

Lahey, Joanna N.

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## The efficiency of a group-specific mandated benefit revisited: The effect of infertility mandates

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# The Efficiency of a Group-Specific Mandated Benefit Revisited: The Effect of Infertility Mandates

Joanna N. Lahey  
*Texas A&M University*

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## **The Efficiency of a Group-Specific Mandated Benefit Revisited: The Effect of Infertility Mandates**

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Joanna N. Lahey  
*Texas A&M University*  
e-mail: [jlahey@nber.org](mailto:jlahey@nber.org)

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### **ABSTRACT**

This paper examines the labor market effects of state health insurance mandates that increase the cost of employing a demographically identifiable group. State mandates requiring that health insurance plans cover infertility treatment raise the relative cost of insuring older women of child-bearing age. Empirically, wages in this group are unaffected, but their total labor input decreases. Workers do not value infertility mandates at cost, and so will not take wage cuts in exchange, leading employers to decrease their demand for this affected and identifiable group. Differences in the empirical effects of mandates found in the literature are explained by a model including variations in the elasticity of demand, moral hazard, ability to identify a group, and adverse selection.

**JEL Classification Codes:** I18, J23, J13

**Key Words:** labor supply, infertility, health insurance, health insurance mandates

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## **I Introduction**

Health insurance mandates require that insurance companies provide coverage for specific services or provider types when they offer insurance to companies. Over the past 30 years, the number of these mandates has greatly increased, from very few mandates in 1965 to almost 2000 mandates in 2008 (Gruber, 1994; Bunce & Wieske, 2008). At the same time, health care costs are rising at a rate much greater than inflation; for example, the Kaiser Family Foundation finds that insurance costs increased 131 percent between 1999 and 2009. The proliferation of health insurance mandates is commonly suggested as a partial cause for this increase (Litow, 2002). In addition to the direct costs to health care, inefficient mandates can have indirect costs through decreasing employment for groups that are unwilling or unable to decrease their wages by the amount of their increased insurance costs.

Academic literature reports mixed effects of these mandates on premium cost and labor market outcomes for the affected workers (e.g. Monheit & Rizzo, 2007; Kowalski, Congdon, & Showalter., 2008; La Pierre et al., 2009; Kapur et al., 2008; Baicker & Chandra, 2006; Kaestner & Simon, 2002). This paper argues that these mixed empirical results are predicted by theory, and provides an empirical example using the incidence of infertility mandates. The bottom line is that when workers value mandates at cost, they will be willing to take a pay cut to offset the cost of the mandates. When they do not fully value these mandates, and employers can discriminate between high cost and low cost groups, then employment of the high cost group will be negatively affected.

Mandates should primarily affect labor market outcomes through the experience-rated effect on premium costs, as these costs make an individual worker more expensive to the firm. In his seminal paper on maternity mandates, Gruber (1994) shows that when firms that offer insurance are required to offer maternity insurance, women's wages shift to compensate for the

value of the additional benefit and their probability of employment does not change. In this situation, women in age groups likely to be affected by the mandate value the additional coverage at least at cost, effectively mitigating the increase in premium costs for these workers to the firm.

However, subsequent papers looking at more general classes of these mandates have found mixed results of the effects of health insurance mandates on both premiums and labor market outcomes. Monheit and Rizzo (2007) provide an extensive literature review of the impact of health insurance mandates on premiums. A large subset of these studies group health insurance mandates together and compare the outcomes of states with more or fewer mandates over time. In general, the effects of grouped mandates on premiums are mixed, with studies finding a decrease in premiums for HMOs, no effect, or finding an increase of 5 percent, depending on assumptions made and populations studied. In later work, Kowalski, Congdon and Showalter (2008) find that the number of mandates in a state increases premium costs in non-group markets by 0.4 to 0.9 percent. However, La Pierre, Conover, Henderson, Seward, and Taylor (2009) find that the overall number of mandates does not have a statistically significant impact on premiums. They also find mixed results of the effect on premiums by mandate type and health plan. Focusing on “expensive” mandates and adjusting for the probability that the mandated item would be included on insurance plans in the absence of the mandate, the CBO (2000) suggests that these mandates increase premiums up to 1.15 percent.

In terms of labor market outcomes, Kaestner and Simon (2002) find no overall effect, looking at all state mandates or at only expensive mandates. In contrast to the decrease in wages found by Gruber (1994) in the case of maternity mandates, they find no wage tradeoff. Cseh (2008) also finds no effect of mental health parity mandates on probability of receiving health insurance, employer contributions to insurance, labor market composition, or lower wages.

More generally, increased worker health costs have been shown to shift both labor supply and wages. Baicker and Chandra (2006) use increases in medical malpractice costs as a source of variation in costs and estimate that a 10 percent increase in premiums decreases hours worked by 2.4 percent and increases probability of being part-time by 1.2 percentage points. Kapur, Escarce, Marquis, and Simon (2008) find that high expected cost workers are less likely to be employed or hired by small firms. In terms of wages, Baicker and Chandra (2006) estimate that a 10 percent increase in premiums decreases wages by 2.3 percent while Daneshvary and Clauretie (2007) estimate that workers accept 16.5 percent to 20 percent lower earnings in return for insurance. Similarly, Adams (2007) finds that wages for more expensive-to-insure older workers increase when community rating, which lowers the relative costs for these workers, is enacted in New York. Bhattacharya and Bundorf (2009) find that obese workers with health insurance accept lower wages while those without health insurance do not. Currie and Madrian (1999) summarize some earlier findings on shifting health insurance costs to wages.

As Kaestner and Simon (2002) note, a “consensus has not been reached” regarding the efficiency of state health insurance mandates. However, a priori, there is no reason to assume that all mandates have the same effects on employment outcomes. The case of maternity mandates is a special case in which a group-specific mandate is valued at least at cost to the worker. Like the case of workers compensation (Gruber & Krueger, 1991; Fishback & Kantor, 1995), this mandate is efficient because it fixes a market failure caused by adverse selection, but does not create large moral hazard incentives. All parties are better off when firms are forced to provide this benefit. Mandates with different attributes should, theoretically, have different or no effects on employment and premium costs.

Infertility mandates provide a natural experiment similar to those of maternity mandates. They are theoretically similar to maternity mandates in every way except that their more elastic

demand creates a moral hazard problem that keeps workers from valuing the increased coverage at full cost.<sup>1</sup> Therefore, theory predicts that wage shifts will not fully offset the increased costs of workers. To test this general theory, this paper uses the March monthly Current Population Survey (CPS) in a difference-in-difference-in-differences (DDD) strategy to compare those in groups most likely to be affected by the mandate in states with infertility mandates before and after the mandate, to those unlikely to be affected by infertility mandates.

This paper finds that on average, conditional on employment, wages do not adjust significantly and the magnitudes of adjustment are small for affected groups upon the receipt of infertility mandates, although the significance of these results vary subject to specification. However, in accordance with theory, employment for these workers decreases by around 1.07 weeks per year, suggesting that women in affected age groups are unwilling to fully offset the increase in benefits through their wages. They are also about two percentage points more likely to be not in the labor force, though these results are only marginally significant in some regressions. Wages are largely unchanged. These results are robust to the inclusion of age\*state\*year fixed effects, sensitivity testing on the treatment group and on the universe studied. The decrease in labor supply does not appear to be the effect of maternity leave after having had children, as childless women appear to show even stronger effects on not working than do all women. These results represent the effect of these laws on the labor market for all women in these age groups and suggest that infertility mandates are inefficient.

The paper proceeds as follows. Section II presents some background on health insurance benefits for infertility from the 1980s to today and discusses the economics of a group-specific

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<sup>1</sup> See section II.B. for information on demand elasticity. Fewer women in the affected group will also utilize infertility benefits compared to maternity benefits, but the theoretical implications of that difference may be mitigated by differences in cost of services and beliefs about infertility risk or utility from the use of services.

mandated benefit. Section III outlines a general theory of how infertility and other group-specific mandates may affect the labor supply for these workers based on their different characteristics. After describing the data and my estimation strategy in Section IV, I estimate the impact of the state mandates on the labor-market outcomes of older women of childbearing age (and their husbands) in Section V. Section VI provides robustness checks and alternate explanations for these findings. Section VII concludes by discussing the welfare implications.

## **II. Background**

### **A. Facts**

Infertility is defined as the inability to conceive after a year of unprotected intercourse. According to the CDC, 7.4 percent of married women, or 2.1 million women, were infertile in the 2002 National Survey of Family Growth (Chandra et al., 2005). Modern medicine provides many diagnoses of and treatments for infertility. The most expensive Artificial Reproductive Technology (ART) treatments are In Vitro Fertilization (IVF), and related procedures Gamete Intrafallopian Transfer (GIFT) and Zygote Intrafallopian Transfer (ZIFT). Infertility treatment is a multi-billion dollar industry with annual estimates of \$1 to \$4 billion depending on the sources (e.g. Winter, 1998; Marketdata, 2009; Saul, 2009).

Infertility treatment is generally not covered by insurance unless firms are required to cover it by a state mandate. Infertility state mandates require that firms that provide health insurance must provide insurance coverage for specific infertility services or providers. Under ERISA, enacted in 1974, state mandates only apply to firms that purchase insurance from an outside provider and do not apply to firms that self-insure. However, larger firms that self-insure generally provide the same mandated coverage that firms with group coverage are required to provide, and the trend of self-insured firms offering mandated coverage has been increasing since the 1980s (General Accounting Office [GAO], 1996; Acs et al., 1996; Jensen & Morrissey,

1999). It is plausible that as adverse selection goes down, large firms are less likely to exclude these benefits.<sup>2</sup> The Alan Guttmacher Institute (1993) finds that IVF is “routinely covered” by 14 percent of large group plans, 16 percent of PPOs and 17 percent of POS and HMOs. According to King and Meyer (1997), half of all workers in Illinois were affected by the Illinois mandate in 1993.

