediments to the adjustment of relative wages to reflect that valuation. As a result, there may be large efficiency costs associated with such a policy.

In considering these results, two caveats are in order. First, I am not studying whether mandated maternity benefits represent sensible social policy, but instead only whether there appear to be large efficiency costs from the financing of such policies. In the conclusion, I will consider more broadly the desirability of mandated maternity coverage. Second, I have focused purely on efficiency considerations and have ignored equity considerations about the source of finance of a group-specific mandate. If the goal of a mandate is not to correct a market failure, but rather to provide benefits to some deprived group in society, then full shifting to wages may not be viewed as a desirable outcome. Thus, in considering the results that follow, it is important to understand the goal of government mandate policy: is it to correct a market failure, or to redirect resources across groups?<sup>11</sup>

## II. Data and Identification Strategy

The goal of the empirical work is to identify the effect of laws passed by certain states (experimental states) which affected

particular groups of individuals (treatment group). Identifying this effect requires controlling for any systematic shocks to the labor-market outcomes of the treatment group in the experimental states that are correlated with, but not due to, the law. I do so in three ways in the estimation below. First, I include year effects, to capture any national trends in the earnings of the treatment group. Second, I include state effects, to control for secular earnings differences in the states that passed the laws and those that did not. Finally, I include state-by-year effects, to control for state-specific shocks over this period which are correlated with the passage of these laws. That is, I compare the treatment individuals in the experimental states to a set of control individuals in those same states and measure the change in the treatments' *relative* outcomes, relative to states that did not pass maternity mandates. The identifying assumption of this “differences-in-differences-in-differences” (DDD) estimator are fairly weak: it simply requires that there be no contemporaneous shock that affects the relative outcomes of the treatment group in the same state-years as the law.<sup>12</sup>

The treatment group here comprises those insured workers who are “at risk” for having a child, or whose health insurance covers someone who is at risk for having a child. The controls are other individuals who were directly unaffected by the law. However, the Current Population Survey (before May 1979) contained no information on health insurance coverage. I am thus unable to identify exactly the employees for whom this was a costly mandate.

I address this problem in two ways in the empirical work below. First, I use as the treatment group married women of ages 20–40. This group will contain the individuals for whom the mandate was most costly (according to Table 1), married women of

<sup>11</sup>For the set of laws under study, the answer appears to be the former, as they were part of a larger set of state insurance-market regulations which appeared in the 1970's. See Gruber (1992b) for more detail on these state mandates.

<sup>12</sup>This name derives from the “differences-in-differences” estimator used, for example, by David Card (1990). Such estimation, in my context, would include only state and year effects and would assume that there were no state-specific shocks.

childbearing age. My control group is all individuals over age 40 and single males of ages 20–40. I exclude single 20–40-year-old women, as well as 20–40-year-old married males, who may also be affected by the laws if their insurance covers their wives.<sup>13</sup> This “treatment-dummy” approach has the virtue that it is relatively nonparametric.

Second, I use data on insurance coverage from other data sets to model the likelihood that individuals were covered by insurance and the type of insurance coverage that they receive, and I assign each individual a cost of the mandate based on these predictions and the cost data in Table 1. This approach has the advantage that I use individual variation, rather than differences across broad demographic groups, to identify the impact of the law. However, it has the disadvantage that it imposes strong parametric assumptions. If the functional form for the expected cost of the mandate is incorrect, then the demographic-group dummy may be a more effective means of capturing the law’s impact. Thus, in the empirical work that follows, I will rely on both the treatment-group dummy and the individually parameterized cost measure.

I examine two sets of law changes in order to identify the effect of the maternity mandates. First, I study several of the states that passed the mandates in the mid-1970’s, comparing them to similar states that did not pass mandates. Second, I study the effect of the federal mandate on the states that had not yet passed maternity mandates, using the states that had passed mandates as controls.

I focus on three of the 23 states that passed maternity mandates before the federal PDA: Illinois, New Jersey, and New

York (the “experimental” states). The choice of these three states was motivated by two considerations. First, all of these laws went into effect between July 1, 1976, and January 1, 1977, so that they can be studied simultaneously, and there is sufficient time to examine their impact before the federal law was put into place (October 1978). Second, the data that I use to study the labor-market impact of these laws, the May Current Population Survey, did not identify all states separately before May 1977, but rather grouped some states into regional classifications. Thus, I can only use those states that were identified separately in the survey before 1977.

My set of “nonexperimental” states was chosen using similar criteria: these states had to be separately identified in these CPS’s, and they had to be able to capture any regional shocks to the experimental states. For Illinois, the control states used are Ohio and Indiana; for New Jersey and New York, the controls are Connecticut, Massachusetts, and North Carolina.<sup>14</sup>

The data consist of observations on all individuals in these sets of experimental and nonexperimental locations for two years before the legislation (1974 and 1975) and for two years after the legislation (1977 and 1978). Because I use the May CPS, the 1978 survey collects data from before the passage of the federal law. The means of the data are presented in the left-hand panel of Table 2, for the experimental states and the nonexperimentals (both for the “before” years and for the “after” years), for all wage-earners.<sup>15</sup>

<sup>13</sup>That is, there are three demographic subsets of costly individuals under the mandate, and the treatment-dummy approach focuses on just one (married women). Approximately 56 percent of working married women had insurance from their employers in 1979; as I will show below, the expected cost of the mandate is roughly comparable across these three groups, as a fraction of wages. I therefore focus on married women for expositional ease; the effects on the other groups, as well as the overall treatment effect, is presented below.

