9-6-2016

The Impact of Mandated Maternity Benefits on the Gender Differential in Promotions: Examining the Role of Adverse Selection

Mallika Thomas
Cornell University, mmt88@cornell.edu

The Impact of Mandated Maternity Benefits on the Gender Differential in Promotions: Examining the Role of Adverse Selection

Abstract

This paper examines how mandated maternity leave policies impact the gender gap in promotions. I present a model of the gender gap in promotions where firms must choose whether to invest in the training of their employees, but they are uncertain about their employees' future choice of hours of work. If women are more likely than men to reduce their hours of work during childrearing years, firms will invest less in women early in their careers, leading to a gender gap in promotions. In the presence of asymmetric information about workers' future preferences, mandated maternity leave policies can exacerbate this gap. Using the Multi-City Study of Urban Inequality and the Panel Study of Income Dynamics, I test the predictions of the model in the context of the Family and Medical Leave Act of 1993 (FMLA). Women hired after the enactment of the FMLA are five percent more likely to remain employed but eight percent less likely to be promoted than those who were hired before the FMLA. Furthermore, I find evidence suggesting that information asymmetry, in addition to selection, is driving the increase in the gender gap in promotions.

Keywords

maternity benefits, gender, promotions, Family and Medical Leave Act of 1993

Disciplines

Benefits and Compensation | Economics | Gender and Sexuality | Labor Economics

Comments

Suggested Citation

Thomas, M. (2016). *The impact of mandated maternity benefits on the gender differential in promotions: Examining the role of adverse selection* (ICS 2016-001). Retrieved [insert date] from Cornell University, ILR School, Institute for Compensation Studies site: http://digitalcommons.ilr.cornell.edu/ics/16

The Impact of Mandated Maternity Benefits on the Gender Differential in Promotions: Examining the Role of Adverse Selection*

Mallika Thomas†

September 6, 2016

Abstract

This paper examines how mandated maternity leave policies impact the gender gap in promotions. I present a model of the gender gap in promotions where firms must choose whether to invest in the training of their employees, but they are uncertain about their employees' future choice of hours of work. If women are more likely than men to reduce their hours of work during childrearing years, firms will invest less in women early in their careers, leading to a gender gap in promotions. In the presence of asymmetric information about workers' future preferences, mandated maternity leave policies can exacerbate this gap. Using the Multi-City Study of Urban Inequality and the Panel Study of Income Dynamics, I test the predictions of the model in the context of the Family and Medical Leave Act of 1993 (FMLA). Women hired after the enactment of the FMLA are five percent more likely to remain employed but eight percent less likely to be promoted than those who were hired before the FMLA. Furthermore, I find evidence suggesting that information asymmetry, in addition to selection, is driving the increase in the gender gap in promotions.

*I am grateful to Gary Becker, Edward Lazear, James Heckman, and Alexander Frankel for invaluable advice and feedback at all stages of this project. I also thank Marianne Bertrand, Alessandra Voena, Pierre-Andre Chiappori, Samuel Scholhofer-Wohl, Bruce Meyer, Elisa Olivieri, Daryl Fairweather, James Marrone, Seth Blumberg, Stefano Mosso, and numerous seminar participants for many useful comments. I gratefully acknowledge the financial support of the American Economic Association's Dissertation Fellowship, the George Stigler Dissertation Fund, University of Chicago's Center for the Study of Gender and Sexuality, and the University of Chicago's Division of Social Sciences.

†Cornell University Department of Economics, mallika.thomas@cornell.edu

1 Introduction

Over the past 20 years, most economically advanced countries have enacted an array of maternity leave benefits, and these countries have witnessed a large increase in female labor force participation in comparison to the United States. However, employed women in the United States are almost three times as likely to reach managerial level positions and much more likely to work full-time than in advanced countries with more comprehensive maternity leave policies.[1] A key question in designing policies that are intended to improve women's labor market outcomes is whether such policies may be facilitating participation in entry level positions but simultaneously reducing the opportunities for women to attain upper level positions.

This paper evaluates whether maternity leave policies can contribute to a widening of this "managerial-entry level" gap, by changing the incentives for employers to invest in their workers. Employers are uncertain about whether workers will reduce their productivity in the future, and in particular, whether employees will reduce their hours of work when they have children. However, they must choose whether to invest in their employees, by training or mentoring them early on in their careers, before learning what their labor supply choices will be in the presence of children. These investments are more profitable to the firm when workers work longer hours. If women, on average, work fewer hours during child-rearing years than men, and firms cannot perfectly distinguish between workers who will work fewer hours and those who will not, the firm will be less likely to invest in and train women during the early years of their careers. Mandated maternity benefits can exacerbate the existing information problem and widen the gender difference in promotions, by changing the distribution of types of women who select into the labor force. Therefore, the expected return on investment in any individual woman may be lower, even among those women who would have otherwise worked as many hours as men.

I formalize this idea using a simple model where workers are heterogeneous in their future preferences for leisure, or time with their family, but information on these preferences is private. Because workers cannot internalize the cost of their training, and firms profit from their ability to sort workers, the private information gives rise to a signaling game. Employers use observed productivity as a signal of future productivity. I show that after a mandated maternity leave policy is enacted, however, observed productivity becomes less predictive of future productivity. Precisely because of the increase in the expected career tenure of family-oriented workers, signals become less informative, and the cost of extracting information about workers' types increases. As a result, firms must set a higher standard in order to profitably promote female workers. The model predicts that while employment and labor force participation among women will increase after the enactment of a maternity leave mandate, the likelihood of promotions for young women will decrease. Moreover, the framework makes clear that the decrease in the likelihood of promotion is generated by both the selection into the labor force of workers with higher labor supply costs as well as a reduced expected

[1] Blau and Kahn (2013) details these findings in a cross-country analysis.

return on firm investment in all female workers, even those with the same ex-ante preferences as men.

I then determine the impact of mandated maternity leave policies in the context of the Family and Medical Leave Act of 1993 (FMLA), a federal mandate in the United States. I exploit the variation in the fertility of women by age and the large decrease in fertility for women at age 40 as well as the variation in state legislation that preexisted the federal mandate, using two different datasets: the Panel Study of Income Dynamics (PSID) and the Multi-City Study of Urban Inequality (MCSUI). I find that women hired after the FMLA are five percent more likely to remain employed, conditional on job tenure, but they are eight percent less likely to be promoted in comparison to those hired before the FMLA. Furthermore, the widening of the existing gender differential in promotions is only observed for women under the age of 40 and is larger among women in age groups with the highest likelihood of conceiving a child.

Consistent with a model of asymmetric information, I find that all women of childbearing age face a reduced likelihood of promotion, including those women who never have children. Furthermore, I take advantage of the detailed data available in the MCSUI, and I construct for each firm, measures of the cost of training. I find the that the widening of the gap in promotions is greater among firms with high training costs, a result that is difficult to reconcile with a model of symmetric information. Finally, I examine several signals of workers' future productivity, including early career hours, indicators of job perfomance, and the choice of potential wage profile. For women hired after the Family and Medical Leave Act, I find a decrease in the return to these signals, in terms of the likelihood of promotion, and moreover, a decrease in the return to these signals specifically for fertile women alone, whereas the return to signaling is unchanged for women over the age of 40, for a wide variety of signals. This is strong evidence that information asymmetry about workers' private labor supply costs, in addition to selection based on these costs, is driving the increase in the gender gap in promotions.

Distinguishing between the implications of a symmetric information model and a model in which asymmetric information plays a role has important welfare implications. In a world of symmetric information, where employers can fully discern which workers will work long hours in the future and which will not, training will be allocated as efficiently as before the enactment of a maternity leave mandate. However, if asymmetric information plays a role, then the cost of increasing female labor force participation is borne, in part, by the "high-hours" women, in terms of a loss of human capital and wage growth over the course of their lifecycle.

1.1 Related Literature

While there is an extensive literature examining the short-term impact of maternity leave policies on female employment and labor supply,[2] a much smaller but growing strand of literature investigates the dynamic effect of maternity leave policies on the long-term human capital accumulation and wage growth of women. Mukhopadyay (2012) , Erosa, Fuster, and Restuccia (2010), and Sanchez-Marcos (2014), for example, all examine the erosion of different forms of human capital that are incentivized as a result of maternity leave policies. [3] Adda, Dustman, and Stevens (2011) analyze the effect of policies that incentivize childrearing on not only unearned wages and the loss of human capital, but also on the change in the path of wage growth due to selection into more child-friendly occupations. To date, however, such work has tended to address the direct effect of leave policy on the incentives for women of a given type, and the effect due to selection has largely been ignored.

This paper not only accounts for selection on the basis of heterogeneity in labor supply costs, but it emphasizes a distinction between adverse selection, where unobserved heterogeneity preexists the policy change and affects the optimal responses of agents, and the direct response of individuals to the incentive structure created by the policy. Much of the previous literature only examines the latter. Furthermore, such literature finds relatively small effects on wages and wage growth from the leave policy itself, especially policies where the length of the leave period is short.[4] Furthermore, the majority of approaches that examine the effect of maternity leave polices through counterfactual analyses attribute, by design, the variation in wages and

[2] Several U.S. studies suggest that time off work is associated with inreases in employment and wages, such as Dalto (1989), SpalterRoth and Hartmann (1990), Waldfogel (1994), (1997), Ruhm (2003), and Higuchi and Abe (1999). Klerman and Leibowitz (1997), Waldfogel (1998), and Baker and Milligan (2008) show that these policies have a substantial effect on female labor force participation. Waldfogel, Higuchi and Abe (1999) find that family leave coverage increases the likelihood that a woman will return to her employer after childbirth in the US, Britain and Japan. There is evidence both in the US and Britain (Waldfogel (1998a)) that women who maintain employment continuity over childbirth have higher wages than those who do not. Ruhm (1998) finds that in nine European countries parental leave legislation is associated with increases in women's employment but wth reductions in their relative wages when leave is mandated at extended durations. Gruber (1994) examines the effect on wages of a US mandate requiring job-protection as well as employer-based coverage for the medical costs of pregnancy among firms providing such benefits for temporariliy disabled workers. This paper finds that this policy, which affects the cost of hiring a member of an identifiable group, reduces wages by the cost to the employer, but has little effect on employment.

[3] Mukhopadyay (2012) builds a dynamic model of labor force participation with the feature that a maternity policy that increases current participation affects future wages through increased work experience as well as future labor force participation. Erosa, Fuster, and Restuccia (2010) assess the impact of mandatory parental leave policies by developing a general equilibrium model of fertility and labor market decisions, considering the effect on women's retention of job-specific human capital. Sanchez-Marcos (2014) argues that mothers on leave do not accumulate human capital, so long lasting leaves may erode their wage prospects, affecting future labor force participation choices as well. Lalive and Zweimuller (2009) finds that leave policies induce a fertility delay in the short-term, and a delay in fertility can lead to substantial increases in career earnings.

[4] Waldfogel (1996) found that the FMLA had a slightly positive employment effect and no discernible wage effect. Adda, Dustman, and Stevens (2011) found relatively small effects on wage growth due to a pro-fertility policy in Germany, examining the channels of the direct incentive of the policy on fertility, and subsequently, occupational choice in anticipation of a change in fertility choices. They find the small estimated impact unsurprising, given that the long-term impact of the policy on fertility is small, even in the context of a paid maternity leave policy. Ruhm (1998) examines mandated paid parental leave in nine European countries and finds that parental leave is associated reductions in their relative wages when only when leave is mandated at extended durations.

employment to a change in fertility or labor force participation incentives for individuals of fixed characteristics. This strand of literature does not allow for a role of asymmetric information.

Thus, this paper also builds on the literature on statistical discrimination, pioneered by Arrow (1972) and Phelps (1972) and formalized by Coate and Loury (1993). This literature emphasizes that group differences can arise endogenously, even without any ex-ante differences in across groups. More recent literature has incorporated private information into the context of the gender-wage gap and its evolution over the course of the lifecycle. Albanesi and Olivetti (2009) show that in the presence of private information about worker's labor market attachment, firms offer labor contracts with lower earnings and performance pay to female workers. The work closest to this paper, in addressing the long-term evolution of the gender gap in human capital accumulation and wages resulting from private information is Gayle and Golan (2012), which formulates a model of labor supply and human capital accumulation in which workers have private information about their labor market participation costs, and employers use the observed labor supply decisions as a signal of the worker's private information. This paper, however, uses the implications of a model of asymmetric information to analyze the effect of introducing a maternity leave policy on the equilibrium labor market outcomes and human capital accumulation of women.

Finally, this papers speaks to a mechanism through which the gender wage gap itself evolves over the course of the lifecycle. There is growing evidence that the wage gap is relatively small when workers are young, and it increases over the course of the lifecycle (Bertrand, Goldin, and Katz (2010), Wood, Corcoran, and Courant (1993)). Moreover, in spite of the significant decline in the gender gap in earnings in the United States over the past 40 years, there is large evidence of a persistent gap among the high ranking and higher earning positions (Blau and Kahn (2006), Bertrand and Hallock (2001), Wolfers (2006)). A large body of literature addresses the array of explanations for this gap (see Altonji and Blank, 1999, for a survey), from human capital explanations (Becker (1985), Mincer and Polachek (1974)) to comparative advantage (Lazear and Rosen (1990)) and occupational choice (Polachek (1981), Adda, Dustman, and Stevens (2011)). The model presented in this paper and the evidence substantiating it adds to this literature by finding support for one of the mechanisms through which the gender wage gap itself can arise. It specifies a model that suggests that information asymmetry is one of the driving forces behind the increase in the gender earnings gap at the upper end of the earnings distribution and addresses the implications for the impact of a mandated maternity leave policy.

The rest of the paper is organized as follows. Section 2 describes the institutional details of the Family and Medical Leave Act. Section 3 develops the theoretical framework. Section 4 presents the main results and discusses alternative explanations. Section 5 concludes.

2 Background and Institutional Detail

Passed in August of 1993, the Family and Medical Leave Act (FMLA) is a federal mandate in the United States that requires covered employers to allow eligible employees to take up to 12 weeks of unpaid leave per 12-month period for the birth and care of a newborn child. [5] Once their leave is over, employees are entitled to reinstatement. The mandate was designed to impose a minimal cost to the employer[6], and it does not require that employees continue to receive any form of wages or compensation while on leave. [7] In order to be eligible for the leave, employees must have worked for a covered employer for 12 months, have worked at least 1,250 hours over the past year, and worked at a location where at least 50 employees were employed within 75 miles.

Following FMLA leave, an employee has the right to be returned to the same position the employee held before or to an equivalent position - one that is identical to the employee's former position in terms of pay, benefits, and working conditions, including privileges and status. The equivalent position must involve the same or substantially similar duties and must require substantially equivalent skill, effort, responsibility, and authority. [8]

The national FMLA was not the first legislation of its kind. Several states had passed similar legislation prior to Congress' 1993 output. Some states, such as California, passed legislation that very closely mirrored the federal FMLA policy. Many states passed legislation well before 1993, but such legislation was not as expansive as what was eventually mandated federally. Other states supplied legislation that was in some ways more progressive than the federal counterpart. Maine, for example, passed a family and medical leave policy in 1987 that applies to firms of 15 or more employees. Oregon only passed a leave requirement in 1995, but it applies to employers of 25 or more persons and only requires that an employee as been employed for 180 and worked an average of 25 hours or more per week during that 180 day period.

In this paper, we take into consideration the variation in the state laws prior to the passage of the federally mandated FMLA. I exploit the variation in the state laws across states and over time, as summarized in Appendix D Table 1.

[5] If an employee is unable to work because of pregnancy, she may take FMLA leave before the child is born. However, she still receives 12 weeks of FMLA leave for the 12-month leave period. Reasons for leave also include placement of a son or daughter for adoption or foster care, to care for an immediate family member with a serious health condition, or medical leave due to an employee's own serious health condition.

[6] Lenhoff and Bell (2002)

[7] Employees are entitled to continue their health benefits while on leave. The employer must continue to pay whatever premiums it would pay if the employee were not on leave. If the employee voluntarily chooses not to return from leave, however, the employer may require the employee to repay the cost of the health care premiums it paid while the employee was on leave.

[8] Equivalent pay includes any bonsues or payments that occurred while the employee was on FMLA leave, if those payments were unconditional.

3 Model

This section provides a simple framework that formalizes the mechanism through which mandated maternity benefits can both increase employment of women with children and reduce the likelihood of promotions among women of childbearing age, even among women who never have children. The model makes clear that the decrease in the probability of promotions, conditional on employment, is generated by (i) the selection into the labor force of workers with higher marginal costs of labor and (ii) a decrease in the expected return on firm investment in low marginal cost types. The model highlights that in the presence of asymmetric information, mandated maternity leave benefits result in a change in the distribution of types of women who select into and are retained in the labor force, and the likelihood of promotion for young women decreases relative to even the symmetric information case. From this model, I derive a series of testable implications that I take to the data in Section 4.

3.1 Economic Environment and Decision Structure

I specify a two-period model in order to capture the phenomenon of workers' privately-known but anticipated increase in their future value of nonmarket time, in the presence of children. There are two types of agents, firms and workers. All are assumed to be risk neutral, and there is no discounting between periods. In Period 1, workers are identical in their marginal value of leisure, but in Period 2, they vary in their marginal value of leisure or nonmarket time, θ, which is unknown to the firm in the first period. I assume that there are three types of workers, A, B, and C, who have marginal values of leisure θ_A, θ_B, and θ_C, respectively, where $\theta_A > \theta_B > \theta_C$. The types are distributed with probabilities $P(A)$, $P(B)$, and $P(C)$, respectively, and the distribution from which these types are drawn is common knowledge. I begin by assuming that no worker has children in Period 1, and all workers have children in Period 2. Later, I relax these assumptions.

At the end of Period 1, firms must choose whether to undertake a training investment in each worker, of cost c to the firm, that generates an increase in the worker's firm-specific human capital. A worker who receives this investment has a greater Period 2 productivity. The decision to invest in a worker is denoted by τ ($\tau = 1$ if the firm invests and $\tau = 0$, if not). This training is complementary with the worker's labor supply in Period 2, h_2, in the firm's production function:

$$Y_2(h_2; \tau) = \alpha(\tau)h_2,$$

where $\alpha(\tau)$ represents the firm's choice of the worker's human capital, and $\alpha(1) > \alpha(0)$.

In Period 1, workers choose their labor supply, h_1, and receive a shock, ε, to their Period 1 productivity, y_1. Employers, however, observe only their productivity, a noisy signal of workers' choice of hours:

$$y_1 = h_1 + \varepsilon, \text{ where } \varepsilon \sim N\left(0, \sigma^2\right)$$

$$Y_1(h_1, \varepsilon) = \alpha(0) y_1$$

Because training is firm-specific, workers cannot fully internalize the cost of their training. Workers sharing the cost of training is also not sufficient to perfectly sort workers, due to incomplete information on the part of the firm about workers' abilities. Previous literature has established that imprecise information with respect to worker quality can function similarly to a credit constraint. In this literature, firms maximize profits by offering a uniform wage contract at the time of hire and paying for training, rather than offering training to workers who accept lower wages.[9] A discussion of how this model can be extended to more formally to include a wage rigidity due to imperfect information with respect to ability is offered in Appendix B. However, it is important to note that this implies that no worker can fully internalize the cost of the training.[10] Thus, firms form beliefs about workers' future productivity using their first period signal.

The salary contract for each period is prespecified such that firms and workers share the rent to human capital. The worker takes a share R_1 in Period 1 and R_2 in Period 2, and the firm takes shares $1 - R_1$ and $1 - R_2$ in Periods 1 and 2 respectively:

$$S_1(h_1; \varepsilon, R_1) = R_1 \alpha(0) \left(h_1 + \varepsilon\right)$$

$$S_2(h_2; \tau, R_2) = R_2 \alpha(\tau) h_2,$$

where $0 < R_1 < 1$ and $0 < R_2 < 1$. Firm-specific training creates a bilateral monopoly situation in wage determination, and R_2 is determined as a result of a bargaining arrangement. Since there is a match-specific surplus generated in Period 2, this surplus will have to be shared by bargaining. This typically implies that firms obtain a fraction of the productivity of the worker as profits.[11]

Labor force participation is determined by whether the worker's utility from working is greater than the

[9] Weiss (1980) demonstrates that when firms have imprecise information concerning the labor market endowment or ability of workers, and the labor market endowment is positively correlated with a worker's outside option, a wage rigidity exists. Reservation wages of workers are an increasing function of productivity, and workers will not be able to increase their probability of securing an employment contract by lowering their reservation wages. Acemoglu and Pischke (1998) demonstrates that asymmetric information with respect to worker ability can function similarly to credit constraints in that workers may not pay for their training. In this case, firms are willing to pay for training and willing to make an additional payment in terms of wages for ex-post monopsony power over workers who are revealed to be more able. In either case, imperfect information about workers' abilities keeps wages artificially high, and thus workers cannot compensate firms for training.

