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THE EFFICIENCY OF A GROUP-SPECIFIC MANDATED BENEFIT: EVIDENCE FROM HEALTH INSURANCE BENEFITS FOR MATERNITY

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I consider the effects of "group-specific mandated benefits", such as mandated maternity leave, which raise the costs of employing a demographically identifiable group. The efficiency of these policies, relative to more broad-based financing of benefits expansions, will largely be a function of the valuation of the mandated benefit by the targeted group. Such valuation should be reflected in substantial shifting of the cost of the mandate to group-specific wages; however, there may be barriers to the adjustment of relative wages which impede such shifting. I study several 1976 state mandates which stipulated that childbirth be covered comprehensively in health insurance plans, increasing the cost of insuring women of child-bearing age by as much as 5% of their wages. 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 the group which benefitted from these mandates; hours rise and employment falls, as may be expected from an increase in the fixed costs of employment. These results are confirmed by using a 1978 Federal mandate as a "reverse experiment".

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In an era of tight fiscal budget constraints, mandating employer provision of workplace benefits to their 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 a recent Democratic health care proposal was mandated employer provision of health insurance (New York Times, June 6, 1991, p. A22). Furthermore, 20 states have mandated some form of maternity leave since 1987, and a federal policy has been under consideration for a number of years.

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 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 which 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 (Gruber and Krueger, 1991).

This efficiency argument, however, may not apply to a certain type of policy which is particularly popular, the "group-specific mandate", which mandates the expansion of benefits for a demographically identifiable group within the workplace. One example is mandated maternity leave; another is the recent recommendation of a Federal advisory panel that there be mandated employer-provided health insurance coverage of all pregnant women and children (NYT, June 4, 1991, p. A18).[1] In these cases, there is likely to be less scope for the free adjustment of wages to reflect the valuation of the benefit by the targeted group, since there are barriers to <u>relative</u> wage adjustment within the workplace (such as anti-discrimination rules or

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[1]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 where workers are distinguished along more subtle dimensions.

workplace relative pay norms) which do not affect the adjustment of overall workplace wage levels. Without the ability of relative wages to adjust, there will be deadweight loss from these mandates even if the benefit is valued by that group. In fact, the deadweight loss from a group-specific mandate without shifting to group-specific wages may be higher than that from a payroll tax on all workers. Thus, in considering the efficacy of mandates relative to more broad-based means of financing group-specific benefit expansions, 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: mandated comprehensive coverage for childbirth in health insurance policies. A commonly accepted feature of health insurance benefits before the mid-1970s 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 (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 child-bearing 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 child-bearing age which are accounted for by maternity benefits. I find that, in 1977, at least 50% of women received lesser benefits for maternity than for comparable illnesses, and that the cost of adding complete maternity coverage to a health insurance plan could be as much as 5% of wages for women of child-bearing age.

I use the Current Population Survey (CPS) to study the extent to which the cost of these group-specific mandates were shifted to the targeted group's wages, and the effect on their hours of work and employment. The estimation controls for contemporaneous (state-specific) labor market shocks by comparing changes in the outcomes of this group to changes in the outcomes of other individuals in the same state who did not benefit from this mandate. This change in relative outcomes for the affected group within states which passed the law is also compared to the relative outcome change within states which did not pass the law, in order to control for any national shocks to relative earnings. By introducing three dimensions of controls (time, location, and demographic group), I am able to impose fairly weak identifying assumptions on my estimate of the impact of the state laws.

I begin by examining changes in wages, hours worked, and employment for married women of child-bearing 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 find that the wages of these women fell substantially. At the same time, their hours of work rose and employment fell, as may be expected from an increase in the fixed costs of employment; total labor input (hours per week per worker) remained unchanged. I then assign each worker an individual-specific cost of the mandate, which is a function of their age-specific cost of maternity coverage, the probability that they receive insurance on the job, and the predicted type of insurance coverage that they receive. This allows me to use individual variation in identifying the impact of the mandate, and to more precisely estimate the extent of shifting. Using this more parameterized model, I again find substantial shifting to wages (on the order of 100% of the cost of the mandate), with little effect on total labor input. This finding suggests that mandates may be a relatively efficient way to finance a group-specific benefit expansion, relative to workplace-wide taxation.

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

PART I: BACKGROUND - MATERNITY HEALTH BENEFITS IN THE 1970s

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 other common illnesses in this era, which was fairly complete.[2] Twenty-three states passed laws in the 1975-1979 period which 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.

The Extent of Differential Coverage

There are two previous estimates of the extent of differential coverage for maternity benefits in this era. Kittner (1978) used a 1976 Labor Department survey of health insurance plans to show that, while over 90% of plans included maternity benefits, nearly 60% 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% 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 HIAA looks only at new policies, which may have been supplementary to existing policies (and therefore less generous), and does not focus on women of child-bearing age.

To obtain more accurate estimates, I use the 1977 National Medical Care Expenditure Survey (NMCES), which collected data on demographics and health insurance coverage for a nationally representative sample of over 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,

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

so that my calculations will represent underestimates of the extent of discrimination in the early 1970s.[3]

The NMCES contains data on approximately 2900 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% of women did not have coverage for maternity benefits when they had coverage for either of these comparable illnesses. There were an additional 30% 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. Another 33% of women did not receive major medical coverage of normal pregnancies in the presence of major medical coverage of comparable illnesses, so that 83% of women faced either differential coverage or benefits.

The Cost of Expanding Maternity Benefits

Estimating the cost of the maternity mandates would require information on the increase in premiums for adding maternity 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 non-differential maternity benefits is now a national mandate. However, as with all Equal Employment Opportunity Commission legislation, this mandate does not apply to firms with less 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 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.[4] For each of several

[3]The data does 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.

[4]The incremental cost of maternity benefits does not seem sensitive to firm size, within the range of this program. My source for this program requested anonymity.

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 percent 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 which is attributable to each.[5] The cost for each group varies widely as a percent of wages, from less than one percent to almost 5%. To the extent that there was coverage for childbirth in health insurance plans before the mandates, but there were differential benefits, the cost figures of Table 1 will be an overestimate of the mandates' cost.

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 $4334 (HIAA, 1989); for married women 20-30 years old, the average probability of having a child in a year was 17.7% (U.S. Department of Health and Human Services, 1987). The annual expected cost of childbirth for this group is thus $767. Compared to the cost of $984 for family coverage for women of this age, this implies an insurance loading factor is 28%, which appears reasonable.[6] The high cost of childbirth meant that this mandate was an expensive one for many insured persons.

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[5]Two-thirds and one-third, respectively; from HIAA (1989). Unfortunately, the detailed CPI for obstetrics was discontinued in 1978.

[6]Furthermore, the costs in Table 1 will account for the possibility of a non-normal delivery; in 1989, cesarean sections cost 66% more than normal births on average (HIAA, 1989). A 28% loading factor is approximately the average for a firm of 50 employees, according to Congressional Research Service (1988b).

PART II: THE ECONOMICS OF GROUP-SPECIFIC MANDATED BENEFITS

The advantages and disadvantages of mandates as tools of social policy are discussed by Summers (1989), McGuire and Montgomery (1982), and others. There are a number of market failures which justify the government's intervention in the economy in order to ensure that certain benefits are provided. For the purposes of this discussion, however, I will take as given that the government's intervention is justified, and I will focus on the means of finance of that intervention. I assume that the government has two tools for finance: a workplace-wide payroll tax, and a mandate.

I focus first, in Figure 1, on the case of a workplace-wide mandate, versus public provision (to workers and non-workers) of the benefit which is financed by a payroll tax on workers. The payroll tax used to finance a public program for the benefit would reduce demand for labor to D_1, causing employment to fall to L_1, and a deadweight loss equal to the area ABC. When the benefit is provided through the workplace only, however, individuals will increase their labor supply in order to take advantage of it. Supply shifts out to S_1, employment only falls to L_2, and deadweight loss is reduced to the area ADE. The key point is that, by providing the benefit only to workers, mandates become benefit taxes; if employees value the benefit they are receiving, the increase in labor supply will reduce the deadweight loss of finance. If valuation is full, then there is no deadweight loss from the mandate. However, even if there is no valuation, there will be approximately the same deadweight loss as a payroll tax financed scheme.[7]

Extending this analysis to the case of a group-specific mandate, such as mandated health insurance for maternity, is straightforward. Figures 2A and 2B represent the labor markets for the group which benefits from the mandate (group A), and all other workers (group B). For the moment, assume that the cross-elasticity of demand between the groups is zero; I will relax this below. Consider the case where group A fully values the benefit which they are receiving. In this case, the mandate leads to a shift in demand to D^1_A and of supply to S^1_A. Wages fall by the cost of the benefit (to W^1_A), and employment remains unchanged (at L^0_A). There is no

[7]This ignores the fact that the mandate will only have to provide benefits to workers, so that less revenue will have to be raised.

deadweight loss from the mandate; a perfect benefits tax is equivalent to lump-sum taxation.[8] Note that, with 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.[9]

This conclusion is robust to a more detailed consideration of the cross-elasticities of demand of these two groups, in a simple model of labor supply and demand. In this model, these two groups are the only inputs into the production process; as such, they are substitutes by definition.[10] In addition, I make two further assumptions: (a) that the labor supply curve is positively sloped for both groups, and (b) that the own elasticity of demand for each group is greater than the cross-elasticity of demand.

The government mandates a benefit for group A which costs the employer C.[11] The members of group A value this benefit at some fraction α of the cost. Thus, labor supply and demand are described by:

$$\begin{gathered} L^S{}_A = L^S{}_A(W_A + \alpha C) \quad ; \quad L^S{}_B = L^S{}_B(W_B) \\ L^D{}_A = L^D{}_A(W_A + C \,,\, W_B) \quad ; \quad L^D{}_B = L^D{}_B(W_B \,,\, W_A + C) \end{gathered} \tag{1}$$

Equating supply and demand in each market, one can solve for the effect of the mandate on the wages of the two groups:

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[8]This point is derived rigorously by Vergara (1990).

