SCREENING FOR COMMITMENT:
THE EFFECT OF MATERNITY LEAVE USE ON WAGES

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Abstract

This paper examines whether firms use participation in work-family policies to screen for a worker’s level of job commitment, an unobservable characteristic that affects productivity. We identify two conditions that differentiate screening from human capital explanations, and test whether the wage penalty associated with usage varies with these conditions. Specifically, we propose that the wage growth penalty from policy usage will increase with monitoring costs and with the quality of the screening technology if firms screen based on usage. We test and provide support for these propositions using the NLSY 1979, with paid maternity leave as the work-family policy of interest. We proxy for monitoring costs using a measure of job autonomy and capture a change in the screen’s quality with passage of the Family and Medical Leave Act (FMLA).
JEL: D82, J16, M50
Keywords: work-family policies, asymmetric information, statistical discrimination

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Acknowledgments: We would like to thank Andrea Ichino, Edward Lazear, and Kathryn Shaw for their comments and suggestions as well as other participants in the NBER summer workshop on empirical personnel economics.
During the last four decades there has been a dramatic increase in the labor force participation of women, particularly among married women (Juhn and Potter, 2006). However, retaining women in the labor force in a continuous, full-time capacity continues to be a challenge because women engage in a disproportionate amount of unpaid labor, such as child care and household management (Cohen and Bianchi, 1999; Gornick and Meyers, 2003; Drago, 2009). Some firms have adopted work-family policies (e.g. parental leave, compressed work week, flexible schedules) with the goal of promoting work-life balance for workers, but the utilization of these policies has been low (Blair-Loy and Wharton, 2004; Kelly and Moen, 2007). Although some workers appear to benefit from these policies (Weeden, 2005), other empirical studies and anecdotal accounts suggest that workers are hesitant to use family-friendly policies because policy use carries negative career consequences (Schwartz 1989; Hochschild, 1997; Crittenden, 2001; Glass, 2004; Bardasi and Gornick, 2008). These negative career consequences may stem from supervisors using participation in work-family policies as a way to differentiate – or screen – workers, and inferring that those who use work-family policies lack work commitment and should not be “fast-tracked.”

The purpose of this paper is to examine this conjecture: Do firms screen workers based on participation in work-family policies? We examine whether policy usage negatively affects wage growth, which we use as a measure of career advancement, and if the nature of the effect is consistent with screening. Evidence that policy usage is associated with lower wage growth, however, is not enough to show that firms screen based on participation in work-family policies serves as a screening device because these outcomes could also be explained by human capital theory. Namely, if participation in work-family policies leads to time out of the labor force or reduced hours, then participation could result in reduced investment in or depreciation of human capital. In order to differentiate screening from human capital, we identify two conditions under which screening, but not human capital depreciation, explains
why participation in work-family policies constrains wage growth. We then test if the wage growth penalty from policy usage is largest under these conditions.

To identify the conditions, we develop a model of asymmetric information in which firms decide whether to infer a worker’s level of job commitment, an unobservable characteristic that affects productivity, by directly monitoring work behaviors or through screening based on work-family policy usage. We make the cost of observing a worker characteristic, which is implicitly prohibitive in standard models of asymmetric information (e.g., Spence 1973), an explicit parameter of the model. Our model predicts that firms are more likely to screen workers based on policy usage when monitoring is costly and when the screen is more informative (i.e., higher quality). Empirically, these propositions imply that the wage growth penalty associated with participation should increase with monitoring costs and with the quality of the screening technology.

To test these propositions, we examine the effect of taking paid maternity leave on wage growth. We use the National Longitudinal Study of Youth, 1979 (NLSY79) and operationalize wage growth as the effect on the slope of the wage-experience profile. We proxy for differences in monitoring costs using a measure of job autonomy because workers with high autonomy are more difficult to monitor. We capture differences in the quality of the information contained in the screen using the passage of the Family and Medical Leave Act (FMLA) in 1993. Before FMLA, highly committed women were less likely to take maternity leave because firms were not required to guarantee post-leave employment. Thus, policy use was a higher quality indicator of work commitment (or less noisy) prior to FMLA.

We find that the wage penalty associated with leave taking is concentrated among workers with high job autonomy prior to the passage of FMLA, which is consistent with screening, but not human capital explanations. The estimated wage growth penalty for women with high job autonomy (e.g., professionals) prior to FMLA was
3.9 percentage points relative to women who did not take leave. The results are robust to specifications that proxy for differences in monitoring costs using job type (i.e. white-collar workers are more difficult to monitor than blue-collar workers) instead of job autonomy, as well as to specifications that restrict the analysis to women in their prime child-bearing years. In all, this paper demonstrates that work-family policies provide an avenue for employers to screen workers, which may explain low utilization rates of work-family policies among workers.