[10] Simply having a minimum wage constraint would impose an inefficient allocation of training only for workers where the minimum wage constraint is binding.

[11] See Hall and Lazear (1984) and Hashimoto and Yu (1980) for analyses of prespecified division of the rent to human capital. Hashimoto and Yu (1980) relies on firm-specific human capital, but Hall and Lazear (1984) uses firm-specific capital only as one example of a surplus generated by the firm-worker match. Both rely on some uncertainty with respect to some aspects of the value of that trade as well as the value of their alternatives and that renegotiation of the contract is costly. Other literature shows that the presence of matching and search costs in the labor market creates a bilateral monopoly, since it is both difficult for workers to find new employers as well as costly for firms to replace their employees. See Mortensen (1982), Diamond (1982) and Pissarides (1990) for analyses of the standard search and matching model. Bargaining, induced by a match-specific surplus, therefore compresses the wage structure.

utility of the outside option. Workers' utility from labor force participation in each period is based on their salary and their cost of supplying labor, which is increasing and convex in hours of work. The marginal cost of labor is dependent on their type. We assume that costs take a quadratic form, with $C_1(h_1) = \frac{1}{2}h_1^2$ and $C_2(h_2;\theta) = \frac{1}{2}\theta h_2^2$.

Notably, each worker has a fixed disutility of labor force participation in each period, of$\gamma(M)$ in period 2, where M is an indicator function for whether the mandated maternity leave policy is enacted, and normalized to 0 in period 1.[12] Thus, the utility of working in each period takes the following form:

$$U_1(h_1;\varepsilon,R_1) = R_1\alpha(0)\,(h_1+\varepsilon) - \frac{1}{2}h_1^2$$

$$U_2(h_2;\theta,\tau,R_2) = R_2\alpha(\tau)h_2 - \frac{1}{2}\theta h_2^2 - \gamma(M),$$

where $M \in \{0,1\}$. I characterize the requirement of the FMLA to hold a job open for a maternity-related absence as lowering the fixed utility cost of labor force participation in Period 2, when children are present, so that $0 < \gamma(1) < \gamma(0)$. This characterization captures the fact that having a maternity leave policy in place lowers the minimum requirements for a woman with a child to sustain a job with the same employer, by offering the option to take more time off for physical recovery, more flexibility within a 12-month period surrounding the birth of a child, or the ability to retain one's job while temporarily unable to work.[13]

This approach has been used in previous literature, including Mukhopadhyay (2012), which analyzes the effects of the 1978 Pregnancy Disrcrimination Act on female labor supply and characterizes the requirement to hold a job open for a pregnancy-related absence as a reduction in the utility cost of supplying labor around the period of childbirth. Although this model does not include an additional period for the time directly around childbirth, a decrease in the Period 2 participation cost is equivalent to a multiperiod model with a reduction in the utility cost of supplying labor around the period of childbirth (shown in Appendix B). Importantly, in a worker's decision about whether or not to remain employed, the worker considers not only the fixed disutility cost, but the total surplus from remaining employed with the same firm, including the wage increase from the firm-specific training. The total lifetime surplus of remaining employed with the same firm depends on the worker's marginal cost of working and their anticipated optimal choice of future labor supply. Therefore, the impact of a temporary reduction in the utility cost of working for new mothers on both employment and hours of work can be captured in a two-period model by a reduction in the Period 2 participation cost.

[12] We consider the impact of a mandated maternity leave policy only on those individuals who participate in the labor force in their early careers.

[13] For example, Ruhm (1998) examines mandated paid parental leave in nine European countries and finds that parental leave is associated with increases in women's employment. Waldfogel (1998) finds that maternity leave raises women's retention over the period of child-birth and allows female employees to retain good job matches. Several U. S. studies suggest that time off for a newborn child is associated with increases in employment and wages (Dalto 1989; SpalterRoth and Hartmann 1990; Waldfogel 1994, 1997).

All employees work in period 1 but remain in the labor force in Period 2 if and only if their second period utility from working is sufficiently high to compensate for the greater disutility of working in Period 2. The outside option in each period is normalized to 0.

To close the model, I also impose a free entry condition on firms in Period 1. Thus, no firm will earn positive profits in equilibrium.

The timing is as follows:

1. Workers decide how many hours to work in Period 1, h_1, knowing their Period 2 valuation of leisure, θ.
2. Workers draw a normally distributed shock to their productivity, ε. Worker productivity is observed by all participants at the end of Period 1.
3. Firms decide whether to undertake a training investment of cost c in each worker. If the firm invests, workers receive firm-specific human capital of $\alpha(1)$ in Period 2. If not, workers' human capital remains at its initial level, $\alpha(0)$.
4. Workers decide whether or not to participate in the labor market and how many hours to work in Period 2, h_2.

The objective is now to study how a reduction in the Period 2 participation cost of working impacts the Period 1 signaling strategies of workers and the likelihood of a promotion for a worker of a given type. In order to analyze this clearly, we assume the following restrictions on θ and $\gamma(M)$:

- $\theta_A > \frac{1}{2}\frac{R_2^2\alpha(1)^2}{\gamma(1)}$
- $\frac{1}{2}\frac{R_2^2\alpha(1)^2}{\gamma(0)} < \theta_B \leq \frac{1}{2}\frac{R_2^2\alpha(0)^2}{\gamma(1)}$
- $\theta_C \leq \frac{1}{2}\frac{R_2^2\alpha(0)^2}{\gamma(0)}$
- $0 < \gamma(1) < \left(\frac{\alpha(0)}{\alpha(1)}\right)^2 \gamma(0)$

3.2 Equilibrium Labor Supply and Firm Investment

Proposition 1 *The Perfect Bayesian Equilibrium of this game consists of a unique threshold, $\underline{y}^*$, and hours of work, $h_1^*(\theta; \underline{y}^*)$, such that all workers with productivity $y_1 \geq \underline{y}^*$ will be promoted and only those workers* *will be promoted.*

Proof. The game is solved by working backwards.

Worker's Period 2 Problem:

The maximization problem of the worker in Period 2, for each type, given the firm's training decision can be written as

$$\max_{h_2} R_2\alpha(\tau)h_2 - \frac{1}{2}\theta h_2^2$$

$$s.t.\ \ R_2\alpha(\tau)h_2 - \frac{1}{2}\theta h_2^2 \geq \gamma(M)$$

The solution to the second (and final) period problem is straightforward - workers optimally choose Period 2 labor supply as a function of their type and whether they received the firm-based investment.

$$h_2^*(\theta;\tau,M) = \begin{cases} \frac{R_2\alpha(\tau)}{\theta} & , \textit{if } \theta \leq \frac{1}{2}\frac{R_2^2\alpha(\tau)}{\gamma(M)} \\ 0 & , \textit{otherwise} \end{cases}$$

Workers with higher marginal costs of labor optimally choose fewer hours of work. Note that although the participation cost is the same for all types of workers, the second period participation cost of supplying labor selectively retains the workers who have the lowest *marginal* costs of working. This is simply due to the fact that the benefit of working is not large enough to compensate for the fixed cost of supplying labor when the marginal cost of working is high. Therefore, the types that anticipate optimally working fewer hours in the future do not participate in the labor force.

Note that Type C will always participate in Period 2, regardless of the maternity policy. Type A will never participate in Period 2. Type B is at the extensive margin of labor supply, incentivized to work in Period 2 only when the maternity leave policy is in place.

Firm's Problem:

Firms choose to promote workers if the expected profit to the firm from promoting a worker, conditional on their first period productivity, is greater than the cost. Without a maternity leave mandate, the optimal decision is to promote a worker who produces y_1 if and only if

$$\pi_C P(C|y_1) \geq c,$$

where $\pi_C = \frac{(1-R_2)R_2\left(\alpha(1)^2-\alpha(0)^2\right)}{\theta_C}$ is the profit to the firm from promoting a Type C worker, and $P(C|y_1)$ is the probability the firm assigns to a worker of being Type C, conditional on observing productivity level y_1 from the worker.

Worker's Period 1 Problem:

We can now characterize the worker's Period 1 decision, $h_1^*(\theta; \underline{y})$, in response to a firm threshold, $\underline{y}$. Only workers who anticipate staying in Period 2 benefit from receiving the training for a promotion. Because the Period 1 signal is noisy, however, no promotion is guaranteed; rather, working additional hours only increases the likelihood of meeting the firm's standard. The Period 1 maximization problem of the worker in Period 1 can be written as

$$\max_{h_1} R_1 a_0 h_1 - \frac{1}{2}h_1^2 + \left\{ A_\theta G\left(\underline{y} - h_1^*\right) + \frac{1}{2}\frac{R_2^2\alpha(0)^2}{\theta} \right\} I_{\theta \le \bar{\theta}_M},$$

where $A_\theta = \frac{1}{2}\frac{R_2^2\left(\alpha(1)^2 - \alpha(0)^2\right)}{\theta}$ is the is the expected increase in surplus from a promotion for a worker of type θ, and $\overline{\theta}_M \equiv \frac{1}{2}\frac{R_2^2\alpha(0)^2}{\gamma(M)}$ is the cutoff type that selects into employment in Period 2.[14] If no maternity mandate has been enacted at the time of hire, $h_1^*(\theta; \underline{y})$ solves the worker's Period 1 problem when

$$\begin{aligned} h_1^*(\theta_A; \underline{y}) &= R_1\alpha(0) \\ h_1^*(\theta_B; \underline{y}) &= R_1\alpha(0) \\ h_1^*(\theta_C; \underline{y}) &= R_1\alpha(0) + A_C g\left(\underline{y} - h_1^*(\theta_C; \underline{y})\right) \end{aligned}$$

Note that $h_1^*(\theta_C; \underline{y}) > h_1^*(\theta_A; \underline{y}) = h_1^*(\theta_B; \underline{y})$, since the second term is strictly positive. $\sigma > \underline{\sigma} = \frac{\sqrt{A_C}}{(2e\pi)^{1/4}}$ guarantees a unique solution.[15]

In words, workers take the firm's promotion standard as given and maximize utility by choosing their Period 1 labor supply. In Period 1, workers who anticipate leaving in Period 2 simply choose hours to maximize their first period utility. Workers who anticipate staying employed in Period 2, however, are willing to work more hours in Period 1 than required by the first-order condition for a static problem. In other words, only the Type C workers are willing to take on an additional cost in utility terms in order to convey their private information to the firm, since only these workers will stay in Period 2 . Although firms only observe the productivity of workers and do not perfectly observe their choice of hours, workers who are committed to the labor force in Period 2 can increase the likelihood of the firm recognizing their anticipated Period 2 participation decision by working more in Period 1. Thus, Type C workers, who plan on staying, are

[14] The Period 1 problem is simplified because of the choice of three types. The problem for continuous types can be written as $\max_{h_1} R_1\alpha(0)h_1 - \frac{1}{2}h_1^2 + E\left[\left(\frac{1}{2}\frac{R_2^2\left(\alpha(1)^2-\alpha(0)^2\right)}{\theta}\right) I_{\theta\le\bar{\theta}_{M,1}} \mid y_1 \ge \underline{y}\right] + E\left[\left(\frac{1}{2}\frac{R_2^2}{\theta}\alpha(0)^2\right) I_{\theta\le\bar{\theta}_{M,0}} \mid y_1 < \underline{y}\right]$, but we use three types here for ease of exposition.

[15] $\sigma > \underline{\sigma}$ ensures that the second order condition holds everywhere. When $\sigma \le \underline{\sigma}$, a unique solution is still guaranteed for $\underline{y} < R_1\alpha(0) + \frac{A}{\sigma\sqrt{2e\pi}}$. As is shown in the Appendix, the equilibrium threshold must be such that $\underline{y} < R_1\alpha(0) + \frac{A}{\sigma\sqrt{2\pi}}$, so only a small range of $\underline{y}$ exists where there may be more than one solution to the first order condition. However, one of these solutions is always a maximum, even for low $\sigma \le \underline{\sigma}$. The idea is that when the variance of the signal is sufficiently low, intermediate choices of hours may actually be a local minimum, and the best choice may be to aim far past the others, since each additional hour more clearly separates Type C's from B's to the firm. For a production function where a promotion generates an increase in salary of \$100 per week, the minimum required standard deviation in observed weekly productivity for a unique solution is 3.47. This is quite reasonable, considering even just the measurement error in observed hours.

partially separated from the Type B and Type A workers, who anticipate leaving in Period 2.

Equilibrium Threshold:

We now show that, given the optimal Period 1 strategy of workers, the firm's optimal decision is to choose a threshold for productivity, above which it will promote workers and below which it will not, based on its ability to distinguish Type C workers from Type B. Let $\underline{y}^*$ be such that

$$\pi_C P(C|\underline{y}^*) = c$$

If $\underline{y}^*$ is the promotion standard, the probability of a worker of Type C producing at $y_1 > \underline{y}^*$ is $g\left(y_1 - h_1^*(\theta_C; \underline{y}^*)\right)$. Hence,

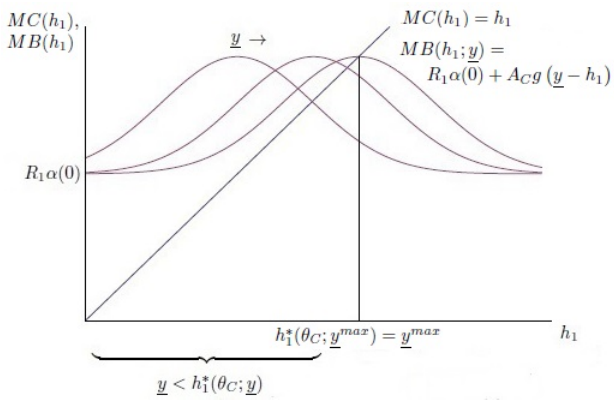
$$P(C|y_1) = \frac{g\left(y_1 - h_1^*(\theta_C; \underline{y}^*)\right) P(C)}{g\left(y_1 - h_1^*(\theta_C; \underline{y}^*)\right) P(C) + g\left(y_1 - h_1^*(\theta_B; \underline{y}^*)\right) (P(B) + P(A))}$$

is increasing in y_1, since $h_1^*(\theta_C; \underline{y}^*) > h_1^*(\theta_B; \underline{y}^*)$ implies that the ratio $\frac{g\left(y_1 - h_1^*(\theta_B; \underline{y}^*)\right)}{g\left(y_1 - h_1^*(\theta_C; \underline{y}^*)\right)}$ is decreasing in y_1 . Therefore, all workers who produce $y_1 \geq \underline{y}^*$ will also be promoted. All workers who produce $y_1 < \underline{y}^*$ will not be promoted for the same reason.

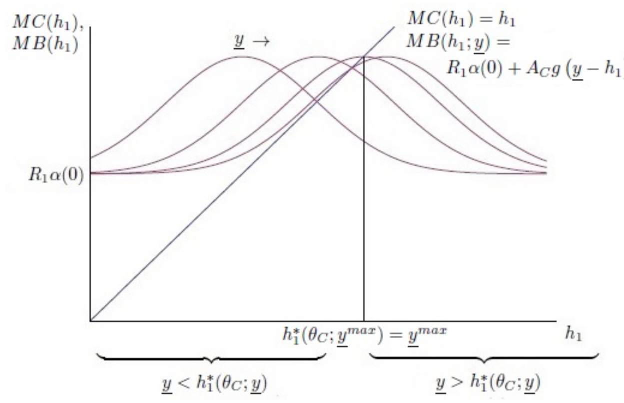
The proof of the existence and uniqueness of such the equilibrium is shown in Appendix A. There, I establish that such a threshold $\underline{y}^*$ exists and is unique as long as the cost of firm training, c, is neither so low nor so high that information about a worker's type is uninformative to the firm's decision. When firm costs are specified such that the signal is informative to the firm's decision, the equilibrium value of $\underline{y}$ falls within a range, $\left(\underline{y}^{min}, \underline{y}^{max}\right)$, specified in Appendix A. The quantity $P(C|\underline{y})$ is strictly increasing in $\underline{y}$ for the feasible range of equilibrium values of $\underline{y}$. Moreover, within the range of equilibrium values of $\underline{y}$ guaranteed by firm costs, $\left(\underline{y}^{min}, \underline{y}^{max}\right)$, $\pi_C P(C|\underline{y}^{min}) < c$ and $\pi_C P(C|\underline{y}^{max}) > c$.

Figure 1 below shows the left- and right-hand sides of the worker's Period 1 first-order condition, for a given firm threshold, $\underline{y}$. The intersection of the two curves indicates the Type C worker's optimal choice of hours for a given $\underline{y}$. As $\underline{y}$ increases, the marginal benefit curve is shifted to the right. The figures show that the worker's Period 1 signal is increasing in the threshold $\underline{y}$, for $\underline{y} < \underline{y}^{max}$, with the maximum number of hours obtained from a Type C worker at $\underline{y} = \underline{y}^{max}$. Figure 2 illustrates the probability distribution of observed productivities for Type C workers shifts to the right as the firm threshold increases, from some $\underline{y}^0 < \underline{y}^{max}$ to $\underline{y}^{max}$, with the maximum difference between the means of the Type B and Type C signals obtained at $\underline{y} = \underline{y}^{max}$.

Figure 1: Type C Worker's Best Response to Promotion Standard $\underline{y}$

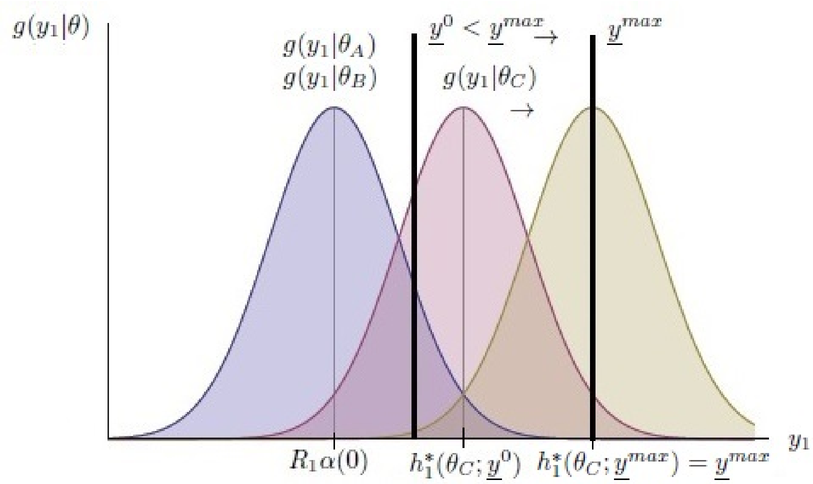


(a) Solution to First-Order Condition for $\underline{y} < \underline{y}^{max}$



(b) Solution to First-Order Condition for $\underline{y} > \underline{y}^{max}$

Figure 2: Response of Distribution of Observed Worker Productivity to Promotion Standard Shift from $\underline{y}^0$ to $\underline{y}^{max}$



Proposition 2 *If a mandate is enacted that decreases the Period 2 participation cost from $\gamma(0)$ to $\gamma(1)$, the Perfect Bayesian Equilibrium of this game consists of a threshold , $\underline{y}^M$, and hours of work, $h_1^M(\theta; \underline{y}^M)$, such that all workers with productivity $y_1 \geq \underline{y}^M$ will be promoted and only those workers will be promoted. For firms with a sufficiently high cost of training, $c > \underline{c}$, $\underline{y}^M > \underline{y}^*$.*

Proof.

The worker's Period 1 and 2 maximization problems remain the same, for a given firm threshold. However, Type B workers now anticipate staying in Period 2, rather than leaving. Therefore, $h_1^M(\theta; \underline{y})$ solves the worker's Period 1 maximzation problem when:

$$\begin{aligned}
h_1^M(\theta_A; \underline{y}) &= R_1\alpha(0) \\
h_1^M(\theta_B; \underline{y}) &= R_1\alpha(0) + A_B g\left(\underline{y} - h_1^M(\theta_B; \underline{y})\right) \\
h_1^M(\theta_C; \underline{y}) &= R_1\alpha(0) + A_C g\left(\underline{y} - h_1^M(\theta_C; \underline{y})\right)
\end{aligned}$$

Note that $h_1^M(\theta_C; \underline{y}) > h_1^M(\theta_B; \underline{y}) = h_1^M(\theta_A; \underline{y})$, since $A_C > A_B$.