[9]Rothschild and Stiglitz (1976) derive the conditions for market failure in competitive insurance markets.

[10]The model considers production along a given isoquant, ignoring the scale effect on labor demand. Ruling out negative cross-elasticities of demand means ignoring some potentially interesting cases, but analyzing this case would require introducing a third factor. See Hamermesh (1986) for a more detailed analysis.

[11]The presumption that a given worker increases the cost to the employer assumes full experience rating of insurance premiums; this is discussed in the appendix.

$$\frac{\partial W_A}{\partial C} = - \frac{(\alpha\eta^S{}_A - \eta^D{}_A)(\eta^S{}_B - \eta^D{}_B) - (\eta^D{}_{AB})^2}{(\eta^S{}_A - \eta^D{}_A)(\eta^S{}_B - \eta^D{}_B) - (\eta^D{}_{AB})^2} \tag{2}$$

$$\frac{\partial W_B}{\partial C} = \frac{\eta^D{}_{AB}}{\eta^S{}_B - \eta^D{}_B} * \left(1 + \frac{\delta W_A}{\delta C}\right) = \frac{\eta^D{}_{AB}\eta^S{}_A(1-\alpha)}{(\eta^S{}_A - \eta^D{}_A)(\eta^S{}_B - \eta^D{}_B) - (\eta^D{}_{AB})^2}$$

where $\eta^S{}_i$ = own supply elasticity for group i
$\eta^D{}_i$ = own demand elasticity for group i
$\eta^D{}_{AB}$ = cross-elasticity of demand

Under the assumptions described above, wages for group A fall. And, as above, there will be full shifting to the wages of group A if there is full valuation of the cost of the mandated benefit by the members of that group ($\alpha = 1$). There will also be full shifting if the groups are perfect substitutes (the cross-elasticity of demand is infinite), since employers will not hire members of group A if the total labor cost rises above that of group B. The effect on group B depends on two components: the extent to which the cost is shifted to A, and a term which is similar to the expression for the incidence of a tax, but with a cross-elasticity (rather than own elasticity) in the numerator. This is because, to the extent that group A doesn't value the benefit, it is like a tax on the employment of group A; with a positive cross-elasticity, this offers a subsidy to the employment of group B.

It is also possible to solve for the effect on the employment of group A:

$$\frac{\partial L_A}{\partial C} = \left(\eta^D{}_A + \frac{(\eta^D{}_{AB})^2}{\eta^S{}_B - \eta^D{}_B}\right)\left(1 + \frac{\partial W_A}{\partial C}\right) \tag{3}$$

Equation (3) shows that, as the cost of the mandate is shifted more to wages, there is less of an employment effect on group A; as in Figure 2A, with full shifting, there is no effect on employment.

There may, however, be a number of barriers to full group-specific shifting which are not present in this simple model. Most obviously, there are anti-discrimination regulations which prohibit differential pay for the same job across groups, or which prevent differential promotion decisions by demographic characteristic.[12] Furthermore, workplace norms which prohibit different pay across groups or union rules about equality of relative pay may have similar effects to anti-discrimination rules. Finally, if the group which benefits is disproportionately composed of workers earning at or near the minimum wage, there may not be scope for shifting to wages. With the exception of the last case, these will not be important considerations for a workplace-wide mandate, such as workers compensation insurance.

The effects of barriers to group-specific adjustment are illustrated in Figure 2A as well, once again for the case of full valuation. As before, I consider the case where the cross-elasticity of demand for the two groups is zero.[13] I also assume that the (compensated) elasticities of supply and demand, respectively, are equal across these groups, in order to make welfare comparisons. The effect of the wage barrier is to restrict wages to remain at W^0_A. Group-specific employment falls to L^1_A, and there is a deadweight loss of area FGH. Thus, even with full valuation, there is a distortion from the mandate.

Furthermore, this distortion will be higher than that which would arise if the benefit was financed by a payroll tax which was assessed on both groups of 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 (Stiglitz, 1986). In the payroll tax case, demand for group A shifts to D^2_A, with deadweight loss FIJ;

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[12]See Ehrenberg and Smith (1987) for a discussion of U.S. anti-discrimination legislation, which was in place well before the mid-1970s. In this discussion, I focus only on laws prohibiting discrimination in rates of pay and/or promotion. In fact, if there are <u>also</u> binding restrictions on relative hiring practices, then employers may be forced to bear the cost of the mandate. If discrimination rules are <u>only</u> binding on the hiring side, then they will not impede group-specific shifting in the case of full valuation.

[13]If the groups are perfect substitutes, then with barriers to relative wage adjustment a mandate may leave all of the members of group A unemployed. In a more complicated model, however, where there is a distribution of ability within each group, employers could react to the mandate by shifting up the ability distribution for group A. That is, by hiring more able members of group A, they can pay the same (dollar) wage and receive a higher marginal product. The result could be full shifting to wages, even with this nominal barrier, and there would be a fall in employment of the low-ability members of group A. With some finite, but non-zero, cross-elasticity, then there will be a substitution of the members of group B for those of group A at the prevailing wage, once again with a fall in the employment of group A.

demand for group B (in figure 2B) shifts to D^1_B, with deadweight loss KLM. The sum of these areas is, by definition, less than the area FGH; since the tax base is broader, the deadweight loss of revenue raising is smaller.

Thus, unlike in the one-group case, a mandate can be worse (in efficiency terms) than a payroll tax when financing a group-specific benefit, if there is an impediment to the free adjustment of relative wages. As Summers (1989) 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 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" (p. 182). A similar conclusion would arise in the case where the benefit is not valued by employees. With a workplace mandate, the deadweight loss would be comparable to that from a payroll tax; with a group-specific mandate, it may be higher.

What Can We Learn From the Empirical Work?

In the empirical work below, I will estimate the extent of group-specific shifting to wages of the cost of mandated health insurance for maternity. This estimate alone cannot resolve the question of whether mandates are more efficient than payroll taxes; this requires a number of assumptions about elasticities of demand and supply.[14] However, the empirical work can address one issue which is central to the debate over mandates. If there is not shifting to wages, then either the group which benefits does not value the mandate, or there are impediments to the adjustment of relative wages to reflect that valuation. Since the efficiency case for mandates rests largely on employee valuation which is reflected in wage adjustments, this is an important consideration. Therefore, this investigation should be viewed as a first step towards assessing empirically the case for mandates, by investigating whether it appears that relative wages can adjust to reveal employee valuation of a group-specific benefit.

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[14]For example, just because there is less than full shifting does not mean that the mandate is necessarily inefficient. Consider the case where the cross-elasticity between the two groups is zero, and they have equal (compensated) labor supply elasticities. If the (compensated) demand for the group which benefits is not very elastic, while demand for the other group is, then a group-specific mandate may still be efficient even with no valuation. The efficiency gains from taxing the less elastic source may outweigh the losses from a narrower tax base.

An important caveat to this discussion is that I have focused purely on efficiency considerations, and 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. Rather, this may be viewed as the mandate being undone by the adjustment of wages. In this case, the deadweight loss from broad-based financing may be a "price" that society is willing to pay in order to direct more resources towards one group. Thus, in considering the results which 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?

PART III: DATA AND IDENTIFICATION STRATEGY

This study will focus primarily on three of the twenty-three states that passed maternity mandates in the 1975-1979 period: 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 which were identified separately in the survey before 1977.

My set of "non-experimental" states were chosen using similar criteria: they had to be separately identified in these CPSs, 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.[15]

The data consists of observations on all individuals in these set of experimental and non-

[15]Pennsylvania could not be used as a "mid-Atlantic" control because it implemented insurance broad sex anti-discrimination 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 very similar if North Carolina is excluded.

experimental locations, for two years before the legislation (1974, 1975), and 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 Table 2, for the experimental states and the controls, for the "before" and "after" years, for all wage earners. Hourly wages are in 1978 dollars. I exclude any individuals who report earning less than $1/hr or more than $100/hr in 1978 dollars. I also exclude any person less than 20 years old or above 65, and the self-employed. The means are unweighted.

There are not many striking differences across the groups of states: the control states have lower wages, are less unionized, and are more manufacturing oriented. Differences in unionization and industry distribution, as well as systematic wage differences across locations, are controlled for in the estimation. Both sets of states saw an increase in the fraction of the workforce represented by women of child-bearing age (20-40). Overall, wages fell more in the experimental states than in the non-experimental states; below, I will assess whether the maternity mandates played any role in this relative fall.

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 state which 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 which passed the laws and those which did not. If I had a large number of states which passed these laws, this "differences-in-differences" strategy would perhaps be sufficient to identify the impact of the law.[16] That is, the difference between changes in outcomes for treatment individuals in the experimental states and changes for similar persons in the non-experimental states would be the impact of the law.

However, my sample of states is small, and the three states are fairly similar. Thus, it is plausible that there were state-specific shocks over this period which are correlated with the passage of these laws. I therefore include a third level of controls, state by year effects. I do

[16]See Card (1990) for an application of this type of analysis.

this by comparing the treatment individuals in the experimental states to a set of control individuals in those same states, and measuring the change in <u>relative</u> outcomes. This change is then compared to the change in relative outcomes in states which did not pass maternity mandates, to control for national shocks to the relative earnings of these groups. The identifying assumption of this "differences-in-differences-in-differences" (DDD) estimator are fairly weak: it simply requires that there be no contemporaneous shock which affect the relative outcomes of the treatment group in the same state-years as the law.

The treatment group here are 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.[17] The controls are other individuals who were directly unaffected by the law. However, the CPS (before May 1979) contained no information on health insurance coverage. I am thus unable to exactly identify 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 from ages 20-40. This group will contain the individuals for whom the mandate was most costly (according to Table 1), married women of child-bearing age. My control group is all individuals over 40, and single males from 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.[18] This "treatment dummy" approach has the virtue that it is relatively non-parametric.