Currently twelve states have mandates requiring that infertility treatment be covered if coverage is offered. Two more states require that a plan that includes infertility treatment be offered to firms offering insurance, but they can price those plans at any price, thus rendering the “mandate to offer” ineffective. The first law mandating infertility treatment was part of HMO regulation in 1977 (prior to the first successful In Vitro Fertilization) and as such only applies to HMO plans. The bulk of infertility mandates were passed in the late 1980s and 1990s, although Connecticut changed its mandate from a mandate to “offer” in its 1989 law to a mandate to “cover” in its 2005 law. Some laws, particularly the ones that only pertain to HMOs, do not mention specific types of coverage and thus have been subject to case law about what kind of coverage is mandated. I code states as having laws if they have “mandate to cover” laws. Results are very similar if “to offer” laws are coded as “to cover” laws.

Other laws specifically mention that IVF, GIFT, and/or ZIFT be covered or be excluded from coverage. Laws that require coverage for these more extensive ARTs should theoretically be more expensive to insurers and thus to employers and should have more of a potential impact on labor market outcomes. For these potentially stronger laws, I code laws that require IVF as

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<sup>2</sup> While larger (thus more likely to self-insure) firms do in general provide more extensive across-the-board benefits than smaller firms, and firms do not generally self-insure as the result of increased state mandates, there may be variation in provision of specific mandated benefits. The issue of spillover effects of mandates to (generally larger) self-insured firms is an important one that deserves further study.

having an IVF mandate. There is some ambiguity as to whether the HMO-specific laws require IVF. These laws are coded as not having coverage for IVF based on commentary by the special interest groups Resolve and the International Council on Infertility Information Dissemination (INCIID). Coding Ohio as mandating IVF prior to 1997, as in Bitler and Schmidt (2010) and Schmidt (2007), the year the Superintendent of Insurance stated that IVF was not mandated, produces very similar results.<sup>3</sup>

Infertility treatment mandates are correlated with increased treatment use after the mandate in states with those mandates (Bartels, 1993; Collins et al., 1995; Neumann, 1997). Prospective patients have more access to centers in states with mandates (Nangia, Likosky, & Wang, 2010) and clinics in mandate states are larger than in non-mandate states (Hamilton & McManus, 2007). The mandates have also been shown to increase fertility, especially among those likely to have been affected (Bundorf, Henne, & Baker, 2007; Bitler, 2008; Jain, Harlow, & Hornstein, 2002; Schmidt, 2007). More information on possible first stage effects is discussed in section IV.B.

## B. Cost

Infertility treatment, or Artificial Reproductive Technology (ART), is expensive, ranging from a few hundred dollars for one use of ovarian stimulatory drugs such as Clomid to around

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<sup>3</sup> Coding Ohio as mandating IVF when it dropped a \$2000 monetary cap on spending in 2000 also slightly attenuates the magnitude of the results but does not change the results overall. Although New York's law was put into place in 1990, it was clarified in 2002 (some argue it was strengthened, others argue it was weakened); coding New York as having the law in 2002 rather than 1990 attenuates results, although they are still qualitatively similar and significant. New York also offers a small fund for IVF although it does not mandate IVF, an act which should not change how employers view the cost of potential employees unless their main concern is maternity leave; coding New York as offering IVF increases the magnitude of the IVF results, but they are still qualitatively similar to the main results for IVF. The websites <http://www.ins.state.ny.us/>, <http://www.asrm.org/>, and <http://www.inciid.org/> provide more discussion on these legal details. There may also be spillover effects from those who work in a state different than their state of residence; these concerns should attenuate the magnitude of the results found.

\$10,000 for a single use of In Vitro Fertilization (IVF) (Bitler & Schmidt, 2010; Collins, 2001; Neumann, 1997; Neumann, Gharib, & Weinstein, 1994). Neumann et al. (1994) cite that the average study estimates between 10 percent and 15 percent of initiated cycles result in at least one live birth, with age, male and female complications, number of cycles undergone, etc., all influencing the effectiveness. More recently, Malizia et al. (2009) estimate a cumulative live birth rate after six IVF cycles of 51 to 72 percent, although the rate decreased to 23 to 42 percent for women over the age of 40. Estimates of the cost of infertility treatment per birth range from \$38,000 to \$800,000 depending on assumptions about which couples select into using IVF and whether or not the increased incidence and cost of multiple births are taken into account in the calculation (Bitler & Schmidt, 2010; Collins, 2001; Neumann, 1997; Neumann, Gharib, & Weinstein, 1994; Reynolds et al., 2003). Because of its expense, some countries outside of the US have targeted ART; Ontario, Canada specifically removed IVF from its insurance system and New Zealand has much higher cost sharing charges for ART than for other health care services (Devlin & Parkin, 2003; Giacomini, Hurley, & Stoodart, 2000).<sup>4</sup>

An additional cost of infertility mandates is that of increased usage from moral hazard. The elasticity of demand for infertility treatment is much higher than that for maternity treatment.<sup>5</sup> Henne and Bundorf (2008) show that poor-prognosis patients move to IVF when

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<sup>4</sup> Garceau et al. (2002) provides a meta review of costs of ART. In one case study, Rabin et al. (1996), a model of a Washington hospital HMO is unable to offset increased break-even capitation rates from an increase in treatment utilization.

<sup>5</sup> Estimates of the elasticity of demand for maternity services are difficult to find. The majority of pregnant women in the United States do give birth in the hospital (MacDorman, Menacker, & Declercq, 2010). Not conditioning on pregnancy, mixed results are found on the effect of Medicaid expansions during the 1980s and 1990s on childbirth itself. Earlier studies find mixed results—Joyce, Kaestner and Kwan (1998) suggest a 5 percent increase overall in the fertility of white women but not black women, Bitler and Zavodny (2010) find that a 10 percent increase in the eligibility cutoff increases fertility by 1.4 percent for non-blacks and 1 percent for blacks, but a more recent study by DeLeire, Lopoo, and Simon (2011) finds no effect of the Medicaid expansion on fertility.

they are in states with generous mandates and cross country comparisons of infertility suggest a utilization rate that is 277 percent higher than the rate in the absence of coverage (Jain, 2006). Collins (2001) estimates a median price elasticity of demand such that a 10 percent reduction in price is associated with a 30 percent increase in cost, and in earlier work, Collins et al. (1995) estimates the elasticity of demand to be between -2.2 and -4.3. These numbers are large compared to the average elasticity of demand for medical services of -0.1 to -0.2 calculated found in the Rand Health Insurance Experiment (Manning et al., 1987). Actuarial studies on these mandates fail to take into account the elasticity of demand for infertility services, greatly underestimating the potential cost of the mandates.

Multiple births and other complications are an additional cost to fertility coverage (GAO, 1996). Theoretically, fertility coverage may increase the number of multiple births on the extensive margin while decreasing them on the intensive margin. That is, the number of women having multiple births can increase or decrease depending on changes in take-up, but conditional on using IVF, multiple births should decrease as doctors implant fewer embryos per cycle. Empirically, on the extensive margin, Bitler (2008) finds that mandates increase twinning for older mothers 10 to 23 percent and increase negative infant health outcomes. Bundorf, Henne and Baker (2007) also find an increase in the probability of multiple births in states with mandates, as does Buckles (2009). However, on the intensive margin, a number of papers using different empirical strategies in different universes find a decrease in embryos transferred per IVF cycle and a decrease in multiple births per IVF cycle in states and countries that required coverage (Martin et al., 2010; Henne & Bundorf, 2008; Hamilton & McManus, 2007; Katz, Nachtigall, & Showstack, 2002; Reynolds, Schieve, Jeng, & Peterson, 2003; Jain, Harlow, & Hornstein, 2002). The strength of the intensive vs. extensive margin may vary depending on the structure of the benefits package offered.

Infertility treatment further increases costs by allowing at-risk women to give birth. For example, the cost per delivery is 3.6 times higher for women over the age of 38 (Devlin & Parkin, 2003). IVF conceived babies may also have higher neonatal intensive care usage, particularly if there is an increased chance of multiples (Devlin & Parkin, 2003; Goldfarb et al., 1996). Chambers et al. (2009) note that the increased cost of perinatal care from multiples is larger than the direct cost from the ART itself. Aggregate cost estimates of the mandates themselves on medical expenditures and premiums are discussed with the first stage in section IV.B.

### C. Political Economy

States do not appear to pass infertility mandates in a manner correlated with prior fertility trends, nor does it appear that more wealthy states are likely to pass mandates first. In fact, infertility mandate passage seems to differ from the general pattern of state innovation as described by Walker (1969). These laws appear to have formed from two separate processes, HMO reform following West Virginia's 1977 law, and mandates specifically directed at provision of fertility services. Three states passed fertility laws within laws for HMO regulation. Specifically, these laws require coverage of "basic health care services ... including ... infertility services" within HMO plans (W. Va. Code §33-25A-2).

For the laws directly targeting infertility treatment, evidence on the political economy of the laws points to the infertility special interest group, Resolve, as instrumental in getting legislation passed in several states. In their case study of Illinois, King and Meyer (1997) describe Resolve's use of political tactics such as delaying opposed members from entering chambers in time to vote in order to get legislation passed. Neumann (1997) discusses the framework of debate about mandating IVF coverage. He suggests that supporters argue that infertility is a disease and it is unfair for only the wealthy to be able to treat it, while opponents

argue that it is expensive, encourages irrational behavior (gambling), substitutes for adoption, or is immoral for religious reasons. Retsinas (1991) argues that infertile couples under the aegis of Resolve are pro-mandate and insurance companies are con-mandate. She describes the battle over state legislatures as one of “family values” and highly available stories against insurance lobbying. Fertility clinics are also pro-, but as of 1991 were not organized into any lobbying efforts.

Estimates of who uses infertility treatment vary based on insurance coverage and the definition of infertility treatment. Staniec and Webb (2007) find that 76 percent of infertile couples seeking treatment had insurance that covered “help getting pregnant.” However, this help may not include the more expensive (or effective) types of infertility treatment. Stephen and Chandra (2000) do not control for mandates and find that 42 percent of the 6.7 million women with fertility problems in 1995 used some form of infertility services, though these services include advice, diagnostics, and miscarriage prevention; only 35 percent used drugs to induce ovulation. Those who were most likely to use these services were older, had ever been married, were college graduates, had a high income, and were non-Hispanic whites, although differences in age, race, and ethnicity disappear when marital status, income, and private health insurance coverage are controlled for. In general, the literature has found increases in overall use once mandates have been implemented, and this justification of the first stage will be discussed in more detail below.