<sup>14</sup>Pennsylvania could not be used as a “mid-Atlantic” control because it implemented broad anti-sex-discrimination insurance regulations during 1977, which included a “maternity mandate.” North Carolina is included as a control in order to avoid comparing New York and New Jersey solely to New England; the results are similar if North Carolina is excluded.

<sup>15</sup>Hourly wages are in 1978 dollars. I exclude any individuals who report earning less than \$1/hour or more than \$100/hour in 1978 dollars. I also exclude any persons less than 20 years old or older than 65, and I likewise exclude the self-employed. The means are unweighted.



TABLE 2—MEANS FOR ALL WAGE-EARNERS

Variable	State laws				Federal PDA			
	Nonexperimental states		Experimental states		Nonexperimental states		Experimental states	
	Before law	After law	Before law	After law	Before law	After law	Before law	After law
Percentage female	41.4 [49.3]	43.9 [49.6]	41.4 [49.3]	43.1 [49.5]	44.4 [49.7]	45.6 [49.8]	44.5 [49.7]	45.8 [49.8]
Average age	38.1 [12.6]	37.6 [12.5]	38.9 [12.6]	38.4 [12.6]	37.6 [12.7]	37.5 [12.4]	37.5 [12.7]	37.2 [12.4]
Percentage married	75.0 [43.3]	70.8 [45.5]	71.6 [45.1]	67.9 [46.7]	65.7 [47.5]	63.8 [48.1]	70.0 [45.8]	67.0 [47.0]
Percentage nonwhite	8.8 [28.3]	9.2 [28.9]	10.2 [30.3]	12.0 [32.5]	12.3 [32.8]	13.5 [34.2]	10.9 [31.2]	11.2 [31.4]
Average education	12.1 [2.87]	12.3 [2.81]	12.4 [2.94]	12.7 [2.88]	12.5 [3.04]	12.7 [2.99]	12.3 [2.97]	12.6 [2.88]
Average hourly wage	5.68 [3.31]	5.59 [3.16]	6.61 [3.98]	6.40 [3.62]	6.33 [4.02]	5.88 [3.74]	5.80 [3.81]	5.49 [3.63]
Percentage union	27.0 [44.4]	26.8 [44.3]	33.4 [47.2]	33.8 [47.3]				
Percentage manufacturing	36.5 [48.2]	35.3 [47.8]	28.5 [45.1]	26.6 [44.2]	25.1 [43.3]	23.9 [42.6]	23.6 [42.5]	22.6 [41.8]
Percentage services	29.7 [47.2]	31.5 [46.5]	35.3 [47.8]	37.3 [48.4]	41.4 [49.3]	42.6 [49.4]	39.9 [49.0]	40.9 [49.2]
Weekly cost of mandate	4.01 [1.68]	3.87 [1.65]	3.92 [1.66]	3.85 [1.63]	3.64 [1.59]	3.56 [1.58]	3.71 [1.60]	3.64 [1.59]
Cost/wages	0.020 [0.010]	0.020 [0.010]	0.018 [0.009]	0.018 [0.010]	0.019 [0.012]	0.021 [0.013]	0.021 [0.013]	0.022 [0.013]
N:	9,954	10,180	10,597	10,636	41,772	45,332	48,713	59,647

Notes: Numbers in square brackets are standard deviations. Observations with wages below \$1/hour or above \$100/hour are dropped, as are individuals younger than 20 or older than 65 and the self-employed. See text for definitions of experimental and nonexperimental states and for definitions of before and after years.

There are not many striking differences across the groups of states: the experimental states have higher wages, are more unionized, and are less manufacturing-oriented. Differences in unionization and industry distribution, as well as systematic wage differences across locations, are controlled for in the estimation. Overall, wages fell more in the experimental states than in the nonexperimental states; below, I will assess whether the maternity mandates played any role in this relative fall.

The federal legislation provides a distinct opportunity to study the impacts of increasing the costs of health insurance coverage for maternity. In this case, the states that had already passed maternity mandates are

the nonexperimentals, and those that had not are the experimentals. The advantage of this “experiment” is that by this later date the CPS was identifying all states separately, so that I am able to use as control states all those states that had passed laws by January 1, 1977 (12 states), and as experimentals all states that did not pass laws before 1979 (28 states).<sup>16</sup> These states are more broadly representative of the country

<sup>16</sup>The controls are Arkansas, California, Colorado, Hawaii, Idaho, Illinois, Iowa, Maryland, New Jersey, New York, Tennessee, and Wisconsin. The experimen-

as a whole, which should help to overcome any problems induced by using three (somewhat similar) states as experimentals in the earlier estimation. The disadvantage is that the PDA was more expansive than the state mandates, covering the entire employment relationship, rather than just health benefits. Thus, there may have been some effect on the cost of employing women of child-bearing age in the nonexperimental states as well.<sup>17</sup> Nevertheless, health-insurance industry representatives estimated that the effects on health benefit plans would represent two-thirds of the cost of implementing the PDA (Commerce Clearing House, 1978). To the extent that the net cost difference across these two sets of states represents the health insurance requirements of the law only, the shifting estimates should be comparable to those from the first (state mandates) strategy.

I use the 1978 and 1979 (before), and 1981 and 1982 (after) March CPS to study the impact of the federal law. The March data differ from the May data used earlier in that the earnings and labor-market data are retrospective; that is, individuals are asked for their annual earnings, weeks worked, and usual hours per week in the previous year.<sup>18</sup> The means for the federal law change are presented in the right-hand panel of Table 2. Once again, the two sets

of states are fairly similar: in this case, the experimental states have slightly lower wages and are less manufacturing-oriented.