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

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[17]Ideally, the treatments would also be restricted to those with differential maternity benefits. As I discuss in Appendix A, however, I am not confident enough in my estimates of the incidence of discrimination to make this an integral part of the analysis.

[18]That is, there are three demographic subsets of costly individuals under the mandate, and the treatment dummy approach focuses on just one (married women). This is done for expositional ease; the effects on these other groups, as well as the overall treatment effect, is presented below.

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 which follows, I will rely on both the treatment group dummy and the individually parameterized cost measure.

PART IV: THE LABOR MARKET IMPACT OF THE STATE LAWS

DDD Estimation

Table 3 illustrates differences-in-differences-in-differences 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 which passed the laws to the change for 20-40 year old married women in the non-experimental states. Each cell contains the mean average real wage for the group labelled on the axes, along with the standard error and the number of observations. There was a 3.4% fall in the real wages of women in the experimental states over this period, compared to a 2.8% rise in the real wages of women in other states. Thus, there was a (significant) 6.2% relative fall in the wages of women of child-bearing age in states which 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 over forty 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%. 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 of these two figures, there is a 5.4% fall in the relative wages of 20-40 year old married women in the states which passed the laws, compared to the change in relative wages in the non-experimental states. This significant DDD estimate provides some evidence that the cost of a group-specific mandate is borne by members of that group. However, the magnitude of the estimate seems unreasonably large given the costs described in

Table 1. Furthermore, 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 the law. First, however, I discuss how the analysis of Table 3 can be expressed within a regression framework.

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 which affect the outcome variables of interest. The regression equation has the following form:

$$W_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 \delta_j + \beta_3 \tau_t + \beta_4 TREAT_i + \beta_5 \delta_j * \tau_t + \beta_6 \delta_j * TREAT_i + \beta_7 \tau_t * TREAT_i + \beta_8 \delta_j * \tau_t * TREAT_i \quad (4)$$

where i indexes individuals
j indexes states (1 if experimental state, 0 if non-experimental)
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
δ_j is a fixed state effect
τ_t is a fixed year effect
TREAT is a dummy for treatment group (1 if treatment, 0 if control)

The analogy of this regression to Table 3 is straightforward. The third-level interaction (β_8) captures all variation in wages specific to the treatments (relative to controls), in the experimental states (relative to the non-experimentals), 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 regression includes fixed year, state, and group effects, as well as second-level interactions of state by year effects, group by year effects, and group by state effects. The set of demographic covariates used includes education, experience and its square, sex, marital status, a marital status by sex interaction, a dummy for non-white, and a control for union status. In addition (not reported), there are dummies for 15 major industries, and separate year dummies for 1974 and 1978.

Table 4 presents the estimates from (4). In the first two columns, the dependent variable is the log hourly wage. In the first column, I leave out the demographic controls, and include only the first, second, and third level interactions. This regression exactly parallels Table 3. Indeed, the estimated effect on the treatment group, a 5.5% fall in wages, is virtually identical to that in Table 3; the difference is due to rounding error.

In column (2), I add the set of demographic controls. The coefficient on the third level interaction indicates that wages fell by 4.3% for the treatment group; it is marginally significant at the 5% level. While this is slightly smaller than the estimate from column (1), 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.

The other coefficients in the regression are of their expected signs and magnitudes. There is an estimated 7% return to an additional year of schooling; earnings increase with experience, but at a decreasing rate; females and blacks earn less; married men earn more and married women earn less; and the estimated union premium is 13.5%. There is a 1.2% fall in wages for the within-state control group (the coefficient on "After*Experimental", the state by year effect). This suggests that the experimental states, on average, saw a negative shock over this period.[19]

The next two columns of Table 4 examine the effects of this mandate on hours of work and probability of employment. The model above predicted that, in the presence of full shifting to wages, total labor input would remain unchanged. However, even if employers are able to shift the cost to wages on average, there is reason to believe that the <u>composition</u> of labor input will change. This mandate represents an increase in the fixed costs of employment, and is thus more costly for low hours employees. If employers are not able to shift the cost to wages in inverse proportion to hours worked, then a natural reaction would be to increase hours and reduce employment. This would enable the employer to reduce the cost per hour of the mandate while leaving total labor input unchanged.

On the other hand, since part-time workers may be more readily excluded from health

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[19]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.

insurance coverage, there may be a countervailing effect, as employers replace full time employees with their (relatively less expensive) part-time counterparts. In this case, hours would fall and employment would rise, and total labor input would remain unchanged.[20] Furthermore, the desired supply response to these mandates from the individual perspective is for increased employment among those out of the labor force, and for part-time workers to increases their hours in order to qualify for health insurance, so that both employment and hours rise. Thus, the effect on hours and employment are uncertain, even if the cost of the mandate is able to be shifted to wages on average.

As Table 4 shows, there is some evidence for the first story. In column (3), the dependent variable is the log of weekly hours of work; hours rose by a significant 4.9% for the treatment group. I measure employment by a dummy variable which equals one if the individual is employed, and zero otherwise (unemployed or out of the labor force); the employment regressions are run as probits. The result in column (4) shows an insignificant fall in employment; it implies that the treatments saw a 1.6% fall in employment over this period, relative to the sets of controls.[21] 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% of average hours per week for the treatment group.[22]

As mentioned above, married women are only one of three groups of workers which are potentially affected by these mandates. The costs of employing single women of child-bearing

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[20]Of course, another option for employers is to drop insurance coverage altogether. However, the 1978 Federal Law contained a provision restricting insurance coverage to be at least as generous as its October 1978 level for one full year (Leshin, 1981). It is unclear whether the state laws contained similar provisions. Gruber (1992) finds that there was little effect of other expensive state mandated benefits on the propensity of firms to offer health insurance.

[21]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.

[22]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.

age will rise as well, as will the costs 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 summarized in Table 5. 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 sizeable 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 (ie. from a regression where the treatment dummy is one for members of all of these groups) is consistent with that for married women only: a decrease in wages (which is significant at the 11% level), a rise in hours, and a fall in employment, with an overall labor input effect which 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 section, I address this heterogeneity by attempting to model the individual specific cost of these mandates.[23]

Individual Parameterization of the Cost of the Mandates

In assessing the cost of these maternity mandates for each individual, one must consider: whether the individual is covered by insurance, and whether that insurance provides differential maternity benefits; whether this coverage is from the individual's own job, or is through a family member; whether the coverage is for the entire family, or just the individual;[24] and the individual's (or spouse's) age-specific probability of child-bearing. Unfortunately, the CPS does

[23]While the results do vary across these three groups, 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.

[24]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 child-bearing. Indeed, the relative cost difference between the two types of policies is almost exactly the same as the difference in the relative probabilities of child-bearing between single and married women (calculated using the data in U.S. DHHS (1987)).

not contain information about insurance coverage during 1974-1978. 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 Current Population Survey Pension Supplement; and data on type of insurance coverage from the 1977 NMCES. These cost calculations are described in detail in Appendix A; 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 asks whether the individual has insurance on the job, 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.

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 probability of having insurance coverage on the job. This yields a predicted weekly cost which varies by the six ten-year age groupings in Table 1.[25] The results of this exercise are presented in Table A1. The cost averages $3.91 a week, which is 1.9% of wages on average; it has a maximum value of 28% of wages. The average cost is not appreciably different across the experimental/non-experimental 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

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[25]In an earlier version of this paper, I also let the cost vary by single-year age-specific probabilities of child-bearing. The results were similar; I rely on the ten 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.

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.

The individually parameterized cost measure can be introduced in place of the treatment dummy in equation (4), to yield:

$$W_{ijt} = \alpha + \beta_1 X_{ijt} + \beta_2 \delta_j + \beta_3 \tau_t + \beta_4 COST_i + \beta_5 \delta_j * \tau_t + \beta_6 \delta_j * COST_i + \beta_7 \tau_t * COST_i \qquad (5)$$
$$+ \beta_8 \delta_j * \tau_t * COST_i$$

where $COST_i = \dfrac{\text{individually parameterized weekly cost}}{\text{average group wages * hours worked per week}}$

In this equation, weekly cost is normalized by both a measure of average wages for the individual's demographic group, and the hours worked per week. The average wages normalization arises from the fact that the calculated cost of the mandate is expressed in dollars, so that it would be interpreted most straightforwardly in a levels wage equation, rather than the log wage specification used earlier. However, since wages are distributed log-normally, a levels wage equation violates the normality assumptions necessary to perform Ordinary Least Squares. 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 calculate average wages for each of 240 demographic group (6) by state (8) by year (5) cells, where the demographic groups are defined by ten-year age intervals for each of the three treatment groups. This average is used to normalize the cost measure. The results where this normalization is not used are quite similar when evaluated at the mean wage for the treatment group; other variations in the specification are presented below.

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.[26] 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.

To the extent that my estimate of the cost of the mandate is correct, a coefficient of -1 on the third-level interaction would indicate 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.[27]

Individual Parameterization - Results

Table 6 presents wage regressions with the individually parameterized costs. In the first column, the cost is normalized by average wages and hours worked. The regression indicates very sizeable shifting to wages, on the order of 240% of the cost of the mandate. While this coefficient is significantly different from zero, it is not significantly different from one, which would imply full shifting to wages.

In the next two columns, I assess the robustness of this finding to changes in the specification of the mandate cost. As discussed above, the individual measure of mandate cost (in dollars) should be normalized by individual wages in a log wage equation; since this was not possible, I used a rough proxy by demographic group. An alternative is to note that the wage equation can be specified as: $W = (e^{\beta X} + COSTNN)e^{\epsilon}$, where COSTNN is the individual hourly mandate cost not normalized by wages, and ϵ is a normally distributed error term. Taking logs

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[26]That is, the fall in wages for a dollar increase in fixed costs of employment is inversely proportional to hours worked.