Because of the reduced participation cost to working, Type B workers act more similarly to the Type C workers, rather than to the Type As. Now, not only the Type C workers but also the Type B workers are willing to take on an additional cost in utility terms in order to convey their private information to the firm, since both types of workers will stay in Period 2 . Type C workers are willing to take on a more costly signal than Type Bs because of the complementarity between training and Period 2 hours in their salaries. However, the signal is noisy, and the mean of the Type B workers' Period 1 productivities is now closer to the mean of the Type Cs. By Bayes' Rule, for any given signal, the employer now assigns a higher probability of being a Type B relative to a Type C.

Firm's Problem Under the Mandate:

Given the optimal Period 1 strategy of workers, the firm chooses a threshold for productivity, above which it will promote workers and below which it will not, based now on its reduced ability to distinguish Type C workers from Type B. With a maternity leave mandate in place, the firm promotes a worker who produces y_1 if and only if

$$\pi_C P^M(C|y_1) + \pi_B P^M(B|y_1) \geq c,$$

where $\pi_B = \frac{(1-R_2)R_2\left(\alpha(1)^2 - \alpha(0)^2\right)}{\theta_B}$ is the profit to the firm from promoting a Type B worker, and P^M denotes the probability of a worker being a given type if hired after the mandate.

Let $\underline{y}^M$ be such that

$$\pi_C P^M(C|\underline{y}^M) + \pi_B P^M(B|\underline{y}^M) = c$$

If $\underline{y}^M$ is the promotion standard, then the expected profit from promoting a worker who produces a signal y_1, $\pi_C P^M(C|y_1) + \pi_B P^M(B|y_1)$, is increasing in y_1, since the ratio $\frac{g\left(y_1 - h_1^M(\theta';\underline{y}^M)\right)}{g\left(y_1 - h_1^M(\theta;\underline{y}^M)\right)}$is decreasing in y_1 for $\theta' > \theta$.[16] Therefore, the expected profit of awarding such a worker a promotion is greater than c for all $y_1 > \underline{y}^M$, and all workers who produce a signal $y_1 > \underline{y}^M$ are promoted. For the same reason, those who produce $y_1 < \underline{y}^M$ are not promoted.

For a worker who signals the pre-mandate threshold, $\underline{y}^*$, *after* the enactment of the mandate, $\pi_C P^M(C|\underline{y}^*) + \pi_B P^M(B|\underline{y}^*) < c$ since $P^M(C|\underline{y}^*) < P^M(C|\underline{y}^*)$, $P^M(B|\underline{y}^*) < P(C|\underline{y}^*)$ when $c > \underline{c}$, and $\pi_B < \pi_C$. $\pi_C P(C|\underline{y}) + \pi_B P(B|\underline{y})$ is increasing in $\underline{y}$ for all $\underline{y}$ in the feasible range of equilibrium thresholds (a range guaranteed by the firm costs, as specified in Appendix A). Therefore, $\underline{y}^M > \underline{y}^*$. The minimum firm cost, $\underline{c}$, and supporting details are found in Appendix A.

Because workers produce a noisy signal, a Type C is more difficult to distinguish from a Type B if hired after the mandate. The Type B workers have something to gain from a promotion and are now willing to "vie" for a promotion. Thus, the return to the signal, in terms of the probability of a promotion, decreases for workers hired after the enactment of the maternity mandate. In equilibrium, Type Cs will have to work harder in Period 1 in order to distinguish themselves from the Type Bs, but they will not be willing to increase their labor supply sufficiently to maintain the same probability of promotion as they received before the mandate.

Figures 3 and 4 below graph the probability distributions of the observed productivities of each type of worker on the same axis. Figure 3 illustrates how the incentive for Type B workers to increase their likelihood of obtaining a promotion reduces the difference in the means of the Type B and C workers' distributions, contaminating the Type C signal. The supporting details can be found in Appendix A.

[16] To see this, note that $\pi_C P(C|y_1) + \pi_B P(B|y_1) = [\pi_C P(C|y_1, B \text{or } C) + \pi_B P(B|y_1,\ B \text{ or } C)]\, P(B \text{ or } C|y_1)$. $P(B \text{ or } C|y_1)$ is increasing in y_1, $P(C|y_1, B\text{or } C)$ is increasing in $y_1$and $P(B|y_1, B\text{or } C)$ is increasing in y_1, since the ratio $\frac{g\left(y_1 - h_1^*(\theta';\underline{y})\right)}{g\left(y_1 - h_1^*(\theta;\underline{y})\right)}$is decreasing in y_1 for $\theta' > \theta$.

Figure 3: Response of Equilibrium Threshold to Change in Signaling Strategy of Type B Workers After Enactment of Mandated Leave

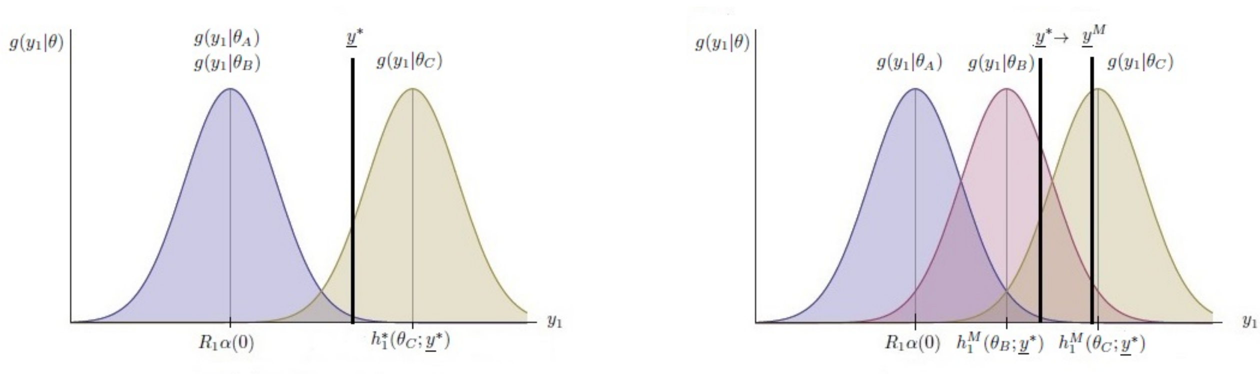
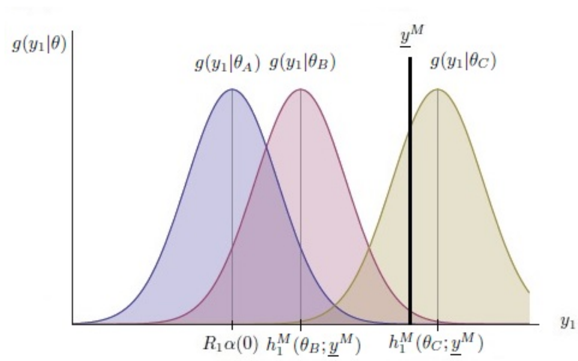


Figure 4: Equilibrium Strategies and Promotion Standard After Enactment of Mandate



We now consider the possibility that rms have the option to provide maternity benets to their workers, even without a government-mandated policy.

Proposition 3 *If firms can choose to take on a cost of providing a benefit to reduce participation costs from* $\gamma(0)$ *to* $\gamma(1)$, *the equilibrium salaries for workers who are hired prior to the mandate and work* $h_1^*(\theta;\underline{y}^*)$ *is* s^*, *and the equilibrium salaries for workers hired after the mandate and work* $h_1^*(\theta;\underline{y}^M)$ *is* $s^M < s^*$.

Proof. See Appendix A.

3.3 Testable Implications

The model makes predictions about the effect of a worker being hired under a mandated maternity leave policy on the likelihood of promotions, employment, labor supply, wages. In addition, the model makes predictions about the return to a wide range of signals about a worker's type, including measures of effort and performance and the worker's choice of a wage contract.

The first prediction characterizes the likelihood of promotions. Because the probability of being a Type C worker is increasing in the signal within the feasible range of equilibrium signals, the employer must raise the standard for promotions in order to invest in workers and promote profitably, as shown in Proposition 2. The likelihood of receiving a promotion, conditional on being employed in Period 2 then decreases for two reasons. First, the selection into the labor force of Type B workers in Period 2, who are less likely to produce a high signal than Type C workers, means the probability of having received a promotion decreases simply by the retention of workers whose optimal strategy is to work fewer hours in Period 1. The first cause is due solely to the retention of workers who have higher marginal valuations of leisure than those who remained employed in Period 2 before the mandate, and these workers are less likely than Type C workers to produce a signal above the equilibrium threshold.

A potential concern is that this prediction alone would also be consistent with a model of symmetric information. However, the asymmetric information model presented here predicts a lower likelihood of promotions even for Type C workers, or the workers with the lowest marginal costs of supplying labor. The reason is that the model predicts the contamination of the Type C workers' signals by the Type B's increased incentive to obtain a promotion. The equilibrium strategy, when hired after the mandate, is such that the fraction of Type C workers who meet the higher equilibrium promotion standard is smaller.[17]

T1. Women of childbearing age hired after the enactment of the mandated maternity leave policy will be less likely to be promoted, conditional on job tenure, including those women with the lowest marginal costs of supplying labor.

T2. Women of childbearing age will have higher employment rates after the mandated maternity leave policy is in place.

T3. Women of childbearing age hired after the mandate will have lower late career labor supply.

[17] Note that when $\underline{y} < \underline{y}^{max}$, $\left|\underline{y} - h_1^*(\theta_C; \underline{y})\right|$ is decreasing in $\underline{y}$ and $\underline{y} < h_1^*(\theta_C; \underline{y})$, so $1 - G(\underline{y} - h_1^*(\theta_C; \underline{y}))$ is decreasing in $\underline{y}$. Also, $h_1^*(\theta_B; \underline{y}^M) < h_1^*(\theta_B; \underline{y}^*) < h_1^*(\theta_C; \underline{y}^*)$, so $1 - G(\underline{y} - h_1^*(\theta_B; \underline{y}^M)) < 1 - G(\underline{y} - h_1^*(\theta_C; \underline{y}^*))$.

The average number of hours in Period 2 is $\frac{R_2\alpha(\tau)}{\theta_C}$ for workers hired before the mandate, and for those hired after the mandate, the model predicts that the retention of higher marginal cost types in Period 2 leads to fewer hours of work, conditional on employment: $\frac{R_2\alpha(\tau)}{P(B)+P(C)}\left(\frac{P(C)}{\theta_C}+\frac{P(B)}{\theta_B}\right)<\frac{R_2\alpha(\tau)}{\theta_C}$. Note that the number of Period 2 hours is predicted to decrease both conditional on whether the worker received a promotion and unconditionally.

T4. Women of childbearing age hired after the mandate will have a lower return to their first period signals, in terms of the likelihood of a promotion.

This is shown in Proposition 2. For each signal, the firm's expected profit from promoting a worker is lower for those hired after the mandate, since the probability of being a Type C for a given signal is lower and $\pi_B < \pi_C$. Therefore, workers are less likely to get promoted for a given first period performance observed by the firm.

T5. Women of childbearing age hired after the mandate will receive a reduced value of wages and fringe benefits.

T6. The effect of the mandate on those hired after its enactment will have larger effects on firms whose cost of training is high.

4 Data and Empirical Analysis of the Reforms

4.1 The data

This paper uses data from both the Panel Study of Income Dynamics (PSID) and the Multi-City Study of Urban Inequality (MCSUI). The PSID provides a long panel on individuals' labor market and childbirth histories for overlapping cohorts. The data are taken from the Individual File, the Family File, and the Childbirth and Adoption History File. The sample contains individuals who were classified as either the *Head* or *Wife* of a household in the year of the interview. I only keep individuals between the ages of 16 and 65 in the sample. After keeping only those individuals with at least one year of labor force participation and employment status data, I have a sample of 14,635 individuals surveyed between the years of 1988 and 2001, of which 55% are women.

The PSID contains key variables that together identify whether a respondent has been promoted since the time of hire. In each year from 1988 onward, respondents were asked when they had started working for their current employer. If the respondent had begun working for his current employer in the year prior to the survey or earlier, he was also asked whether his current position had changed in the previous year. The possible categories for the response to this last question include promotion with higher pay, a major change in duties but with the same pay, other, and unknown. These questions allow one to determine whether a promotion has taken place within a firm, rather than simply a wage increase within the same position or a lateral position change that does not result in wage or salary growth, and at what tenure level the promotion occurred.

The data is then partitioned into employment spells using the method Brown and Light (1992) suggests is most accurate. An individual is assumed to have started a new spell with an employer when the reported starting date is after the date of the last survey.[18] Tenure for each survey year is determined by using the time elapsed between the reported date of hire and the survey date. [19]An individual is then categorized as having been promoted since the time of hire if they have been promoted in the previous year at least once within the employment spell. Because we can only determine from the survey questions whether respondents have been promoted in the previous year, we lack promotion information on those individuals who were hired before 1987. Therefore, whether the respondent has been promoted since the time of hire is categorized as missing if the respondent was hired before 1987. Some promotions will not be identified, if the respondent is both promoted and begins a new employment spell in less than one year. This is addressed in the empirical analysis. [20]

This cumulative incidence of a promotion is used, as opposed to the promotion hazard rate because of the concern of unobserved heterogeneity. The primary predictable effect of the presence of unobserved heterogeneity is that the results will be biased toward finding a declining hazard rate with respect to job tenure. Those with a relatively high probability of being promoted due to characteristics unobserved to the econometrician will be promoted out of the risk pool more quickly than those with low probabilities of being promoted. Because the goal is to examine changes in gender differences in promotions, unobserved heterogeneity could potentially bias downward or even reverse the sign of the coefficient of interest. For example, if unobserved characteristics (unobservable to the econometrician, but observable to the employer) contributed more to the

[18]If the exact date of hire is missing, an individual is assumed to have started a new spell when the reported starting year is greater than or equal to the year of the last survey.

[19]Brown and Light (1992) has noted that internal inconsistency in tenure within employment spells can be a concern in the PSID. First, misreporting of tenure data is not as problematic for the survey years used here, 1988-2001, compared to the earlier years used in Brown and Light (1992). From 1988 onward, the PSID asks for the date of hire with the current employer, rather than the length of time for which the respondent has been employed with his current employer. While Brown and Light examine the correlation between the time elapsed between surveys and the reported tenure to determine consistency, the data here only requires that the reported start dates within an employment spell are consistent. I address reporting errors in start dates by by using the date of hire reported in the previous survey year if the date of hire differs within an employment spell for only one survey year.

[20]The data used here does not suffer from the same degree of underidentification of position changes as in McCue (1996) because we only use data from the 1988-2001 survey years. Prior to 1984, for an observation to be included in the promotion category, the respondent must have both reported positional tenure below some bound and then have indicated that he had changed position because he was promoted. From 1988 onward, the sequence of questions was revised. When the respondent reports having been in the current position for less than one year, he is then asked whether the position changed at any time in the previous year. Therefore, all changes from the calendar year prior to the survey are captured, as long as the employed respondents remain with this employer.

likelihood of promotion for men than for women, the negative duration dependence of the hazard rate would be overestimated more for men than for women. In other words, the hazard rate would be biased toward declining more steeply for men than for women. In an extreme case, where observable characteristics were the only factors affecting the employer's promotion decision for women, but unobservable characteristics are taken into account for men, such as in a case of statistical discrimination against women on the basis of observable characteristics, it might, in fact, appear that women have a higher probability of being promoted than men within a given period, especially at high levels of tenure. Because the differential use of observable characteristics in a promotion decision is central to the context addressed in the model, the cumulative incidence of promotion is better able to capture the effect of interest. There may still be a bias that the cumulative incidence of promotion is increasing less steeply for men than for women, but the measure that is relevant is the relative likelihood of promotion for a given job tenure is affected by the policy change. An effect of the policy change on discrimination and the use of information may not be captured by the hazard rate, while it may be captured by the cumulative incidence, since the sign of that effect will not be affected by the bias.

The second dataset I use, the Multi-City Study of Urban Inequality, is a cross-sectional survey designed to broaden understanding of labor market dynamics and comprises data for two surveys: a survey of households and a survey of employers. The employer survey records detailed information from 3510 firms in four major metropolitan areas: Boston, Chicago, Atlanta, and Los Angeles. In particular, the survey data contains a substantial number of questions about each firm's most recently hired worker, an these questions form the basis for the empirical analysis. Several unique features of the data are important to the analysis. First, the dataset contains a wide variety of firm characteristics in addition to standard information on occupation and industry, enabling us to control for these variables while still relying on a broad, representative sample, as opposed to a single-firm or a narrowly-defined population. For example, the survey includes an indicator for nonprofit status. The data shows that women are more heavily represented in the nonprofit than in the for-profit sector; the fraction working in the for-profit sector is 74 percent for women versus 87 percent for men. Recent empirical work by DeVaro and Samuelson (2005) documents a pronounced difference in promotion rates between for-profit and nonprofit organizations, with promotions less likely in nonprofits. These considerations suggest that nonprofit status should be controlled in analyses of gender differences in promotion rates. In addition to nonprofit status, I control for industry, establishment size, number of sites of operation, whether or not the firm is a franchise, and the percentage of workers covered by collective bargaining agreements. While the PSID is a nationally-representative panel with detailed worker characteristics, it is thin on firm characteristics. Second, because the main dataset used is information asked of the employer, all worker characteristics are based on employer knowledge or perceived knowledge, which is more relevant for an examination of employer expectations than the actual worker characteristics, such as age. Furthermore, the data includes include job-specific worker performance ratings, allowing us to control for performance more precisely than commonly-used skill indicators such as educational attainment or tenure would allow. Importantly, it also contains employer-based performance evaluations of all workers, so the performance measure used in the analysis is normalized based on the distribution of employer ratings. Therefore, we can also view performance as a costly signal received by the employer. The dataset also contains detailed information not only on wages, but on a variety of fringe benefits provided to employees. Lastly, we have detailed information on firm practices, and in particular, a measure of the cost of firm training, both formal and informal training.

As an additional resource, the MCSUI data also contain a number of variables measuring wages and the wage growth attached to promotions. Four variables pertain to the wages of the most recently hired worker: starting wage, current wage at the time of the survey, wage the employee is expected to receive if promoted, and highest wage an employee could attain without a promotion in the same position as the most recently hired employee. These variables are useful for examining whether the definition of a promotion has simply changed over time and what effect a promotion has on wage growth.

After keeping in the dataset only firms whose last hire was aged 16 to 65, I am left with a sample of 3,510 firms, of which 1,774 firms' most recent employee hired was female. The majority of firms indicated that their most recent employee hired was hired in 1993 (48.12% of firms hired their most recently hired employee in 1993) , so this dataset is particularly useful because most of the variation in whether the employee was hired before or after the enactment of the FMLA has little variation in the year of hire, but rather, the information set of the employer.

4.2 Empirical Analysis

The Family and Medical Leave Act provides the distinct opportunity to study the impact of increasing access to job-protected leave on the employment, human capital accumulation, labor supply, and wages of workers whose labor force participation is most likely affected by the job-protected leave. Using the PSID, I exploit variation in state legislation on family and medical leave prior to and since the 1993 federal legislation on family leave. The variation in state legislation is summarized in Appendix Table 1. The U.S. Department of Labor (DOL) provides a comparison of the FMLA and the family and medical leave legislation of 11 states and the District of Columbia.[21] In addition to the 11 states and the District of Columbia, I consider the state legislation passed by Massachusetts, Montana, North Carolina, and Tennessee, as considered in the legislative dataset constructed by Stutts and Heiland (2006), since these states also passed legislation on family and medical leave prior to and since the federal mandate. There is some amount of variation in the exact terms of the legislation for each state.[22] I focus on the impact of increasing access to job-protected leave for

[21] See http://www.dol.gov/esa/programs/whd/state/fmla/index.htm. States included (as of 25 February 2006) are California, Connecticut, Hawaii, Maine, Minnesota, New Jersey, Oregon, Rhode Island, Vermont, Washington, Wisconsin, and the District of Columbia.