These findings contributes to the literature by developing a new method for differentiating human capital from screening explanations that uses testable implications derived from an economic model. In particular, we build on the career interruptions literature, which has likewise debated the merits of screening versus human capital depreciation as explanations for why career interruptions constrain wages (Jacobsen and Levin, 1995; Baum, 2002; Hotchkiss and Pitts, 2007). Previous work has sought to differentiate the two competing theories by examining whether the wage penalty from career interruptions varies by type of leave, namely parental leave versus other types of leave (Albrecht et al., 1999; Buligescu et al., 2008). These studies find more severe wage penalties for parental leave: men who take parental leave experience slower wage growth relative to those who took other types of leave (Albrecht et al. 1999), and the wage growth penalty associate with maternity leave lasts longer than that associated with other types of leave (Buligescu et al., 2008). Hence, these papers suggest that the wage penalty from parental leave cannot be fully explained by human capital theory and suggest that firms may use parental leave as an indicator of low career commitment. However, the empirical strategy used by these papers is only applicable to leave taking, which is only one of many types of work-family policies. In addition, both papers examine the effect of policy usage in two countries with extensive parental leave policies (Sweden and Germany), which may limit the applicability of the results to the United States, in which parental leave polices are
less generous.

We extend the existing literature by deriving a more general method for evaluating whether firms use participation in work-family policies to make inferences about a worker’s commitment that can be applied to a variety of work-family policies, including parental leave. Specifically, we demonstrate that the wage growth penalty associated with paid maternity leave is determined by two parameters, monitoring cost and information quality, and find evidence consistent with a screening explanation using a United States sample. The larger penalty for workers with higher monitoring costs could be explained by human capital theory if, for example, there are differences in depreciation rates by this job characteristic, yet human capital theory does not generate predictions based on information quality. Thus, human capital theory alone cannot fully explain the wage penalty associated with paid maternity leave in the United States.

The rest of the paper is organized as follows. In Section 1 we outline the model and list the propositions derived from the comparative statics, which are evaluated in the Appendix. Section 2 describes how we implement the propositions empirically for the case of paid maternity leave and gives the empirical specification. The data and sample restriction are described in Section 3. Section 4 presents and discusses the results and provides robustness checks, while Section 5 concludes the paper.

1 COMMITMENT AND WORKER SCREENING

1.1 Conceptual Framework

In this paper, job commitment is a characteristic of the worker that affects productivity. Commitment affects a worker’s behavior on the job through a willingness to put forth extra work effort, such that workers with high commitment are willing to sacrifice their time and energy for the betterment of the firm, or in general, attain
the “ideal worker” norm (Acker, 1990; Hays, 1996; Williams, 2000). Not only are workers with high commitment less costly to the firm because they have lower absenteeism and higher retention rates, they also generate value by applying extra work effort that results in innovation and high quality work (Mathieu and Zajac, 1990). Thus, commitment affects productivity, although it is not equivalent to output.

We assume that workers are heterogeneous in their level of job commitment. Firms want to identify high-commitment workers and position them in roles that maximize the value of their marginal product, shuttling them into higher-level positions within the firm by way of the “fast track.” This career advancement is associated with steeper earnings profiles, or higher wage growth. On the other hand, workers who are classified as low-commitment, receive fewer opportunities for advancement and are concentrated among lower levels of the firm (Landers et al., 1996; Weeden, 2005). The lack of advancement results in lower wage growth for workers identified as having low commitment.

However, a worker’s commitment level may be difficult for the firm to observe. If the level of commitment is known by the worker, but not by the employer, the firm is at an informational disadvantage. This informational asymmetry may cause the party with the informational disadvantage (the firm) to engage in behaviors to elicit the worker’s type – high commitment or low commitment – to efficiently distribute workers to jobs. Because commitment affects a worker’s behavior through a willingness to put forth extra work effort, a firm could learn about a worker’s commitment level by monitoring work effort, or alternatively, it could make a judgment about a worker’s commitment level based on more a readily observable behavior.\footnote{Because the firm (uninformed party) engages in a behavior to infer the worker’s type (informed party), we employ a screening model instead of a signaling model. However, we often refer to the information captured by policy usage as an indicator or signal of the worker’s type.}

Participation in work-family policies could be used by firms to screen for worker commitment because, by participating in these policies, the worker reveals to the
firm a competing priority for his/her time and effort. While the intention of work-family policies may have been to allow workers to pursue both career and family goals, supervisors could use participation in these policies as a means of assessing the commitment level of workers and allocate these workers to “second-tier” jobs, or jobs with fewer opportunities for advancement (Mason and Ekman, 2007). In what follows, we develop a model that allows for the possibility that firms use participation in work-family policies as an indicator of low commitment.