### **III. Theory**

As noted in the introduction, empirical studies of mandates on premium costs and employment outcomes have provided mixed results (Monheit & Rizzo, 2007 provide an excellent review of the literature and note that, “Studies of the impact of mandates on premiums provide a mixed and incomplete picture”). However, these mixed results are exactly what is

predicted by economic theory, as identified by Summers (1989). Different mandates will have different effects depending on the characteristics of those mandates. This section will briefly discuss the general theory of insurance mandates, then will show that moral hazard can lead to a group not valuing a mandate at cost, and finally will demonstrate that labor market outcomes for a group-specific mandate will be determined by whether or not the specific group values that mandate at cost.

#### A. Economics of Mandated Benefits

The theory for how different mandates should work is illustrated in the flowchart in Figure 1. The ideal mandate solves a market failure problem generally caused by adverse selection and makes all parties better off.<sup>6</sup> Assuming that a mandate (ideal or otherwise) solves an adverse selection problem, the mandate will still have different possible effects based on other characteristics. If a mandate has very little cost to the insurance company (for example, the use of midwives or cleft palate repair), then the mandate should have little to no effect on premiums or on employment outcomes. When a mandate does have cost to the insurance company, then it is important to know whether or not the workers are able to adjust their wages to accommodate the increase in compensation or are unable to, as with the case of minimum wage or scale work. If workers are unable to adjust wages, then there are two possible outcomes. If the mandate affects an identifiable group (assuming substitutes for that group exist), then the mandate will theoretically lead to decreased employment for that group because employers will substitute

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<sup>6</sup> There may also be public goods or paternalistic reasons to have a mandate, as in the case of vaccines or infant industries. However, in these cases a mandate may not provide the most efficient solution to the market failure problem; general revenue taxes may produce smaller deadweight loss. This section only considers mandates that counter an adverse selection problem. Note that if the mandate has positive spillover effects to other health costs, it is likely that the insurance company will already cover the treatment or provider.

towards less expensive workers. If there is not an identifiable group, then the company will not be able to avoid the cost of the mandate and profits will drop.

Even the assumption that wages can adjust to accommodate increased mandate costs does not directly mean that wages will adjust. Again, the theory depends on different aspects of the mandate and workers. If the firm cannot differentiate between groups likely to be affected by the mandate, then outcomes will vary by whether or not the workers value the mandate at cost. If workers value the mandate at cost, then wages will adjust (though perhaps not fully depending on individual valuation), and profits will decrease to the extent that mandates are not fully valued by individuals. If workers do not value the mandate, then profits will fall.

A final situation is one in which the mandate creates a cost for insurance companies, employees are not at the minimum wage, and there is an identifiable group, that is, it is a group-specific mandate. If the members of the group value the mandate at cost, then their wages will adjust and employment will not be affected. The canonical example of this type of mandate is a maternity mandate (Gruber, 1994). However, if members of the group do not fully value the mandate at cost, then wage decreases will not fully offset the increased cost to the employer, and the employer will substitute towards less expensive workers. That outcome is the example provided in this paper, the effect of infertility mandates.

B. Moral hazard can lead to a group not valuing the mandate at cost.

Assume that the utility of success from a fertility procedure is normalized to 1 and the utility from failure is 0. The simplest model is a one period model where only one treatment can be attempted in that period. More complicated models allow multiple periods with an attempt in each period, and allow for Bayesian updating of the probability of infertility and the probability of success with each try of the method. In this model, the probability of success is given.

For either procedure:

$$E[\text{value of procedure}] = P_{\text{infertility}} * (P_{\text{success}} * U(\text{success}) + P_{\text{failure}} * U(\text{failure}) - \text{Cost})$$

$$\text{Then } E[\text{value of procedure}] = P_{\text{infertility}} * (P_{\text{success}} - \text{Cost})$$

Let there be two types of procedures, the High Cost Treatment (HCT), and the Low Cost Treatment (LCT).  $\text{Cost}_{\text{HCT}} > \text{Cost}_{\text{LCT}}$ .

In the absence of insurance (NI), an infertile woman will choose the low cost treatment if:

$$P_{\text{LCT, success}} - \text{Cost}_{\text{LCT, NI}} > P_{\text{HCT, success}} - \text{Cost}_{\text{HCT, NI}}$$

This simple model assumes that the cost to the user is the same for either treatment once insurance has been offered. This assumption would be directly comparable to a set co-pay per office visit and procedure (such as that for many insurance plans in Massachusetts), but is not unreasonable given the high cost of any fertility treatment compared to deductibles on plans with co-insurance. A more complicated model would allow a smaller difference between the costs of two treatments with insurance than without.

$$\text{Thus: } \text{Cost}_{\text{LCT, Ins}} = \text{Cost}_{\text{HCT, Ins}}$$

In this case, a woman will choose whichever treatment provides  $\max(P_{\text{LCT, success}}, P_{\text{HCT, success}})$ .

Case 1: If  $P_{\text{LCT, success}} > P_{\text{HCT, success}}$ , the woman will choose the low cost treatment in both cases and will never use the high cost treatment. The insurance will set the price to that of the low cost treatment and there will be no moral hazard.

Case 2: Assume then that  $P_{\text{HCT, success}} > P_{\text{LCT, success}}$ . If  $P_{\text{HCT, success}} - \text{Cost}_{\text{HCT, NI}} > P_{\text{LCT, success}} - \text{Cost}_{\text{LCT, NI}}$ , then the woman will choose the high cost treatment in both cases and never use the low cost treatment. The insurance company will set the price to that of the high cost treatment and there will be no moral hazard.

Case 3: Assume  $P_{\text{HCT, success}} > P_{\text{LCT, success}}$ , and  $P_{\text{LCT, success}} - \text{Cost}_{\text{LCT, NI}} > P_{\text{HCT, success}} - \text{Cost}_{\text{HCT, NI}}$ , then the woman would have chosen the low cost treatment in the absence of health insurance.

The woman then will choose the higher cost treatment when insured and the lower cost treatment when not insured. She has the following expected values:

$$E[\text{value} \mid \text{no insurance}] = P_{\text{inf}}^* (P_{\text{LCT, success}} - \text{Cost}_{\text{LCT, NI}})$$

$$E[\text{value} \mid \text{insurance}] = P_{\text{inf}}^* (P_{\text{HCT, success}} - \text{Cost}_{\text{HC, Ins}})$$

In case 3, the insurance company has no choice but to set the cost of insurance at the expected value of the higher cost procedure. Therefore:

$$\text{Price of infertility insurance} = P_{\text{inf}}^* (P_{\text{HCT, success}} - \text{Cost}_{\text{HCT, NI}}).$$

The woman, however, only values this treatment at the price =  $P_{\text{inf}}^* (P_{\text{LCT, success}} - \text{Cost}_{\text{LCT, NI}})$

Thus, moral hazard causes women not to value the infertility mandate at cost.

When a mandate is not valued at cost, employees will be unwilling to take a pay-cut equivalent to the cost of the mandate. In the next part, the effect on employment is shown for the simplest example, assuming that labor supply is positively sloped and own elasticity of demand is greater than cross-elasticity of demand.<sup>7</sup>

### C. The effect of employee value of a group-specific mandate on labor market outcomes

Figure 2 illustrates the potential effects of a group-specific mandate on the group directly affected, group A, and on employment by other workers in the firm, group B. Panel I demonstrates the effect on employment of both groups when group A values the mandate at cost. In this example, the demand curve for A shifts down as these workers become more expensive and the supply shifts out as these workers are attracted by the benefit. These effects balance each other out such that the number of group A workers is unchanged, but the wage of workers decreases by the amount of the benefit. Because employment of workers in group A is unchanged, there will be no effect on the employment outcomes of workers in group B.

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<sup>7</sup> More complicated models including welfare implications and comparisons to optimal taxation are available in Gruber (1992) and Hamermesh (1986).

Panel II illustrates the effect on employment of groups A and B when group A only values the mandate at a fraction of the cost,  $\alpha C$ . In this example, the demand curve for A shifts down as before. However, the supply curve does not shift out the same amount as it did when group A valued the mandate fully. Thus wages do not fully decrease to offset the cost of the mandate, and employment for group A decreases. Workers in group A are not willing to pay for the full cost of the mandate in terms of lower wages. Because employment of group A has changed, the firm has an incentive to substitute with workers from group B. If A and B are perfect substitutes, the labor demand curve of group B will shift to the right and the labor supply and wages for group B will both increase. If A and B are perfect complements, the labor demand curve will shift left, decreasing both labor supply and wages for group B. If A and B have no cross elasticity of demand, then the labor demand curve for B will be unchanged and there will be no effect on employment outcomes of workers in B.

#### **IV. Data and Identification Strategy**

##### **A. Data**

This paper uses data from the Census Bureau's March Current Population Survey (CPS) for the years 1980-2007 as a repeated cross section. The CPS provides consistent annual information on employment and demographic controls throughout this time period. Because of possible differences in treatment across different racial groups, the main results are restricted to the largest racial group, whites (Bitler & Schmidt, 2006).<sup>8</sup> The main regressions are also restricted to the universe of people between the standard working ages of 25 and 64.

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<sup>8</sup> Although fertility problems are more common among nonwhite women than among white women (Bitler & Schmidt, 2010), Schmidt (2007) finds no significant effect of the mandates on first births for black women and Bitler and Schmidt (2010) finds that mandates did not expand access to treatment for nonwhites.

The CPS provides information to study the effects on not working, weeks worked, and wage outcomes. Weeks worked and wage outcomes are reported for the previous year only. Wages must be imputed from total wages in the previous year, weeks worked in the previous year, and hours worked in the previous year. The measure of hourly wages in the survey year for workers paid by the hour is not available for the entire sample and is not used. To mitigate differences in coding across years and measurement error resulting in extreme outliers, wage data are trimmed 1 percent at the top and bottom of the wage distribution in the wage regressions.<sup>9</sup>

Laws were compiled in two ways. First, all previous compilations of infertility laws from previous papers examining the effect of infertility laws on fertility and access as well as the *Resolve* website were collected (Bitler & Schmidt, 2006, 2010; Bundorf, Henne, & Baker, 2007; Buckles, 2007; Hamilton & McManus, 2007; Jain, Harlow, & Hornstein, 2002; Schmidt, 2007). Second, the original law and any law changes were compiled from Westlaw and from superseded state statutes from the Harvard law library. Legal interpretation from Westlaw and selected websites informed decisions on coding when the law was vague on the subject of coverage as discussed in footnote 3.