[22] The DOL breaks down the FMLA into 14 elements, including the types of employers covered by the legislation, employee eligibility, leave amount, type of leave allowed, whether there is a key employee exception, whether health benefits are maintained during leave, and whether medical certification is required.

the birth and care of a newborn child on workers hired under this regime. I define states as nonexperimental or “control” states if state legislation before August of 1993 required job-protected leave for private sector employees for the birth and care of a newborn child, for at least eight weeks.[23] I consider those that did not to be “treatment” states. These states vary in their geographic region and are broadly representative of the country as a whole, as shown in Table 2. Observations in a few states updated their laws after the enactment of the FMLA to become significantly more expansive than the federal law. Because these states varied greatly in what aspects of their legislation was designed to surpass the requirements of the federal law, observations in the years in which state legislation expanded beyond the federal requirements are insufficient to provide accurate data for such a quasi-experiment; thus, they will not be used for causal interpretation.[24]

Testable Implication T1: Likelihood of Promotions

To examine the impact of the being hired after the enactment of the Family and Medical Leave Act on the likelihood of promotion, I estimate the following equation for an individual i employed with firm j, in state s, and in year t:

$$\begin{aligned} promoted_{i,j,s,t} &= \beta_1(HiredAfter_{i,j} \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(HiredAfter_{i,j} \cdot TreatmentState_s) \\ &+ \beta_4(HiredAfter_{i,j} \cdot FemaleUnder40_{i,t}) + \beta_5 HiredAfter_{i,j} + \beta_6 FemaleUnder40_{i,t} \\ &+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \delta_{i,t,s} + \varepsilon_{i,j,s,t} \end{aligned} \tag{1}$$

The vector X contains a set of controls for sex, age, age squared, tenure, tenure squared, level of education attained, marital status, marital status interacted with sex, marital status interacted with FemaleUnder40, and a dummy for nonwhite. The vector Z contains a set of controls for the characteristics of the firm and the

[23] Hawaii is considered a treatment state because its state law would not apply to private sector employees until January 1, 1994, after the FMLA was passed. Montana is considered a treatment state because its law, passed in 1975, much more closely mirrored the Pregnancy Discrimination Act of 1979, by making it illegal to terminate a woman’s employment because she was pregnant and required that employers allow the women on maternity leave any compensation or other benefits for which they would have been eligible under any other form of disability leave. After the Pregnancy Discrimination Act of 1979 amended Title VII of the Civil Rights Act of 1964, pregnancy became a protected class under discrimination law, and discrimination in employment on the basis of pregnancy was prohibited by law in every state. This amendment included the requirement to provide disability benefits to women "disabled by pregnancy" the same amount of compensation and for the same length of time as they provide for any other type of disability.While Montana’s 1975 legislation also mandated that employers allow women “a reasonable leave of absence” for pregnancy, the terms and length of this leave were not specified and the exact terms were left to the discretion of the employer and employee. Therefore, I do not include it in the group of states that mandated job-protected leave for the birth and care of a newborn child.

[24] The North Carolina General Assembly, for example, passed two acts in relation to family leave, one, in 1993 and, the other, in 1997. Unlike most states being examined in this paper, North Carolina’s original 1993 legislation did not mirror or even resemble the federal FMLA. North Carolina’s Chapter 509, Section 1 (sections 2 and 3 of Chapter 509 addressed issues outside of family leave) allowed parents in the State to take protected leave from work to be involved with their child’s school activities. Other states whose legislation eventually became significantly more progressive than the federal legislation were Maine, in 1997, New Jersey, in 1995, and Oregon, in 1995.

job. λ_t denotes year fixed effects, c_s, state fixed effects, and f_i, individual fixed effects. $\delta_{i,t,s}$ denotes a set of state-specific year fixed effects and year fixed effects specific to the demographic group of interest, women under the age of 40. The dependent variable *promoted* is constructed to represent whether the respondent has ever been promoted in the duration of the employment spell. I consider the likelihood of promotion for respondents hired under two different maternity leave regimes. *HiredAfter* is a dummy variable for whether the respondent was hired after August of 1993, when the FMLA was enacted. *TreatmentState* is a dummy variable equal to 1 for the states that did not already have state legislated job-protected parental leave.

The coefficient β_1 captures the effect of interest. It captures the effect of being hired after the enactment of the FMLA on the likelihood of promotion for women under the age of 40, relative to men and women over the age of 40, in the treatment states, relative to the control states, the states that had already mandated some form of job-protected leave. β_7 captures the time-invariant characteristics of the experimental states, β_6 captures the existing differential in the likelihood of promotion for a woman under the age of 40,[25] β_5 controls for the changes over time in the likelihood of promotions for contracts and employment spells initiated at later dates, β_4 captures the changes over time for women under the age of 40, in particular, β_3, changes over time in likelihood of promotions specific to the experimental states, and β_2 captures the time-invariant likelihood of promotion for women under the age of 40 specific to the experimental states.

The first row of Table 1 presents the estimates of the third-level interaction from Equation 1, β_1, using fixed-effect OLS regressions for different specifications. Column 1 is the baseline specification, which includes the set of controls contained in vector X as well as year fixed-effects, state fixed-effect and individual fixed-effects. Column 2 includes characteristics of the employment position: whether the respondent works for a private, non-goverment company and whether the job is covered by a union contract. Columns 3 through 5 include detailed occupation and industry controls. Column 3 controls for 12 occupational categories using the 12 standard occupational divisions used in the 1970 census occupational classification system. The occupations in this system are organized into several large groupings of roughly descending socioeconomic status. Column 4 uses a different division of occupations, with 12 occupational categories, as described earlier, and divisions made in order to capture potential selection into more child-friendly occupations, based on a literature that measures the rates of human capital depreciation and wage growth across occupations and how these choices are affected by planned fertility.[26] Column 5 uses an even finer division of occupational categories, controlling for 24 standard divisions, as used in the census occupational classification system, as well as 8 standard industry level divisions. The coefficient of interest, β_1, responds little to the changes in division and the increasing fineness of the division of occupational categories.

[25] Note that with individual fixed effects, when β_6 is negative, this captures the increase in the likelihood of promotions for the same female individual when she reaches the age of 40.

[26] See, in particular Adda, Dustmann, and Stevens (2011)

Appendix C presents a set of robustness checks on these results. The results shown in Figure 13 demonstrate that while women under the age of 40 face a lower likelihood of promotion when hired after the enactment of the FMLA, women over the age of 40 are unaffected in comparison to men. I also show that women under the age of 40 are unaffected by the policy change in control states. Furthermore, while table 1 compared women under the age of 40 to a control group comprised of men and women over the age of 40, I show in Tables 11 and 12 that using either of two control additional groups yields similar results: (i) men under the age of 40 only or (ii) women over the age of 40 only. Appendix Tables 15-17 include linear time trends to confirm that differential trends in the year of hire do not drive the results. The potential concern is that if treatment states already had more steeply declining promotion rates for young women relative to men, with respect to the date hired, than did control states, then the estimates from equation (1) would pick up a pre-existing trend. I show that the results are robust to the inclusions of state-specific, demographic-specific, and state-demographic-specific linear time trends in the year of hire. While women under the age of 40 do witness a slight increase in the likelihood of being promoted over time relative to men, this trend exists for all women and is not specific to women under the age of 40. Thus, the time trends reveal that the enactment of the FMLA retards the progress in the growth over time of women's likelihood of promotion relative to men, for women of childbearing age. The inclusion of time trends has little effect on the estimates when the control group is made up exclusively of women over the age of 40. Therefore, there is no evidence of a systematic bias from omitted factors that affect fertile women differentially over time in treatment states.

However, omitting state- or demographic-specific factors that influence the likelihood of promotions over time only bias the estimated effect of being hired after the FMLA if there is a systematic relationship between the trend in promotion rates and the adoption of state legislation on family and medical leave. Inclusion of state-demographic-specific trends may be too restrictive a specification, if there is a dynamic component to the effects of the policy, since the year trend may control for exactly the effect we want to observe[27]. Therefore, I also examine individual year-treatment effects, as opposed to year trends, in order to observe dynamic component to the treatment effect. Figure 5 shows the results of year-treatment coefficients and the 95 percent confidence intervals of the estimated coefficients. The estimated effect is relative to the those hired in the pre-FMLA 1993 period (January - July 1993). The figure also shows the estimates of year-treatment coefficients when the control group consists of men under the age of 40 only or older women only, and the results are robust to the choice of control group.

Next, I exploit the variation in fecundity for women by age and the large decrease in fecundity for women at age 40 in examining whether the gender gap in promotions was widened for women with the highest likelihood of having children. Figure 6 shows the estimates of age-treatment coefficients and the 95 percent confidence intervals of the estimated coefficients for the change in the likelihood of promotions for women

[27] This argument was made by Wolfers (2006) in examining the dynamic effect of the introduction of unilateral divorce laws on divorce rates

relative to men. For each of the 5-year age bins, being hired after the FMLA widens the gender gap in promotions, but only for those women of childbearing age. Among each of the older age groups, women hired after the FMLA face no significant effect in their likelihood of promotion. Panels (a) and (b) of Figure 6 demonstrate that the effect on each group of younger women is significant and negative in treatment states, but there is no significant effect on any of the groups of younger women in control states.

Finally, the federal FMLA requires employees to have worked more than 1,250 hours over the past 12 months before becoming eligible. An additional estimation exploits the variation in eligibility for the FMLA across jobs, and, therefore, the variation in likely future eligibility for the FMLA. [28]

$$\begin{aligned} promoted_{i,j,s,t} &= \beta_1(HiredAfter_{i,j} \cdot EligFemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(EligFemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(HiredAfter_{i,j} \cdot TreatmentState_s) \\ &+ \beta_4(HiredAfter_{i,j} \cdot EligFemaleUnder40_{i,t}) + \beta_5 EligFemaleUnder40_{i,t} \\ &+ \beta_6 HiredAfter_{i,j} + \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \delta_{i,t,s} + \varepsilon_{i,j,s,t} \end{aligned} \tag{2}$$

The estimates of the Equation (2), using fixed-effect OLS regressions with the specifications described earlier, are presented in Table 2.

One potential concern is that the passage of the FMLA not only had an effect on those hired after the mandate through adverse selection (unobserved heterogeneity preexists the contractual relationship and constrains its form), but also on the behavior of a *given* type of worker (whereby behavior directly responds to the incentive structure created by the contract). The latter is the effect conventionally studied in analyses of mandated maternity and family leave policies, where individuals in a given employment relationship potentially adjust future labor supply and fertility decisions. These individuals experience a change in the likelihood of promotions in the survey years after the FMLA was passed, due to a direct effect of the mandate on the incentives for a given type of worker. However, the information that the employer has about the worker's type is fixed, implicitly, and only the mapping from the type to the behavior changes due to a change in incentives. In this paper, we are primarily concerned with the existence of an effect due to adverse selection. However, ensuring that the effect that we observe in the data is not driven by the "incentive" effect of the policy, for a

[28] The federal FMLA also requires employees to have been employed for a total of 12 months and to have worked more than 1,250 hours over the past 12 months before becoming eligible. I did not use the tenure requirement in determining eligibility because in this paper, I test the hypothesis that costly employer investment is reduced in anticipation of women's selection into the labor force (or the selective retention of female employees) in the future, when the investment will pay off. The model relies on a change in the average group behavior among those who are mostly likely to have reduced participation costs in the future. Whether an employee currently has at least one year of tenure is not important for determining the risk of future FMLA eligibility because an employee with less than one year of tenure may be eligible in the future, by the time children are conceived. However, I assume that jobs that do not require more than 1,250 hours of work over the course of the year are likely to continue to require fewer than 1,250 hours of work per year in the future. Although the stylized model gives workers a choice of hours in the first period, workers who choose job offers with hours below the minimum eligibility requirements for coverage by FMLA are selecting into employment positions that are not affected by the FMLA.

given type, is a crucial task. I show that the change in the likelihood of promotion is not driven by the effect of the mandate on a given type of worker. In the results shown in Table 1, we include year fixed effects, state-specific year fixed effects, and year fixed effects specific to the demographic group of interest, women under the age of 40, but also state-demographic-specific year fixed effects. This specification allows for the possibility that the FMLA affected the likelihood of promotion over the time period in which it was enacted, through a direct effect on the behavior of those already in employment relationships. Appendix Table 13 presents the estimates from Equation (1), but the panel data is restricted to observations from the period after the Family Medical Leave Act was passed, still exploiting the variation in whether the respondent was hired before or after its enactment. The restricted sample isolates the selection effect from the incentive effect, and the estimates are consistent with those from the unrestricted sample. Appendix Tables 18 and 19 and Figure 15 address the concern about the potential impact of the FMLA on a given type and demonstrate that there appears to be little effect of the introduction of the FMLA on the population hired before its enactment who had already entered into their employment relationships.

In Appendix Table 14, I examine the effect of being hired after the enactment of the FMLA on the likelihood of promotion for women under the age of 40, excluding the state-specific, demographic-specific, and state-demographic-specific year fixed-effects. This allows us to investigate the effect of the FMLA on individuals hired before the FMLA, including a potential direct incentive effect on individuals of a given type once the mandate is in place. We see from the first row of Table 14 that the total change in the likelihood of promotion is still significantly negative for women under the age of 40 hired after the FMLA in treatment states, in spite of a possible improvement in promotion probabilities for a given type in the years following the mandate.

While the results in Table 1 are striking, they are also compatible with an alternative model of symmetric information, in which women who are more likely to benefit from the mandate select into the labor force, but full information about their types is known to the employer. Therefore, I examine the effect of being hired after the enactment of the FMLA on women under 40 who do not benefit from the mandate and are not on the extensive margin of participation due to its enactment: women who never have children. I estimate equation (1), but I include two additional demographic groups of interest: women under 40 who never have children and women under 40 who eventually have children. By also including their respective interaction terms[29], I examine whether the mandate affects the likelihood of promotions for either of these two groups differently from other women under the age of 40. Columns (1) and (2) of Table 3 show that the the effect on the likelihood of promotion is not significantly different for women who never have children. In other words,

[29] Columns (1) and (2) of Table 3 estimates the following equation: $promoted_{i,j,s,t} = (\beta_{1,n}FemaleUnder40NeverHasKids_{i,t} + \beta_{1,e}FemaleUnder40EverHasKids_{i,t} + \beta_1 FemaleUnder40) \cdot HiredAfter_{i,j} \cdot TreatmentState_s + (\beta_{2,n}FemaleUnder40NeverHasKids_{i,t} + \beta_{2,e}FemaleUnder40EverHasKids_{i,t} + \beta_2 FemaleUnder40) \cdot TreatmentState_s + (\beta_{3,n}FemaleUnder40NeverHasKids_{i,t} + \beta_{3,e}FemaleUnder40EverHasKids_{i,t} + \beta_3 FemaleUnder40) \cdot HiredAfter_{i,j} + \beta_{4,n}FemaleUnder40NeverHasKids_{i,t} + \beta_{4,e}FemaleUnder40EverHasKids_{i,t} + \beta_4 FemaleUnder40EverHasKids_{i,t} + \beta_5 HiredAfter_{i,j} + \beta_6(HiredAfter_{i,j} \cdot TreatmentState_s) + \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \varepsilon_{i,j,s,t}$

the likelihood of promotions is decreased for all women under the age of 40, including the women who never have children. Column (2) is restricted to a sample of women only, so the control group is comprised only of women over the age of 40. The results here demonstrate that the likelihood of promotions decreases for all women who have the potential to have children in the future, regardless of their realized fertility. Column (3) is also restricted to a sample of women only, but here the treatment group is restricted to only women under the age of 40 who never have children. The results show that there is, indeed, a reduced likelihood of promotions for women under the age of 40 who never have children.

Testable Implication T2: Employment

To examine the impact of the being hired after the enactment of the Family and Medical Leave Act on the likelihood of promotion, I estimate the following equation for an individual i employed with firm j, in state s, and in year t:

$$\begin{aligned} employed_{i,s,t} &= \beta_1(After_t \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(After_t \cdot TreatmentState_s) \\ &+ \beta_4(After_t \cdot FemaleUnder40_{i,t}) + \beta_5 After_t + \beta_6 FemaleUnder40_{i,t} \\ &+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \lambda_t + c_s + f_i + \varepsilon_{i,j,s,t} \end{aligned} \tag{3}$$

Here, we examine the likelihood of employment in the periods before and after the enactment of the FMLA. The model implies that it is only the participation constraint that affects the employment, so here, the information that the employer has about the worker does not affect the worker's decision to continue participating in the employment relationship. Table 4 shows that employment among women of childbearing age increases by 3 percent in treatment states relative to control states, after the enactment of the FMLA. Column (1) of Table 4 controls for state fixed effects, year fixed effects, and individual fixed effects, while Column (2) includes state-specific and demographic-specific year fixed effects. Figure 8 shows the estimates of the year-treatment coefficients from 1988 to 2001 and confirms that the rise in employment after the FMLA was not due to pre-existing trends.

Testable Implication T3: Hours

To examine the impact of the being hired after the enactment of the Family and Medical Leave Act on the realized labor supply of women with children, I estimate the following equation for an individual i employed with firm j, in state s, and in year t:

$$
\begin{aligned}
hours_{i,j,s,t} &= \beta_1(HiredAfter_{i,j} \cdot FemaleWithKids_{i,t} \cdot TreatmentState_s) \\
&+ \beta_2(FemaleWithKids_{i,t} \cdot TreatmentState_s) + \beta_3(HiredAfter_{i,j} \cdot TreatmentState_s) \\
&+ \beta_4(HiredAfter_{i,j} \cdot FemaleWithKids_{i,t}) + \beta_5 HiredAfter_{i,j} + \beta_6 FemaleWithKids_{i,t} \\
&+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \lambda_t + c_s + f_i + \delta_{i,t,s} + \varepsilon_{i,j,s,t}
\end{aligned} \tag{4}
$$

This equation is analagous to Equation (1), except that the demographic group of interest is no longer those who are at risk of having children in the future. Here, we examine the labor supply effects on those for whom the risk to the employer was realized. Table 5 shows the estimates of equation (4). As in the literature, women with children do work fewer hours per week. However, the results indicate that there is a decrease in late-career weekly hours of work among the women hired after the mandate. In other words, fertility is not the only determining factor of labor supply, and the types of women with children who are still employed at high levels of tenure work fewer hours than the types who were hired before the mandate. Employer beliefs based on the specified model are rational.

Testable Implication T4: Return to Signals

In order to test whether information asymmetry and the informativity of a signal is indeed affected by the introduction of the FMLA, I now turn to the second dataset, the Multi-City Study of Urban Inequality to examine the impact of the Family and Medical Leave Act on the return to costly signals. This dataset is unique in that it collects detailed information from each firm on performance evaluations of its employees. The prediction of the model is that the signal of those women who anticipate working a high number of hours would be contaminated by the signaling of those women who now anticipate working in the future but anticipate working a low number of hours. Table demonstrates that these performance evaluations are, in fact, predictive of the likelihood that a worker receives a promotion. In fact, there is a nonlinear increase in the likelihood of promotion with performance. For women, a performance evaluation above the 75th percentile leads to a 7 percent increase in the likelihood of promotion. The results in Column (1) of Table 7 demonstrate that at each level of performance, the marginal return to a higher performance level is lower for fertile women hired after the mandate, while the return to signaling is unchanged for women over the age of 40. This is a direct implication of the main mechanism of the model: signal contamination from the anticipated future selection of women whose optimal choice of hours is lower. As a result of the increased participation of low hours types, the information conveyed from a given signal is lower. Here, we also find that the return to the performance level that yielded the greatest increase in the likelihood of promotions, the 75th percentile, is also lower for fertile women hired after the mandate. This is shown in Columns (2)-(5) of Table 7. Lastly, we see in Column (6) that the marginal return of performing above 75th percentile is reduced for women of childbearing age.

Testable Implication T5: Effect on Wages and Fringe Benefits

The MCSUI is unique in that it contains detailed information on each firm's last hire and the wages and fringe benefits offered to an employee of this position. I examine the effect of being hired after the enactment of the FMLA on these various forms of compensation. Among starting wage, starting salary, health insurance provision, family health insurance provision, dental coverage, pension plan contributions, bonuses, and other benefits given to employees of the position in question, there is no effect on women of childbearing age relative to older women. However, among two particular benefits, flexible hours and day care provision, we see a large reduction in the likelihood of those benefits being provided when the last hire was a female under the age of 40, relative to older women. This provides evidence that the positions for which women under the age of 40 are hired, after the enactment of the FMLA, provide lower compensation in terms of fringe benefits, conditional on starting salary and starting wage.