1.2 Theoretical Framework

Using a model of asymmetric information regarding a worker’s level of commitment, we generate propositions regarding the pattern of wage growth penalties that would be expected if firms screen workers for low commitment based on policy usage. We extend a basic screening model by making the cost of learning the workers’ characteristics explicit. We demonstrate that changes in key parameters, namely the cost of directly learning the characteristic through monitoring work behaviors and the quality of the screen (i.e., policy use), affect the propensity to screen versus monitor. Our theoretical framework is related to the model developed by Huang and Cappelli (2006) and by Landers et al. (1996), in which a firm screens applicants based on work ethic or willingness to work long hours, respectively; however, we build on these models by using participation in work-family policies as the screening mechanism. We do not explicitly model whether firms offer work-family policies such as paid maternity leave solely to screen workers, or whether they make leave available to help workers improve work-life balance, but then end up using participation to screen workers. The latter represents a case in which a personnel department institutes paid leave, but then supervisors use leave to differentiate workers.\(^2\)

\(^2\)Through our discussions with personnel departments, we found that this is a common scenario and future work should attempt to explicitly model this dynamic.
are employed by the firm. Workers are assumed to be heterogeneous in their level of commitment, θ_i, which is either low (θ_L) or high (θ_H) with 0 ≤ θ_L < θ_H ≤ 1. A worker’s type is known to the worker, but unknown to the firm; the fraction of the workforce that has high commitment is λ. A worker’s commitment level affects the disutility of working, or applying effort, such that this cost is higher for θ_L-types. The worker decides whether to participate in the work-family policy, where p is an indicator variable for the participation decision, which reduces the cost of effort. The worker’s utility is given by:

$$U(p, \theta) = w - c(p, \theta)$$ (1)

where w is wage income and c(p, θ) is the disutility from working. The properties of c(p, θ) are as follows: \(c_\theta(p, \theta) < 0\), \(c_p(p, \theta) < 0\), and \(c_{p,\theta}(p, \theta) > 0\). These properties imply that participation reduces the disutility from working by more for low-types relative to high-types, therefore, participation in the work-family policy potentially provides a way for the firm to differentiate the two types. The structure of the costs in the utility function implies that firms may use policy participation as an indicator of low commitment.

In the first period all workers start working in job 1, which has a production function that does not depend on θ: \(f_1(\theta) = h\). In the second period the firm can promote a subset of workers into job 2 in which productivity does depend on θ: \(f_2(\theta) = g(\theta)h\). We assume that \(g(\theta_H)h > h > g(\theta_L)h\) so that it is only efficient to promote workers with high commitment. In the first period, the firm can monitor a worker’s behavior at a cost of m, which may include the detailed documentation of tasks and time taken to complete tasks, to learn about a worker’s type. Alternatively, the firm can infer a worker’s type based on work-family policy usage. Let \(\phi_H\) be the probability that a \(\theta_H\)-type uses the work-family policy and \(\phi_L\) be the probability a \(\theta_L\)-
type uses. The screening technology is binary; therefore, if the firm uses participation in the policy as an indicator of type, all users must be classified as $\theta_L$-types. The quality of the information captured in the screening mechanism is given by $s = \frac{\phi_H}{\phi_H + \phi_L}$. If the screen were perfect ($\phi_H = 0$ and thus $s = 1$), then all users would be $\theta_L$-types. The quality of screen decreases (or becomes more “noisy”) as $\phi_H$ increases. Stated differently, the variance of $\theta$ conditional on usage increases with $\phi_H$.

The timing of the model is as follows. The firm employs a continuum of workers and allocates them to job 1 with a wage of $w_1$. The firm (or supervisor at the firm) decides whether it will screen for commitment via policy usage or whether it will detect commitment only through monitoring. Note that if the firm decides to screen by inferring that all policy users are of low commitment, it will still need to monitor non-participants. Following the firm’s decision, workers decide whether to participate in the policy. At the end of the first period, workers who are identified via monitoring as being of high commitment are promoted to job 2 for the second period and paid wage of $w_2^j$, which depends on the firm’s strategy $j$. All remaining workers stay in job 1 for period 2. If screening is used, no participants are promoted to job 2. At the end of period 2, the workers retire.

Solving first for the workers’ strategies conditional on whether the firm screens or not, it is straightforward to show that all workers use the policy if the firm decides not to screen (i.e. chooses monitor-only strategy). However, if the firm screens and thus infers that all participants are of low commitment, then all workers with low commitment participate in the policy. Workers with high commitment will refrain from participating only if $\frac{w_2^S - w_1}{2} > c(0, \theta_H) - c(1, \theta_H)$ where $w_2^S$ is the wage in job 2 when the screening strategy is used. The full solution is given in the Appendix, which results in $0 \leq \phi_H \leq \phi_L \leq 1$.