[27]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 prediction equations used fit fairly well, predicting between 73% and 85% of the cases correctly.

of both sides of this equation, we obtain: $\log(W) = \log(e^{\beta X} + COSTNN) + \epsilon$. This non-linear model thus has both a normally distributed error and a directly interpretable coefficient on the individual mandate cost in dollars. In column (2), I estimate this model, where all variables which do not involve an interaction with the mandate cost are exponentiated, and the mandate cost and all interactions with it are not. I find a similar coefficient to that in column (1).[28]

In column (3), I return to the standard log wage specification, but I remove the normalization of the mandate cost for hours worked (while retaining that for average wages). At the mean hours worked for the treatment group, there is 109% shifting to wages, but the estimate is not significant (although it is not significantly different that in column (1)). 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 these workers. However, only 20% 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 prediction 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 (4) of Table 6, I focus only on full time workers (35 hours per week or more); over 75% 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

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[28] 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 percent 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.

quite high for this group.[29] The results reveal that the conclusion of group-specific shifting was not driven by low hours workers. The shifting estimate for full time workers is significant at the 6% level, and it lies between the estimates of columns (1) and (3).

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 (5) and (6) of Table 6. In column (5), the dependent variable is the log of hours worked, and the cost is not normalized by average wages or hours.[30] The results reveal a rise in hours worked which is marginally significant at the 10% level; for a \$1 increase in cost, hours are predicted to rise by about .5% of their average level.

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 their demographic group (single females, married females and married males). That is, I assume that if non-employed 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 (6) of Table 6 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% fall in probability of employment.[31] Taken together with the hours coefficient, this implies a rise in total labor input per worker of 0.63% of its average value for a 100% rise in cost. This

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[29]The shifting estimate is similar is cost is normalized (-1.32 rather than -1.60), but it is only significant at the 10% level.

[30]While normalizing by hours is once again theoretically appropriate, it would induce a spurious negative correlation between predicted cost and hours worked. Furthermore, the prediction equation for the probability of insurance coverage used here does not control for hours worked, since this would induce a spurious positive correlation.

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

can be contrasted to the estimate of a fall in wages of 4.7% of their average value. Thus, the estimated effect on net labor input is small, which confirms the conclusion of substantial shifting to wages.[32]

Specification Checks

Since the results above represented the composite effects of three separate "experiments", it is useful to assess whether one of the experiments is driving the results. I do this in two ways in Table 7, which presents the coefficients on the first, second, and third level interactions from the wage regression model used in Table 6 (normalized by hours).[33] In columns (1) and (2), I run the regressions separately within region, comparing Illinois only to Ohio and Indiana ("Mid-West"), and New Jersey and New York only to Massachusetts, Connecticut, and North Carolina ("Atlantic"). The coefficient is significant within each region, and is virtually identical across regions; this implies that the findings are not driven by region effects.

In column (3), I look separately at the two Atlantic states, by freeing up the experimental location dummy by state within the Atlantic region. The coefficient for New York is quite large, although not significantly different from that for Illinois, and the coefficient for New Jersey is zero. There is no obvious difference in the wording of the three laws which explains this discrepancy. In any case, it is important to note that the overall result is not driven by any one state's experience; the cost coefficient remains significant (at the 10% level) even if New York is excluded.

In Table 8, I examine the regional variation in the labor input effects of the mandates. I present only the coefficient of interest (the third-level interaction) from regressions such as those in Table 6. In the first column, the dependent variable is the log of hours worked. Column (2) shows a probit regression for employment, and column (3) interprets the coefficient as the change in probability of employment for a one dollar increase in the cost of the mandate.

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[32]I also tried using a dummy for part-time work (less than 35 hours per week) as a dependent variable in these regressions. There was a sizeable and significant negative coefficient on the third level interaction, which is consistent with an increase in the fixed costs of employment.

[33]The regression includes, but I do not report the coefficients on, the usual set of demographic controls, year dummies, and industry dummies.

The rows in this table correspond the columns in Table 7.

The final column of this table calculates the change in total labor input per worker (as a percentage of its average value) for a 100% increase in the cost of the mandate. Overall, there is a 0.63% increase in total labor input, as mentioned above. Within the Mid-West, there is a 1.8% increase in labor input, as the rise in hours is quite large relative to the employment fall. Within the Atlantic states, there is a small net fall of 0.4% of labor input, which represents the weighted average of a rise of 1.2% in New Jersey and a fall of 2.2% in New York.[34] Thus, while the individual regressions offer little by way of statistical significance, they are supportive of the conclusion of a rise in hours and a fall in employment which leaves net labor input unchanged.

An alternative interpretation of the findings described thus far would be that they were driven by group-specific demand shocks to the experimental states; that is, the identifying assumption of the DDD estimator is not valid. One way to assess the role of demand shocks is to look for a control group for whom labor demand should be more closely tied to that of the treatment group. There is a large literature which discusses "vintage effects" in labor market performance; that is, older workers may not be readily substitutable for younger ones (Freeman, 1979). To the extent that the labor market is segregated by age, then older workers may be insufficient controls for state-specific shocks which affect the treatment group. I address this possibility in Tables 7 and 8 by only using the remaining group of non-treatment young workers, 20-40 year old single males, as the control group. As column (4) of Table 7 shows, the shifting estimate is somewhat smaller relative to this restricted set of controls. However, it is still significant (at the 5.5% level), and it is insignificantly different from that in Table 6. Furthermore, the labor input estimates in the sixth row of Table 8 are close to those in the first row.

Another way to assess the role of demand shocks in driving these results is to enrich the set of individual controls included in the regression. These regressions have included a set of

[34]Note that, while the net fall in labor input is largest for New York, the wage effects are largest for New York as well, implying (at mean cost and wages) a fall in wages of 6.6%. Thus, the fall in net labor input is small relative to the wage fall.

15 industry dummies, to capture fixed pay differences across industries. Column (5) of Table 7 assesses the importance of changes in the distribution of wages by industry on the findings, by allowing these industry dummies to vary by state and year.[35] This is a very general specification, which allows for area-specific, year-specific, and area by year-specific shocks by industry. Nevertheless, the shifting estimate is virtually identical to that in Table 6; the within-region results (not reported) are quite similar as well. While this does not rule out the possibility of demand shocks along other dimensions, it shows that general changes in the wage structure by industry, location, and time are not driving these results.

Summary

Tables 4-8 find extensive shifting of the cost of the maternity mandates to group-specific wages, both in a relatively unrestrictive treatment group dummy model, and in a more parametric specification which tried to capture individual variation in the cost of the mandates. This finding was consistent across a variety of different specifications. The labor input results were also consistent with full shifting, along with a compositional reaction to an increase in the fixed costs of employment. Nevertheless, these results emerged from the analysis of only three experiments, using a select set of control states. Furthermore, the main findings are not consistent across all three states. This suggests the desirability of finding an example of a group-specific mandate which affected a broader range of states. The Federal PDA of 1978 provides such an example.

PART V: THE FEDERAL EXPERIMENT

The Experiment and the Data

The state laws studied in Part IV were the prelude to the Federal Pregnancy Discrimination Act, which was implemented in October, 1978. This legislation was more expansive that the state mandates, covering the entire employment relationship, rather than just health benefits. Nevertheless, health insurance industry representatives estimated that the effects on health benefit plans would represent two-thirds of the cost of implementing the PDA

[35]Actually, by before/after the law change (only two "years").

(Commerce Clearing House, 1978).

To the extent that the Federal legislation simply duplicated existing state law for those states with maternity mandates, it provides a distinct opportunity to study the impacts of increasing the health insurance costs of maternity. In this case, the states which had already passed laws are the controls, and the states which had not are the experimentals. Furthermore, by this later date the CPS was identifying all states separately, so that I am able to use as control states all those states which had passed laws by January 1, 1977 (12 states), and as experimentals all states which did not pass laws during 1977 and 1978 (28 states).[36] These states are more broadly representative of the country as a whole, which should help to overcome any problems induced by using three (fairly similar) states as experimentals in the earlier estimation.

I use the 1978 and 1979 (before), and 1981 and 1982 (after) March CPS to study the impact of the Federal law, within the regression framework developed above. 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.[37] The only other differences from the regressions above are that there is no data on union status and that the year dummies are now for 1978 and 1981.

The Results

Table 9 presents several estimates of the impact of the PDA on wages. In column (1), I repeat the estimation using the demographic dummy, which is once again equal to one for married women aged 20-40, and zero 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

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[36]The controls are: Arkansas, California, Colorado, Hawaii, Idaho, Illinois, Iowa, Maryland, New Jersey, New York, Tennessee, and Wisconsin. The experimentals 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 non-discrimination rules for all groups in 1979.

[37]Thus, the actual labor market data come from 1977, 1978, 1980, and 1981. The use of the Marches was dictated by the fact that, starting in 1979, the May CPS only asked earnings information of one-quarter of the sample.

is approximately one-half that of the earlier regressions; it is significant at the 10% level. The coefficient for single females (not reported) is about one-half as large, and is only as large as its standard error; for married males, wages fall by less than 1%, which is insignificant.

In columns (2)-(6), I present the results where the individual cost of the law has been modelled in the same way as in Tables 6-8.[38] In all cases, the cost is normalized by demographic group average wages for 48 location (2) by year (4) by demographic group (6) cells. In column (2), the cost is normalized by hours worked, and in column (3), 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 63% shifting, but the estimate is insignificant. When it is not normalized, the shifting estimate rises to 90% and it is marginally significant at the 5% level.

One reason for the worse results when cost is normalized by hours could be the fact that hours per week in the March survey 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 which 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 (4). The non-normalized estimate is slightly smaller than that in column (3), and is only significant at the 12% level. Nevertheless, the finding is similar for this restricted sample, and it indicates shifting of about 77% of the cost of the mandate; the result is very similar when it is normalized by hours for this group.