Testable Implication T6: Effect on Firms with High Costs of Training

An important implication of the model is that not all firms raise the standard of promotions when the retention of women with high marginal costs of labor is increased. In particular, the cost to the firm of training a worker in the early career must be high enough that the firm suffers a loss when investing training in a worker who will optimally choose to work part-time in the future. If this were not the case, the profit to the firm of training a worker who will work part-time in the future would be lower, but as long as the profit were positive, the expected profit to the firm increases under the mandate because the firm retains the worker and reaps some profit in the future. However, it is only when the cost of training is sufficiently high that the firm optimally chooses a threshold to separate the full-time workers from the part-time, rather than the stayers from the leavers. When the information on hours, conditional on future labor force participation is more informative to the firm's promotion decision than the participation decision, then the firm raises its promotion standard. In other words, only when firm costs are sufficiently high that retention of part-time workers does not benefit the firm does the introduction of a maternity leave mandate have a negative effect on the likelihood of promotion for women of childbearing age, conditional on observing their first-period behavior. Table 9 shows the estimated coefficients from the following regression:

$$\begin{aligned} promoted_{i,j,t} &= \beta_1(HiredAfter_{i,j} \cdot FemaleUnder40_i \cdot HighTraining_j) \\ &+ \beta_2(FemaleUnder40_i \cdot HighTraining_j) + \beta_3(HiredAfter_{i,j} \cdot HighTraining_j) \\ &+ \beta_4(HiredAfter_{i,j} \cdot FemaleUnder40_i) + \beta_5 HiredAfter_{i,j} + \beta_6 FemaleUnder40_i \\ &+ \beta_7 HighTraining_j + \beta_8 X_i + \lambda_t + c_c + \varepsilon_{i,j,t} \end{aligned} \tag{5}$$

The first row of Table 9 demonstrates that the reduction in the likelihood of promotions among women under the age of 40 is significantly larger among firms with high costs of training, whereas there is no significant effect among firms with low costs of firm-sponsored training. This is a particularly important empirical finding because it refutes the alternative explanation that the decrease in likelihood of promotion is entirely due to selection. If this were the case, we should not expect to see a larger effect among firms with a high cost of training or mentorship.

5 Concluding Remarks

This paper sheds light on the hetereogeneous welfare consequences of mandated maternity leave policies. I show that maternity leave mandates can both increase employment and, yet, decrease promotion rates for women. I then show, in two different datasets, that women who were hired after the enactment of a minimal maternity leave mandate were five percent more likely to be employed but eight percent less likely to be promoted than those who were hired before its enactment. Moreover, I find a series of empirical results that would be difficult to reconcile with alternative explanations: the likelihood of promotions decreases even among women who do not benefit from the mandate, women who never have children, the labor supply among women with children is reduced by more than ten percent, even among those who were promoted, and that the widening of the gender gap in promotions is largest among firms where the cost of training is high. Finally, using a series of measures of signals of future productivity, including job performance, early career hours of work, and importantly, the selection of wage contracts, I find that the return to this signal, in terms of the likelihood of promotion, decreased for women hired after the enactment of a maternity leave mandate. Moreover, it decreases for women under the age of 40 alone. This set of evidence is difficult to reconcile with an explanation based on purely on selection and strongly suggests that information asymmetry between firms and workers is at the heart of the problem.

An innovative feature of the model is that it is precisely because the policy lengthens the expected career tenures of some women that the information problem is exacerbated. Women who expect to completely separate from their employers upon having children have little incentive to invest or take on costly signals in order to advance within the firm. Because the maternity policy reduces participation costs for women who would have otherwise left the labor force, such workers are retained. These workers have an additional incentive, under a maternity policy, to take on costs in order to increase the likelihood of obtaining a promotion. However, these workers are also more likely to work part-time than those who were willing to vie for a promotion before a maternity policy. The increased incentive for the less committed types to obtain a promotion as well "muddies" the signals from the most career-oriented workers and increases the cost of sorting workers.

The empirical results are consistent with these predictions and have powerful welfare implications for such policies. Because of the limited ability of firms to sort workers and allocate training efficiently, mandated maternity leave policies can result in a lower rate of human capital accumulation and wage growth for all women over the course of their lifecycle, and fewer advancement opportunities within the firm. While such policies may enhance welfare for women who would not have otherwise participated in the labor market, the cost of increasing the labor force participation of women is borne, in part, by the most career-oriented women, through a loss of their human capital accumulation and lower wage growth over the course of their lifecycles. Studies examining only the short-term impact of maternity leave policies may not fully capture the welfare consequences of maternity leave policies, and empirical work focused only on the effect due to changes in the direct incentives for workers' choice of human capital and occupation miss a crucial welfare consequence of such policies.

References

[1] Abbring, Jaap H., Pierre-Andre Chiappori, James Heckman, and Jean Pinquet (2003), "Adverse Selection and Moral Hazard in Health Insurance: Can Dynamic Data Help to Distinguish?" *Journal of the European Economic Association*, 1, 2-3, 512-521.

[2] Adda, Jerome, Christian Dustman and Katrien Stevens (2011), "The Career Costs of Children," *IZA Discussion Paper No. 6201.*

[3] Acemoglu, Daron and Jorn-Steffen Pischke (1998), "Why Do Firms Train? Theory and Evidence," *The Quarterly Journal of Economics*, 113, 1, 79-119.

[4] Acemoglu, Daron and Jorn-Steffen Piscke (1999), "Beyond Becker: Training in Imperfect Labour Markets," *The Economic Journal,* 109, 453, F112-F142.

[5] Albanesi, Stefania and Claudia Olivetti (2008), "Home Production, Market Production and the GenderWage Gap: Incentives and Expectations", Review of Economic Dynamics, 12, 1, 80–107.

[6] Altonji, Joseph and Rebecca Blank (1999), "Race and Gender in the Labor Market", in *Handbook of Labor Economics* edited by Orley Ashenfelter and Albert Rees, 3, 3143–3259.

[7] Arrow, Kenneth J. (1973), "The Theory of Discrimination" in *Discrimination in Labor Markets,* edited by Orley Ashenfelter and Albert Rees. Princeton: Princeton University Press, 3-33.

[8] Autor, David (2001), "Why Do Temporary Help Firms Provide Free General Skills Training?" *Quarterly Journal of Economics,* 116, 4, 1409-1448.

[9] Attanasio, Orazio, Hamish Low and Virginia Sanchez-Marcos (2008), "Explaining Changes in Female Labor Supply in a Life-Cycle Model", *American Economic Review*, 98, 4, 1517–1552.

[10] Barron, John, Dan Black and Mark Lowenstein (1993), "Gender Differences in Training, Capital and Wages", *Journal of Human Resources*, 28, 2, 343–364.

[11] Becker, Gary (1962), "Investment in Human Capital: A Theoretical Analysis," *Journal of Political Economy*, 70, 5, 9-49.

[12] Becker, Gary (1971), *The Economics of Discrimination*, 2nd ed, Chicago: The University of Chicago Press.

[13] Becker, Gary (1985), "Human Capital, Effort, and the Sexual Division of Labor," *Journal of Labor Economics*, 3, 1, S33-S58.

[14] Blau, Francine D. and Jed DeVaro (2006), "New Evidence on Gender Differences in Promotion Rates: An Empirical Analysis of a Sample of New Hires," NBER Working Paper 12321, National Bureau of Economic Research.

[15] Blau, Francine D. and Lawrence M. Kahn (1997), "Swimming Upstream: Trends in the Gender Wage Differential in the 1980's," *Journal of Labor Economics*, 15, 1, 1-42.

[16] Blau, Francine D. and Lawrence M. Kahn (2006), "The U.S. Gender Pay Gap in the 1990's; Slowing Convergence," *Industrial and Labor Relations Review*, 60, 1, 45-66.

[17] Blau, Francine D. and Lawrence M. Kahn (2013), "Female Labor Supply: Why is the US Falling Behind?" NBER Working Paper 18702, National Bureau of Economic Research

[18] Blau, Francine D. and Lawrence M. Kahn (2013), "Female Labor Supply: Why is the US Falling Behind?" *American Economic Review*, 103, 3, 251-256.

[19] Brown, James and Audrey Light (1992), "Interpreting Panel Data on Job Tenure," *Journal of Labor Economics,* 10, 3, 219-257.

[20] Chiappori, Pierre-Andre and Bernard Salanie (2002), "Testing Contract Theory: Some Recent Work," *CESIFO Working Paper No. 738.*

[21] Chiappori, Pierre-Andre and Bernard Salanie (2000), "Testing for Asymmetric Information in Insurance Markets," *Journal of Political Economy*, 108, 1, 56-78.

[22] Coate, Stephen and Glenn C. Loury (1993), "Will Affirmative Action Policies Eliminate Negative Stereotypes?" *The American Economic Review*, 83, 5, 1220-1240.

[23] Dalto, Guy (1989), "A Structural Approach to Women's Hometime and Experience-Earnings Profiles: Maternity Leave and Public Policy," *Population Research and Policy Review,* 8, 3, 247-266.

[24] Eckstein, Zvi and Kenneth Wolpin (1989), "Dynamic Labor Force Participation of Married Women and Endogenous Work Experience," *The Review of Economic Studies,* 56, 3, 375-390.

[25] Erosa, Andres, Luisa Fuster and Diego Restuccia (2002), "Fertility Decisions and Gender Differences in Labor Turnover, Employment, and Wages," *Review of Economic Dynamics,* 5, 4, 856-891.

[26] Erosa, Andres, Luisa Fuster and Diego Restuccia (2010), "A General Equilibrium Analysis of Parental Leave Policies," *Review of Economic Dynamics,* 13, 4, 742-758.

[27] Francesconi, Marco (2002), "A Joint Dynamic Model of Fertility and Work of Married Women," *Journal of Labor Economics,* 20, 2, 336-380.

[28] Gayle, George-Levi and Limor Golan (2012), "Estimating a Dynamic Adverse-Selection Model: Labour-Force Experience and the Changing Gender Earnings Gap 1968-1997," *Review of Economic Studies,* 79, 1, 227-267.

[29] Gayle, George-Levi and Robert Miller (2006), "Life-Cycle Fertility Behavior and Human Capital Accumulation," *2006 Meeting Papers 784*, Society for Economic Dynamics.

[30] Gibbons, Robert and Lawrence Katz (1991), "Layoffs and Lemons," *Journal of Labor Economics,* 9, 4, 351-380.

[31] Gruber, Jonathan (1994), "The Incidence of Mandated Maternity Benefits," *The American Economic Review,* 84, 3, 622-641.

[32] Kahn, Lisa (2013), "Asymmetric Information between Employers," *American Economic Journal: Applied Economics,* 5, 4, 165-205.

[33] Kahn, Lisa and Fabian Lange (2014), "Employer Learning, Productivity and the Earnings Distribution: Evidence from Performance Measures," *Review of Economic Studies,* 81, 4, 1575-1613.

[34] Hart, Oliver and John Moore (1988), "Incomplete Contracts and Renegotiation," *Econometrica,* 56, 4, 755-785.

[35] Hall, Robert and Edward Lazear (1984), "The Excess Sensitivity of Layoffs and Quits to Demand," *Journal of Labor Economics,* 2, 2, 233-257.

[36] Hashimoto, Masanori and Ben Yu (1980), "Specific Capital, Employment Contracts, and Wage Rigidity," *The Bell Journal of Economics*, 11, 2, 536-549.

[37] Heckman, James and Thomas Macurdy (1980), "A Life-Cycle Model of Female Labour Supply," *Review of Economic Studies*, 47, 1, 47-74.

[38] Laffont, Jean-Jacques and David Martimort (2002), *The Theory of Incentives: The Principle-Agent Model*, Princeton: Princeton University Press.

[39] Laffont, Jean-Jacque and Jean Tirole (1988), “The Dynamics of Incentive Contracts,” *Econometrica*, 56, 5, 1153-1175.

[40] Lazear, Edward and Sherwin Rosen (1990), “Male-Female Differentials in Job Ladders,” *Journal of Labor Economics*, 8, 1, S106-S123.

[41] Lalive, Rafael and Josef Zweimuller (2009), “How Does Parental Leave Affect Fertility and Return to Work? Evidence From Two Natural Experiments,” *Quarterly Journal of Economics*, 124, 3, 1363-1402.

[42] Low, Hamish and Sanchez-Marcos, Virginia (2014), “Female Labor Market Outcomes and Maternal Leave Policies,” Working Paper.

[43] McCue, Kristin (1996), “Promotions and Wage Growth,” *Journal of Labor Economics,* 14, 2, 175-209.

[44] Mukhopadhyay, Sankar (2012), “The Effects of the 1978 Pregnancy Discrimination Act on Female Labor Supply,” *International Economic Review,* 53, 4, 1133-1153.

[45] Pallais, Amanda (2014), “Inefficient Hiring in Entry-Level Labor Markets,” *The American Economic Review*, 104, 11, 3565-3599.

[46] Phelps, Edmund S. (1972), “The Statistical Theory of Racism and Sexism,” *The American Economic Review*, 62, 4, 659-661.

[47] Ruhm, Christopher (1998), “The Economic Consequences of Parental Leave Mandates: Lessons From Europe,” *The Quarterly Journal of Economics*, 113, 1, 285-317.

[48] Spence, Michael (1973), “Job Market Signaling,” *The Quarterly Journal of Economics*, 87, 3, 355-374.

[49] Thomas, Jonathan and Tim Worrall (2007), “Limited Commitment Models of the Labor Market,” *CESIFO Working Paper No. 2109.*

[50] Waldfogel, Jane (1998), “Understanding the 'Family Gap' in Pay for Women with Children,” *The Journal of Economic Perspectives,* 12, 1, 137-156.

[51] Weiss, Andrew (1980), “Job Queues and Layoffs in Labor Markets with Flexible Wages,” *Journal of Political Economy*, 88, 3, 526-538.

[52] Wolfers, Justin (2006), “Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results,” *The American Economic Review*, 96, 5, 1802-1820.

6 Tables and Figures

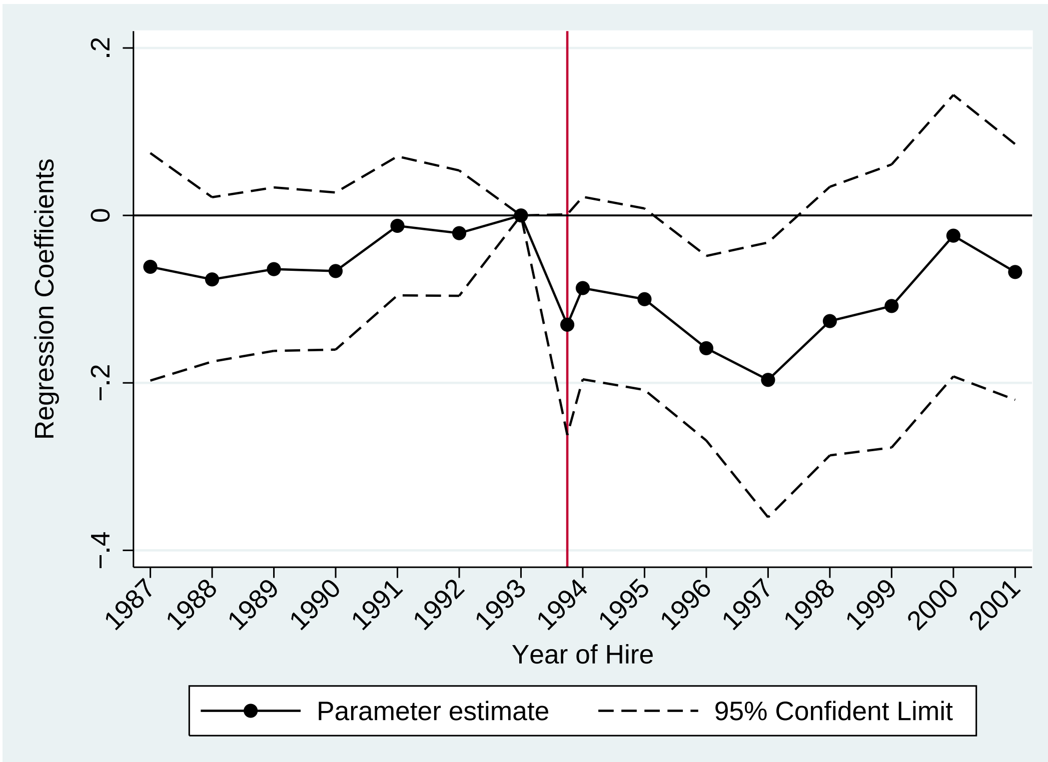
Table 1: Probability of Promotion Since Hire (T1)

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.06**	-0.08***	-0.08***	-0.08***	-0.08***
	[0.024]	[0.026]	[0.025]	[0.025]	[0.026]
Female Under 40* Treatment State	0.02	0.02	0.02	0.02	0.03
	[0.023]	[0.025]	[0.025]	[0.026]	[0.026]
Hired After*Treatment State	0.04***	0.05***	0.05***	0.05***	0.05***
	[0.011]	[0.013]	[0.014]	[0.014]	[0.014]
Hired After*Female Under 40	0.02	0.05**	0.06***	0.06***	0.05**
	[0.019]	[0.021]	[0.019]	[0.020]	[0.020]
Female Under 40	-0.08***	-0.07***	-0.07***	-0.07***	-0.07***
	[0.019]	[0.020]	[0.021]	[0.021]	[0.020]
Hired After	-0.01	-0.03**	-0.03**	-0.03**	-0.03**
	[0.011]	[0.014]	[0.014]	[0.014]	[0.014]
Treatment State	0.02	0.09	0.03	0.03	0.03
	[0.035]	[0.059]	[0.037]	[0.038]	[0.039]
Tenure	0.05***	0.05***	0.05***	0.05***	0.05***
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	57,269	49,108	48,987	48,987	48,637
Individual Fixed Effects	13,913	12,904	12,889	12,889	12,857
Mean	0.07	0.07	0.07	0.07	0.07

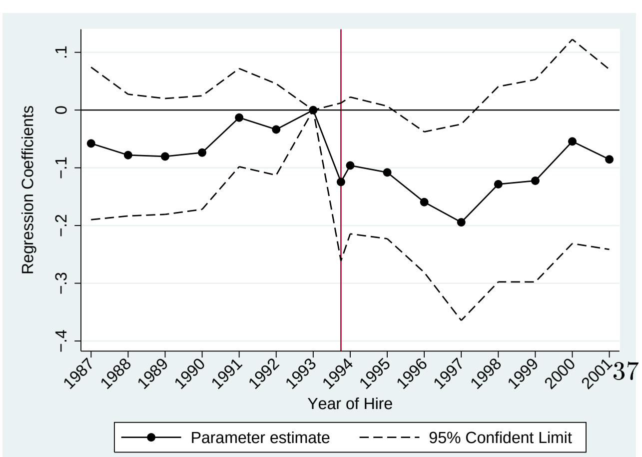
Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients reported are the estimated coefficients from Equation (1). This sample is restricted to respondents hired in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions include year, state, and individual-fixed effects. All regressions also include state-specific, demographic group-specific, and state-demographic specific year fixed effects, where the demographic group of interest is women under the age of 40. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

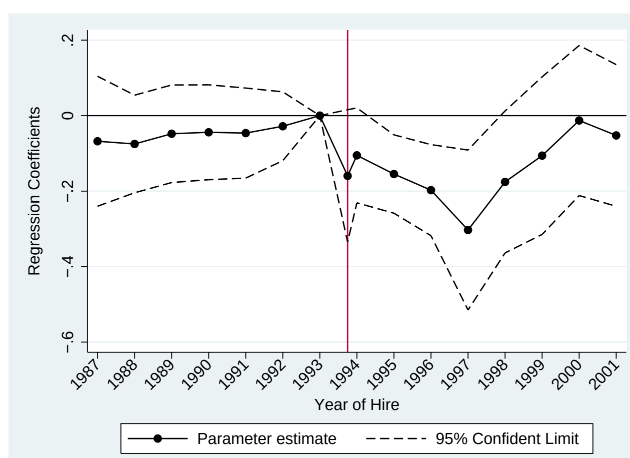
Figure 5: Effect of Introduction of FMLA On Promotion Rate of Women Under 40



Notes: The coefficients $\beta_{\tau,1}$ and 95% confidence intervals reported are obtained from estimating
$promoted_{i,j,s,t} = \sum_{\tau=1987}^{2001} \beta_{\tau,1}(HiredYear\tau_{i,j} \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s)$
$+\beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \sum_{\tau=1987}^{2001} \beta_{\tau,3}(HiredYear\tau_{i,j} \cdot TreatmentState_s)$
$+ \sum_{\tau=1987}^{2001} \beta_{\tau,4}(HiredYear\tau_{i,j} \cdot FemaleUnder40_{i,t}) + \sum_{\tau=1987}^{2001} \beta_{\tau,5} HiredYear\tau_{i,j}$
$+\beta_6 FemaleUnder40_{i,t} + \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \delta_{i,t,s} + \varepsilon_{i,j,s,t}$.
Standard errors are clustered at the state level. The sample is restricted to respondents hired in 1987 or later. The regression controls for sex, age, age squared, tenure, tenure squared, level of education attained, marital status, marital status interacted with sex, marital status interacted with FemaleUnder40, and a dummy for nonwhite. The regression also controls for job characteristics (private firm, union), 24 standard occupational categories, and 12 standard industry categories from the census occupational classification system.