In the Appendix we use the solution to the workers’ problem to compare the firm’s profits between the screening and monitor-only strategies when the screen is
perfect to demonstrate that the relative profitability of the combination strategy is increasing in the cost of monitoring. Finally, we compare the profits under the two strategies when the screen is not perfect to show that the profitability of the combination strategy relative to monitor-only strategy is increasing in the quality of the screening mechanism (or decreasing in the noisiness of the mechanism). In terms of the implications for wage growth, the key result from the workers’ strategies is that wages are independent of participation when the monitoring-only strategy is used because both types participate in the policy. However, when the firm screens, wage growth is lower for policy users relative to non-users.

The comparative statics outlined in the Appendix give us the following propositions.

Proposition 1: \textit{Firms are more likely to use participation in the work-family policy to screen for worker commitment when the costs of monitoring are high.}\n
This proposition implies that the wage penalty associated with policy usage should be larger when monitoring costs are high. It is important to reiterate that either screening or human capital theory could be used to explain why a wage penalty results from using work-family policies as participation may causes depreciation or lower investment in skills. In addition, Proposition 1 alone does not provide sufficient support for screening over human capital explanations because differences in the wage penalty could be explained by differences in monitoring costs if, for example, human capital depreciation varies by this job characteristic. In order to differentiate these two competing explanations, we need an additional test.

To demonstrate our second proposition, in the Appendix we compare the two strategies when the screen is imperfect to evaluate how the profitability of the screening strategy relative to the monitor-only strategy changes with a decrease in the quality of the information captured by the screening mechanism. This results in our next proposition.
Proposition 2: *Firms are more likely to use participation in the work-family policy to screen for worker commitment when the quality of the information captured by the screening mechanism is high.*

This implies that the wage penalty associated with policy usage should be larger when the screen in more informative, or when policy usage among high-commitment types is low. Human capital theory *cannot* be used to generate or explain this condition. Taken together, these propositions imply that the wage penalty associated with usage should be largest when monitoring costs are high and when the quality of the screen is high if firms use policy usage to screen for worker commitment.

# 2 EMPIRICAL METHODOLOGY

## 2.1 Empirical Implementation

We use paid maternity leave as our application because, like other work-family policies, the stated goal of the policy is to help an employee balance work demands with family responsibilities. This application is related to that examined by Aghion and Hermalin (1990) in the context of legal restrictions, which proposes that workers may attempt to indicate their type (high commitment) by forfeiting maternity benefits at the time of hire. Our paper takes this idea a step further by suggesting that maternity leave use can serve as a mechanism for inferring a worker’s type after the initial hire.

We examine the effect of leave taking on wage growth, while most prior research on maternity leave has focused on how these policies affect female labor supply (Glass and Riley, 1997; Joesch, 1997; Gornick, Meyers, and Ross, 1998; Ruhm, 1998; Klerman and Leibowitz, 1999; Gornick and Meyers, 2003; Baker and Milligan, 2008). We restrict out analysis to the effect of leave taking on wage growth within a single employer because we expect that firms use participation as an indicator
of low commitment, and information regarding policy use at past employers is not readily available to current employers. Baum (2002) and Waldfogel (1998a) find that the penalty due to maternity leave is substantially reduced (actually reversed) when women return to their pre-birth employers due to the preservation of firm-specific human capital. This finding suggests our sample restriction may bias us toward not finding a penalty.

We assess if the wage growth penalty associated with paid leave changes with the key parameters of our model, namely the cost of monitoring and the quality of the screening mechanism. We test whether the wage penalty is higher when monitoring costs are high by using variation in job autonomy, a measure of how much discretion a worker has to complete tasks, to capture differences in monitoring costs across jobs. Workers in jobs with high autonomy are more costly to monitor and, therefore, learning about a worker’s job commitment by observing work behaviors is more costly in these types of jobs. Job autonomy is a valid proxy for monitoring costs because jobs with high autonomy are characterized by complex tasks that lack routine (Arai, 1994), which makes monitoring work behaviors difficult. A measure of job autonomy is available through O*NET, which is a comprehensive system of occupational categorization that is replacing the Dictionary of Occupational Titles (DOT) as a resource that has previously been used in the literature, for example, to characterize the nature of job tasks (Autor, Levy, and Murnane, 2003). In O*NET, job autonomy is captured by rating occupations based on the degree of work supervision in different jobs. We merged this information using the three-digit occupation code available in the NLSY79 and averaged this measure at the one-digit occupation level. Consistent with Proposition 1, we hypothesize that the wage growth penalty due to maternity leave will be greater in high autonomy jobs if firms use leave taking

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3It is important to reiterate that while job commitment affects productivity, it is not equivalent to output. Therefore, the measurability of output does not constitute an appropriate test of the model.
as a screening device.