One problem with comparing these results to the previous ones is the Federal law outlawed pregnancy discrimination in any facet of the employment relationship. As such, it may have imposed additional costs of employing women of child-bearing age which would not affect the costs of employing their husbands; the predicted cost for men may now be overstated relative to that for women I examine this in columns (5) and (6) of Table 9, where I use as the

[38]The only difference is that the prediction equations now no longer include controls for union status.

treatment group just women, and exclude 20-40 year old married men from the regression. The result where cost is not normalized by hours worked is stronger than that in column (3), and indicates shifting on the order of 100%; the hours-normalized result is almost identical to that in column (2).

Table 10 examines the effect of the PDA on labor input, using the specifications employed above, both with and without married men included in the treatment group. For the treatment dummy case for married women (the final row), there is an increase in hours and a fall in employment, as before, but the net negative effect is fairly sizeable (about -2%), and is of the same size as the fall in wages in column (1) of Table 9 (although neither the hours nor the employment coefficient are as large as their standard errors). However, for the parameterized results for the entire treatment group, the result is actually the opposite of that uncovered earlier; there is now a <u>fall</u> in hours and a <u>rise</u> in employment. Nevertheless, both the hours coefficient and the employment coefficient are completely insignificant, and there is no net effect on labor input. For women treatments only, there is a rise in both hours and employment, although both are once again insignificant; there is a small net rise in labor input of 1.3%. Thus, while the shifting estimates are somewhat smaller than those for the evaluation of the state laws, they are consistent with the conclusion of substantial shifting to group-specific wages, with little net effect on labor input.

PART VI: 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 this market failure. By exploiting the fact that employees value the benefit which 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 freely adjust 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, however, supports the contention that there will be group-specific shifting of the costs of mandates such as comprehensive health insurance coverage for maternity. While this does not prove that women of child-bearing age and their husbands valued the benefit which they were receiving, it does suggest that wages are free to reflect such valuation. This is an important precondition for arguing that mandates are an efficient tool of social policy.

However, while this finding was robust to a variety of different specifications of the effect of these maternity mandates, there was some variation in the effects of these mandates across the several natural experiments used; in particular, the results for the Federal law were weaker than those for the state level regulations. This suggests the value of further evaluations of group-specific mandates, such as the recent spate of maternity leave laws which have been passed at the state level.

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 against mandates. First, the goal of the mandate may be to redistribute resources towards 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 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 (Feldstein, 1973). Indeed, it is interesting to note that the number of cesarean births per 1000 population doubled from 1975 to 1981, and that cesarean sections are now the second most frequently performed surgical procedure in the country (U.S. Department of Commerce, 1989; HIAA, 1989). More research is needed on the effects of increased coverage for maternity after the mid-1970s 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 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 1000 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.

APPENDIX A: THE COST OF THE MANDATES

This appendix details the creation of the individual-specific measure of the cost of the maternity mandates. The cost is calculated distinctly for each of three groups of persons aged 20-40: married women, single women, and married men. For all others, it is set to zero. It is calculated as:

$$COST_i = \frac{(COSTINDIV_i * pr(COVINDIV_i) + COSTFAM_i * pr(COVFAM_i)) * pr(INSCOV_i)}{4 * DEFL * AVWAGE_i * HRSWK_i} \quad (A1)$$

where $COST_i$ = individual cost for someone with demographic characteristics i

$COSTINDIV_i$ = the cost of adding maternity benefits to individual health insurance coverage for person i

$pr(COVINDIV_i)$ = the probability of having individual coverage, given that i has insurance coverage

$COSTFAM_i$ = the cost of adding maternity benefits to family health insurance coverage

$pr(COVFAM_i)$ = the probability of having family coverage, given insurance coverage

$pr(INSCOV_i)$ = the probability of being covered by insurance on the job

DEFL = deflator to 1978 dollars

$AVWAGE_i$ = average wage for i's demographic group*state*year cell

$HRSWK_i$ = hours of work per week

Data on dollar costs (COSTINDIV, COSTFAM) comes from Table 1; the ten-year age specific cost is used, so that there are six dollar cost categories for the three treatment groups. The probability that individual i is covered by health insurance on the job is modelled using an extract from the 1979 May CPS Pension Supplement of all workers in each of the three demographic groups with non-missing data on health insurance coverage. A dummy variable for having health insurance is regressed on: education, experience and its square, a dummy for non-white, union status, hours worked, a dummy for working less than 18 hours per week, union status times hours worked, union status times the 18 hours dummy, and 15 industry dummies. These regressions were run separately for each of the three demographic groups. They were run as linear probability models (LPM), using Generalized Least Squares, and as probits.[1] These two predictors were highly correlated for each group, and yielded similar results in the outcome (wages and hours) regressions; the LPM results were used.

The type of insurance coverage was modelled using an extract from the NMCES of all employed individuals in each of these groups who had health insurance coverage on the job and who were the primary insured for their household. I created a dummy variable which was equal to one if the individual had family coverage, and zero if the individual had individual coverage. This was regressed on: education, age and its square, a dummy for non-white, head of household status (for single women), nine

[1]The LPM is run using weighted least squares, where the weights are: $1/(\sqrt{\hat{p}*(1-\hat{p})})$, where $\hat{p}$ is the predicted probability of health insurance coverage from an OLS regression.

industry dummies, spouse's employment status, and nine dummies for spouse's industry. Once again, separate linear probability models were run for each of the three demographic groups, using GLS. These six regressions (three for predicting insurance coverage, three for type of insurance) are available upon request.

The 1990 costs are deflated to 1978 levels using a weighted average of the detailed CPI for hospital services (2/3 weight) and physician services (1/3 weight). Since the cost in the insurance calculator is monthly, I normalize by 4 weeks; this assumes full-month work, and so it will underestimate the cost for part-month workers. The fact that the cost is monthly implies that this may not be a problem for seasonal workers. As discussed in the text, I normalize by average wages and by hours worked. Average wages are tabulated for each of 240 groups: six demographic categories (ten year age groups within each of the three demographic groups), for each of the eight states, for each of the five years.

Another factor which will determine the effect of these laws is the extent of differential benefits, which I have chosen not to model. There is substantial uncertainty of what constitutes differential benefits, as well as the extent to which different types of differentials are costly to eradicate. Furthermore, the NMCES only gives details of insurance coverage for a small number of insured individuals, and I cannot control for which states had passed maternity mandates by 1977. Thus, it was not feasible to model the extent of discrimination at the individual level.

The analysis of this appendix assumes that employers will bear the full additional premium costs of hiring a worker in one of the treatment groups. This will only be true under full experience rating of employer health insurance premiums. According to CRS (1988a), commercial insurers have used experience rating since their growth began in the 1940s; in 1978, commercial insurers had a 50% market share nationwide (based on premium income, from HIAA (1981)). Blue Cross and Blue Shield plans had traditionally "community rated" insurance premiums, which would mean that employers would not bear the additional cost of an employee of child-bearing age. However, during the 1950s, many Blue Cross/Blue Shield plans began experience rating large employers, and others moved to "demographic rating", under which the demographic composition of the workforce determines the rate (so that the mandate would be costly for the marginal employee). By the late 1980s, the Blues experience rated all larger groups, and even some small employers (CRS, 1988a, p. 21). While the exact extent of experience and demographic rating cannot be estimated for the late 1970s, it seems to have been fairly widespread by that point.

Finally, under the Employee Retirement Income Security Act (ERISA) of 1974, self-insured employers are exempt from state regulations, including state mandated benefits. Thus, while all firms would have been subject to the Federal PDA, this implies that large firms that could afford to self-insured may have avoided the effect of the state laws. However, while over 40% of firms with more than 500 employees self-insured in 1989 (Gruber, 1992), self-insurance was not very widespread before 1980. Jensen and Gabel (1988) find that only 21% of firms with more than 100 employees self-insured in 1981, and that self-insurance was growing rapidly over the early 1980s; presumably, therefore, the extent of self-insurance in 1978 was even lower.

Table A1 below gives the mean weekly and hourly cost of the mandate, as well as the hourly cost as a fraction of hourly wages, for: all treatment individuals; treatment individuals by experimental/non-experimental state and before/after the experiment; and different groups of treatments.

Table A1: The Cost of the Mandate - Individual Parameterization

	Cost per Week	Cost per hour	Cost/Wages
All Treatments	3.91 (1.66) [7.50]	0.099 (0.046) [0.919]	0.019 (0.010) [0.277]
Non-Experimental States in Before Years	4.01 (1.68) [7.50]	0.100 (0.043) [0.917]	0.020 (0.010) [0.277]
Non-Experimental States in After Years	3.87 (1.65) [7.50]	0.096 (0.045) [0.914]	0.020 (0.010) [0.138]
Experimental States in Before Years	3.92 (1.66) [7.45]	0.102 (0.055) [0.919]	0.018 (0.009) [0.173]
Experimental States in After Years	3.85 (1.63) [7.43]	0.097 (0.042) [0.906]	0.018 (0.010) [0.196]
Married Females 20-40 Years Old	2.83 (1.33) [6.78]	0.078 (0.033) [0.564]	0.019 (0.010) [0.151]
Single Females 20-40 Years Old	2.40 (0.85) [5.05]	0.063 (0.021) [0.334]	0.015 (0.007) [0.070]
Married Males 20-40 Years Old	5.13 (1.02) [7.50]	0.123 (0.045) [0.919]	0.020 (0.011) [0.277]

Notes:

1) Cells in rows 1-4 contain average for treatment individuals; cells in rows 5-7 contain average over all states/years

2) Standard deviations in parentheses; maxima in square brackets.