### III. The Labor-Market Impact of the State Laws

#### A. DDD Estimation

Table 3 illustrates DDD estimation of the effect of the maternity mandates on wages. The top panel compares the change in wages for 20–40-year-old married women in the states that passed the laws to the change for 20–40-year-old married women in the nonexperimental states. Each cell contains the mean average real wage for the group labeled on the axes, along with the standard error and the number of observations. There was a 3.4-percent fall in the real wages of women in the experimental states over this period, compared to a 2.8-percent rise in the real wages of women in other states. Thus, there was a (significant) 6.2-percent relative fall in the wages of women of child-bearing age in states that passed these laws; this is the differences-in-differences estimate of the law's impact. This figure seems somewhat large given the magnitude of the costs identified in Table 1.

However, if there was a distinct labor-market shock to the experimental states over this period, this estimate does not identify the impact of the law. I examine this in the bottom panel of Table 3, where I perform the same exercise for the control group, all those older than 40 and single males ages 20–40. For that group, I do find a fall in wages in the experimental states, relative to the other states, of 0.8 percent. Although not significant, this suggests that it may be important to control for state-specific shocks in estimating the impact of the law.

Taking the difference between the two panels of Table 3, there is a 5.4-percent fall in the *relative* wages of 20–40-year-old married women in the states that passed the laws, compared to the change in relative wages in the nonexperimental states. This statistically significant DDD estimate provides some evidence that the cost of a group-specific mandate is borne by

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tals are Alabama, Alaska, Delaware, the District of Columbia, Indiana, Kentucky, Louisiana, Maine, Massachusetts, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Rhode Island, South Carolina, South Dakota, Texas, Utah, Vermont, Washington, West Virginia, and Wyoming. Connecticut was excluded in this part of the study because the state mandated benefit nondiscrimination rules for all groups in 1979.

<sup>17</sup>For example, another major cost of the PDA was the requirement that firms offering disability coverage extend that coverage to include pregnancy.

<sup>18</sup>Thus, the actual labor-market data come from 1977, 1978, 1980, and 1981. The use of the March CPS was dictated by the fact that, starting in 1979, the May CPS only asked earnings information of one-quarter of the sample. The March CPS also does not report data on union status.

members of that group. However, its interpretation is problematic, since there may be important variation in the effect of the law within the set of married 20–40-year-old women; for example, only some of these women will have insurance on the job. This source of variation will be exploited below, where I build individual-specific measures of the impact of the law. First, however, I discuss how the analysis of Table 3 can be expressed within a regression framework.

### B. Regression Framework for DDD Estimation

The sampling variance of the DDD estimate in Table 3 can be reduced by moving to a regression framework, which allows me to control for other observables that affect the outcome variables of interest. The regression equation has the following form:

$$(1) \quad W_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 \tau_t + \beta_3 \delta_j \\ + \beta_4 \text{TREAT}_i + \beta_5 (\delta_j \times \tau_t) \\ + \beta_6 (\tau_t \times \text{TREAT}_i) \\ + \beta_7 (\delta_j \times \text{TREAT}_i) \\ + \beta_8 (\delta_j \times \tau_t \times \text{TREAT}_i).$$

In this equation,  $i$  indexes individuals,  $j$  indexes states (1 if experimental state, 0 if nonexperimental), and  $t$  indexes years (1 if after the law, 0 if before).  $W$  is the log real hourly wage,  $X$  is a vector of observable characteristics,  $\delta_j$  is a fixed state effect,  $\tau_t$  is a fixed year effect, and TREAT is a dummy for treatment group (1 if treatment, 0 if control).

The analogy of this regression to Table 3 is straightforward. The fixed effects control for the time-series changes in wages ( $\beta_2$ ), the time-invariant characteristics of the experimental states ( $\beta_3$ ), and the time-invariant characteristics of the treatment group ( $\beta_4$ ). The second-level interactions control for changes over time in the experimental states ( $\beta_5$ ), changes over time for the treatment group nationwide ( $\beta_6$ ), and time-invariant characteristics of the treatment group in the experimental states ( $\beta_7$ ). The third-level interaction ( $\beta_8$ ) captures all

variation in wages specific to the treatments (relative to controls) in the experimental states (relative to the nonexperimentals) in the years after the law (relative to before the law). This is the DDD estimate of the extent of shifting of the cost of the mandate to group-specific wages. The set of demographic covariates used includes education, experience and its square, sex, marital status, a marital-status  $\times$  sex interaction, a dummy for nonwhite, a dummy for union status, dummies for 15 major industries, and separate year dummies for 1974 and 1978.

The first row of Table 4 presents the estimates of the third-level interaction from (1),  $\beta_8$ . The coefficient indicates that wages fell by 4.3 percent for the treatment group; it is marginally statistically significant. While this is slightly smaller than the estimate from Table 3, the standard error has been reduced as well, so that the significance is approximately the same. The fact that introducing the other covariates did not have a sizeable impact on this coefficient is comforting, given the experimental interpretation of the estimate.<sup>19</sup>

The next two columns of Table 4 examine the effects of this mandate on hours of work and probability of employment. If this benefit is fully valued, on average, by workers in the treatment group, there should be no change in their net labor input. However, even with full valuation on average, it is possible that the mandate could affect the *composition* of labor input. This mandate represents an increase in the fixed costs of employment and is thus more costly (as a

<sup>19</sup>The other coefficients in the regression are of their expected signs and magnitudes. There is a 1.2-percent fall in wages for the within-state control group. This suggests that the experimental states, on average, saw a negative shock over this period. Alternatively, the mandate itself could be causing this fall for the control group, if the groups are complements or if there is cross-subsidization across groups due to relative pay restrictions. However, given the findings of substantial shifting to group-specific wages, such spillover seems unlikely. The full set of coefficients are reported in Gruber (1992a).