We also evaluate if the wage growth penalty relates to changes with the quality of the screening mechanism, or \( s \). We propose that the passage of FMLA in 1993 increased the utilization of maternity leave by high-commitment workers relative to low-commitment workers and therefore decreased the quality of the screen (i.e. decreased \( s \)). Before FMLA, highly-committed women were unlikely to take leave because there was no assurance of job continuation. In 1993, FLMA guaranteed that workers employed at establishments with 50 or more workers could return to their jobs (or similar jobs) after taking a leave that lasted no more than twelve weeks. Thus, FMLA increased the legitimacy of taking maternity leave and the likelihood that high commitment worker will take leave by making it a protected right. Consistent with this idea, Han, Ruhm, and Waldfogel (2009) found that college-educated women were ten percent more likely to take unpaid maternity leave after FMLA, while FMLA had no significant effect on leave-taking among women who had not gone to college. Assuming that college-educated individuals are more likely to be high-commitment types relative to those without a college education, this result is consistent with the notion that FMLA increased leave taking among high-commitment workers. Notably, FMLA mandated unpaid leave, yet it likely affected employers’ and workers’ attitudes toward maternity leave in general (Waldfogel 1999), as it had symbolic importance for increasing the legitimacy of taking maternity leave (Ruhm, 1997).

Consistent with this idea, the NLSY79 data confirm that paid leave taking increased after FMLA: 39% of women who gave birth took paid maternity leave prior to FMLA as compared to 55% after FMLA. In all, this evidence suggests that FMLA marked a decrease in the quality in the screening mechanism, because high commitment workers were more likely to take maternity leave after FMLA. Based on Proposition 2, we therefore hypothesize that the wage penalty associated with leave
taking will be larger prior to the passage of FMLA. While it is possible that firms may also use unpaid leave to screen for a worker’s type, we focus on paid leave because FMLA directly affected a firm’s decision to offer unpaid leave through a mandate. Using paid leave allows us to sidestep this issue and allows us to use the passage of FMLA as having an indirect effect on paid leave taking by changing attitudes and increasing the legitimacy of leave taking.

Together, the two propositions generate the hypothesis that the penalty for usage will be largest for workers with high job autonomy prior to the passage of FMLA, a prediction that can be explained by screening, but not human capital theory. Testing this hypothesis is the focus of our paper’s empirical strategy.

2.2 Empirical Specification

We evaluate the two propositions empirically by examining how maternity leave usage affects wage growth rates. Using wage growth rate as the outcome variable allows us to evaluate the effect of policy usage on career advancement, an outcome relevant to inferences about a worker’s commitment. Examining wage growth, or the slope of the wage-experience profile, is consistent with prior work (e.g., Loughran and Zissimopoulos (2009) and also ameliorates concerns about compensating wage differentials associated with access to paid maternity leave.

We estimate the effect of taking maternity leave usage on wage growth controlling for human capital measures and occupation characteristics that may change over time. We directly model wage growth using the following econometric specification:

\[
\Delta \ln\text{wage}_{i,j,t} = X'_{i,t}\beta + Z'_{j,t}\gamma + \alpha_1 \text{Usage}_{i,j,t} + \alpha_2 M_{j,t} + \alpha_3 \text{Usage}_{i,j,t} \times M_{j,t} + \xi_i + \epsilon_{i,j,t} \tag{2}
\]

where \(\Delta \ln W_{i,j,t} = \ln \frac{\text{wage}_{i,j,t}}{\text{wage}_{i,j,t-1}}\), \(X_{i,t}\) includes a quadratic in labor market experience and firm tenure, hours of work, and a set of dummies for marital status and edu-
cation, \( Z_{j,t} \) includes union status, access to maternity leave, and one-digit industry dummies, \( \xi_i \) represents individual-level time-invariant characteristics, and \( \epsilon_{i,j,t} \) represents unobservable heterogeneity.

Consistent with our screening model, we consider the impact of policy participation on wage growth only within employment spells at a single employer because information on past usage is not readily available to outside firms.\(^5\) The variable \( Usage_{i,j,t} \) reflects usage of paid maternity leave at a particular employer, such that:

\[
Usage_{i,j,t} = \begin{cases} 
1 & \text{if took paid leave from firm } j \text{ in } t - 1 \\
1 & \text{if } Usage_{i,j,t-1} = 1 \\
0 & \text{Otherwise} 
\end{cases}
\]

which captures the idea that women are classified as users for all years following their initial taking leave as long as they remain at the same employer. The term \( M_{i,j,t} \) represents our proxy for monitoring costs; the interaction between the indicator for usage and \( M_{i,j,t} \) in equation 2 allows the effect of usage on wage growth to vary with this proxy. To implement our second proposition, we restrict the data to either before (1983 to 1992) or after (1993 to 2004) FMLA. We estimate this specification using a fixed effects model to remove the individual-specific component of wage growth to avoid biases induced if \( \xi_j \) is correlated with the decision to use maternity leave. This would occur, for example, if individuals with lower wage growth were more likely to use maternity leave. Furthermore, including an individual fixed effect removes variation due to an individual’s actual commitment level. In addition, we cluster the standard errors at the individual-level.