There are several ways to code the presence of infertility laws. The first approach would be to code all states that require infertility coverage of any kind as having a law. Mandates that require that insurance for infertility treatment be offered but not covered are theoretically unlikely to have an effect as insurance companies can price the offer with infertility coverage out of the market, and these laws have not been found to have an effect on coverage or treatment

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<sup>9</sup> Results are nearly identical with larger top and bottom cuts, e.g. 2%, 5%, 10%, 20%.

(Bundorf, Henne, & Baker, 2007; Hamilton & McManus, 2007).<sup>10</sup> Laws that require or do not exclude the most expensive form of treatment, IVF (or similar procedures of GIFT or ZIFT), are likely to have a stronger effect than laws that specifically exclude this procedure. Thus, IVF coverage could be considered separately, or in more complicated specifications could be considered a “strong law.” Three states receive their infertility coverage through regulation of the Health Maintenance Organization (HMO) market and these infertility mandates only apply to HMO plans.<sup>11</sup> Although these laws are vague about what specific infertility treatments must be covered, in practice they have severely limited the amount of coverage mandated.

Table 1 shows summary statistics for those in states with mandate laws and those in states without mandates. Those in states with mandate laws are approximately the same age and gender composition, slightly less likely to be married, and somewhat more educated. Labor market outcomes are also very similar. The average year for law implementation is 1991 and year of passage ranges from 1977 with West Virginia to 2005 with Connecticut.

#### B. First stage

In order for infertility mandates to have an effect on employment outcomes, it must be empirically true that these mandates increase actual or perceived costs for these workers. For costs to increase or to be perceived to increase, it must first be true that mandates increase usage of infertility services. For usage to increase, access must first increase. Previous literature has thoroughly explored the first stage effects of the mandates and is detailed below.

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<sup>10</sup> Coding “offer” laws as having the same effect as “cover” laws provides similar results to the main specification in Table 2, with a slightly larger coefficient of -1.321 (0.334) weeks worked. This increase in magnitude seems to be driven by the state of California—there is no difference in magnitude when coding just TX and CT (prior to 2005) as “cover” states.

<sup>11</sup> Removing these HMO only states from the regression provides very similar results to those in Table 2. Coding HMO only states as not having a law provides nearly identical results to those in Table 2.

Access to fertility treatment is correlated with mandates. Nangia, Likosky, and Wang (2010) find that the median population within 60 minutes of an Assisted Reproductive Technology (ART) center was higher in states with mandates than in those without. More cycles are attempted and there is higher per-capita utilization of IVF in states and countries that cover IVF than in those that do not (Bartels, 1993; Collins et al., 1995; Neumann, 1997). Controlled studies also find a link between mandates and treatment use. In mandate states, highly educated older women, of whom only 10% sought help prior to the mandate, had a 4.1 percentage point increase in the probability of ever having had sought medical help to get pregnant (Bitler & Schmidt, 2010). Hamilton and McManus (2007) found that the number of IVF cycles increased by 89 percent for women under age 35 and by 91 percent for women over age 35 with the introduction of a mandate. Several controlled studies find that mandates increase fertility among groups most likely to be affected by them (Bundorf, Henne, & Baker, 2007; Jain, Harlow, & Hornstein, 2002; Schmidt, 2007).

In general, infertility mandates are perceived to be among the most expensive mandates offered.<sup>12</sup> The Council for Affordable Health Insurance suggests a 3-5 percent increase in costs from these mandates. Collins et al. (1995) have estimates of an annual increase of \$15.69 per employee assuming a 500 percent increase in usage. Even actuarial studies that do not take into account moral hazard concerns find increases in costs. For example, Goodman and Matthews (1997) report that a Milliman and Roberts actuarial study estimates a premium increase of \$105 to \$175 per family policy per year.

Note that many estimates underestimate the potential costs of a woman in the treatment group. These costs per employee are measured across all employees, not just those likely to utilize the mandate, and thus underestimate the cost of employees most likely to use the mandate.

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<sup>12</sup> See Frankfurter (2003) for further literature review of cost estimates.

Moreover, even small per-employee costs can lead to large costs across the entire firm. Additionally, as summarized in Section II.B, most estimates do not include moral hazard or the high elasticity of demand for these services (Collins, 1995, 2001; Jain, 2006). Nor do they generally account for spillover costs to other medical expenditures, such as maternity or neonatal care (GAO, 1996; Goldfarb et al., 1996; Devlin & Parklin, 2003). Indirect costs may also increase as controlled papers have found an increase in more costly multiple births with the introduction of mandates (Bundorf, Henne, & Baker, 2007; Bitler, 2008).

### C. Reduced form

The reduced form equation is a triple difference strategy comparing women likely to have been affected by the law in states and years where the law was in place to the control groups of unmarried men, women not in the age groups, and states in years when the law is not in effect.

The DDD equation is given by:

$$(1) \quad Y_{it} = \beta_1 havelaw_{st} * agegroup_i * female_i + \beta_2 havelaw_{st} * female_i + \beta_3 havelaw_{st} * agegroup_i + \beta_4 female * agegroup_i + \beta_5 havelaw_{st} + \beta_6 female_i + X_i \beta_7 + \delta_a + \sigma_s + \theta_t + \zeta_{st} + \varepsilon_{ist}$$

where  $Y_{it}$  is an outcome variable including Weeks Worked, whether or not a person is in the labor force, or a measure of wage income. The dichotomous variable  $havelaw_{st}$  is 1 in states  $s$  years  $t$  when there is a law mandating that infertility treatment be covered and 0 when there is not. Similarly,  $female_i$  takes the value of 1 when the individual is female and 0 when male, and  $agegroup$  is 1 when the individual is in the age group likely to be affected by the law and 0 when not. Controls  $X$  include dummies for education and marital status. A full set of age fixed effects  $\delta_a$ , state fixed effects  $\sigma_s$ , and year fixed effects  $\theta_t$  are included in order to control for observable heterogeneity between treatment and control groups. In some regressions a state specific time trend  $\zeta_{st}$  is included to control for state specific differences that trend over time.  $\varepsilon_{ist}$  is an error

term. Regressions are also robust to the inclusion of interacted state\*year\*age fixed effects in place of a state-specific time trend.

In the main specification, the choice of treatment group is women age 28-42 in states and years with mandates. These ages were chosen because women's fertility begins to decline in their late 20s (cf. Dunson, Colombo, & Baird, 2002) and egg quality deteriorates enough at age 42 for many fertility clinics to restrict transfers at those ages. Note that the treatment group in these employment based regressions is different than that of regressions examining fertility outcomes. First, all women of later child-bearing age are potentially affected by the treatment; these results provide the general equilibrium results of the effect on employment for women in this age group, thus providing information on the labor market efficiency of these mandates. Second, unlike the case of fertility outcomes, it is not appropriate to limit the treatment group to people who are employed in jobs that provide health insurance when the outcome being measured is employment. Unmarried men have been placed in the control group because they are not as likely to be affected by the law from an employer standpoint. Married men are omitted from the universe as they are less likely than women in the treatment to be treated directly, but they may be indirectly treated because they may provide coverage to their wives. Empirical and theoretical justification of this choice is discussed in VI. A, Robustness Checks.<sup>13</sup>

## **V. Results: The Labor Market Impact of the State Laws**

### **A. Regression Framework**

Table 2 presents the main results. Women likely to be affected by the mandate, those between the ages of 28 and 42, work 1.07 fewer weeks per year than other working age people in the universe, which is 3 percent lower than the average number of weeks worked for all people

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<sup>13</sup> Specification checks with different age ranges provide very similar results. Back of the envelope calculations getting a bound on the effect for women most likely to be offered infertility treatment are discussed in the discussion section.

(except married men) of 34.64 weeks. These women are also 0.02 points more likely to be out of the labor force off of a base of 0.28 for the average person in the sample, an increase of 7 percent.

Logged hourly wages conditional on working, shown in Table 2 columns (5) and (6), are not significantly affected by the mandates. As a caution, hourly wages were calculated by inflating the annual wage from the previous year by the CPI calculator provided by the BLS, dividing that by the actual weeks worked in the previous year, and dividing that measure by the usual number of hours worked per week. As with all wage results using this imputed variable, there will be measurement error attenuating the results. Additionally for these regressions, the universe of wages has been cut off at the 1 percent level at both top and bottom to adjust for differences in top coding and other outliers over time. Different methods of adjusting for these topcodes and outliers produce very similar results.

#### B. Distributional differences

In general, more educated women are most likely to use infertility treatment. Therefore we might expect to see differences in employment outcomes based on education level. It appears that women with the lowest amounts of education, those who are not high school graduates, are unlikely to be affected by the mandates (Bitler & Schmidt, 2006). Among the universe of those without a high school degree, the treated population works a statistically insignificant 0.464 fewer weeks per year ( $se(0.644)$ ). However, conditioning on the woman being a high school graduate, there does not seem to be an increasing effect of having a law on employment outcomes with education.

Table 3 presents results on distributional differences and robustness checks. Results may be stronger using states with stronger laws as the treatment states. However, Table 3 column (1) shows the effect of using laws that require that IVF to be covered for the havelaw variable (states

with laws that exclude IVF are in the control), and the magnitude of the effect decreases to 0.931 fewer weeks per year, suggesting that the effect is not stronger in IVF states; that is, employers do not differentiate between states that offer full infertility coverage and states that offer some infertility coverage.<sup>14</sup> Similarly, in states where infertility coverage is only required for HMOs, the laws may have less of an effect. Indeed, the results when removing states that require coverage for HMO plans from the universe in Table 3 column (2) do show a larger magnitude, with treated women working 1.385 fewer weeks per year. It may also take time for these laws to have an effect on employers or on the labor force composition. Therefore, columns (3) and (4) test to see if the effect is stronger by lagging the law one or two years. The coefficient from the one year lag is smaller than that of the regression with no lag, and the coefficient is somewhat smaller when 2 years of lag time is introduced, suggesting that the effect from the mandate begins within the first year of the law.