TABLE 3—DDD ESTIMATES OF THE IMPACT OF STATE MANDATES ON HOURLY WAGES

Location/year	Before law change	After law change	Time difference for location
<i>A. Treatment Individuals: Married Women, 20–40 Years Old:</i>			
Experimental states	1.547 (0.012) [1,400]	1.513 (0.012) [1,496]	–0.034 (0.017)
Nonexperimental states	1.369 (0.010) [1,480]	1.397 (0.010) [1,640]	0.028 (0.014)
Location difference at a point in time:	0.178 (0.016)	0.116 (0.015)	
Difference-in-difference:		–0.062 (0.022)	
<i>B. Control Group: Over 40 and Single Males 20–40:</i>			
Experimental states	1.759 (0.007) [5,624]	1.748 (0.007) [5,407]	–0.011 (0.010)
Nonexperimental states	1.630 (0.007) [4,959]	1.627 (0.007) [4,928]	–0.003 (0.010)
Location difference at a point in time:	0.129 (0.010)	0.121 (0.010)	
Difference-in-difference:		–0.008: (0.014)	
<b>DDD:</b>		<b>–0.054</b> <b>(0.026)</b>	

*Notes:* Cells contain mean log hourly wage for the group identified. Standard errors are given in parentheses; sample sizes are given in square brackets. Years before/after law change, and experimental/nonexperimental states, are defined in the text. Difference-in-difference-in-difference (DDD) is the difference-in-difference from the upper panel minus that in the lower panel.

fraction of labor payments) for low-hours employees. If employers are able to lower each worker's wages by the lump-sum cost of the mandate, then neither hours nor employment should change. However, if employers are not able to implement a percentage reduction in pay that is inversely proportional to hours worked, then part-time workers will become more expensive. Employers may thus react by increasing hours and lowering employment, reducing

the cost per hour of the mandate while leaving total labor input unchanged.

Of course, if the wage offset is lower for low-hours workers, workers will demand the opposite outcome; there will be increasing demand for part-time work, with hours falling and employment increasing. Furthermore, since part-time workers may be more readily excluded from health insurance coverage, there may also be a countervailing effect on the employer side, as full-time

TABLE 4—TREATMENT-DUMMY RESULTS ACROSS DEMOGRAPHIC GROUPS

Group	Log hourly wage	Log hours/week	Employment (probit)	Percentage changes in labor input
Married women, ages 20–40	–0.043 (0.023)	0.049 (0.022)	–0.047 (0.048) [–0.016]	1.40
Single women, ages 20–40	–0.042 (0.026)	–0.014 (0.024)	–0.095 (0.064) [–0.030]	–5.95
Married men, ages 20–40	–0.009 (0.018)	0.030 (0.015)	–0.139 (0.072) [–0.038]	–1.08
All treatments	–0.023 (0.015)	0.027 (0.014)	–0.079 (0.039) [–0.024]	–0.88

*Notes:* Standard errors are given in parentheses. The coefficient is that on the third-level interaction in equation (1). The treatment group is the group indicated for each row. The control group is the same as that for Table 3 (all those older than 40 and single men younger than 40). The number in brackets in the employment column is the marginal probability (see text). The change in total labor input is the change in hours at the average-employment/population ratio plus the change in employment in terms of average hours per employed person. This is then divided by the ratio of employment to population to get per-worker figures and then divided by average hours per week for the treatment group to get a percentage change.

employees are replaced with their (uninsured) part-time counterparts. In this case as well, hours would fall, and employment would rise.<sup>20</sup> Thus, the effects on hours and employment are uncertain, even if the cost of the mandate can be shifted to wages on average.

Table 4 confirms the conclusion of full shifting, on average, but does show some compositional changes. In the second column, the dependent variable is the log of weekly hours of work; hours rise by a significant 4.9 percent for the treatment group. I measure employment by a dummy variable which equals 1 if the individual is employed, and 0 otherwise (unemployed or out of the labor force); the employment regressions are run as probits. Table 4 also shows an insignificant fall in employment; it implies that the treatments saw a 1.6-percent fall in employment over this period, relative to the

sets of controls.<sup>21</sup> There is a small net positive effect on total labor input of 0.48 hours per week per worker; this amounts to a rise in hours of about 1.4 percent of average hours per week for the treatment group.<sup>22</sup> This is consistent with the large wage offsets uncovered in the columns for log hourly wage and log hours/week.

As mentioned above, married women represent only one of three groups of workers that are potentially affected by these mandates. The costs of employing single women of childbearing age rose as well, as did costs

<sup>20</sup>Of course, another option for employers is to drop insurance coverage altogether. Gruber (1992b) finds that there was little effect of other expensive state-mandated benefits on the propensity of firms to offer health insurance.

<sup>21</sup>This is calculated by using the probit coefficients to predict the probability of employment as if all individuals in the experimental state/years were treatments, then predicting the probability as if none were treatments, and taking the average of the differences of these predictions across individuals.