\(^4\)Labor market experience and tenure are measured net of time taken for paid maternity leave. \(^5\)This is consistent with pregnancy being among the “protected classes” in Federal employment law.


3 DATA AND SAMPLE SELECTION

This paper utilizes data on a sample of female workers from the NLSY79 to examine the relationship between maternity leave usage and wage growth. In order to conduct the empirical tests of the hypotheses generated from our model’s propositions, it is necessary to have consecutive wage information (i.e. to construct wage growth) along with records of paid maternity leave usage that are available in both the pre- and post-FMLA periods. The NLSY79 satisfies these conditions and it also provides detailed information about job characteristics, such as Standard Occupation Classification (SOC) codes, which we use when we proxy for monitoring costs. The NLSY79 is a nationally-representative longitudinal sample of individuals who were aged 14-21 on January 1, 1979 and living within the United States or who were on active military duty outside the United States. It includes annual (between 1979 and 1994) and biennial (since 1994) information on these individuals. For the analysis, we restrict the sample to only females respondents and apply the following sample selection criteria.\(^6\)

We used information for the primary job, which is defined by the NLSY79 as the current and most recent job in which weekly hours were the highest at the survey date. In order to limit the influence of temporary employment when wages may be more variable, we only included full-time positions (weekly working hours of at least 30 hours) and employment spells that last 104 weeks or more. These rules excluded 1,636 women who never had a full-time position between 1979 and 2004. In addition, we excluded 248 women who reported wage information for only one time period because we cannot construct wage growth for these individuals.

Wage information is obtained from “hourly rate of pay,” and the wages are inflated to 2004 dollars using the CPI-U. Individuals with reported hourly wages of more than

\(^6\)It is important to note that our results will suffer from sample selection because we only observe wages for those who participate in market work; to the extent that workers who are more heavily penalized from usage exit the labor force, our results will be biased toward zero.
$500 or less than $1 are excluded from the sample. In addition, if an individual’s wages were missing in one survey year but she did not switch employers, we imputed the missing wage using the average wage from the previous and subsequent year.\textsuperscript{7} Because the survey was conducted biennially from year 1994 to 2004, we used the geometric mean to compute the single-year wage growth rate for those years to make wage growth comparable with earlier years.

To preserve the sample size, we imputed missing values when other pieces of information could be used to confidently make these imputations. For example, in case a woman did not report her marital status, tenure, or union status in a particular year because she did not participate in the survey, we imputed this information from other years.\textsuperscript{8,9} The results are robust to these imputations.

In the NLSY79 interview, women were asked whether they had taken paid maternity leave in the prior year, providing start and end dates for that leave. Although this set of questions was not included until 1988, a retrospective component was included the first time this set of questions was asked in the interview. Hence, the earliest year of available maternity leave usage information is 1986 and the latest year is 2004. Starting in 1983, respondents were asked whether they had access to maternity leave at the employer.\textsuperscript{10} Because we control for access to maternity leave, we include information from all birth cohorts (1959 through 1964), but limit the survey years to 1983 to 2004 for the analysis (excludes 89 individuals and 2,103 person-year observations).

In this paper a woman is classified as having used maternity leave if she reported

\textsuperscript{7}457 missing values are replaced to the average wage between previous and subsequent wages by this imputation procedure. We conducted robustness check by dropping these 457 imputed values when we analyze the data and the results were unchanged.

\textsuperscript{8}This procedure excluded one woman from the sample because she did not ever provide tenure information although she reported she worked for two interview years.

\textsuperscript{9}There were 97 women who did never provided union status information and were thus excluded from the sample. In addition, one woman was dropped for the analysis by job type because she did not report her occupation.

\textsuperscript{10}The NLSY survey question pertaining to access does not distinguish between paid and unpaid leave.
start dates and end dates for a leave while she was employed by her current firm. Accordingly, we define “maternity leave user” as a woman who reported the start and end dates of maternity leave in her primary job.\textsuperscript{11} A woman is classified as a “user” in all years following her leave as long as she remains at that employer; however, she is classified as a non-user in the years prior to usage at that employer. In the event that a woman used maternity leave multiple times in a same job, only the first usage of maternity leave is tracked; multiple usages do not affect the value of the indicator for maternity leave usage. If the woman leaves her employer, she is no longer classified as a user.