### C. Potential Pathways

The decrease in weeks worked could be coming from changes in job accession patterns, changes in job separation patterns, or a movement from full-time work to part-time work or leaving employment. To explore the impact of hiring and job separation outcomes for older workers, I construct measures of separations and accessions (hires) by matching CPS rotation groups as in Bleakley, Ferris, and Fuhrer (1999). An accession is recorded when someone who was not employed in month  $m$  is employed in month  $m+1$ . Similarly, an individual is coded as having experienced a separation in month  $m$  if he or she is employed in any month  $m$  and not in month  $m+1$ . Neither hires nor separations include people who change jobs without leaving employment.

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<sup>14</sup> It is also possible that the smaller sample size within the treatment group instead causes the results to be less precise.

If mandated workers are unwilling to take wage cuts, employers may shift employment for the mandated group by either decreasing hires of these groups or increasing job separations from these groups. Because it is generally easier for employers to treat a protected group of workers differentially at the hiring margin than at the firing margin, it is likely that there would be a stronger negative impact on accessions than on separations, if there is an impact on separations. Table 4 explores pathways through which the treated group could be working fewer weeks per year. Results from equation (1) with “Accession” or “Separation” as the *Y* variable are presented in Table 4, columns (1) and (2). Although the magnitude of 0.0019 is small, there is significant evidence of a decrease in job accessions for the treated group. The coefficient for separations is smaller, negative, and not significant, providing little evidence for increased separations as a pathway for decreased work for this group.

Changes in type of work are investigated by cutting the universe to only those working full-time in the previous year and examining the probability of them not working or working part-time in the survey year, again using equation (1) in a probit framework with not working or part-time as the *Y* variable (marginal effects are reported). These regressions show a significant increase in not working of 0.139 percentage points and a marginally significant increase in part-time work of 0.029 percentage points, in Table 4 columns (3) and (4) respectively.<sup>15</sup> These results suggest that in addition to the shift into not working, there may also be a shift from full-time work to part-time work among the treated who are employed full-time. Part-time jobs are not required to provide health insurance even in companies that provide health insurance for full-time workers and thus do not have to adjust wages or employment because of mandate costs.

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<sup>15</sup> Using a multinomial logit framework provides similar results. When married men are added to the control group, the increase in non-working becomes only marginally significant and the increase in part-time becomes significant.

An additional pathway question is what ages are most affected by the law changes. Table 4, Panel B provides information by 5 year age intervals on relative effects within the treatment group. It appears that the treatment effect on weeks worked is concentrated on ages 28-32. However, a caution should be added to these results. When married men are included in the control group, weeks worked decreases more as ages increase within the 28-42 range, suggesting greater effects of the mandate as age increases.

## **VI. Robustness Checks**

### *A. Identification assumptions*

Although mitigated by the triple difference strategy, an important concern is that states with mandates have different labor market trends than states without mandates for reasons unrelated to the mandates themselves, and would show the same differential outcomes in the absence of the mandate. To check for prior trends, Table 5 presents a falsification exercise that adds a “fake” law variable that turns on five years prior to the law. As would be expected in the absence of a pre-trend, coefficients on the fake interaction variables are close to zero and insignificant, while the main interaction term using the actual law remains significant for the weeks worked regressions and marginally significant for the NILF regressions. These results lend credence to the idea that the DDD is picking up a true effect and not a trend. The results are less clear for the  $\ln(\text{hourlywage})$  outcome, where it appears that an effect of the prior “fake law” exactly cancels out an opposite-signed trend in the actual law passage. However, Hamilton and McManus (2007) do not find any correlation between mandate passage and household income in their first stage estimates in their work.

The previous first stage literature also checks for correlations between mandates and other outcomes that could affect first stages on fertility use or multiple births. Bitler and Schmidt (2010) find no correlation with seeking help to prevent miscarriage, number of abortions,

number of pregnancy losses, or pregnancies at the time of or prior to the passage of the law. In addition to their null finding on median household income, Hamilton and McManus (2007) find no difference in the characteristics of states that got mandates compared to those who never had mandates, including female labor force participation rates, female educational attainment or average family size. They do find some correlation between having a mandate and support of government intervention in medical markets, as well as a few other political factors that might make it more likely to pass such a mandate while not affecting differential labor market or other outcomes.

It is also important to note that although the treatment may affect some members of the control group, the effect is differentially stronger for those in the treatment. For example, there may be some effects of the treatment for women age 28-42 who live in control states but work in treatment states, which will lead to attenuation of the results. Married men or women outside the age range may also be affected, but these effects will, on average, be smaller for these groups than for the treated groups, and may result in further attenuation. Table 6 provides robustness checks adding married men to, and removing single women from, the regression universe. Table 6 Panel I provides results adding married men to the universe,<sup>16</sup> and the results are very similar to the main results, with women in states with mandate laws in the treated group working 1.16 fewer weeks per year than controls and no effect on  $\ln(\text{hourly wage})$ . Tables 3a and 3b, columns (5) and (6), demonstrate the results using different age groups in the treatment; the results are larger with a larger age group, working 1.44 fewer weeks per year, and not as strong when

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<sup>16</sup> While married men who cover their wives may be treated, rendering them an imperfect control, married men as a whole will be less treated than the women in our treatment group. Married men provide a better control in some respects than the other groups in our sample, but are not perfect.

focusing at those age 35-44, working .062 fewer weeks per year.<sup>17</sup> Single women may not be as affected by the law, at least in the view of the employer, and Table 6 Panel II provides results eliminating single women entirely from the universe. These results are somewhat larger than, but substantively similar to, those in the main specification, with women in the treatment group working 1.4 fewer weeks per year and again no significant effect on wages. Table 6 Panel III adds married men and removes single women, and finds that women in the treatment group work 1.6 fewer weeks per year than those in the control group.

A similar concern, illustrated in Figure 2, Panel II, is that the elasticity of substitution for the workers may affect the control group as well as the treatment group. If there is no cross elasticity of substitution, then the control group will be unchanged by the treatment. If the control group contains substitutes for the treatment group, then the coefficients will overestimate the direct effect of the treatment on the treated. If the control group contains complements, then the coefficients in the regressions will underestimate the direct effect of the treatment on the treated.

#### *B. Robustness to specification*

The main results are robust to more complicated specifications, including a full set of age\*state\*year, as shown in Table 3, column (7) and clustering at the state\*year level rather than at the state level as shown in Table 3, column (8). As mentioned in the previous section, results are robust to different age groups in the treatment. Shortening the universe to the years when most, but not all, of the laws were put into effect, 1980-1991, attenuates the results, with affected women working 0.973 fewer weeks per year, as shown in Table 3, column (9). This attenuation

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<sup>17</sup> Results are also robust to treatment age groups 25-42, 30-44 and 35-39, although significance decreases to the 10 percent level with some of these specifications. Results are also robust to limiting the universe to ages 28-64. When married men are included in the control group, these different treatments remain significant at the 5 percent level.

may be a result of the decreased sample and treatment sizes; however, it could be that access to more expensive infertility treatments may not have been as large during the early years of this time period.

There may be a concern that the reason for change in employment is because of new mothers leaving the labor force after pregnancy.<sup>18</sup> Theoretically, childless women would be subject to employer beliefs about their likelihood of using infertility treatment, but would not be able to leave the labor force for maternity purposes. Table 7 limits the entire universe to childless people and estimates the effect of being in the affected age group under a mandate against childless people unaffected by the mandate. The results are very similar, with treated women working 1.2 fewer weeks per year than the control and an additional 0.02 point probability of not being in the labor force off of a base of 0.265, an increase of 8 percent.

A similar concern is that time off the labor market to obtain and use fertility treatment is causing the decrease in time worked. Eisenberg et al. (2010) finds that infertile women seeking treatment spend a median of 51.5 hours on infertility-related activities over an 18 month period. Using this information and information on the percentage of white women age 28-42 that use infertility services for 2002 calculated from the National Survey of Family Growth (NSFG), I am able to get an estimate of the average number of hours that all white women in this age group spend on fertility treatment. Thus:  $51.5 \text{ hours per 18 months} * (12/18) * 0.166 = 5.70 \text{ hours per year}$ . Using similar estimates for 1995 data or looking at all women rather than just white women, these hours range from 3.81 hours per year to 6.87 hours per year. This time spent on

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<sup>18</sup> For example Cristia (2008) studies women actively trying to get pregnant with their first children and who saw a healthcare professional for fertility reasons, and finds that labor force participation drops when their first children are less than one year old. The effect measured in the current study is that of a stereotype on a larger group of women who may not be trying to get pregnant or whose decision to seek fertility treatment is more marginal. It is important to note also that the effect Cristia finds is on a much smaller, more selected group, and may have a more elastic labor supply response to fertility.

fertility treatment itself would decrease the estimates of the magnitude of the results on weeks worked by 0.095 to 0.17 weeks per year depending on the assumptions made.

## **VII. Discussion and welfare implications.**

Not all mandates have the same labor market effects. This paper has shown that infertility mandates, group-specific mandates which are not valued at cost, decrease the labor supply of women most likely to be affected by these mandates. Weeks worked per year and labor force attachment decrease. This decrease is similar to that found for the decrease in weeks worked for men in states with stronger age discrimination laws in Lahey (2008), and smaller than that found by Acemoglu and Angrist (2001) for the Americans with Disabilities Act. Wages are only marginally affected by the addition of these laws. Results are robust to several specification checks and are not driven by decreased labor force participation after childbirth.

The finding that firms treat more expensive workers differentially fits with previous findings on health insurance. For example, Kapur, Escarce, Marquis, and Simon (2008) look at small firms only and find that workers with high expected health costs are less likely to be employed at firms with insurance compared to those without insurance, despite workers' greater demand for insurance. Similarly, Lahey (2007) finds less employment shifting for older workers when younger workers become relatively more expensive because of mandates.