<sup>22</sup>I calculate the change in total labor input as the change in hours at the average employment-to-population ratio plus the change in employment at average hours per employed person. This is then divided by the employment-to-population ratio to get per-worker figures.

of employing married males, who may cover their wives in their insurance policies. The effects on these other groups, as well as the effect on all of these groups together, are presented in the remaining rows of Table 4. There are some differences in the results across groups: shifting to wages is small and insignificant for married males, and there is evidence of a sizable fall in total labor input for single females, which is of the same magnitude as the fall in wages for that group. However, the overall results across all groups (i.e., from a regression in which the treatment dummy is 1 for members of all of these groups) is consistent with that for married women only: a decrease in wages, a rise in hours, and a fall in employment, with an overall labor input effect that is small relative to the wage effect.

The reasons for this differential effect across the demographic subgroups may be heterogeneity in the impact of this law across the groups, due to differential probabilities of insurance coverage and costs of extending maternity health-insurance benefits. In the next subsection, I address this heterogeneity by attempting to model the individual-specific cost of these mandates.<sup>23</sup>

### C. Individual Parameterization of the Cost of the Mandates

In assessing the cost of these maternity mandates for each individual, one must consider (i) whether the individual is covered by insurance and whether that insurance provides differential maternity benefits; (ii)

<sup>23</sup>As I discuss in the next subsection, the expected costs of the mandates across the three groups are roughly equal. The reason for the much smaller wage effect for married males is therefore unclear; it may be that employers perceived the law as having a larger effect on the cost of employing women for other reasons. In any case, the coefficients across the different groups are not significantly different from each other, and the more appropriate test of the effect of the laws is to use the variation in the cost across individuals, which I do next. The overall conclusions from the individually parameterized results below are not sensitive to the exclusion of any one of these three groups from the analysis.

whether this coverage is from the individual's own job or is through a family member; (iii) whether the coverage is for the entire family, or just the individual;<sup>24</sup> and (iv) the individual's (or spouse's) age-specific probability of childbearing. Unfortunately, the CPS does not contain information about insurance coverage during the 1974–1978 period. I have thus calculated *predicted* individual-specific costs, drawing on three sources of data: the estimates of age-specific costs from the premium program; data on the probability of insurance coverage in the 1979 May CPS Pension Supplement; and data on type of insurance coverage from the 1977 NMCES. These cost calculations are described in detail in the appendix to Gruber (1992a); I will briefly review them here.

For all individuals over age 40, and for single males ages 20–40, a cost of zero is assigned. I divide the remaining 20–40-year-olds into three treatment groups: single females, married females, and married males. I use the CPS Pension Supplement, which collects data on employer-provided insurance, to model the probability of insurance coverage as a function of individual demographics, hours of work, union status, and industry of employment. Separate predictor regressions are run for each of the three groups. I then create an extract from the NMCES of all persons in each of these three groups who are employed, who have insurance on the job, and who are the primary insured for their household. For each group I model the probability that a worker will have family coverage versus individual coverage as a function of demographics, industry, spouse's employment status, and spouse's industry.

<sup>24</sup>The premium-pricing program described earlier assigns a much higher incremental cost of adding maternity benefits to family coverage than to individual coverage. Presumably, this proxies for differences in the probability of childbearing. Indeed, the relative cost difference between the two types of policies is almost exactly the same as the difference in the relative probabilities of childbearing between single and married women (U.S. Department of Health and Human Services, 1987).

Finally, I use Table 1 to assign age-specific costs: I take a weighted average of the costs of individual and family coverage (where the weights are the predicted probabilities of each type of coverage from the NMCES) and multiply by the predicted probability of having insurance coverage on the job. This yields a predicted weekly cost which varies by the six 10-year age groupings in Table 1.<sup>25</sup> The results of this exercise, for all treatment individuals, are presented at the bottom of Table 2. The cost averages \$3.91 per week, which is 1.9 percent of wages on average; it has a maximum value of 28 percent of wages. The average cost is not appreciably different across the experimental/nonexperimental locations, nor does it change much over time. The weekly cost is highest for married males, reflecting both the high cost for that group from Table 1 and the fact that they are more likely than married females to be the primary insured. However, once costs are normalized by wages, they are roughly equal for married men and women, and slightly lower for single women.

Since a fixed-cost-of-employment mandate is more costly for part-time workers, the predicted cost should also be normalized by hours worked per week, given that the probability of insurance coverage has been appropriately downweighted for that group in the predictor equation. However, for workers who report weekly wages in the CPS, the hourly wage is calculated as the weekly wage divided by hours worked. Thus, if there is measurement error in hours, this may induce a spurious correlation between

hourly wage and hourly mandate cost. I will present results below both with and without the normalization for hours worked, as the results are sensitive to the specification chosen.