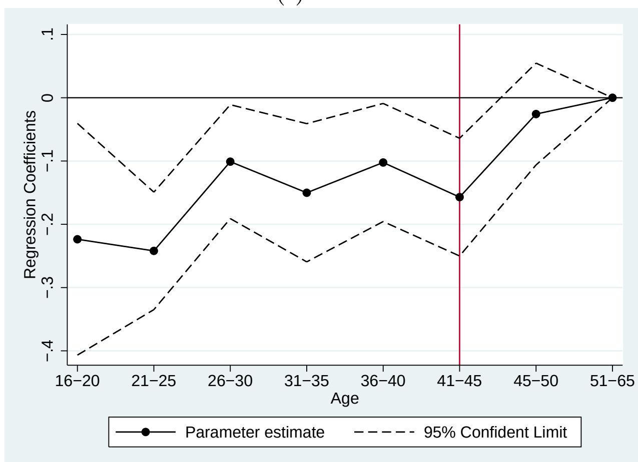
(a) Control Group = Men Under 40 Only



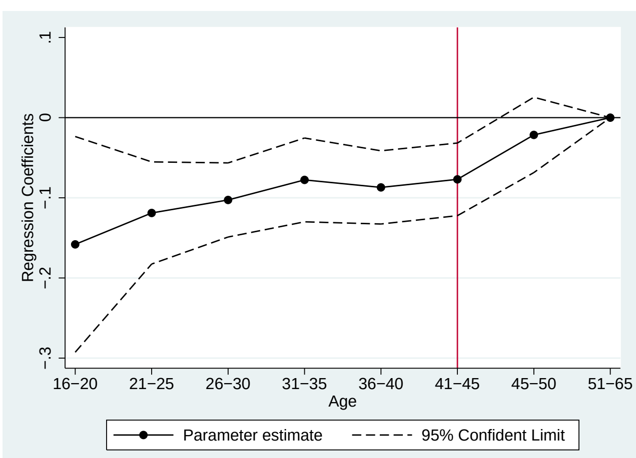
(b) Control Group = Women Over 40 Only

Figure 6: Effect of Mandate on Gender Differences in Promotions Relative to Older Women

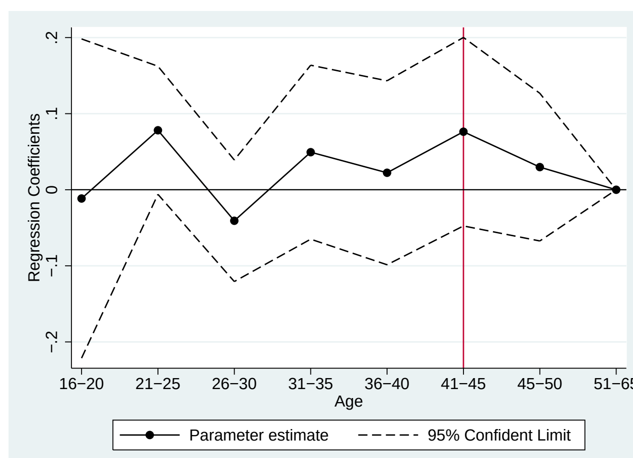
(I) PSID



Notes: The coefficients $\beta_{a,1}$ and 95% confidence intervals reported are obtained from estimating
$promoted_{i,j,s,t} = \sum_{a=15(5)}^{45} \beta_{a,1}(FemaleAged\ a\ to\ (a+4)_{i,t} \cdot HiredAfter_{i,j} \cdot TreatmentState_s)$
$+ \sum_{a=15(5)}^{45} \beta_{a,2}(FemaleAged\ a\ to\ (a+4)_{i,t} \cdot TreatmentState_s) + \beta_3(HiredAfter_{i,j} \cdot TreatmentState_s)$
$+ \sum_{a=15(5)}^{45} \beta_{a,4}(FemaleAged\ a\ to\ (a+4)_{i,t} \cdot HiredAfter_{i,j}) + \beta_5 HiredAfter_{i,j}$
$+ \sum_{a=15(5)}^{45} \beta_{a,6}(FemaleAged\ a\ to\ (a+4)_{i,t} + \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \delta_{i,t,s} + \varepsilon_{i,j,s,t}$.
Standard errors are clustered at the state level. The sample is restricted to respondents hired in 1987 or later. The regression controls for sex, age, age squared, tenure, tenure squared, level of education attained, marital status, marital status interacted with sex, marital status interacted with FemaleUnder40, and a dummy for nonwhite. The regression also controls for job characteristics (private firm, union), 24 standard occupational categories, and 12 standard industry categories from the census occupational classification system.



(a) Treatment States



(b) Control States

Notes: The coefficients $\beta_{a,4}$ and 95% confidence intervals reported are obtained from estimating
$promoted_{i,j,s,t} = \sum_{a=15(5)}^{45} \beta_{a,4}(FemaleAged\ a\ to\ (a+4)_{i,t} \cdot HiredAfter_{i,j}) + \beta_5 HiredAfter_{i,j}$
$+ \sum_{a=15(5)}^{45} \beta_{a,6}(FemaleAged\ a\ to\ (a+4)_{i,t} + +\beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \delta_{i,s} + \varepsilon_{i,j,s,t}$.
Standard errors are clustered at the state level. The sample is restricted to respondents hired in 1987 or later. The regression controls for sex, age, age squared, tenure, tenure squared, level of education attained, marital status, marital status interacted with sex, marital status interacted with FemaleUnder40, and a dummy for nonwhite. The regression also controls for job characteristics (private firm, union), 24 standard occupational categories, and 12 standard industry categories from the census occupational classification system.

Table 2: Probability of Promotion Since Hire In Jobs Eligible for FMLA Coverage

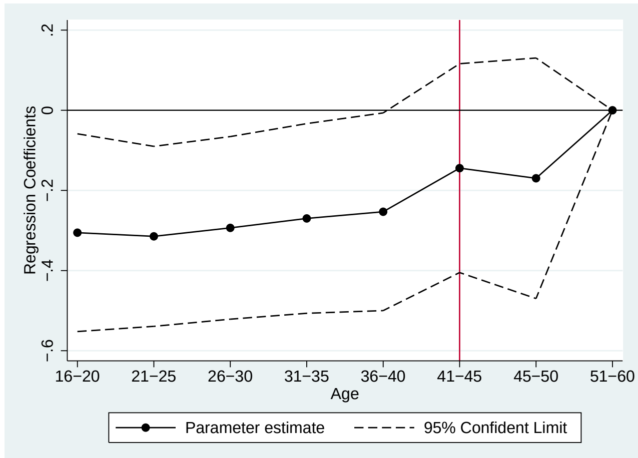
VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Eligible Female Under 40* Hired After*Treatment State	-0.07**	-0.11***	-0.10***	-0.10***	-0.10***
	[0.033]	[0.034]	[0.033]	[0.033]	[0.034]
Eligible Female Under 40* Treatment State	0.02	0.03	0.03	0.03	0.04
	[0.028]	[0.031]	[0.030]	[0.030]	[0.031]
Eligible Female Under 40* Hired After	0.03	0.08***	0.07***	0.07***	0.07**
	[0.027]	[0.028]	[0.027]	[0.027]	[0.028]
Eligible Female Under 40	-0.10***	-0.10***	-0.10***	-0.10***	-0.11***
	[0.025]	[0.029]	[0.027]	[0.027]	[0.027]
Tenure	0.05***	0.05***	0.05***	0.05***	0.05***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	38,431	32,952	32,878	32,878	32,638
Individual Fixed Effects	11,436	10,484	10,475	10,475	10,439
Mean	0.10	0.10	0.10	0.10	0.10

Robust standard errors in brackets, clustered at the state level

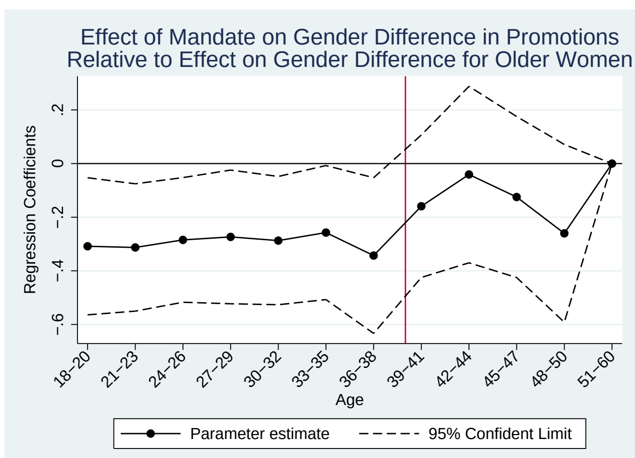
*** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients reported are the estimated coefficients from Equation (2). "Eligible Female Under 40" is defined as women under the age of 40 who have worked at least 1250 hours in their current employment position in the previous year. The control group consists of men and women over the age of 40. The sample is restricted to workers hired in 1987 or later and to those respondents who had a non-missing response to the number of hours worked over the previous 12 months. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions include year, state, and individual-fixed effects. All regressions also include state-specific, demographic group-specific, and state-demographic specific year fixed effects, where the demographic group of interest is women under the age of 40. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Figure 7: Effect of Mandate on Gender Differences in Promotions Relative to Older Women



Notes: The coefficients $\beta_{a,1}$ and 95% confidence intervals reported are obtained from estimating
$promoted_{i,j,t} = \sum_{a=15(5)}^{45} \beta_{a,1}(FemaleAged\ a\ to\ (a+4)_{i,t} \cdot HiredAfter_{i,j}) + \beta_2 HiredAfter_{i,j}$
$+\sum_{a=15(5)}^{45} \beta_{a,3}(FemaleAged\ a\ to\ (a+4)_{i,t}$
$+\sum_{a=15(5)}^{45} \beta_{a,3}(Aged\ a\ to\ (a+4)_{i,t} + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_c + \varepsilon_{i,j,t}$.
All regressions control for years of age, age squared, education level, a dummy for nonwhite, a standardized performance level, 24 standard occupational categories, and 8 standard industry divisions. The excluded education level is high school graduate.



Data: MCSUI

Notes: The coefficients $\beta_{a,1}$ and 95% confidence intervals reported are obtained from estimating
$promoted_{i,j,t} = \sum_{a=18(3)}^{45} \beta_{a,1}(FemaleAged\ a\ to\ (a+2)_{i,t} \cdot HiredAfter_{i,j}) + \beta_2 HiredAfter_{i,j}$
$+\sum_{a=18(3)}^{45} \beta_{a,3}(FemaleAged\ a\ to\ (a+2)_{i,t} + \sum_{a=18(3)}^{45} \beta_{a,3}(Aged\ a\ to\ (a+2)_{i,t} + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_c + \varepsilon_{i,j,t}$.
All regressions control for years of age, age squared, education level, a dummy for nonwhite, a standardized performance level, 24 standard occupational categories, and 8 standard industry divisions. The excluded education level is high school graduate.

Table 3: Effect of Mandate on Probability of Promotion
Among Women Under 40 Who Never Have Children

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted
Female Under 40 Never Has Kids*	0.01	0.01	-0.08*
Treatment State*Hired After	[0.053]	[0.054]	[0.048]
Female Under 40 Ev. Has Kids*	-0.03	-0.04	
Treatment State*Hired After	[0.040]	[0.042]	
Female Under 40*	-0.09***	-0.09**	
Treatment State*Hired After	[0.024]	[0.033]	
Tenure	0.05***	0.05***	0.04***
	[0.003]	[0.003]	[0.010]
Tenure Squared	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.001]
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Firm Characteristics	Yes	Yes	Yes
Occupation Controls	24	24	24
Industry Controls	8	8	8
Observations	28,496	14,475	4,364
Individual Fixed Effects	10,054	5,295	1,828
Mean	0.09	0.08	0.07

Robust standard errors in brackets
Standard errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: This sample is restricted to respondents hired in 1987 or later and year observations from August 1993 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions include year, state, and individual-fixed effects. All regressions also control for 24 standard occupation divisions and 8 standard industry divisions. The dependent variable is whether the respondent has been promoted since the time of hire. Columns (2) and (3) restricts the sample to women only.

Table 4: Effect of Mandate on Employment and Labor Force Participation of Women Under 40 (T2)

VARIABLES	(1) Employed	(2) Employed	(3) LFP	(4) LFP
Female Under 40*Treatment State* Post-Period	0.03**	0.03*	0.05***	0.05***
	[0.015]	[0.017]	[0.012]	[0.013]
Female Under 40*Treatment State	-0.04***	-0.04***	-0.05***	-0.05***
	[0.015]	[0.015]	[0.012]	[0.012]
Treatment State*Post-Period	-0.01	-0.00	-0.02**	-0.01
	[0.012]	[0.011]	[0.009]	[0.010]
Female Under 40*Post-Period	0.00	-0.02	-0.00	-0.02
	[0.014]	[0.022]	[0.011]	[0.017]
Treatment State	0.01	0.01	0.03	0.04
	[0.032]	[0.034]	[0.025]	[0.027]
Post-Period	-0.01	-0.00	-0.00	-0.01
	[0.009]	[0.010]	[0.008]	[0.008]
Female Under 40	0.01	-0.01	0.03	0.00
	[0.020]	[0.020]	[0.018]	[0.018]
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Year F.E.	No	Yes	No	Yes
Demographic-Specific Year F.E.	No	Yes	No	Yes
Observations	129,064	129,064	129,064	129,064
Individual Fixed Effects	22,105	22,105	22,105	22,105
Mean	0.75	0.75	0.79	0.79

Robust standard errors in brackets, Standard Errors Clustered at the State Level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients reported are the estimated coefficients from Equation (3). All regressions control for age, age squared, education level, marital status, marital status interacted with sex, marital status interacted with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions include year, state, and individual-fixed effects. Columns (2) and (4) also include state-specific and demographic-specific year fixed effects.

Figure 8: Effect of Mandate on Employment Rates of Women Under 40

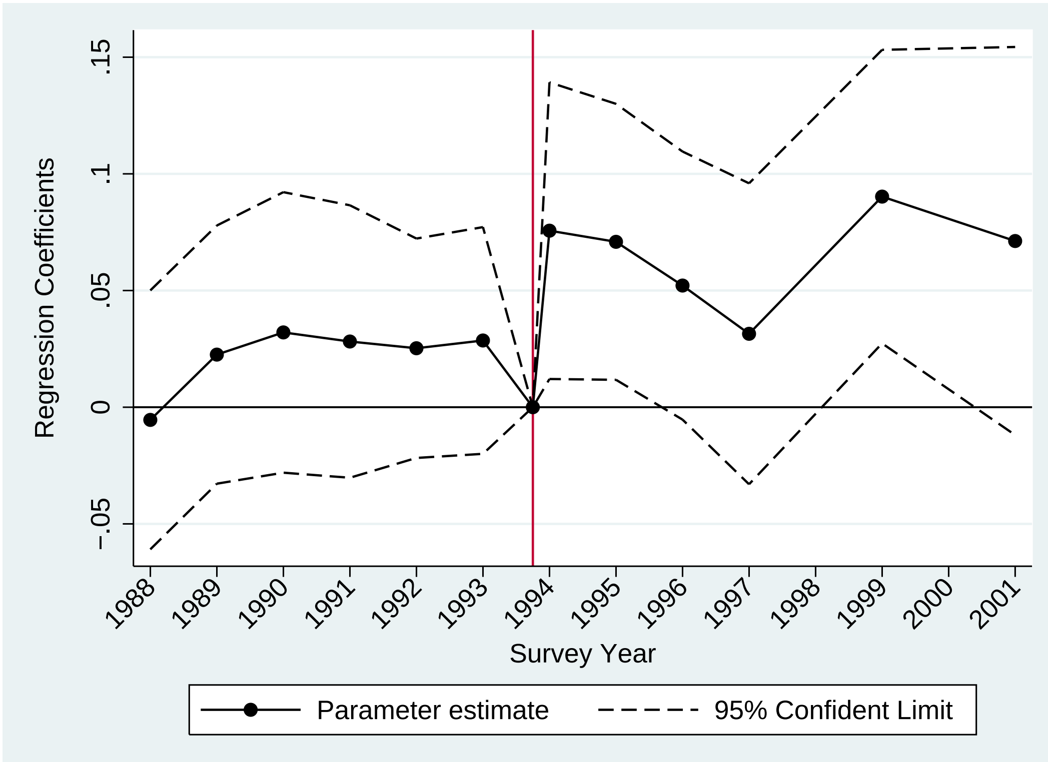


Table 5: Effect of Mandate on Late-Career Labor Supply of Women Under 40 (T3)

VARIABLES	(1) Weekly Hours	(2) Total Weekly Hours	(3) Part-Time	(4) Overtime
Female With Children*Hired After*	-4.73**	-4.09**	0.09	-0.11*
Treatment State	[2.127]	[1.956]	[0.060]	[0.060]
Female With Children*	0.43	0.04	-0.01*	0.00
Treatment State	[0.650]	[0.246]	[0.006]	[0.008]
Hired After*Treatment State	2.21*	1.39	0.01	0.01
	[1.199]	[1.284]	[0.019]	[0.042]
Female With Children*Hired After	4.25**	4.26**	-0.10*	0.11**
	[1.979]	[1.788]	[0.053]	[0.053]
Female With Children	-4.40***	-3.49***	0.13***	-0.10***
	[0.487]	[0.441]	[0.014]	[0.018]
Hired After	-1.61	-1.16	-0.01	-0.02
	[1.032]	[1.104]	[0.013]	[0.032]
Treatment State	0.78	2.27	0.10***	0.15**
	[2.081]	[1.816]	[0.019]	[0.058]
Tenure	-0.00	-0.01	-0.00*	-0.00*
	[0.034]	[0.036]	[0.001]	[0.001]
Tenure Squared	0.00	0.00	0.00	0.00***
	[0.001]	[0.001]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Year F.E.	Yes	Yes	Yes	Yes
Demographic-Specific Year F.E.	Yes	Yes	Yes	Yes
Occupation Controls	24	24	24	24
Industry Controls	8	8	8	8
Observations	50,481	50,481	50,481	50,481
Individual Fixed Effects	11,747	11,747	11,747	11,747
Mean	42.33	42.85	0.10	0.21

Robust standard errors in brackets, Standard Errors Clustered at the State Level
*** p<0.01, ** p<0.05, * p<0.1

Notes: This sample is restricted to respondents with at least four years of tenure with their current employer. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions include year, state, and individual-fixed effects, as well as state-specific and demographic-specific year-fixed effects. All regressions also control for 24 standard occupation divisions and 8 standard industry divisions. "Weekly Hours" is defined as the average number of hours a week the respondent worked on in their main job in the previous year. "Total Weekly Hours" is defined as the average number of hours a week the respondent worked plus the average weekly hours of overtime (determined from the annual hours of overtime reported). "Part-Time" is defined as average weekly hours of work is below 35 hours per week, and "Overtime" is defined as average hours of work is greater than or equal to 50 hours of work per week.