The results in this paper pertain to the labor market for affected women as a whole. Unlike previous work on infertility mandates that examines the direct effect of insurance on fertility, it is not necessary or desirable to compare women directly impacted because they are already employed in firms that are covered by the law. Indeed, such employment is endogenous to the mandate precisely because the treated group, if employed, may be forced to sort into firms that are not mandated to provide infertility insurance. However, following Bitler (2008), the direct effect on women likely to be directly affected by these mandates can be imprecisely

estimated using a back-of-the-envelope calculation in which the coefficient on the interaction term is inflated by the share of women who are included in an employer group health insurance plan or an employer paid health insurance plan in a state-year cell. Using these variables, the number of weeks worked decreases between 2.6 and 3 weeks per year among the treated group and there is no effect on wages, as might be expected. An additional check would be to use firm size in these calculations, as small firms are less likely to have insurance and large firms are not required to carry such insurance (though there is evidence that self-insured firms do provide mandated coverage (e.g. GAO, 1996; Acs et al., 1996; Jensen & Morrissey, 1999)).<sup>19</sup> Unfortunately, the variable for firm-size does not exist in the CPS until 1988, making such estimates unreliable for this sample.

Direct cost-benefit analysis is complicated when the mandate involves childbirth because the benefit is difficult to value. The price of a life created may not be given the same value as the price of a life saved in hedonic analysis (Devlin & Parkin, 2003). Additionally, there are wide-ranging direct cost estimates of infertility mandates for employers. Note though that costs to the economy may actually be higher if fertility drugs lead to more complicated births, more premature infants, and other increased costs not generally included.

It is important to acknowledge that, while this paper focuses on the direct effects of a group-specific mandate on labor force participation and wages, increased health insurance costs may affect labor market outcomes in paths other than the ones explored in this paper. For example, employers could shift the more expensive employees to work more hours in order to

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<sup>19</sup> These results provide only information on the effect of state mandates, not the effect of a federal mandate, which would directly affect firms that self-insure. As such, the main results in the paper provide a lower bound of the effect of a federal law. Because not all firms are covered by state mandates, potentially more expensive women are able to sort into firms that are not mandated to offer benefits, which would decrease the extent of labor market inefficiencies caused by the mandate. A federal mandate would not allow that sorting.

decrease the average cost of the fixed benefit. Examining the overall effects of having any insurance, Cutler and Madrian (1998) document that in addition to some wage shifting, insured workers work 3 percent longer hours when insurance costs increase over the 1980s. Similarly, there may be a move towards substituting not only across groups in the case of group-specific mandates, but across types of workers who are covered by insurance. Cutler and Madrian (1998) also discuss the mixed literature regarding the increase in part-time or temporary workers who are not covered by health insurance to substitute for full-time workers who are covered.

Another method that employers could use in the face of increased mandate costs is to either drop insurance entirely or move to self-insurance. Because I cannot separate between firms that offer insurance from those that do not for most of the years in my CPS sample, my results tend to underestimate the actual treatment effect as some firms in the treatment are not actually covered by the mandate (and indeed, may have to compensate for lower benefits through higher wages). However, other literature has found very little evidence of either of these effects occurring. Jensen and Gable (1992) find some evidence of shifts towards small firms dropping health insurance, but Gruber (1992) is able to use a somewhat cleaner identification strategy and better data and finds no such effects.

In conclusion, not all mandates are created equal. When group-specific mandates are not valued at cost, the group affected is unwilling to make the trade-off between wages and benefit provision, and thus members of that group become more expensive to employers. While some mandates may increase efficient provision of services, others provide larger labor market inefficiencies which work against the interest of the group they are meant to benefit. Unlike maternity mandates, infertility mandates belong to this latter set. If society values access to infertility treatment, then publicly provided benefits would not provide the same group-specific labor market distortions that mandates do, although publicly provided benefits present their own

dead-weight losses from tax-financed provision (Summers, 1989). The mixed findings on the effects of insurance mandates are not a mystery, but are exactly what is predicted by theory. Policy makers should examine each mandate individually based on its own merits rather than categorizing them as the same.

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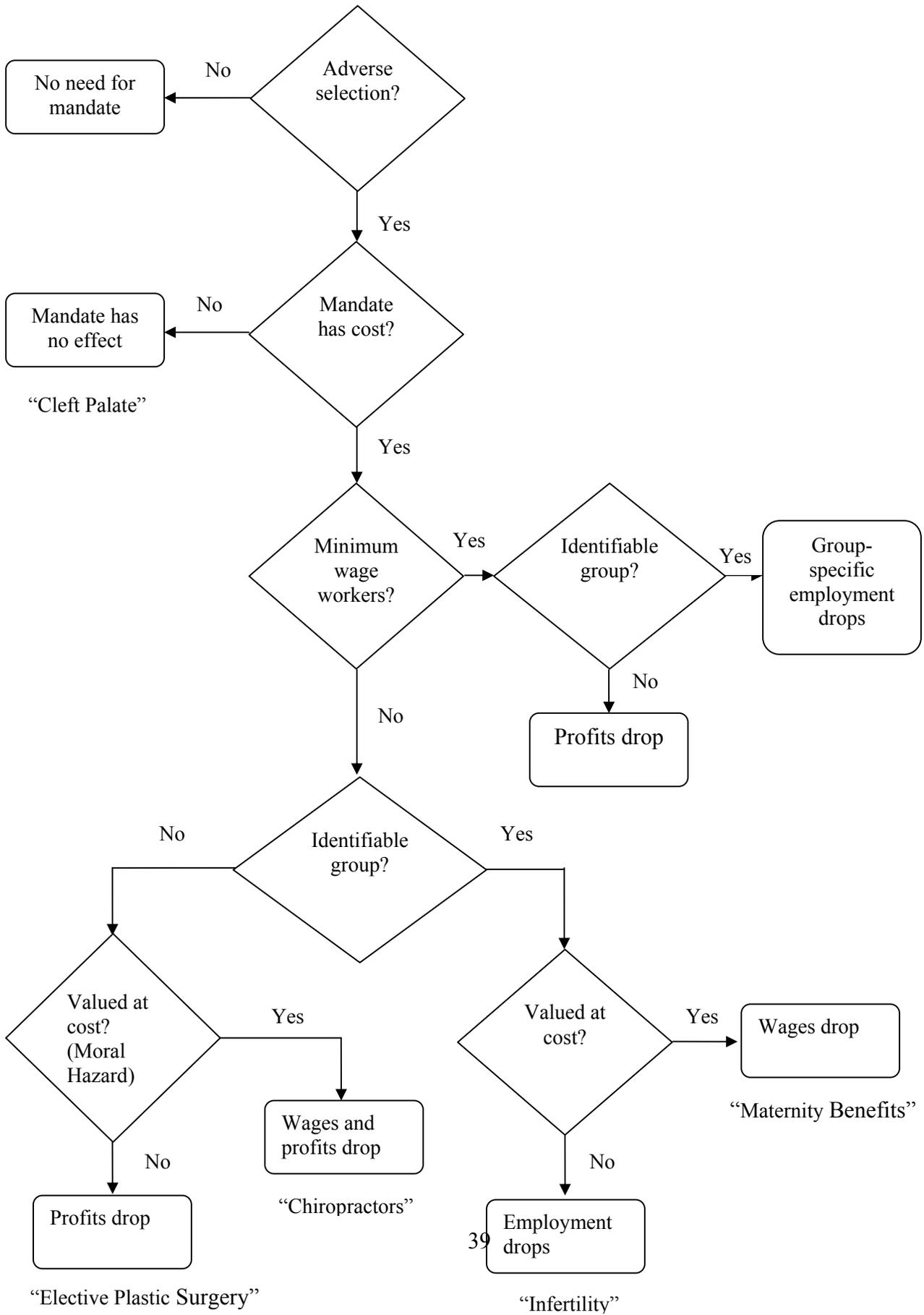
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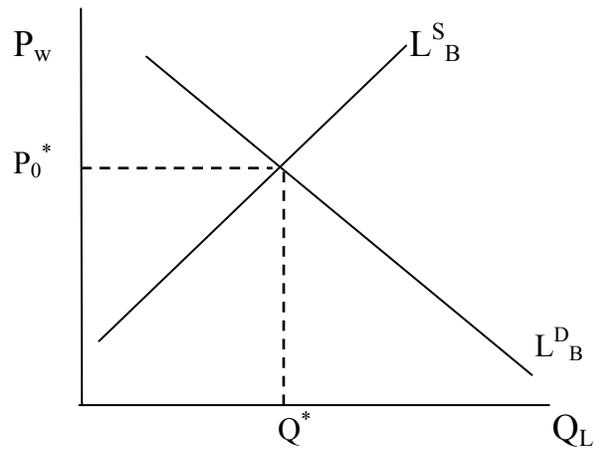
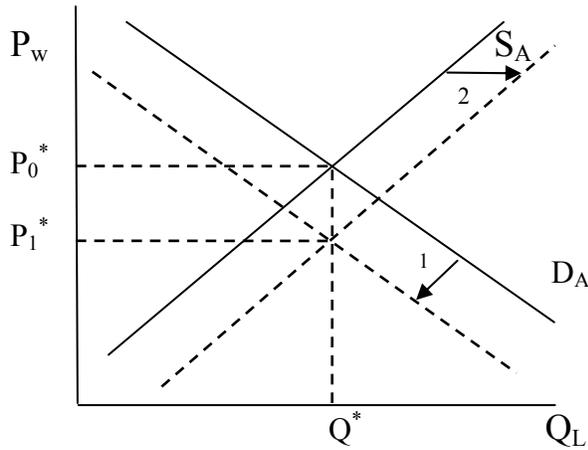
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**Figure 1: Theoretical Effects of Mandated Benefits Flowchart**



**Figure 2: Effect of Mandate Valuation on Labor Supply**

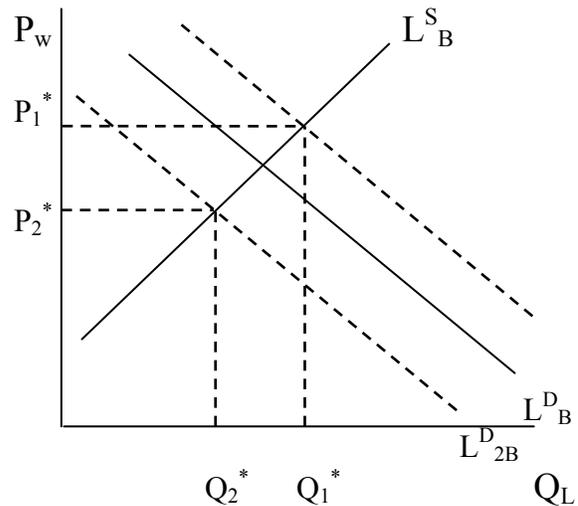
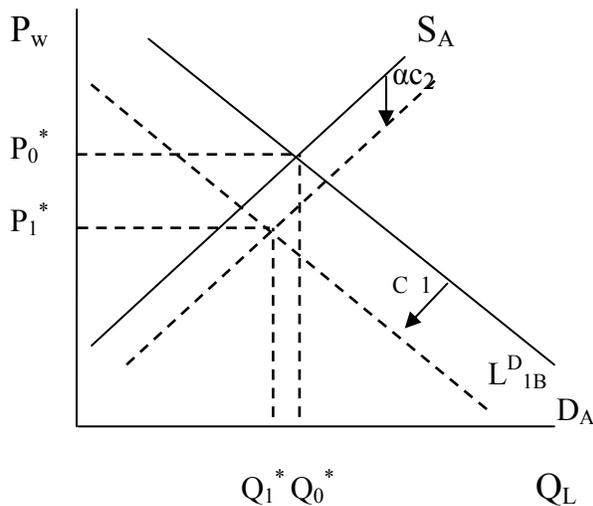
Panel I: A values benefit fully, B is not directly affected by benefit