The individually parameterized cost measure can be introduced in place of the treatment dummy in equation (4). Since the calculated cost of the mandate is expressed in dollars, it would be interpreted most straightforwardly in a levels wage equation, rather than the log wage specification used earlier. However, since wages are distributed lognormally, a levels wage equation is potentially misspecified. In a log wage equation, the linear cost measure estimates the percentage fall in wages for a one-dollar increase in cost, which varies along the wage distribution. Ideally, this problem could be solved by normalizing costs by individual wages, but this would induce a spurious negative correlation between the dependent and independent variables in the wage regression. Instead, I note that the wage equation can be specified as  $W = (e^{\beta X} + \text{COSTNN})e^{\epsilon}$ , where COSTNN is the individual hourly mandate cost, and  $\epsilon$  is a normally distributed error term. Taking logs of both sides of this equation, one obtains:  $\log(W) = \log(e^{\beta X} + \text{COSTNN}) + \epsilon$ . This nonlinear model thus has both a normally distributed error and a directly interpretable coefficient on the individual mandate cost in dollars.<sup>26</sup>

To the extent that my estimate of the cost of the mandate is correct, a coefficient of  $-1$  on the third-level interaction would in-

<sup>25</sup>In an earlier version of this paper, I also let the cost vary by single-year age-specific probabilities of childbearing. The results were similar; I rely on the 10-year averages because this seems to be the level at which the costs of insurance vary. For married males, I use own age rather than wife's age, since this appears to be the relevant variable for the premium calculation. Ideally, I would also control for the probability that the individual has differential benefit coverage; however, I am not confident enough in my estimates of the incidence of differential benefits to make this an integral part of the analysis. This implies that the result may underestimate the extent of shifting.

<sup>26</sup>The results are quite similar when the cost is included linearly in the log wage regression and normalized by average wages (see Gruber, 1992a). I have also tried entering hourly cost, not normalized by wages, into a linear wage equation: the estimated third-level interaction is  $-3.76$ , with a standard error of  $0.99$ . Furthermore, the coefficient on cost (not normalized by wages) in a log wage regression should fall as the wage rises, since a dollar cost increase represents a smaller percentage of wages. This prediction is testable by cutting the sample by some measure of *permanent* income, such as education. In fact, the shifting coefficient for workers who did not graduate from high school is twice that of those who did, although the estimates are not significantly different from each other.

TABLE 5—WAGES AND LABOR INPUT RESULTS—PARAMETRIZED COST OF THE MANDATE

Coefficient	Specification				
	(i) Log wage	(ii) Log wage (no hours)	(iii) Log wage (full-time)	(iv) Log hours/week	(v) Employment (probit)
$\beta_8$	-2.140 (0.759)	-0.028 (0.019)	-0.037 (0.019)	0.0049 (0.0031)	-0.027 (0.011) [-0.022]
Shifting (percentage):	214	109	156		
N:	41,367	41,367	35,868	41,367	84,305

Notes: All regressions are estimated by nonlinear least squares, as described in the text. Standard errors are given in parentheses. The mandate cost in columns (ii) and (iii) is not normalized by hours worked; shifting is calculated at average hours for the treatments. The sample in column (iii) is restricted to those who work at least 35 hours per week. Column (v) is a probit. Cost is assigned by demographic group average. The number in brackets shows the change in the probability of employment for a \$1 increase in costs.

dicate full shifting to wages. Even if the level of the estimate is incorrect, however, so long as I capture the *relative costliness* across individuals appropriately, I will gain efficiency in estimation over the treatment-dummy case by using individual variation in relative costs.<sup>27</sup>

#### D. Individual Parameterization: Results

Table 5 presents the coefficient of interest from wage regressions with the individually parameterized costs. In column (i), the cost is normalized by hours worked. The regression indicates very sizable shifting to wages, on the order of 210 percent of the cost of the mandate. While this coefficient is significantly different from 0, it is not significantly different from 1, which would imply full shifting to wages.

In column (ii), I remove the normalization of the mandate cost for hours worked. At the mean hours worked for the treat-

ment group, there is 109 percent shifting to wages, but the estimate is not significant. This reduction in the shifting coefficient implies that the fall in wages was greater for the low-hours workers, since they saw the greatest increase in predicted costs when predicted costs were normalized by hours worked. To the extent that these part-time workers were covered by health insurance, this is a sensible finding, since the hourly cost of the mandate was highest for them. However, only 20 percent of individuals who worked less than 35 hours per week in 1979 were covered by health insurance (based on tabulations from the May 1979 CPS). The predictor equation for the probability of insurance coverage controls for hours worked, a dummy for part-time work, and interactions of union status with hours of work and the part-time dummy; nevertheless, it would be disturbing if these results were driven solely by low-hours workers.

Thus, in column (iii) of Table 5, I focus only on full-time workers (35 hours per week or more); over 75 percent of this group is covered by insurance on the job. Cost is not normalized by hours worked for full-time workers, since the noise-to-signal ratio in hours is presumably quite high for this group.<sup>28</sup> The results reveal that the conclu-

<sup>27</sup>Using this estimated cost in place of a demographic dummy does introduce more imprecision into the estimation, since I have predicted the cost from earlier regression models. This imprecision will not be appropriately reflected in the standard errors in my outcome regressions, which will therefore be too small. However, this problem can be shown to disappear as the precision of the predictor equations increases; the predictor equations used fit fairly well, predicting between 73 percent and 85 percent of the cases correctly.

<sup>28</sup>The shifting estimate is similar if cost is normalized.



sion of group-specific shifting was not driven by low-hours workers. The shifting estimate for full-time workers lies between the estimates of columns (i) and (ii).

If these estimates are correct, and there is full shifting of the cost of this mandate to the wages of the treatment group, then there should be no net effect on labor input. I test this in columns (iv) and (v) of Table 5. In column (iv), the dependent variable is the log of hours worked, and the cost is not normalized by average wages or hours.<sup>29</sup> The regression reveals a rise in hours worked by about 0.5 percent of their average level for a \$1 increase in cost.