Table 6: Non-Linear Return to Performance Among Female Employees

VARIABLES	(1) Promoted
50th %	0.03
	[0.030]
75th %	0.07**
	[0.029]
80th %	-0.06
	[0.040]
90th %	0.04
	[0.042]
Under 40	0.08*
	[0.045]
Standardized Performance	-0.08
	[0.116]
Observations	933
R-squared	0.11
Mean	0.07

Standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: This sample is restricted to firms whose most recent hire was female. All regressions control for years of age, age squared, education level, and a dummy for nonwhite. Regressions also all include controls for standardized performance evaluations, firm characteristics, city dummies, 24 standard occupational categories, and 8 standard industry categories. The excluded education level is high school graduate. The standardized performance level is equal to the employer's evaluation of worker performance minus the average performance rating given to workers in the firm, on a scale of 0 to 1. "Xth

Table 7: Marginal Return to Signals Among Female Employees (T4)

Table 8 - Effect of Mandate on Return to Performance

VARIABLES	Promoted
50th %*Female Under 40*After	0.06
	[0.136]
75th %*Female Under 40*After	-0.27**
	[0.114]
80th %*Female Under 40*After	0.17
	[0.333]
90th %*Female Under 40*After	-0.17
	[0.324]
Performance	-0.06
	[0.116]
Sample	Females
Observations	933
R-squared	0.11
Mean	0.07

Standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Notes: This sample is restricted to firms whose most recent hire was female. All regressions control for years of age, age squared, education level, and a dummy for nonwhite. Regressions also all include controls for standardized performance evaluations, firm characteristics, city dummies, 24 standard occupational categories, and 8 standard industry categories. The excluded education level is high school graduate. The standardized performance level is equal to the employer's evaluation of worker performance minus the average performance rating given to workers in the firm, on a scale of 0 to 1. "Xth

Table 8: Effect of Mandate on Wages and Fringe Benefits (T5)

VARIABLES	(1) Starting Wage	(2) Health Insurance	(3) Dental	(4) Pension	(5) Paid Vacation	(6) Other Benefits	(7) Additional Pay	(8) Bonuses	(9) Flexible Hours	(10) Day Care
Female Under 40*Hired After	0.06	-0.08	-0.10	-0.03	-0.07	-0.05	0.05	-0.02	-0.17*	-0.19***
	[0.914]	[0.078]	[0.098]	[0.097]	[0.066]	[0.091]	[0.092]	[0.097]	[0.097]	[0.046]
Female*Hired After	-0.13	0.14*	0.08	0.06	0.05	0.06	-0.05	-0.01	0.24**	0.15***
	[0.910]	[0.078]	[0.098]	[0.097]	[0.066]	[0.091]	[0.092]	[0.097]	[0.097]	[0.046]
Hired After	0.25	-0.04	0.05	0.02	-0.02	-0.01	-0.05	-0.03	-0.04	0.02
	[0.366]	[0.031]	[0.039]	[0.039]	[0.026]	[0.036]	[0.037]	[0.039]	[0.039]	[0.019]
Female Under 40	1.17*	0.22***	0.10	0.06	0.02	0.04	-0.07	-0.01	0.05	0.10***
	[0.695]	[0.059]	[0.074]	[0.073]	[0.050]	[0.069]	[0.070]	[0.074]	[0.074]	[0.035]
Female	-1.90***	-0.20***	-0.02	-0.06	-0.01	0.01	0.08	0.03	-0.04	-0.05
	[0.666]	[0.056]	[0.071]	[0.070]	[0.048]	[0.066]	[0.067]	[0.070]	[0.071]	[0.034]
Under 40	-0.54	-0.08	0.04	0.02	-0.01	-0.07	-0.04	-0.09	-0.01	-0.03
	[0.653]	[0.056]	[0.070]	[0.069]	[0.047]	[0.065]	[0.066]	[0.070]	[0.070]	[0.033]
Tenure	0.49**	-0.01	-0.06**	-0.03	0.02	-0.03	0.06**	0.08***	0.02	0.03**
	[0.247]	[0.021]	[0.027]	[0.026]	[0.018]	[0.025]	[0.025]	[0.026]	[0.026]	[0.013]
Tenure Squared	-0.01	-0.00	0.01	0.00	-0.01**	0.00	-0.00	-0.00	0.00	-0.00*
	[0.035]	[0.003]	[0.004]	[0.004]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]	[0.002]
Observations	1,576	1,690	1,688	1,685	1,690	1,685	1,686	1,690	1,685	1,691
R-squared	0.30	0.13	0.11	0.13	0.10	0.08	0.19	0.11	0.12	0.16
Mean	8.50	0.80	0.57	0.57	0.86	0.71	0.56	0.41	0.56	0.07

Standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: All regressions control for years of age, age squared, education level, and a dummy for nonwhite. Regressions also all include controls for standardized performance evaluations, firm characteristics, city dummies, 24 standard occupational categories, and 8 standard industry categories. The excluded education level is high school graduate. Wages and salaries are in terms of 1990 dollars.

Table 9: Effect of Mandate on Likelihood of Promotion Among Firms with High Costs of Training (T6)

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted	(6) Promoted
High Training*Female Under 40*	-0.14	-0.22*	-0.22*	-0.23*	-0.26**	-0.26**
Hired After	[0.111]	[0.124]	[0.126]	[0.128]	[0.128]	[0.128]
Female Under 40*Hired After	-0.07	-0.07	-0.10	-0.10	-0.11	-0.11
	[0.068]	[0.072]	[0.073]	[0.075]	[0.075]	[0.077]
Female Under 40*High Training	0.02	0.03	0.02	0.03	0.05	0.05
	[0.059]	[0.062]	[0.068]	[0.069]	[0.070]	[0.070]
Hired After*High Training	0.13	0.19	0.20*	0.20*	0.22*	0.20
	[0.104]	[0.116]	[0.118]	[0.120]	[0.120]	[0.121]
Hired After	0.06	0.06	0.07	0.07	0.07	0.08
	[0.065]	[0.068]	[0.069]	[0.071]	[0.071]	[0.073]
Female Under 40	0.10*	0.08	0.09	0.10	0.09	0.09
	[0.056]	[0.059]	[0.062]	[0.063]	[0.063]	[0.064]
High Training	0.00	0.00	0.00	0.01	-0.02	-0.01
	[0.055]	[0.057]	[0.063]	[0.064]	[0.065]	[0.065]
Tenure	0.12***	0.13***	0.10***	0.10***	0.10***	0.09***
	[0.018]	[0.020]	[0.028]	[0.028]	[0.029]	[0.029]
Tenure Squared	-0.01***	-0.01***	-0.00	-0.00	-0.00	-0.00
	[0.003]	[0.003]	[0.006]	[0.006]	[0.006]	[0.006]
Performance Control		Yes	Yes	Yes	Yes	Yes
Firm Characteristics			Yes	Yes	Yes	Yes
City Controls			Yes	Yes	Yes	Yes
Occupation Controls				12	24	24
Industry Controls						8
Observations	1,031	927	757	752	750	745
R-squared	0.08	0.09	0.10	0.11	0.12	0.13
Mean	0.07	0.07	0.07	0.07	0.07	0.07

Standard errors in brackets
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: The sample is restricted to firms whose last hire was female. All regressions control for years of age, age squared, education level, and a dummy for nonwhite. The excluded education level is high school graduate. The dependent variable is whether the employer's last hire has been promoted. "High Training" is defined as the number of hours of training the last hire received from coworkers and supervisors since the time of hire was greater than or equal to 50 hours. "Hired After" is defined as whether the worker was hired after the enactment of FMLA. "Under 40" is defined as whether the worker was aged 40 or less at the time of hire. Column (2) uses a standardized performance level that is equal to the employer's evaluation of worker performance minus the average performance rating given to workers in the firm, on a scale of 0 to 1. Occupations were divided into a set of 12 standard categories in Column (4) and 24 categories in Column (5), although no observations were in the 24th standard category, military occupations. Column (6) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 10: Effect of Mandate on Fertility of Employed Women (T7)

VARIABLES	(1) Has Children	(2) Ever Has Children	(3) Has Child LT 5	(4) Has Child LT 1
Hired After* Female* Treatment State	0.04**	0.04**	0.04**	0.03*
	[0.018]	[0.016]	[0.018]	[0.016]
Female* Treatment State	0.01	0.01	0.01	0.00
	[0.008]	[0.007]	[0.008]	[0.007]
Hired After*Treatment State	-0.01	-0.01	-0.03**	-0.02**
	[0.012]	[0.011]	[0.013]	[0.011]
Hired After*Female	-0.01	-0.03*	-0.05***	-0.05***
	[0.016]	[0.015]	[0.016]	[0.014]
Female	0.13***	0.13***	0.11***	0.09***
	[0.010]	[0.009]	[0.010]	[0.009]
Hired After	-0.00	0.02	0.03***	0.04***
	[0.012]	[0.010]	[0.012]	[0.010]
Treatment State	0.03***	0.02***	-0.02***	-0.01**
	[0.005]	[0.005]	[0.005]	[0.005]
Tenure	0.00	-0.00	-0.00	-0.00**
	[0.000]	[0.000]	[0.001]	[0.000]
Tenure Squared	-0.00***	-0.00	-0.00**	0.00
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	98,020	98,747	98,747	98,747
R-squared	0.20	0.08	0.35	0.30
Mean	0.78	0.86	0.32	0.20

Standard errors in brackets

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This sample is restricted to respondents whose employment spell began in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include controls for whether the respondent was a member of a union and whether the employer was private Occupations are divided into a set of 24 standard occupation divisions and 8 standard industry divisions.

7 Appendix A

Proof of Proposition 1

Such a threshold $\underline{y}^*$ exists as long as the cost of firm training is neither so low nor so high that information about a worker's type is uninformative to the firm's decision. Such a threshold does not exist if the cost of training were sufficiently low that promoting a worker without any additional information about their type is still profitable:

$$c < \pi_C P(C) \equiv c_{min}$$

In this case, the firm would promote every worker. Furthermore, if the cost of training were sufficiently high such that setting the threshold at the value of $\underline{y}$ that maximizes $P(C|\underline{y})$, does not generate a high enough probability that workers producing this signal are of Type C, then promoting no one, regardless of their signal, maximizes firm profit. This occurs if

$$c > \pi_C P(C|\underline{y}), \forall \underline{y}$$

or, equivalently, if $\frac{\pi_C}{c} - 1 < \frac{g\left(\underline{y} - R_1\alpha(0)\right)}{g\left(\underline{y} - h_1^*(\theta_C;\underline{y})\right)}$for all promotion standards $\underline{y}$. The right-hand side of this inequality attains its minimum at the threshold $\underline{y}^{max}$ such that $h_1^*(\theta_C;\underline{y}^{max}) = \underline{y}^{max}$. This is true when

$$\underline{y}^{max} = \frac{A}{\sigma\sqrt{2\pi}} + R_1\alpha(0)$$

Note that when $\underline{y} < h_1^*(\theta_C;\underline{y})$, $h_1^*(\theta_C;\underline{y})$ is increasing in $\underline{y}$, when $\underline{y} > h_1^*(\theta_C;\underline{y})$, $h_1^*(\theta_C;\underline{y})$ is decreasing in $\underline{y}$, and when $\underline{y} = h_1^*(\theta_C;\underline{y})$, $h_1^*(\theta_C;\underline{y})$ is the maximum of the Type C worker's best response function, as a function of the promotion standard, $\underline{y}$. This maximum number of hours from a Type C worker is attained by $\underline{y}^{max}$. In other words, if the firm chooses the threshold that maximizes the the number of hours worked by the Type C worker, thereby maximizing the difference between the means of the Type B and Type C signals, and promoting a worker who meets this threshold is still not profitable, there is no threshold that the firm can set to distinguish profitable promotions from unprofitable ones. No worker will be promoted for

$$c > c_{max} = \frac{\pi_C}{\left(1 + g\left(\frac{A}{\sigma_\varepsilon\sqrt{2\pi}}\right)\sigma\sqrt{2\pi}\frac{P(A)+P(B)}{P(C)}\right)}$$

The quantity $P(C|\underline{y}) = \frac{g\left(\underline{y} - h_1^*(\theta_C;\underline{y})\right)P(C)}{g\left(\underline{y} - h_1^*(\theta_C;\underline{y})\right)P(C) + g\left(\underline{y} - R_1\alpha(0)\right)(P(B)+P(A))}$ is increasing in $\underline{y}$ for the feasible range of the equilibrium threshold $\underline{y}^*$, $\left(\underline{y}^{min}, \underline{y}^{max}\right)$, a range guaranteed by firm costs, $c\epsilon\,(c_{min}, c_{max})$.[30] $\pi_C P(C|\underline{y}^{min}) < c$ and $\pi_C P(C|\underline{y}^{max}) > c$. Therefore, the equilibrium $\underline{y}^*$ exists for $c\epsilon\,[c_{min}, c_{max}]$.

[30] This is because $\frac{g\left(\underline{y} - h_1^*(\theta_C;\underline{y})\right)}{g\left(\underline{y} - R_1\alpha(0)\right)}$ is increasing in $\underline{y}$ for all $\underline{y}\epsilon\left(\underline{y}^{min}, \underline{y}^{max}\right)$ and decreasing in $\underline{y}$ for all $\underline{y}\epsilon\left(0, \underline{y}^{min}\right)$, for some $\underline{y}^{min} < R_1\alpha(0)$. If $c > c_{min}$, $\pi_C P(C) < c$, and therefore, $\pi_C P(C|\underline{y}) < c$ for all $\underline{y}\epsilon\left[0, \underline{y}^{min}\right]$ as well, so it must be that the equilibrium threshold satisfies $\underline{y}^* > \underline{y}^{min}$. $c < c_{max}$ guarantees $\underline{y}^* < \underline{y}^{max}$. In other words, $\underline{y}^*$will not be less than $\underline{y}^{min}$ nor greater than $\underline{y}^{max}$ because we have specified a problem with firm costs such that the signal is informative to the firm's decision.

Figure 9: Type C Worker's Best Response to Promotion Standard $\underline{y}$

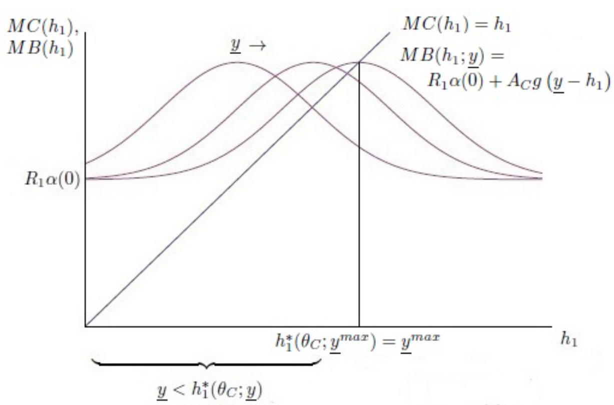


Figure 10: (a) Solution to First-Order Condition for $\underline{y} < \underline{y}^{max}$

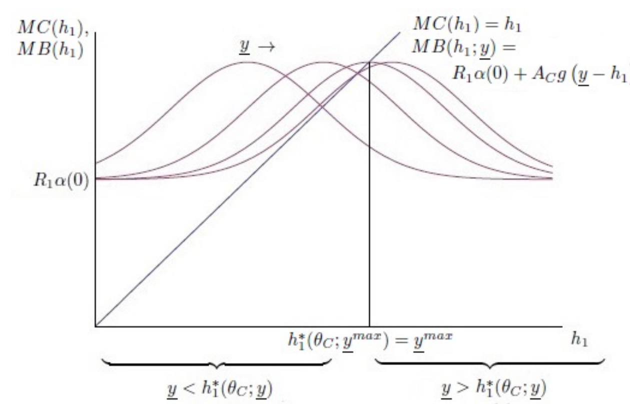
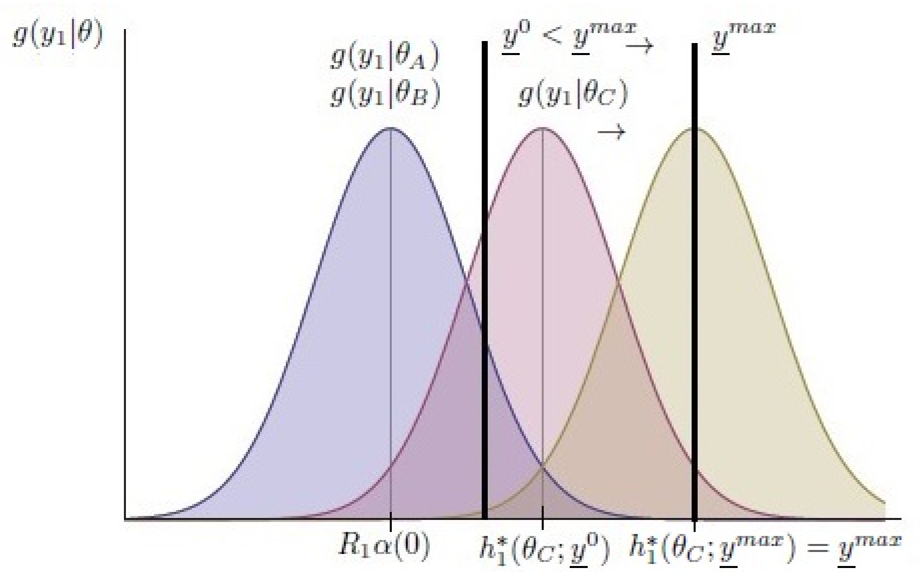


Figure 11: (b) Solution to First-Order Condition for $\underline{y} > \underline{y}^{max}$

Figure 12: Response of Distribution of Observed Worker Productivity to Promotion Standard Shift from $\underline{y}^0$ to $\underline{y}^{max}$

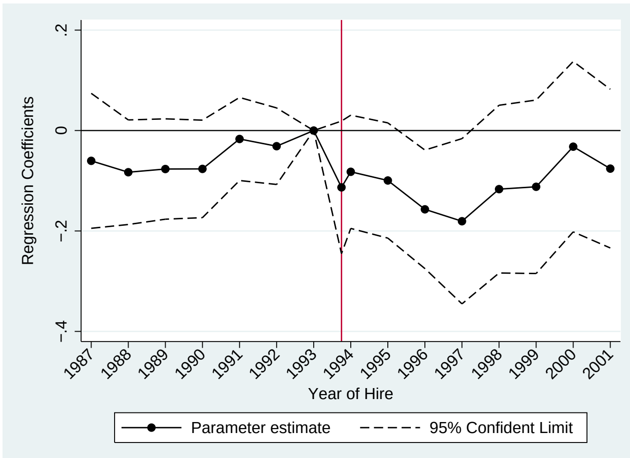


Uniqueness comes from the fact that $\pi_C P(C|\underline{y})$ is strictly increasing in $\underline{y}$ for all $\underline{y}\epsilon\left(\underline{y}^{min}, \underline{y}^{max}\right)$.

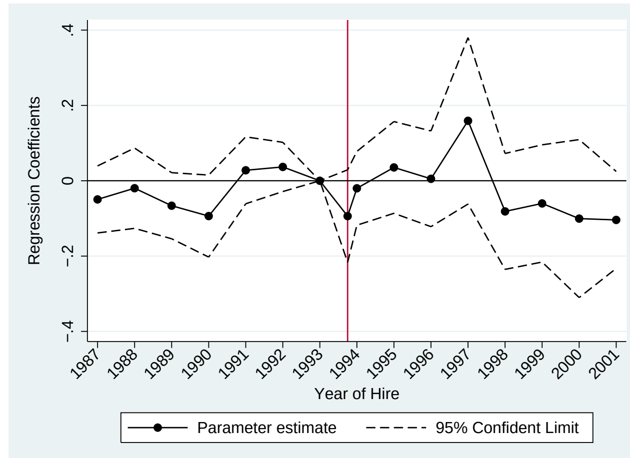
8 Appendix B: Robustness Checks

In this Appendix, I present a series of robustness checks to the results described in section 3. First, I show that the control groups are unaffected by the policy change; in control states, women under the age of 40 hired after the mandate face similar likelihood of promotion as those hired before. Furthermore, while women under the age of 40 face a lower likelihood of promotion when hired after the enactment of the FMLA, women over the age of 40 are unaffected in comparison to men. Second, I show that the change in the likelihood of promotion is not driven by the effect of the mandate on a given type of worker. Table 13 shows results for the change in the probability of promotion when we restrict to observations from survey years after the FMLA, still exploiting the variation in whether the respondent was hired before or after its enactment. Table 14 and Figure 14 demonstrate that the results are not driven by the inclusion of state- or demographic-specific time fixed effects. To confirm that differential year trends do not drive the results, I show in Tables 15 - 17 results including state-specific, demographic-specific, and state-demographic-specific trends in the year of hire, separately for each control group. While men under the age of 40 do witness a slight time trend in that women under the age of 40 have an increasing likelihood of being promoted over time relative to men under 40, the enactment of the FMLA still retards that relative growth for women under the age of 40. Tables 18 and Figure 15 address the concern about the impact of the FMLA on a given type and demonstrate that there appears to be little effect when restricting the sample to those who were hired before the FMLA.