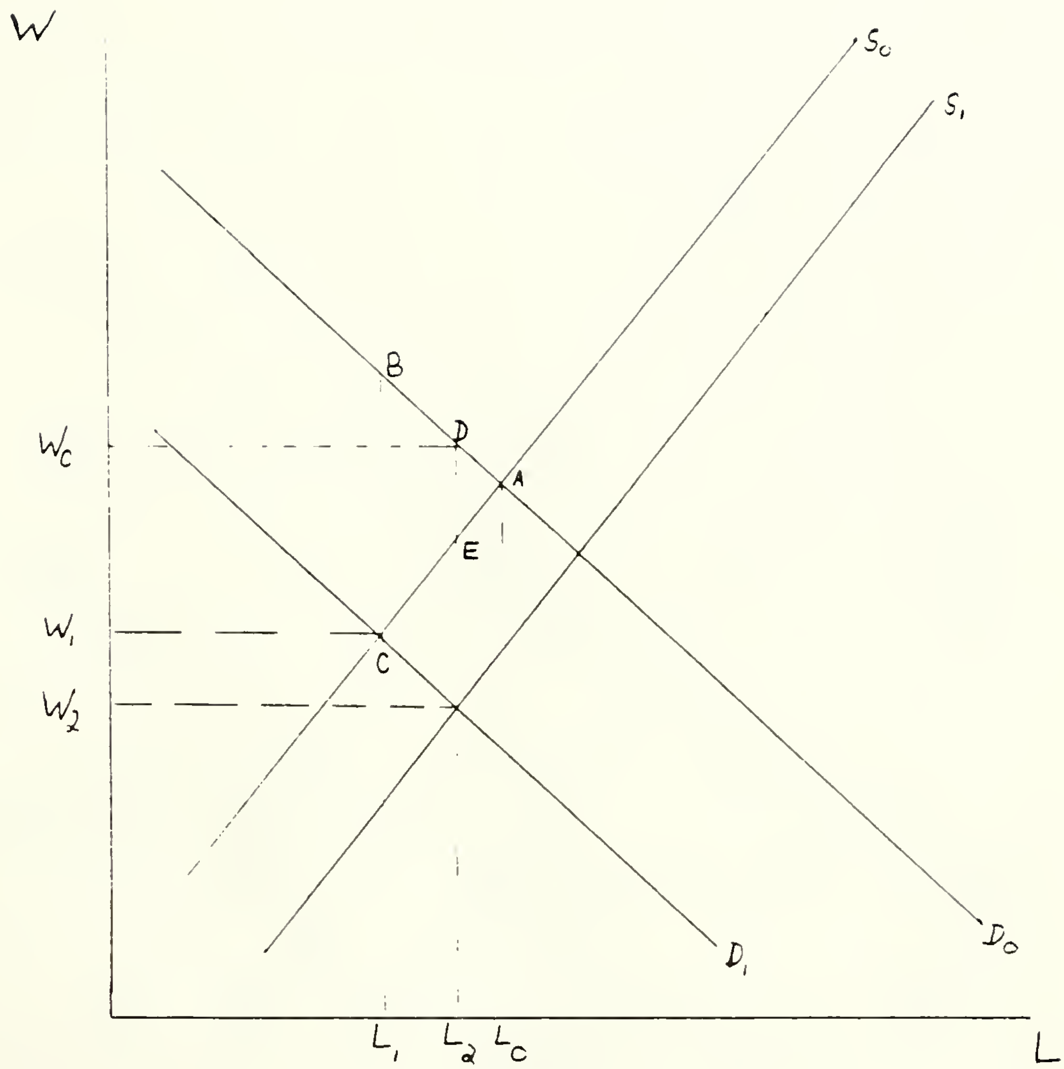

FIGURE 1: THE LABOR MARKET EFFECTS OF A MANDATE

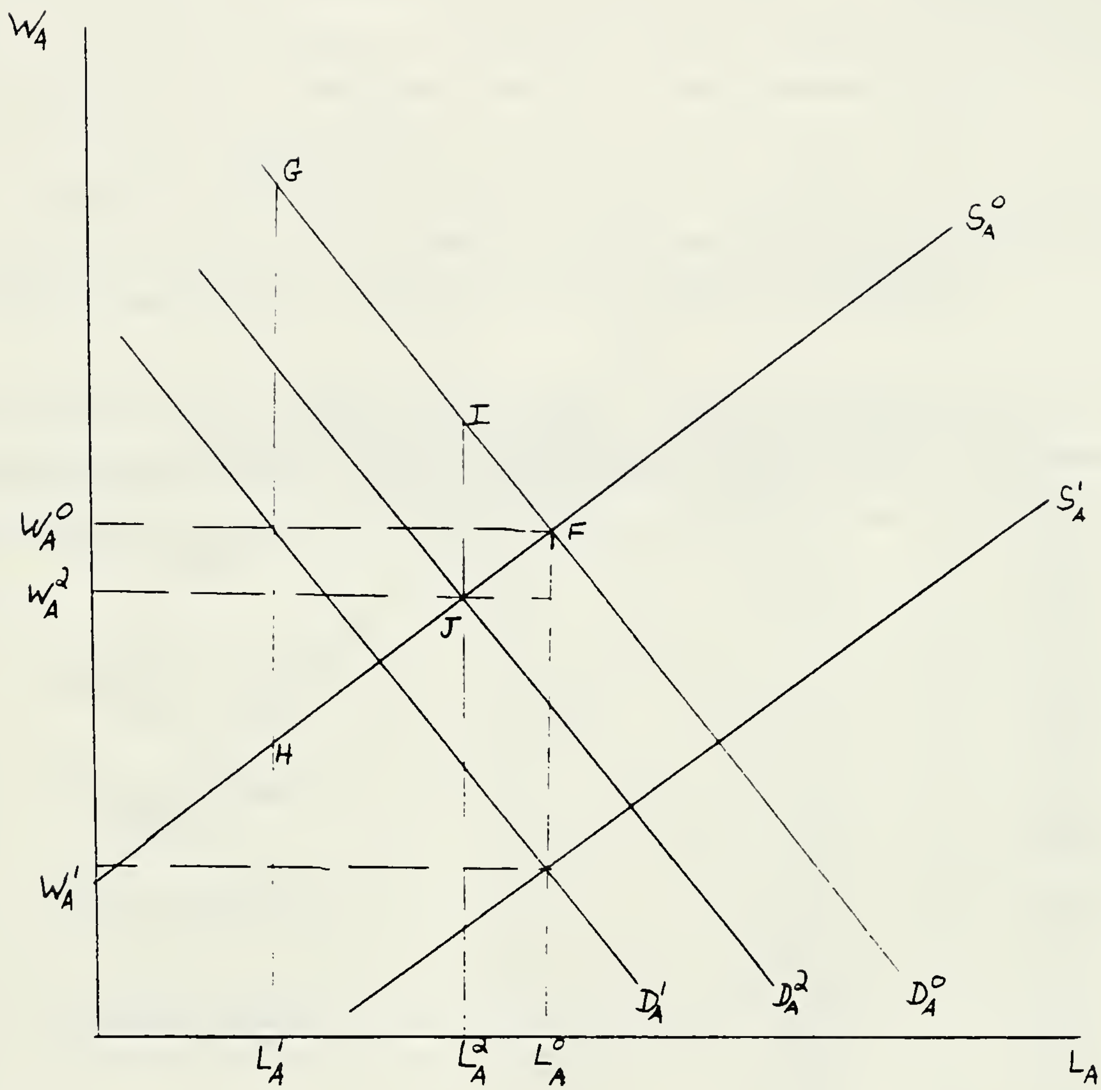

FIGURE 2A: GROUP SPECIFIC MANDATE - GROUP A

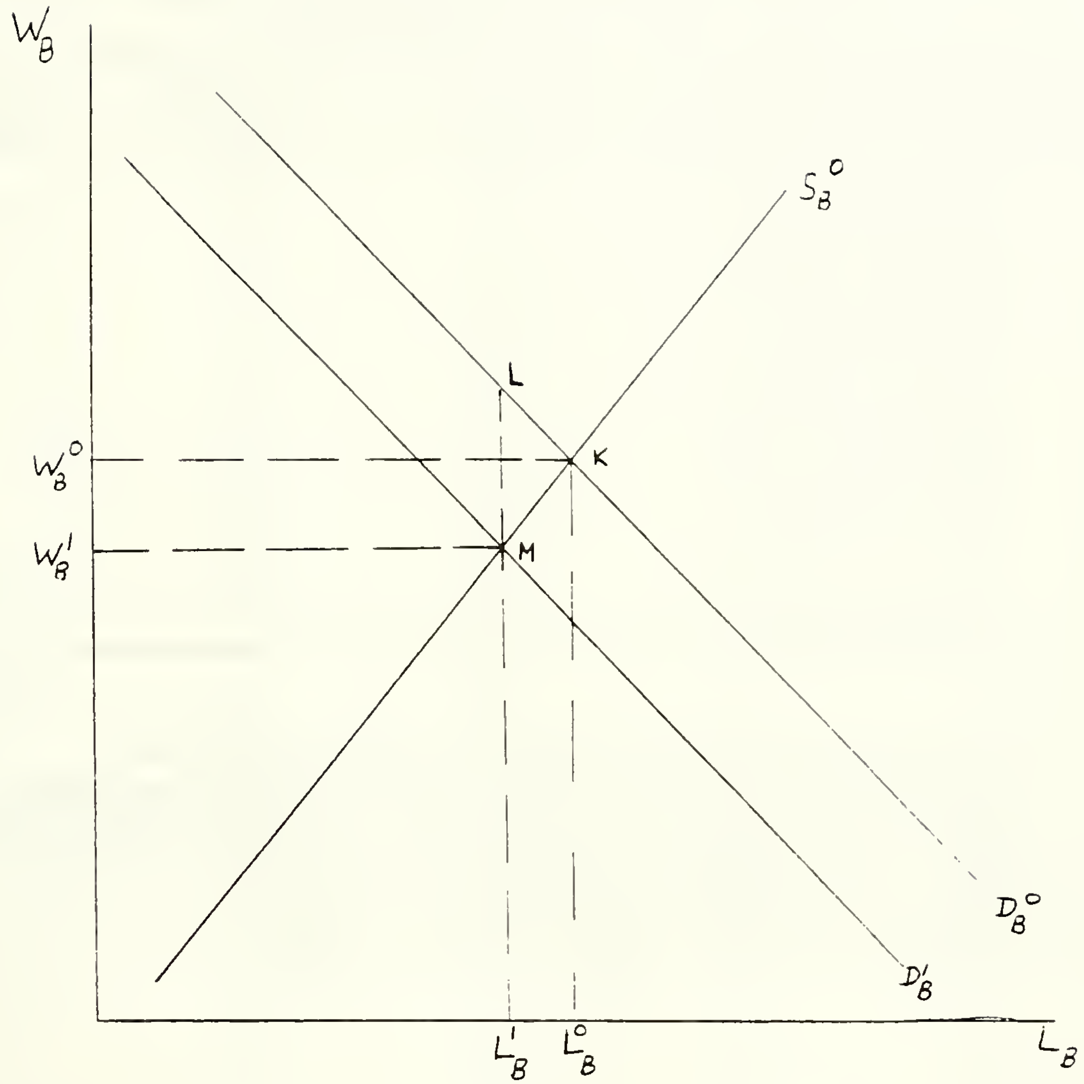

FIGURE 2B: GROUP SPECIFIC MANDATE - GROUP B

Table 1: The Cost of Adding Maternity Benefits to a Health Insurance Package			
Demographic Group	Annual Cost (1990 Dollars)	Annual Cost (1978 Dollars)	Cost as a % of 1978 Wkly Earnings
20-29 Year Old Females - Family Coverage	$984	$360	4.6%
30-39 Year Old Females - Family Coverage	$756	$277	3.5%
20-29 Year Old Females - Individual Coverage	$324	$119	1.5%
30-39 Year Old Females - Individual Coverage	$252	$92	0.9%
20-29 Year Old Males - Family Coverage	$984	$360	2.9%
30-39 Year Old Males - Family Coverage	$756	$277	1.7%

Notes:

1) Source of data is premium calculation program from an anonymous insurance carrier.
2) 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.
3) 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 2/3 and 1/3, respectively.
4) 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.

Table 2: Means for all Wage Earners				
	Non-Experimental States		Experimental States	
	Before Law Δ	After Law Δ	Before Law Δ	After Law Δ
% Female	41.4 [49.3]	43.9 [49.6]	41.4 [49.3]	43.1 [49.5]
Average Age	38.1 [12.6]	37.6 [12.5]	38.9 [12.6]	38.4 [12.6]
% Married	75.0 [43.3]	70.8 [45.5]	71.6 [45.1]	67.9 [46.7]
% Non-White	8.8 [28.3]	9.2 [28.9]	10.2 [30.3]	12.0 [32.5]
% 20-40 Female	23.6 [42.5]	27.1 [44.4]	22.7 [41.9]	25.2 [43.4]
Average Education	12.1 [2.87]	12.3 [2.81]	12.4 [2.94]	13.7 [2.88]
Average Hrly Wage	5.68 [3.31]	5.59 [3.16]	6.61 [3.98]	6.40 [3.62]
% Union	27.0 [44.4]	26.8 [44.3]	33.4 [47.2]	33.8 [47.3]
% Agric, Mining & Construction	6.4 [24.4]	6.1 [23.8]	5.2 [22.1]	4.9 [21.7]
% Manufacturing	36.5 [48.2]	35.3 [47.8]	28.5 [45.1]	26.6 [44.2]
% Transport, Comm, & Util	6.4 [24.4]	6.2 [24.1]	8.2 [27.4]	8.0 [27.2]
% Trade	19.6 [39.7]	20.2 [40.1]	21.4 [41.0]	21.9 [41.4]
% Services	29.7 [47.2]	31.5 [46.5]	35.3 [47.8]	37.3 [48.4]
N	9954	10180	10597	10636

Notes:

1) Numbers in square brackets are standard deviations.
2) Before law change is years 1974-1975; after is 1977-1978.
3) Experimental states are those which passed maternity mandates (Illinois, New Jersey, and New York); non-experimental states are Connecticut, Indiana, Massachusets, North Carolina, and Ohio.
4) Observations with wages below $1/hr and above $100/hr are dropped, as are individuals younger than 20 or older than 65, and the self-employed.
5) Means are unweighted

Table 3: Differences-in-Differences-in-Differences Estimates of the Impact of State Mandates on Hourly Wages

Treatment Individuals: Married Women, 20-40 Years Old			
Location/ Year	Before Law Δ	After Law Δ	Time Difference For Location
Experimental States	1.547 (0.012) [1400]	1.513 (0.012) [1496]	-0.034 (0.017)
Non-Experimental States	1.369 (0.010) [1480]	1.397 (0.010) [1640]	0.028 (0.014)
Location Diff at a Point in Time	0.178 (0.016)	0.116 (0.015)	
		Difference-in-Difference	-0.062 (0.022)

Control Group: Over 40 and Single Males 20-40			
Location/ Year	Before Law Δ	After Law Δ	Time Difference For Location
Experimental States	1.759 (0.007) [5624]	1.748 (0.007) [5407]	-0.011 (0.010)
Non-Experimental States	1.630 (0.007) [4959]	1.627 (0.007) [4928]	-0.003 (0.010)
Location Diff at a Point in Time	0.129 (0.010)	0.121 (0.010)	