For the cost measure in the employment regression, I cannot predict individual probabilities of insurance coverage or type of coverage, since I cannot measure industry of employment or hours worked for the unemployed. I thus assign each individual the average probability of insurance coverage and the average probability of family/individual coverage for his or her demographic group (single females, married females, and married males). That is, I assume that if nonemployed individuals were employed, they would face the same probabilities of insurance coverage and buy the same type of insurance as their demographic counterparts who are employed. As before, the employment regression is run as a probit.

As column (v) of Table 5 shows, there is a significant fall in employment for the treatment group. The probit coefficient implies that a \$1 increase in cost would lead to a 0.22-percent fall in probability of employment.<sup>30</sup> Taken together with the hours co-

efficient, this implies a rise in total labor input per worker of 0.63 percent of its average value for a 100-percent rise in cost. This can be contrasted to the estimate of a fall in wages of 4.7 percent of their average value. Thus, the estimated effect on net labor input is small, which confirms the conclusion of substantial shifting to wages.<sup>31</sup>

#### IV. The Federal Experiment

Tables 4 and 5 find extensive shifting of the cost of the maternity mandates to group-specific wages, both in a relatively unrestricted treatment-group-dummy model and in a more parametric specification which tried to capture individual variation in the cost of the mandates. However, these results emerged from the analysis of only three experiments, using a select set of control states. This suggests the desirability of finding an example of a group-specific mandate that affected a broader range of states. The federal PDA of 1978 provides such an example.

The federal law can be studied within the same regression framework used above, with the exceptions that there are no data on union status and that the year dummies are now for 1978 and 1981. These results

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in predicted probability of employment across treatment individuals gives the effects of a dollar increase in costs.

<sup>31</sup>I have also performed two specification checks to assess the robustness of this result. First, I assessed whether one of the law changes was driving the results, by running the regression separately by state, relative to the regional controls. While I found stronger results for New York and weaker results for New Jersey, the result was not driven by any one state's experience; the shifting estimate was significant at the 10-percent level even if New York was excluded. In all cases the net labor-input effect was very small relative to the wage effect, confirming the conclusion of full shifting. Second, in an attempt to include more detailed controls for industry-specific shocks which may be driving the results, I allowed the industry dummies to vary by state and year. This is a very general specification, which allows for state-specific, year-specific, and state  $\times$  year-specific shocks by industry. Nevertheless, the shifting estimate is virtually identical to that in column (i) of Table 5. These results are reported in Gruber (1992a).

<sup>29</sup>While normalizing by hours is once again theoretically appropriate, it would induce a spurious negative correlation between predicted cost and hours worked. Furthermore, the predictor equation for the probability of insurance coverage used here does not control for hours worked, since this would induce a spurious positive correlation.

<sup>30</sup>This is calculated similarly to the earlier case: predicted employment is calculated at average cost and at average cost plus one dollar for the treatment group in the experimental state/years; the average difference

TABLE 6—FURTHER RESULTS—FEDERAL EXPERIMENT

Demographic group/treatment	Specification					
	(i) Log wage	(ii) Log wage (no hours)	(iii) Log wage (full-time)	(iv) Log hours/week	(v) Employment (probit)	(vi) Change in total labor input
Married women, ages 20–40	–0.021 (0.012)			0.0012 (0.0098)	–0.018 (0.028) [–0.0055]	–0.0071
Single women, ages 20–40	–0.014 (0.014)			0.0157 (0.0101)	0.0184 (0.0374) [0.0050]	0.0219
Married men, ages 20–40	–0.008 (0.0012)			–0.0008 (0.0073)	0.0020 (0.0046) [0.0005]	–0.0003
All treatments	–0.0014 (0.0009)			0.0032 (0.0064)	0.0001 (0.0233) [0.00004]	0.0033
Individual parameterization	–0.587 (0.412)	–0.023 (0.010)	–0.017 (0.010)	–0.0002 (0.0015)	0.0007 (0.0068) [0.00005]	–0.0005
Shifting (percentage):	59	90	75			

Notes: The coefficient is  $\beta_8$  in equation (1). Standard errors are given in parentheses. In column (v), the number in brackets interprets the probit coefficient for employment, by calculating the change in probability of employment for a \$1 increase in the cost of the mandate. Change in total labor input is the change in total hours per week per worker for a 100-percent rise in the cost of the mandate. It is calculated by adding the change in hours at average employment to the change in employment at average hours, for a \$1 rise in cost, for the relevant treatment group. This is then divided by average labor input (hours times employment/population ratio) for the treatment group (and multiplied by cost per week in the parameterized cost case) to get the percentage change in labor input for a 100-percent rise in the dollar cost.

are reported in Table 6. The first row repeats the estimation using the demographic dummy, which is once again equal to 1 for married women aged 20–40, and 0 for all others (excluding married 20–40-year-old males and single 20–40-year-old women). There is evidence of shifting to wages, although the magnitude is approximately half that of the earlier regressions and is only significant at the 10-percent level. There is also an increase in hours and a fall in employment, as before, although neither the hours nor the employment coefficient is as large as the respective standard errors. The net effect on labor input is approximately zero. Thus, once again one finds a fall in group-specific wages with no effect on net labor input.

The next three rows of Table 6 examine the other demographic groups. The wage results for both single women and married men are weaker than those for married

women, although in no case are the estimates significantly different from each other. The overall treatment effect is about two-thirds the size of the effect for married women only. The labor-input results are once again mixed; overall, there is a small rise in hours and no effect on employment.