The final sample contains 4,211 women in an unbalanced panel structure (26,979 observations). In our sample, 647 women used paid maternity leave at least once during their career (813 observations). The descriptive statistics for the sample are given in Table 1. The mean age of women in our sample is 31 years with average labor market experience of just under 11 years. The majority of women work in white collar positions (approximately 72 percent) and most are non-unionized. The average job autonomy rating measured on a 5-point scale is 3.3, with professionals having the highest rating (3.9) and laborers having the lowest (2.5). Unconditionally, we find that the wage growth rate following usage of paid maternity leave is significantly lower, on average, than the growth rate experienced prior to usage.

4 RESULTS

4.1 Maternity Leave and Wage Growth

The results from estimating our basic empirical model (i.e. without incorporating our propositions) are given in the first column of Table 2. We find that maternity leave usage reduces wage growth using our full sample, but the effect is very small and not

\footnote{We excluded cases where maternity leave usage length was more than 365 days. This procedure excluded 24 observations.}
significant at conventional levels. We proxy for monitoring costs using job autonomy and assume that monitoring costs increase with the degree of job autonomy. We find a negative coefficient on the interaction between usage and job autonomy (Table 2, column 2), which implies that the penalty for usage is higher for employees with greater job autonomy. This is consistent with Proposition 1.

This result, however, does not provide sufficient evidence that the wage penalty is due to screening because this wage penalty reflects actual productivity changes (i.e. workers with greater job autonomy experience a more human capital depreciation). We therefore incorporate Proposition 2 by if the effect differs before versus after FMLA. We find that the negative interaction between job autonomy and policy usage is concentrated in the time period before FMLA (column 3 versus 4 in Table 2). For workers with high job autonomy (i.e., autonomy rating of 3.93 on a 5-point scale), taking paid maternity leave prior to FMLA reduces wage growth by 3.9 percentage points \((-0.0390 = -0.0747 \times 3.93 + 0.2546\) relative to those who did not take paid leave. If the negative effect of policy use on wage growth was due to human capital depreciation, we would not expect the effect to vary by the passage of FMLA. One might also expect to find a smaller wage growth difference between those who do and do not take leave after FMLA if higher-commitment types, who are also likely to have higher wage growth, take leave after FMLA. However, we control for individual differences in worker commitment by using fixed effects analysis. Thus, the penalty we estimate is over and above individual differences in worker commitment.

As a robustness check, we also proxy for differences in monitoring costs using job type (white-collar vs. blue-collar), and assume that White-collar workers have higher monitoring costs relative to blue-collar workers. This assumption is consistent with past research on efficiency wage theory, which finds that white-collar workers receive larger establishment-size wage premium than blue-collar workers, implying that monitoring costs are higher for the former group (Krueger and Summers, 1988;
Fox, 2009). The results presented in Table 3 show that the penalty associated with using paid maternity leave is concentrated among white collar workers prior to FMLA (column 1 vs. 2). In fact, we find that blue-collar workers who used leave received a wage boost prior to the passage of FMLA (column 3). Even though we control for access to leave (paid or unpaid) in the regression, this premium could be due to more limited access to paid maternity leave for these types of workers and thus policy usage may proxy for the presence of firm-specific human capital.

In terms of our proxy for the change in the quality of the screening device, one may be concerned that the lack of wage penalty after FMLA may be due to increased risk of litigation as a result of the Act. However, FMLA guarantees the return to one’s same job or similar job after taking leave and does not specify requirements for wages. Further, FMLA stipulated job protection for unpaid leave, but did not impose requirements for paid leave. Hence, by examining the effect of taking paid maternity leave on wage growth, our empirical tests are defensible against the concerns of direct policy effect on wages.

Although our primary interest is whether employer’s use paid maternity leave as a screening mechanisms, we also consider whether the results are consistent with firms using unpaid leave as a screening mechanism. However, capturing unpaid leave using the NLSY79 is imperfect; the best available proxy is to use information on within employment gaps. If a respondent experiences a gap within an employment spell, they are asked to choose from 15 different reasons for why the gap occurred. The best proxy for unpaid maternity leave is spells that the respondent indicated was due to pregnancy. When we include a control of this episode of unpaid leave, our results do not change. In addition, the pattern of our results are also robust to grouping episodes of paid and unpaid leave together, although the significance level is reduced.\textsuperscript{12} Results that incorporate unpaid leave are available from the authors.