		Difference-in-Difference	-0.008 (0.014)
		Diff-in-Diff-in Diff	**-0.054** **(0.026)**

Notes:

1) Cells contain mean log hourly wage for the group identified on the axes. Standard errors in parentheses; sample size in square brackets.
2) Years before/after law change, and experimental/non-experimental states, are defined after Table 2.
3) Difference-in-difference-in-difference is the difference-in-difference from the upper panel minus that in the lower panel.

Table 4: Wage and Labor Input Effects - Treatment Group Dummy				
Specification	(1) Log Hourly Wage	(2) Log Hourly Wage	(3) Log Hours/ Week	(4) Employment (Probit)
Education		0.069 (0.001)	0.007 (0.001)	0.046 (0.002)
Experience		0.020 (0.001)	0.005 (0.001)	0.037 (0.002)
Experience Squared/1000		-0.308 (0.014)	-0.115 (0.014)	-1.078 (0.032)
Female		-0.217 (0.012)	-0.044 (0.011)	-0.123 (0.026)
Married		0.175 (0.009)	0.065 (0.009)	0.650 (0.024)
Married*Female		-0.214 (0.013)	-0.185 (0.013)	-1.161 (0.032)
Non-White		-0.050 (0.008)	0.019 (0.008)	0.011 (0.018)
Union		0.135 (0.006)	0.045 (0.005)	
"After" Dummy	-0.003 (0.010)	0.010 (0.009)	0.015 (0.009)	0.046 (0.022)
Experimental State Dummy	0.129 (0.010)	0.102 (0.008)	-0.016 (0.007)	-0.078 (0.018)
Treatment Dummy	-0.262 (0.015)	0.086 (0.014)	-0.016 (0.014)	-0.285 (0.030)
After*Experimental	-0.007 (0.014)	-0.012 (0.011)	-0.010 (0.010)	-0.029 (0.026)
After*Treatment	0.032 (0.020)	0.007 (0.016)	-0.015 (0.016)	0.157 (0.035)
Experimental* Treatment	0.049 (0.021)	0.040 (0.016)	-0.026 (0.016)	-0.022 (0.033)
After*Experimental *Treatment	**-0.055** **(0.029)**	**-0.043** **(0.023)**	**0.049** **(0.022)**	**-0.047** **(0.048)** **[-0.016]**
N	27033	27033	27033	62575

Notes:

1) Standard errors in parentheses.

2) Regression specification is equation (6) in the text; regressions include two year dummies and 15 industry dummies.

3) Regression in column (1) includes only fixed effects, second level interactions, and third level interaction.

4) "After" dummy is equal to one in 1977-78 and zero otherwise; "Experimental" dummy is equal to one if experimental state and zero otherwise; "Treatment" dummy is equal to one if 20-40 year old married woman and zero if above 40 or a 20-40 year old single male.

5) Employment is a dummy which is equal to one if working and zero otherwise (unemployed or out of labor force). Employment regression is estimated as a probit. Number in brackets is marginal probability - see text.

Table 5: Treatment Dummy Results Across Demographic Groups				
Group	Log Hourly Wage	Log Hours/Week	Employment (Probit)	% Change in Labor Input
Married Women 20-40	-0.043 (0.023)	0.049 (0.022)	-0.047 (0.048) [-0.016]	1.40
Single Women 20-40	-0.042 (0.026)	-0.014 (0.024)	-0.095 (0.064) [-0.030]	-5.95
Married Men 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:

1) Standard errors in parentheses.
2) Coefficient is that on third level interaction in regressions such as those in Table 4.
3) The treatment group is the group indicated for each row.
4) The control group is the same as that for Table 4 (all those over forty and single men under 40).
5) Number in brackets in employment column is marginal probability - see text.
6) Change in total labor input is 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, and divided by average hours per week for the treatment group to get a percentage change.