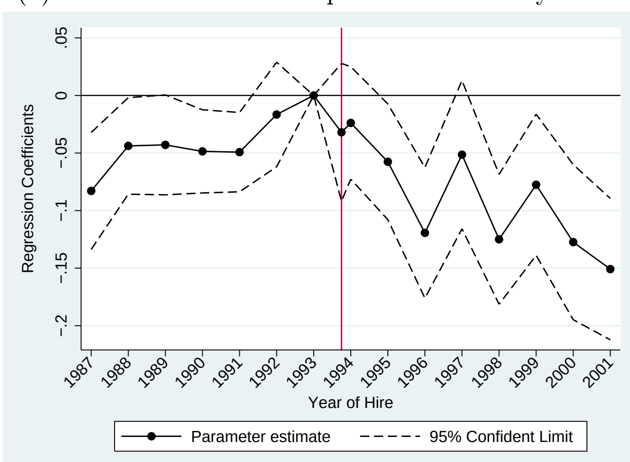
Figure 13: Effect of Mandate on Gender Differences in Promotion Rates



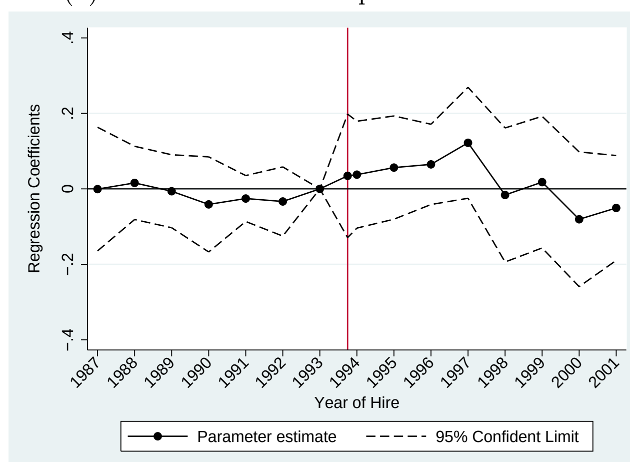
(a) Women Under 40 Compared to Men Only



(b) Women Over 40 Compared to Men



(c) Treatment States



(d) Control States

Notes: The coefficients $\beta_{\tau,4}$ and 95% confidence intervals reported are obtained from estimating $promoted_{i,j,s,t} = \sum_{t=1988}^{2001} \beta_{t,1}(SurveyYeart_t \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s)$ $+\beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \sum_{t=1988}^{2001} \beta_{t,3}(SurveyYeart_t \cdot TreatmentState_s)$ $+\sum_{\tau=1987}^{2001} \beta_{\tau,4}(HiredYear\tau_{i,j} \cdot FemaleUnder40_{i,t}) + \sum_{\tau=1987}^{2001} \beta_{\tau,5} HiredYear\tau_{i,j}$ $+\beta_6 FemaleUnder40_{i,t} + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \delta_{i,t,s} + \varepsilon_{i,j,s,t}$, where the sample in Panel (c) is restricted to treatment states only, and the sample in panel (d) is restricted to control states only. Standard errors are clustered at the state level. The sample is restricted to respondents hired in 1987 or later. The regression controls for sex, age, age squared, tenure, tenure squared, level of education attained, marital status, marital status interacted with sex, marital status interacted with FemaleUnder40, and a dummy for nonwhite. The regression also controls for job characteristics (private firm, union), 24 standard occupational categories, and 12 standard industry categories from the census occupational classification system.

Table 11: Probability of Promotion Since Hire, Compared to Men Under 40

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.08***	-0.11***	-0.10***	-0.10***	-0.10***
	[0.023]	[0.025]	[0.025]	[0.025]	[0.025]
Female Under 40* Treatment State	0.04**	0.05***	0.05***	0.05***	0.05***
	[0.014]	[0.017]	[0.016]	[0.017]	[0.017]
Hired After*Treatment State	0.07***	0.08***	0.08***	0.08***	0.08***
	[0.024]	[0.027]	[0.026]	[0.025]	[0.025]
Hired After*Female Under 40	0.01	0.04	0.04*	0.03*	0.04*
	[0.019]	[0.021]	[0.021]	[0.021]	[0.020]
Female Under 40	-0.03**	-0.05**	-0.05***	-0.05**	-0.05***
	[0.016]	[0.018]	[0.017]	[0.017]	[0.017]
Hired After	-0.00	-0.01	-0.01	-0.01	-0.01
	[0.023]	[0.027]	[0.026]	[0.026]	[0.025]
Treatment State	0.03	0.06	0.02	0.02	0.02
	[0.042]	[0.071]	[0.060]	[0.059]	[0.060]
Tenure	0.05***	0.06***	0.05***	0.05***	0.05***
	[0.002]	[0.003]	[0.003]	[0.003]	[0.003]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	29,558	25,353	25,295	25,295	25,295
Individual Fixed Effects	7,218	6,775	6,770	6,770	6,753
Mean	0.06	0.06	0.06	0.06	0.06

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The sample here is restricted to individuals under the age of 40 whose employment spell began in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 12: Probability of Promotion Since Hire, Compared to Women Over 40

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.08***	-0.11***	-0.10***	-0.10***	-0.10***
	[0.023]	[0.025]	[0.025]	[0.025]	[0.025]
Female Under 40* Treatment State	0.04**	0.05***	0.05***	0.05***	0.05***
	[0.014]	[0.017]	[0.016]	[0.017]	[0.017]
Hired After*Treatment State	0.07***	0.08***	0.08***	0.08***	0.08***
	[0.024]	[0.027]	[0.026]	[0.025]	[0.025]
Hired After*Female Under 40	0.01	0.04	0.04*	0.03*	0.04*
	[0.019]	[0.021]	[0.021]	[0.021]	[0.020]
Female Under 40	-0.03**	-0.05**	-0.05***	-0.05**	-0.05***
	[0.016]	[0.018]	[0.017]	[0.017]	[0.017]
Hired After	-0.00	-0.01	-0.01	-0.01	-0.01
	[0.023]	[0.027]	[0.026]	[0.026]	[0.025]
Treatment State	0.03	0.06	0.02	0.02	0.02
	[0.042]	[0.071]	[0.060]	[0.059]	[0.060]
Tenure	0.05***	0.06***	0.05***	0.05***	0.05***
	[0.002]	[0.003]	[0.003]	[0.003]	[0.003]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	29,558	25,353	25,295	25,295	25,295
Individual Fixed Effects	7,218	6,775	6,770	6,770	6,753
Mean	0.06	0.06	0.06	0.06	0.06

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The sample here is restricted to females whose employment spell began in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 13: Probability of Promotion Since Hire, Restricted to Survey Dates August 1993 -2001

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.07***	-0.09***	-0.09***	-0.09***	-0.09***
	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]
Female Under 40* Treatment State	0.02	0.02	0.02	0.02	0.02
	[0.017]	[0.020]	[0.020]	[0.020]	[0.020]
Hired After*Treatment State	0.02*	0.03**	0.03**	0.03**	0.03**
	[0.012]	[0.013]	[0.014]	[0.014]	[0.013]
Hired After*Female Under 40	0.03	0.06**	0.06***	0.06***	0.06**
	[0.022]	[0.022]	[0.021]	[0.021]	[0.021]
Female Under 40	-0.01	-0.00	-0.00	-0.00	-0.00
	[0.020]	[0.022]	[0.023]	[0.022]	[0.022]
Hired After	0.00	-0.01	-0.01	-0.01	-0.01
	[0.010]	[0.011]	[0.011]	[0.011]	[0.011]
Treatment State	-0.05	-0.01	0.05	0.01	-0.00
	[0.043]	[0.053]	[0.056]	[0.059]	[0.061]
Tenure	0.04***	0.05***	0.05***	0.05***	0.05***
	[0.002]	[0.002]	[0.002]	[0.002]	[0.003]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
State*Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	34,202	29,088	28,992	28,992	28,798
Individual Fixed Effects	11,122	10,249	10,228	10,228	10,197
Mean	0.09	0.09	0.09	0.09	0.09

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients reported are the estimated coefficients from the following equation:

$$\begin{aligned} promoted_{i,j,s,t} &= \beta_1(HiredAfter_{i,j} \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(HiredAfter_{i,j} \cdot TreatmentState_s) \\ &+ \beta_4(HiredAfter_{i,j} \cdot FemaleUnder40_{i,t}) + \beta_5 HiredAfter_{i,j} + \beta_6 FemaleUnder40_{i,t} \\ &+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \varepsilon_{i,j,s,t} \end{aligned}$$

The sample is restricted to observations from August of 1993 or later and respondents whose employment spell began in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 14: Probability of Promotion Since Hire, Without State- or Demographic-Specific Time Fixed Effects

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.03*	-0.05**	-0.05**	-0.05**	-0.05**
	[0.019]	[0.021]	[0.021]	[0.021]	[0.021]
Female Under 40* Treatment State	0.02**	0.02**	0.02**	0.02**	0.02**
	[0.008]	[0.009]	[0.009]	[0.009]	[0.009]
Hired After*Treatment State	0.02**	0.03**	0.03**	0.03**	0.03**
	[0.011]	[0.013]	[0.013]	[0.013]	[0.013]
Hired After*Female Under 40	0.02	0.04**	0.04***	0.04***	0.04**
	[0.013]	[0.014]	[0.014]	[0.014]	[0.014]
Female Under 40	-0.04**	-0.04**	-0.04**	-0.04**	-0.04**
	[0.016]	[0.018]	[0.017]	[0.017]	[0.017]
Hired After	-0.01	-0.02	-0.02	-0.02	-0.02
	[0.010]	[0.011]	[0.012]	[0.012]	[0.011]
Treatment State	0.01	0.08	0.02	0.02	0.03
	[0.031]	[0.052]	[0.032]	[0.033]	[0.034]
Tenure	0.05***	0.05***	0.05***	0.05***	0.05***
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	57,269	49,108	48,987	48,987	48,637
Individual Fixed Effects	13,913	12,904	12,889	12,889	12,857
Mean	0.07	0.07	0.07	0.07	0.07

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients reported are the estimated coefficients from the following equation:

$$\begin{aligned} promoted_{i,j,s,t} &= \beta_1(HiredAfter_{i,j} \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(HiredAfter_{i,j} \cdot TreatmentState_s) \\ &+ \beta_4(HiredAfter_{i,j} \cdot FemaleUnder40_{i,t}) + \beta_5 HiredAfter_{i,j} + \beta_6 FemaleUnder40_{i,t} \\ &+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \varepsilon_{i,j,s,t} \end{aligned}$$

The sample is restricted to respondents hired in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Figure 14: Effect of Mandate on Promotion Rate of Women Under 40

Without State- or Demographic-Specific Time Fixed Effects

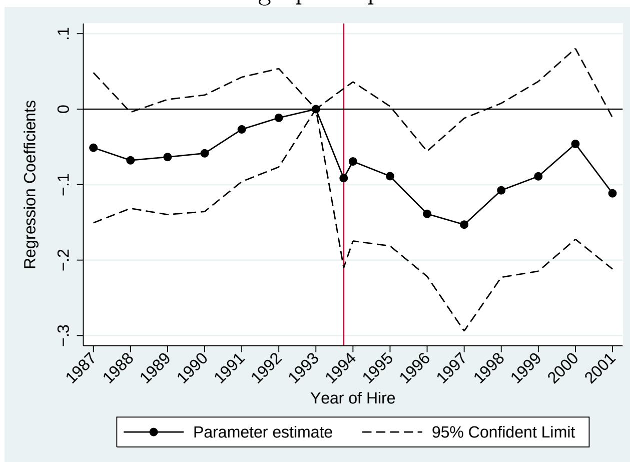


Table 15: Probability of Promotion Since Hire, With Time Trends in the Year of Hire

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40*	-0.06*	-0.09***	-0.09***	-0.09***	-0.08***
Treatment State	[0.030]	[0.032]	[0.032]	[0.032]	[0.031]
Female Under 40*	0.01	0.01	0.01	0.01	0.01
Treatment State*Year Hired	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Female Under 40*	-0.00	-0.00	-0.00	-0.00	-0.00
Year Hired	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Female*	0.01***	0.01***	0.01***	0.01***	0.01***
Year Hired	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Year Hired	-0.03***	-0.03***	-0.03***	-0.03***	-0.03***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
State*Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	57,269	49,108	48,987	48,987	48,637
Individual Fixed Effects	13,913	12,904	12,889	12,889	12,857
Mean	0.07	0.07	0.07	0.07	0.07

Robust standard errors in brackets, clustered at the state level
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This sample is restricted to respondents hired in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 16: Probability of Promotion Since Hire, With Time Trends in the Year of Hire Among Women Only

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.09**	-0.12***	-0.12***	-0.12***	-0.12***
	[0.042]	[0.040]	[0.041]	[0.040]	[0.040]
Female Under 40* Treatment State*Year Hired	-0.00	-0.00	-0.00	-0.00	-0.00
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Female Under 40* Year Hired	-0.01	-0.01	-0.02	-0.02	-0.02
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
Year Hired	-0.01	-0.01*	-0.02*	-0.02**	-0.02*
	[0.007]	[0.008]	[0.008]	[0.008]	[0.008]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
State*Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	29,558	25,353	25,295	25,295	25,118
Individual Fixed Effects	7,218	6,775	6,770	6,770	6,753
Mean	0.06	0.06	0.06	0.06	0.06

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: This sample is restricted to women hired in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 17: Probability of Promotion Since Hire, With Time Trends in the Year of Hire Among Men and Women Under 40 Only

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Hired After*Female Under 40* Treatment State	-0.05*	-0.08**	-0.08**	-0.08**	-0.07**
	[0.030]	[0.033]	[0.032]	[0.033]	[0.032]
Female Under 40* Treatment State*Year Hired	0.00	0.00	0.00	0.00	0.00
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
Female Under 40* Year Hired	-0.00	-0.00	-0.00	-0.00	-0.00
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Year Hired	-0.03***	-0.03***	-0.03***	-0.03***	-0.03***
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
State*Year Hired Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	47,662	41,323	41,219	41,219	40,922
Individual Fixed Effects	12,095	11,251	11,234	11,234	11,202
Mean	0.08	0.08	0.08	0.08	0.08

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: This sample is restricted to respondents under the age of 40 hired in 1987 or later. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 18: Probability of Promotion Since Hire, Restricted to Those Hired Before Aug. 1993

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Post-Period*Female Under 40* Treatment State	0.04**	0.04*	0.04*	0.04*	0.04*
	[0.017]	[0.020]	[0.020]	[0.020]	[0.020]
Female Under 40*Treatment State	-0.00	0.00	0.01	0.01	0.01
	[0.010]	[0.012]	[0.013]	[0.013]	[0.013]
Post-Period*Treatment State	-0.04***	-0.05***	-0.05***	-0.05***	-0.05***
	[0.015]	[0.016]	[0.017]	[0.017]	[0.017]
Post-Period*Female Under 40	-0.02	-0.03	-0.03	-0.03	-0.03
	[0.016]	[0.018]	[0.018]	[0.018]	[0.019]
Female Under 40	-0.04*	-0.04*	-0.04	-0.04	-0.04*
	[0.023]	[0.025]	[0.024]	[0.025]	[0.025]
Female	0.00	0.00	0.00	0.00	0.00
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Post-Period	0.01	0.02	0.02	0.02	0.02
	[0.013]	[0.015]	[0.015]	[0.015]	[0.015]
Treatment State	0.10**	0.12***	0.23***	0.23***	0.13***
	[0.041]	[0.045]	[0.061]	[0.061]	[0.044]
Tenure	0.04***	0.05***	0.05***	0.05***	0.05***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Tenure Squared	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	39,067	33,529	33,456	33,456	33,206
Individual Fixed Effects	10,194	9,284	9,273	9,273	9,249
Mean	0.11	0.11	0.11	0.11	0.11

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: The coefficients reported are the estimated coefficients from the following equation:

$$\begin{aligned} promoted_{i,j,s,t} &= \beta_1(PostPeriod_t \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(PostPeriod_t \cdot TreatmentState_s) \\ &+ \beta_4(PostPeriod_t \cdot FemaleUnder40_{i,t}) + \beta_5 PostPeriod_t + \beta_6 FemaleUnder40_{i,t} \\ &+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \varepsilon_{i,s,t} \end{aligned}$$

This sample is restricted respondents hired between 1987 and August of 1993. "Post-period" is an indicator variable equal to 1 if the survey date took place after August of 1993. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Table 19: Probability of Promotion Since Hire, Restricted to Those Hired Before Aug. 1993 With Year Trends

VARIABLES	(1) Promoted	(2) Promoted	(3) Promoted	(4) Promoted	(5) Promoted
Post-Period*Female Under 40* Treatment State	0.01	0.01	0.01	0.01	0.01
	[0.020]	[0.021]	[0.020]	[0.020]	[0.021]
Female Under 40* Treatment State*Survey Year	0.01**	0.01**	0.01**	0.01**	0.01**
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Female Under 40* Survey Year	-0.00	-0.00	-0.00	-0.01	-0.00
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Survey Year	0.02***	0.02***	0.02***	0.02***	0.02***
	[0.004]	[0.005]	[0.005]	[0.005]	[0.005]
State-Specific Year Trends	Yes	Yes	Yes	Yes	Yes
Survey Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Characteristics		Yes	Yes	Yes	Yes
Occupation Controls			12	12	24
Industry Controls					8
Observations	39,067	33,529	33,456	33,456	33,206
Individual Fixed Effects	10,194	9,284	9,273	9,273	9,249
Mean	0.11	0.11	0.11	0.11	0.11

Robust standard errors in brackets, clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

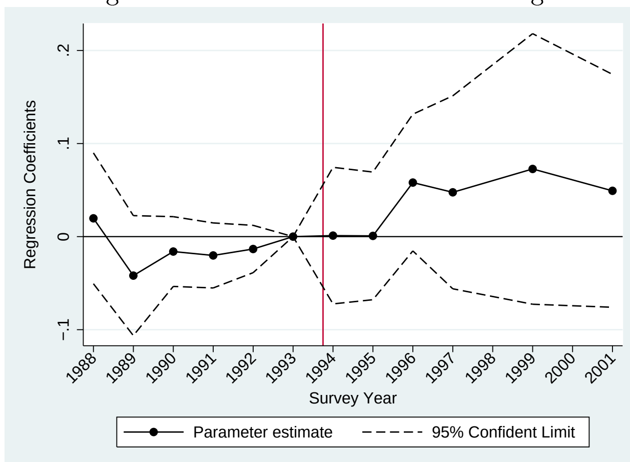
Notes: The coefficients reported are the estimated coefficients from the following equation:

$$\begin{aligned} promoted_{i,j,s,t} &= \beta_1(PostPeriod_t \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s) \\ &+ \beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \beta_3(PostPeriod_t \cdot TreatmentState_s) \\ &+ \beta_4(PostPeriod_t \cdot FemaleUnder40_{i,t}) + \beta_5 PostPeriod_t + \beta_6 FemaleUnder40_{i,t} \\ &+ \beta_7 TreatmentState_s + \beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \varepsilon_{i,s,t} \end{aligned}$$

This sample is restricted respondents hired between 1987 and August of 1993. "Post-period" is an indicator variable equal to 1 if the survey date took place after August of 1993. All regressions control for age, age squared, tenure, tenure squared, education level, marital status, marital status interacted with sex, marital status interaction with a dummy for female under 40, and a dummy for nonwhite. The excluded education level is high school graduate. All regressions also include year, state, and individual-fixed effects. The dependent variable is whether the respondent has been promoted since the time of hire. Column (2) includes controls for whether the respondent was a member of a union and whether the employer was private. Occupations were divided into a set of 12 standard categories in Column (3) and 12 categories reflecting variation in human capital depreciation in Column (4). Column (5) includes both 24 standard occupation divisions and 8 standard industry divisions.

Figure 15: Gender Differences in Promotions in the Pre- and Post-Mandate Periods

Among Women Under 40 Hired Before Aug. 1993



Notes: The coefficients $\beta_{\tau,1}$ and 95% confidence intervals reported are obtained from estimating
$promoted_{i,j,s,t} = \sum_{t=1988}^{2001} \beta_{t,1}(SurveyYeart_t \cdot FemaleUnder40_{i,t} \cdot TreatmentState_s)$
$+\beta_2(FemaleUnder40_{i,t} \cdot TreatmentState_s) + \sum_{t=1988}^{2001} \beta_{t,3}(SurveyYeart_t \cdot TreatmentState_s)$
$+\sum_{t=1988}^{2001} \beta_{t,4}(SurveyYeart_t \cdot FemaleUnder40_{i,t}) + \beta_6 FemaleUnder40_{i,t} + \beta_7 TreatmentState_s$
$+\beta_8 X_{i,t} + \beta_9 Z_j + \lambda_t + c_s + f_i + \varepsilon_{i,s,t}$.
Standard errors are clustered at the state level. The sample is restricted to respondents hired in August of 1993 or later. The regression controls for sex, age, age squared, tenure, tenure squared, level of education attained, marital status, marital status interacted with sex, marital status interacted with FemaleUnder40, and a dummy for nonwhite. The regression also controls for job characteristics (private firm, union), 24 standard occupational categories, and 12 standard industry categories from the census occupational classification system.