Demand A decreases, and Supply A increases  
 ⇒ W decreases by amount of benefit  
 L is unchanged

If A and B are perfect substitutes  
 ⇒ No effect on B  
 If A and B are perfect complements  
 ⇒ No effect on B  
 If A and B have no cross-elasticity of demand  
 ⇒ No effect on B

Panel II: A values benefit at a fraction of cost,  $\alpha c$ , B is not directly affected by benefit



Demand A decreases and Supply A increases  
 ⇒ W decreases by less than amount of benefit  
 L decreases.

If A and B are perfect substitutes,  $L^D$  shifts right to  $L^D_{1B}$ , L increases, W increases  
 If A and B are perfect complements,  $L^D$  shifts left to  $L^D_{2B}$ , L decreases, W decreases.  
 If A and B have no cross elasticity,  $L^D_B$  is unchanged.

**Figure 3***Years of Law Implementation*

State	Year	Cover	Offer	IVF	HMO
Arkansas	1987	X		Only	
California	1989		X	No, GIFT	
Connecticut	1989		X	Yes	
Connecticut	2005	X		Yes	
Hawaii	1987	X		Only	
Illinois	1991	X		Yes	
Maryland	1985	X		Only	
Massachusetts	1987	X		Yes	
Montana	1987	X			Only
New Jersey	2001	X		Yes	
New York	1990	X		No	
Ohio	1991	X			Only
Rhode Island	1989	X		Yes	
Texas	1987		X	Only	
West Virginia	1977	X			Only

NOTE--The table reports the type of mandate each state enacted and its provisions. In the IVF column, "yes" indicates access to comprehensive IVF treatments, such as GIFT and ZIFT, while "only" specifies that the mandate only covers/offers limited IVF treatment. New York is coded as "no" in the IVF column because while it is coded as a mandate state, it does not mandate IVF but instead has a small fund for IVF. In the HMO column, state mandates that only apply to HMO plans are coded as "only."

**Table 1***Summary Statistics, CPS 1980-2007*

	(1)	(2)	(3)
	Everybody	With Law	Without Law
	(N=1,267,033)	(N=203,422)	(N=1,063,611)
Age	41.559	42.004	41.474
Married	0.569	0.537	0.575
White	1.000	1.000	1.000
Female	0.801	0.789	0.803
No HS	0.149	0.121	0.155
HS	0.378	0.363	0.381
Some College	0.238	0.240	0.238
College Grad	0.167	0.184	0.163
weeks worked	34.638	35.495	34.474
NILF*	0.277	0.265	0.280
ln(hourlywage)**	1.861	1.957	1.842
Year Law Passed	1991		

NOTE-- Universe includes all people aged 25-64 in the March CPS from 1980-2007 except married men. Hourly wages are adjusted using a CPI inflator where 1982-1984 = 100. \*Uses concurrent CPS, number of observations are 1265645, 203245, 1062400. \*\*Uses concurrent CPS, number of observations are 893658, 146061, 747597.

**Table 2**  
*Main Results*

	(1)	(2)	(3)	(4)	(5)	(6)
	Weeks Worked		NILF		ln(hourly wage)	
law*female*2842	-1.069**	-1.067**	0.022**	0.022**	0.000	0.000
	(0.371)	(0.373)	(0.008)	(0.008)	(0.009)	(0.009)
law*2842	0.293	0.301	-0.000	-0.000	0.004	0.003
	(0.210)	(0.206)	(0.005)	(0.005)	(0.007)	(0.007)
female*2842	-2.539**	-2.534**	0.083**	0.083**	0.030**	0.030**
	(0.146)	(0.146)	(0.003)	(0.003)	(0.005)	(0.005)
law*female	1.963**	1.964**	-0.045**	-0.045**	0.045**	0.045**
	(0.366)	(0.361)	(0.007)	(0.007)	(0.015)	(0.015)
female	-0.991**	-0.993**	0.034**	0.034**	-0.182**	-0.182**
	(0.246)	(0.246)	(0.006)	(0.006)	(0.009)	(0.009)
have law	-0.872*	-1.015*	0.022**	0.021**	-0.035*	-0.019
	(0.383)	(0.388)	(0.008)	(0.008)	(0.016)	(0.020)
Observations	1,266,977	1,266,977	1,265,596	1,265,596	877,441	877,441
State Trend	No	Yes	No	Yes	No	Yes

NOTE--Robust standard errors are in parentheses and are clustered on state. The table reports interactions in regressions that include married, white, education dummies, age dummies, year dummies, and state dummies. OLS results are reported in the Weeks Worked and ln(hourly wage) columns. The marginal of the probit coefficient is reported in the NILF column. Weeks worked and wage information refer to the previous year. Regressions are weighted at the person level. The universe was limited to whites, ages 25 to 64 for years 1980 and above. Married men were also dropped from the universe.

\* significant at 5%; \*\* significant at 1%

**Table 3**

*Distributional Differences and Robustness Checks*

	A. Weeks Worked Last Year								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	IVF as mandate variable	No HMO	Have Law One Year Later	Have Law Two Years Later	Age Group 25-44	Age Group 35-44	Age State Year Fixed Effects	Clustering on State*Year	1980- 1991
law*female*agegrp	-0.931*	-1.385**	-0.999**	-0.982**	-1.441**	-0.623	-1.092**	-1.069**	-0.973
	(0.398)	(0.345)	(0.337)	(0.322)	(0.415)	(0.450)	(0.297)	(0.258)	(0.707)
law*agegrp	0.260	0.282	0.248	0.337	0.481	-0.188	-0.123	0.293	0.577
	(0.230)	(0.217)	(0.212)	(0.229)	(0.278)	(0.238)	(0.255)	(0.216)	(0.715)
female*agegrp	-2.642**	-2.538**	-2.559**	-2.568**	-3.356**	0.508**	-2.531**	-2.539**	-1.480**
	(0.168)	(0.150)	(0.147)	(0.150)	(0.241)	(0.155)	(0.151)	(0.132)	(0.193)
law*female	2.258**	2.033**	2.040**	2.078**	2.308**	1.696**	2.007**	1.963**	1.391**
	(0.462)	(0.414)	(0.373)	(0.387)	(0.421)	(0.396)	(0.370)	(0.242)	(0.456)
female	-0.848**	-0.954**	-0.985**	-0.973**	0.106	-2.370**	-1.046**	-0.991**	-1.411**
	(0.189)	(0.252)	(0.243)	(0.238)	(0.277)	(0.286)	(0.243)	(0.104)	(0.318)
have law	-1.379**	-1.008*	-0.866*	-0.961*	-1.021*	-0.687	-1.845**	-0.872**	-0.775
	(0.485)	(0.380)	(0.405)	(0.435)	(0.421)	(0.360)	(0.601)	(0.226)	(0.597)
Observations	1,266,977	1,185,816	1,266,977	1,266,977	1,266,977	1,266,977	1,266,977	1,266,977	525,221
	B. Ln(Hourly Wage)								
law*female*agegrp	0.016*	0.006	0.002	0.005	-0.012	0.010	0.001	0.000	-0.010
	(0.007)	(0.010)	(0.008)	(0.008)	(0.015)	(0.013)	(0.009)	(0.009)	(0.023)
law*agegrp	0.005	0.004	0.004	0.002	0.009	-0.006	0.002	0.004	0.010
	(0.011)	(0.008)	(0.007)	(0.008)	(0.011)	(0.010)	(0.014)	(0.008)	(0.022)
female*agegrp	0.029**	0.029**	0.030**	0.030**	0.068**	-0.025**	0.029**	0.030**	0.048**
	(0.004)	(0.005)	(0.005)	(0.005)	(0.009)	(0.005)	(0.005)	(0.004)	(0.007)
law*female	0.042**	0.054**	0.047**	0.047**	0.055*	0.042**	0.041*	0.045**	0.057
	(0.016)	(0.015)	(0.015)	(0.015)	(0.021)	(0.013)	(0.016)	(0.008)	(0.033)
female	-0.178**	-0.180**	-0.182**	-0.182**	-0.217**	-0.160**	-0.181**	-0.182**	-0.228**
	(0.007)	(0.009)	(0.009)	(0.008)	(0.012)	(0.008)	(0.009)	(0.004)	(0.006)
have law	-0.025	-0.041*	-0.034*	-0.037*	-0.040*	-0.031*	0.014	-0.035**	-0.012
	(0.017)	(0.016)	(0.016)	(0.016)	(0.018)	(0.015)	(0.021)	(0.009)	(0.038)
Observations	877,441	823,248	877,441	877,441	877,441	877,441	877,441	877,441	346,623
Age*State*Year Fixed Effects	No	No	No	No	No	No	Yes	No	No

NOTE-- Robust standard errors are in parentheses and are clustered on state. The table reports interactions in regressions that include married, white, no high school, high school graduate, some college, college graduate, age dummies, year dummies, and state dummies. Ln(hourly wage) was adjusted using a CPI inflator where 1982-1984 = 100. Weeks worked last year has been lagged a year in order to align actual data from weeks worked last year with the year the law was enacted. Regressions are weighted by person weight. The universe includes whites, age 25 to 64 for years 1980 to 2007. Married men have been dropped from the universe. Column (1) reports results where IVF is the mandate variable instead of infertility. Column (2) reports results where states with HMO coverage only (Montana, Ohio, and West Virginia) were dropped from the dataset. Column (3) and column (4) report results where the base is given 1 and 2 years leads respectively. Column (5) and column (6) report results where the age group has been cut to 25-44 and 35-44. Column (7) reports the results of regressions that included the age\*state\*year variable as an additional fixed effect. Column (8) presents the results of regressions that were run with clustering on state and year. Column (9) reports regression results where the years in the universe have been cut to 1980-1991.