As above, moving to the individually parameterized cost yields further variation which can be used to pin down the extent of shifting to wages more precisely. The individually parameterized cost is calculated in the same way as for the state laws, and this is presented at the bottom of the right-hand side panel of Table 2.<sup>32</sup> The results are quite similar to those from the state laws,

<sup>32</sup>The only difference is that the predictor equations now no longer include controls for union status and its interaction with hours worked and part-time status.

with the cost averaging about 2 percent of wages.

The regression results using the individually parameterized costs are reported in Table 6; the same nonlinear specification described earlier is used. In column (i) the cost is normalized by hours worked, and in column (ii) it is not normalized. Here, the results are reversed from the previous case; the shifting estimate is higher and more significant when cost is *not* normalized. When cost is normalized by hours, the estimate indicates 60-percent shifting, but the estimate is insignificant. When it is not normalized, the shifting estimate rises to 90 percent, once again indicating approximately full shifting to wages, and it is statistically significant.

One reason for the worse results when cost is normalized by hours could be the fact that hours per week in the March CPS are for the previous year, while in the May survey they are the usual hours per week worked currently. The May measure may be a less noisy proxy for actual hours, which makes the estimate of cost per hour more precise and reduces the problems that arise from dividing both the dependent and independent variables by hours. To address this point, as well as to reduce the possible spurious influence of low-hours workers who are not covered by health insurance, I focus only on individuals who worked 35 hours per week or more in column (iii). The non-normalized estimate is similar for this restricted sample, and it indicates shifting of about 75 percent of the cost of the mandate; it is significant at the 10-percent level.

The fifth row of Table 6 reports labor-input results for the individually parameterized costs. Here, the results are the opposite of those uncovered earlier; there is now a *fall* in hours and a *rise* in employment. However, both the hours coefficient and the employment coefficient are completely insignificant, and there is no net effect on labor input. This confirms the conclusion that, on average, the cost of the mandate was fully shifted to wages.<sup>33</sup>

<sup>33</sup>I have also performed the two specification checks described above for the federal law change. First, I

## V. Conclusions

Mandated employer provision of employee benefits is a topic of increasing interest in America today, and many of the proposed mandates are group-specific ones. When there is a market failure in the provision of a particular benefit, a mandate may be an efficient means of correcting the failure. By exploiting the fact that employees value the benefit that they are receiving, mandates act as a benefits tax, and can (in the limit) be as efficient as lump-sum financing of the benefit expansion. However, this argument rests crucially on the ability of wages to adjust freely to reflect employee valuation of the mandated benefit; in the case of group-specific mandates, there may be a number of impediments to such free adjustment of relative wages.

The evidence in this paper supports the contention that there will be group-specific shifting of the costs of mandates such as comprehensive health insurance coverage for maternity. This finding was robust to a variety of different specifications of the effect of these maternity mandates. The fact that the wages of women of childbearing age and their husbands were free to reflect the valuation of these benefits suggests that group-specific mandates do not change the relative cost of employing the targeted group of workers. This is an important precondition for arguing that mandates are an efficient tool of social policy.

It is important to highlight that this paper focused only on the efficiency case for mandates as a tool of public policy. In fact, there are at least two equity arguments

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reran the regressions within each of four regions of the country: the Northeast, the Midwest, the South, and the West. Within each region, there was evidence of wage offsets, with the shifting estimates (from the regressions in which cost is not normalized by hours) ranging from 56 percent to 122 percent. However, none of the estimates was individually statistically significant, and none was significantly different from any of the others. Second, I controlled for industry-specific shocks by once again including industry-by-area-by-year controls. As with the earlier state laws, this had no effect on the coefficient of interest.

against mandates. First, the goal of the mandate may be to redistribute resources toward a certain group in society. In this case, group-specific shifting of the costs of a mandate undoes this redistributive policy. Second, mandates may be relatively regressive policies for financing benefit expansions. As Rodrigo Vergara (1990) shows, a tax on all labor which finances a benefit expansion will be more progressive than a mandate if the distribution of income is sufficiently unequal.

Furthermore, the case of maternity health benefits may illustrate how correcting one market failure can serve to exacerbate another. Health economists have shown that full insurance may lead to large welfare losses through the overutilization of medical resources (Martin S. Feldstein, 1973). Indeed, it is interesting to note that the number of cesarean births per 1,000 population doubled from 1975 to 1981 and that cesarean sections are now the second most frequently performed surgical procedure in the country (Health Insurance Association of America, 1989; U.S. Department of Commerce, 1990). More research is needed on the effects of increased coverage for maternity after the mid-1970's on the costs of childbirth. Did full insurance coverage lead to more costly treatment of the complications of childbirth?

Finally, this analysis has focused solely on the financing of expansions of insurance coverage and has ignored the potential benefits of mandates. If expanded coverage of maternity did lead to a change in the style of treatment of childbirth, this may have had beneficial effects on birth outcomes. Similarly, if maternity-leave provisions increase the continuity of labor-force participation of women, there could be important gains in terms of reducing workplace inequality. There have also been almost 1,000 other mandated benefits at the state level which are similar to these maternity mandates; that is, they dictate the inclusion of minimum levels of certain benefits in existing health insurance plans. Some mandates, such as mental illness and alcoholism treatment, may have substantial "offset" effects in terms of reducing medical expenditures in other parts of the health-care system

(McGuire and Montgomery, 1982). If these benefits can be estimated, they could be weighed against the wage costs to employees in evaluating the efficacy of future workplace benefit expansions.

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