Table 6: Wage and Labor Input Results - Parametrized Cost of the Mandate

Specification	(1) Log Wage	(2) Log Wage (NLLS)	(3) Log Wage (No hours)	(4) Log Wage (Full-Time)	(5) Log Hrs/ Week	(6) Empl. (Probit)
Education	0.071	0.073	0.070	0.071	0.007	0.065
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Experience	0.024	0.024	0.022	0.022	0.008	0.042
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Experience Squared/1000	-0.370	-0.366	-0.360	-0.349	-0.151	-1.067
	(0.012)	(0.012)	(0.012)	(0.012)	(0.010)	(0.027)
Female	-0.214	-0.225	-0.190	-0.172	-0.086	-0.246
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.020)
Married	0.123	0.110	0.150	0.147	0.031	0.597
	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.024)
Married* Female	-0.157	-0.147	-0.180	-0.169	-0.156	-1.166
	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)	(0.027)
Non-White	-0.068	-0.071	-0.065	-0.077	0.001	-0.116
	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.015)
Union	0.116	0.118	0.122	0.102	0.020	
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	
"After" Dummy	0.004	0.007	0.008	0.013	0.008	0.063
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.021)
Experimental State Dummy	0.090	0.097	0.107	0.099	-0.017	-0.094
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.017)
Parametrized Cost	2.321	3.308	0.028	0.015	0.009	-0.005
	(0.043)	(0.415)	(0.011)	(0.011)	(0.002)	(0.007)
After* Experimental	-0.006	-0.007	-0.016	0.002	-0.007	-0.023
	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.025)
After*Cost	0.139	-0.098	-0.007	-0.018	0.0003	0.032
	(0.515)	(0.607)	(0.013)	(0.013)	(0.002)	(0.008)
Experimental* Cost	3.229	2.521	0.033	0.028	-0.002	0.009
	(0.535)	(0.520)	(0.014)	(0.014)	(0.002)	(0.008)
After*Exper* Cost	**-2.460**	**-2.140**	**-0.028**	**-0.038**	**0.0049**	**-0.027**
	(0.763)	**(0.759)**	**(0.020)**	**(0.020)**	**(0.0031)**	**(0.011)**
						[-0.022]
Shifting	**246%**	**214%**	**109%**	**160%**		
N	41367	41367	41367	35868	41367	84305

Notes:

1) Wage and Hours regressions include 2 year dummies and 15 industry dummies.

2) Column (2) is estimated by non-linear least squares, and the mandate cost is not normalized by demographic group average wages; the specification is described in the text.

3) Mandate Cost in col (3) & (4) is not normalized by hours worked; shifting is calculated at average hours for the treatments.

4) Sample in column (4) is restricted to those who work at least 35 hours per week.

5) Column (6) is a probit. Cost is assigned by demographic group average. Number in brackets is the change in the probability of employment for a $1 increase in costs.

Table 7: Varying the Specification - Wage Results Dependent Variable is Log Hourly Wage					
Specification	(1) Mid-West Only	(2) Atlantic Only	(3) Atlantic Only	(4) Versus Sing Men	(5) Ind*Year *State
"After" Year	-0.002 (0.010)	0.006 (0.011)	0.009 (0.012)	-0.017 (0.013)	
Experimental State	0.092 (0.011)	0.104 (0.009)		0.068 (0.012)	
New York			0.102 (0.010)		
New Jersey			0.112 (0.013)		
Parametrized Cost	2.132 (0.575)	2.577 (0.646)	2.587 (0.520)	-1.977 (1.030)	2.582 (0.435)
After* Experimental	0.018 (0.015)	-0.021 (0.013)		-0.012 (0.017)	
After*New York			-0.024 (0.014)		
After*New Jersey			-0.018 (0.018)		
After*Cost	0.028 (0.661)	0.286 (0.799)	0.289 (0.798)	0.808 (0.707)	0.106 (0.530)
Experimental* Cost	3.198 (0.862)	3.319 (0.737)		4.330 (0.728)	3.102 (0.542)
New York*Cost			4.072 (0.798)		
New Jersey*Cost			1.508 (1.034)		
After* Experimental*Cost	**-2.651** **(1.204)**	**-2.555** **(1.052)**		**-1.977** **(1.030)**	**-2.386** **(0.951)**
After*New York* Cost			**-3.666** **(1.140)**		
After*New Jersey* Cost			**0.173** **(1.493)**		
N	17739	23626	23626	24229	41366

Notes:

1) All regressions include: education, experience and its square, female, married, and non-white dummies, female*married interaction, union status, year dummies for 1974 and 1978, and 15 industry dummies.
2) Regression in column (3) was run by freeing up the experimental, experimental*after, experimental*cost, and experimental*after*cost effects by state (New York and New Jersey).
3) Regression in column (4) restricts sample to those 20-40 years old.
4) Regression in column (5) absorbs 240 industry (15) by year (2 - before/after) by state (8) effects in the estimation.

Table 8: Varying the Specification - Labor Input Results Coefficient is that on After*Experimental*Cost Interaction				
Column	(1)	(2)	(3)	(4)
Specification	Hours (Individual)	Employment	Employment ($1 ΔC)	Δ in Total Labor Input
Basic	0.0049 (0.0031)	-0.027 (0.011)	-0.0022	0.0063
Mid-West Only	0.0066 (0.0047)	-0.027 (0.018)	-0.0015	0.0177
Atlantic Only	0.0033 (0.0043)	-0.030 (0.016)	-0.0029	-0.0042
New York	0.0029 (0.0046)	-0.037 (0.017)	-0.0057	-0.0220
New Jersey	0.0041 (0.0057)	-0.013 (0.021)	-0.0007	0.0119
Vs. Single Males	0.0052 (0.0045)	-0.026 (0.018)	-0.0021	0.0081

Notes:

1) Coefficient is that on third level interaction (Experimental*After*Cost) in regressions such as those in Table 5.
2) Separate estimates for New York and New Jersey were obtained by freeing up the experimental, experimental*after, experimental*cost, and experimental*after*cost effects by state.
3) Column (2) is run as a probit, using demographic group average costs.
4) Column (3) interprets the coefficients from column (2), by calculating the change in probability of employment for a $1 increase in the cost of the mandate (see text).
5) Change in Total Labor Input is change in total hours per week per worker for a 100% 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 to get the percent change in labor input for a 100% rise in the dollar cost.

Table 9: Wage Results - Parametrized Cost of the Mandate - Federal Experiment
Dependent Variable is Log Hourly Wage

Specification	(1) Treat Dummy	(2) Mandate Cost	(3) Not norm by hours	(4) Full-Time Non-norm	(5) Women Norm hrs	(6) Women Non-norm
Education	0.064	0.066	0.065	0.068	0.063	0.063
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Experience	0.025	0.027	0.026	0.028	0.024	0.024
	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)
Experience Squared/1000	-0.362	-0.397	-0.389	-0.404	-0.364	-0.358
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Female	-0.272	-0.232	-0.209	-0.234	-0.248	-0.236
	(0.007)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Married	0.198	0.164	0.189	0.152	0.223	0.232
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Married*Female	-0.255	-0.210	-0.231	-0.198	-0.262	-0.268
	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Non-White	-0.062	-0.069	-0.066	-0.078	-0.068	-0.066
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
"After" Dummy	-0.080	-0.079	-0.082	-0.075	-0.078	-0.081
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Experimental State Dummy	-0.090	-0.089	-0.088	-0.094	-0.086	-0.088
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treat Dummy/ Param Cost	0.129	2.650	0.018	0.055	4.533	0.083
	(0.008)	(0.308)	(0.008)	(0.008)	(0.388)	(0.010)
After* Experimental	0.023	0.021	0.023	0.018	0.021	0.023
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
After*Cost	0.030	0.483	0.024	0.014	0.884	0.040
	(0.009)	(0.351)	(0.009)	(0.009)	(0.438)	(0.012)
Experimental* Cost	0.002	0.149	0.005	0.010	-0.469	-0.002
	(0.009)	(0.343)	(0.009)	(0.008)	(0.429)	(0.011)
After*Cost *Experimental	**-0.021**	**-0.628**	**-0.023**	**-0.018**	-0.647	-0.029
	(0.012)	**(0.455)**	**(0.012)**	**(0.011)**	(0.570)	(0.015)
Extent of Shifting		63%	90%	77%	65%	103%
N	131512	195463	195463	166417	154823	154823

Notes:

1) In column one, a dummy for 20-40 year old females is used as treatment effect; in columns 2-7, the parametrized cost is used.

2) After is a dummy which equals one if the year is 1980 or 1981; it equals zero if 1977 or 1978.

3) Experimental is a dummy which equals zero if the state had passed a maternity mandate by January 1, 1977; it equals one if the state did not pass a maternity mandate during 1977 or 1978.

45 Column (4) uses only those who worked at least 35 hours per week.

4) Columns (5) and (6) exclude married 20-40 year old males from the treatment group.

6) In columns (3), (4) and (6) the mandate cost is not normalized by hours worked; the extent of shifting in columns (3), (5), and (6) is calculated at average hours for the treatment group included in the regression.

Table 10: Labor Input Results - Federal Experiment
Coefficient is that on After*Experimental*Cost Interaction

Column	(1)	(2)	(3)	(4)	(5)	(6)
Specification	Hours (Dummy)	Hours (Param)	Empl (Dummy) (Probit)	Empl (Param) (Probit)	Empl (Param) ($1 ΔC)	Δ in Tot Labor Input
Basic		-0.0002 (0.0015)		0.0007 (0.0068)	0.00005	-0.0005
Women Treatments Only		0.0009 (0.0029)		0.0034 (0.0094)	0.00029	0.0013
Married Women Treatments	0.0012 (0.0098)		-0.018 (0.028)		-0.0055	-0.0197

Notes:

1) Coefficient in columns (1) and (3) is that on third level interaction (Experimental*After*Treatment Dummy) in regressions such as those in Table 4, where the treatment group is married women aged 20-40, with a dummy variable equal to one if individual is in treatment group.
2) Coefficient in columns (2) and (4) is that on third level interaction (Experimental*After*Cost) in regressions such as those in Table 5, where the treatment group is all women 20-40 and married men 20-40, where individuals are assigned individually parametrized costs in column (2), and demographic group average costs in column (4).
3) The second row restricts the treatment group to be women aged 20-40.
4) Column (5) interprets the coefficients from column (4), by calculating the change in probability of employment for a $1 increase in the cost of the mandate.
5) Change in Total Labor Input is change in total hours per week per worker for a 100% 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 to get the % change in labor input for a 100% rise in the dollar cost.

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