\* significant at 5%; \*\* significant at 1%

**Table 4**  
*Pathways*

A. Labor Market Transitions				
	(1)	(2)	(3)	(4)
	Accession	Separation	Fulltime to NW	Fulltime to PT
law*female*2842	-0.0019** (0.0005)	-0.0002 (0.0010)	0.139** (0.051)	0.029 (0.020)
law*2842	0.0010 (0.0005)	0.0000 (0.0007)	-0.107** (0.039)	-0.027* (0.014)
female*2842	0.0023** (0.0003)	0.0006 (0.0004)	-0.069* (0.029)	-0.046** (0.009)
law*female	0.0005 (0.0005)	-0.0017* (0.0007)	-0.086 (0.047)	-0.021 (0.011)
female	-0.0039** (0.0003)	-0.0046** (0.0003)	-0.094** (0.024)	0.006 (0.006)
have law	0.0017* (0.0008)	0.0028** (0.0006)	0.104** (0.035)	0.026* (0.010)
Observations	9,148,491	9,252,568	17,267	120,658
B. Five Year Age Intervals				
	Weeks Worked		ln(hourly wage)	
law*female*2832	-1.522** (0.319)	-1.524** (0.318)	-0.019 (0.012)	-0.020 (0.012)
law*female*3337	-0.897 (0.636)	-0.896 (0.636)	0.013 (0.010)	0.013 (0.010)
law*female*3842	-0.800 (0.493)	-0.793 (0.501)	0.017 (0.017)	0.017 (0.017)
Observations	1266977	1266977	877441	877441
State Trend	No	Yes	No	Yes

NOTE--Robust standard errors are in parentheses and are clustered on state. Married men were dropped from the universe. Both panels report interactions in regressions that control for married, white, and education. The regressions were also run with a full set of age dummies, state dummies, and year dummies. OLS results are reported throughout the table. Regressions are weighted by person weight. The universe includes whites, age 25 to 64 for years 1980 to 2007. In Panel A, column (1) an accession is recorded when someone who was not employed in month m is employed in month m+1. In column (2), an individual is coded as having experienced a separation in month m if he or she is employed in any month m and not in month m+1. Column (3) reports results for individuals transitioning from fulltime to not working. Column (4) reports results on individuals transitioning from fulltime to part-time. Fulltime in the previous year is defined as working 35 hours or more and at least 40 weeks that year. Part-time is defined as working less than 35 hours. Not working is defined by workers who are either not in the labor force or are unemployed. Panel B presents the results of triple interactions, which have age intervals of five years, for weeks worked and ln(hourly wage). There were also main effects for law\*2832, law\*3337, law\*3842, female\*2832, female\*3337, female\*3842, law\*female, female, and have law.

\* significant at 5%; \*\* significant at 1%

**Table 5***Robustness Check Regressions*

	(1)	(2)	(3)	(4)	(5)	(6)
	Weeks Worked		NILF		ln(hourly wage)	
law*female*2842	-1.233** (0.440)	-1.230** (0.440)	0.018 (0.010)	0.018 (0.010)	-0.042** (0.015)	-0.041** (0.015)
fakelaw*female*2842	0.164 (0.590)	0.163 (0.587)	0.005 (0.010)	0.005 (0.010)	0.047** (0.012)	0.046** (0.012)
law*2842	1.618** (0.534)	1.628** (0.530)	-0.018 (0.013)	-0.018 (0.013)	0.044** (0.007)	0.043** (0.006)
fakelaw*2842	-1.465** (0.511)	-1.458** (0.515)	0.021 (0.015)	0.020 (0.015)	-0.045** (0.011)	-0.044** (0.011)
female*2842	-2.537** (0.152)	-2.531** (0.152)	0.082** (0.003)	0.082** (0.003)	0.026** (0.004)	0.026** (0.004)
law*female	3.922** (0.606)	3.917** (0.608)	-0.083** (0.011)	-0.082** (0.011)	0.095** (0.019)	0.094** (0.019)
fakelaw*female	-2.163** (0.647)	-2.155** (0.647)	0.048** (0.015)	0.047** (0.015)	-0.055** (0.020)	-0.055** (0.020)
female	-0.789** (0.209)	-0.792** (0.210)	0.030** (0.005)	0.030** (0.005)	-0.177** (0.009)	-0.177** -0.009
have law	-2.895** (0.739)	-3.207** (0.720)	0.069** (0.017)	0.070** (0.016)	-0.077** (0.017)	-0.063** (0.011)
fake law	1.464 (0.747)	1.443* (0.651)	-0.034* (0.016)	-0.038* (0.015)	0.046** (0.012)	0.028 (0.027)
Observations	1,266,977	1,266,977	1,265,596	1,265,596	877,441	877,441
State Trend	No	Yes	No	Yes	No	Yes

NOTE--Robust standard errors are in parentheses and are clustered on state. The table reports interactions in regressions that include married, white, no high school, high school graduate, some college, college graduate, age dummies, year dummies, and state dummies. The table also includes fake law regressions. Fake law is defined as 5 years prior to law passage. OLS results are reported in the Weeks Worked and Ln(hourlywage) columns. The marginal of the Probit coefficient is reported in the NILF column. Weeks worked and wage information refer to the previous year. Regressions are weighted by person weight. The universe includes whites, age 25 to 64 for years 1980 to 2007. Married men have been dropped from the universe.

\* significant at 5%; \*\* significant at 1%

**Table 6**  
*Different Universes*

	A. With Married Men		
	Weeks Worked	NILF	ln(hourly wage)
law*female*2842	-1.163** (0.342)	0.007 (0.005)	-0.002 (0.010)
law*2842	0.398 (0.205)	0.010 (0.006)	0.009* (0.004)
female*2842	-1.482** (0.155)	0.088** (0.002)	0.049** (0.003)
law*female	2.473** (0.377)	-0.039** (0.005)	0.051** (0.019)
female	-9.815** (0.258)	0.175** (0.004)	-0.355** (0.011)
have law	-1.250** (0.327)	0.019** (0.006)	-0.030* (0.013)
Observations	1979091	1967714	1455688
	B. No Single Women		
law*female*2842	-1.402* (0.555)	0.031** (0.011)	0.011 (0.009)
law*2842	0.250 (0.207)	0.001 (0.005)	0.004 (0.007)
female*2842	-3.007** (0.167)	0.095** (0.003)	0.036** (0.005)
law*female	2.785** (0.451)	-0.062** (0.009)	0.048** (0.016)
female	-6.948** (0.335)	0.154** (0.006)	-0.190** (0.009)
have law	-1.196** (0.432)	0.030** (0.010)	-0.035* (0.015)
Observations	973,124	971,869	656,800
	C. With Married Men and No Single Women		
law*female*2842	-1.615** (0.538)	0.016* (0.006)	0.006 (0.009)
law*2842	0.458* (0.199)	0.008 (0.005)	0.010* (0.004)
female*2842	-2.023** (0.181)	0.107** (0.003)	0.053** (0.004)
law*female	2.999** (0.414)	-0.043** (0.006)	0.050** (0.017)
female	-13.110** (0.312)	0.248** (0.005)	-0.429** (0.011)
have law	-1.302** (0.327)	0.018** (0.006)	-0.026* (0.012)
Observations	1685238	1673987	1,235,047

NOTE--Robust standard errors are in parentheses and are clustered on state. The table reports interactions in regressions that include married, white, education dummies, age dummies, year dummies, and state dummies. The regressions include state trends. OLS results are reported in the Weeks Worked and Ln(hourly wage) columns. The marginal of the probit coefficient is reported in the NILF column. Weeks worked and wage information refer to the previous year. Regressions are weighted by person weight. The universe includes no married men, whites, age 25 to 64 for years 1980 to 2007. For Panel A, married men were included in the universe. In Panel B, single women were deleted from the regression universe. For Panel C, the universe includes married men and no single women. \* significant at 5%; \*\* significant at 1%

**Table 7***Childless Women*

	(1)	(2)	(3)	(4)	(5)	(6)
	Weeks Worked		NILF		ln(hourly wage)	
law*female*2842	-1.220**	-1.194**	0.022**	0.021**	-0.005	-0.005
	(0.398)	(0.402)	(0.008)	(0.008)	(0.011)	(0.011)
law*2842	0.181	0.154	0.001	0.001	0.007	0.006
	(0.253)	(0.248)	(0.005)	(0.005)	(0.009)	(0.009)
female*2842	0.485	0.483	0.014**	0.014**	0.055**	0.055**
	(0.250)	(0.250)	(0.005)	(0.005)	(0.006)	(0.006)
law*female	1.592**	1.554**	-0.036**	-0.035**	0.044**	0.044**
	(0.386)	(0.388)	(0.007)	(0.007)	(0.016)	(0.016)
female	-0.599**	-0.595**	0.028**	0.028**	-0.151**	-0.151**
	(0.215)	(0.215)	(0.005)	(0.005)	(0.008)	-0.008
have law	-1.275**	-0.711	0.026**	0.009	-0.034*	-0.003
	(0.440)	(0.546)	(0.008)	(0.009)	(0.015)	(0.017)
Observations	628,931	628,931	628,030	628,030	450,405	450,405
State Trend	No	Yes	No	Yes	No	Yes

NOTE--Robust standard errors are in parentheses and are clustered on state. The table reports regressions on the universe of the childless only. The table reports interactions in regressions that include married, white, education dummies, age dummies, year dummies, and state dummies. OLS results are reported in the Weeks Worked and Ln(hourlywage) columns. The marginal of the probit coefficient is reported in the NILF column. Weeks worked and wage information refer to the previous year. Regressions are weighted by person weight. The universe includes whites, age 25 to 64 for years 1980 to 2007. Married men have been dropped from the universe.

\* significant at 5%; \*\* significant at 1%