\textsuperscript{12}The weaker result for unpaid leave suggests that firms view usage of paid maternity leave as a stronger indicator of type, which is plausible because paid leave taking is more costly to the firm.
Our empirical specification models the effect of maternity leave usage on wage growth or the slope of the wage-experience profile. While this specification provides a valid test of the empirical implications derived from our model’s propositions, for a robustness check we also allow policy usage to affect both wage levels and growth rates because modeling wages using a log-level specification is the most common empirical strategy in the literature. Loughran and Zissimopoulos (2009) provide a method for estimating both an intercept (“level effect”) and slope effect (“wage growth effect”) in their analysis of how marriage and childbearing affect the wages of women and men. We adopt their specification and apply it to use of paid maternity leave. We find that compared to workers who did not take leave, usage of maternity leave reduced wages (in levels) of women with high job autonomy by 5.2 percent and find that the estimate of the effect of usage on wage growth is larger in magnitude than our main specification. Hence, our results are robust to this alternative specification that incorporates the effect of maternity leave on wage growth as well as wage levels. Results are available upon request.

One limitation of using the NLSY79 for our analysis is that the individuals in our data only vary in age by a span of eight years. Therefore, the difference in wage penalties we see before and after the passage of FMLA could be due to aging rather than a change in the quality of the screening technology. In particular, women who use maternity leave early in their career may suffer a larger penalty than those who use later in their career due to greater human capital depreciation. To address this concern, we re-estimated each specification after restricting the women’s age to prime-child bearing years (ages 25 to 36). The results, available upon request, are robust to this specification: the wage growth penalty for women with high job autonomy who used paid leave prior to FMLA was 3.4 percentage points.

than unpaid leave.
In summary, we find that the wage penalty is concentrated among workers with higher monitoring costs, and within a period when the quality of the screening was high, which supports our model’s propositions. This result is consistent with firms screening workers based on policy usage; one would not expect this pattern if the reductions in wage growth were due to human capital depreciation alone. These results indicate that firms penalized users based on an inference of low commitment, rather than based on observed changes in productivity.

4.2 Maternity Leave Penalty vs. Motherhood Penalty

Many researchers have examined the effect of motherhood on wages and have hypothesized about the mechanisms that could explain why mothers earn lower wages. Early work by Mincer and Polachek (1974) investigated the role of human capital investment and depreciation as a potential explanatory mechanism; however, subsequent work has found that a negative wage effect remains after controlling for differences in human capital (Korenman and Neumark, 1992; Jacobsen and Levin, 1995; Waldfogel, 1997, 1998a, 1998b; Budig and England, 2001). For example, Budig and England (2001) estimate that wages of mothers are five percent lower per child relative to non-mothers after controlling for reductions in labor market experience as well as for differences in “mother-friendly” characteristics of jobs, which could generate compensating wage differentials. They argue that the motherhood penalty is due to either discrimination or to actual reductions in the productivity of these women. Evidence consistent with discrimination against mothers has also been found in recent audit and laboratory studies (Cuddy and Fiske, 2004; Correll, Benard, and Paik, 2007). The model developed in this paper could be easily broadened to allow employers to screen for commitment level using motherhood. However, we have chosen to focus on policy usage as the screening mechanism because it not only incorporates the fact that she has family responsibilities, but that she has brought them
into the work environment through policy usage, making this competing demand more salient.

We can, however, examine the extent to which the wage growth penalties we observe reflect maternity leave use versus motherhood, by comparing the penalty due to leave with the penalty due to childbirth or adoption. In our sample 781 women (913 observations) reported having a child, but did not take paid maternity leave. Table 4 shows the results for wage growth when women who gave birth and used leave (“Maternity Usage”) are distinguished from those women who gave birth and did not use leave (“Birth, No Use”). For ease of interpretation, we use job type as the proxy for monitoring costs. Using all the years and both job types, we see that the effect is negative but not statistically significant from zero (column 1). When we examine only white-collar workers, the magnitude increases slightly. When we restrict the sample to the years before FMLA, the effect is more negative and is statistically significant at the 5 percent level. These results clearly show that it is policy usage that is driving the result: the wage growth rate for users is 2.4 percentage points lower than non-users and is statistically significant from zero at the 10-percent level, while there is no negative effect associated with birth for those who do not use the policy (column 3). Therefore, our results suggest that maternity leave usage rather than birth of a child is the behavior firms used as the screening mechanism. While our paper does not explicitly examine the motherhood penalty, our results suggest that the motherhood penalty may also not be due to human capital explanations alone because we find evidence of worker screening based on maternity leave usage.

4.3 Evidence of Statistical Discrimination

We find that the wage growth penalty due to maternity leave usage is concentrated among workers with high job autonomy prior to the passage of FMLA. This pattern
of wage growth penalties provides evidence that employers used maternity leave to screen for worker commitment and cannot be fully explained by human capital depreciation. Assuming that this screen was imperfect, our results also provide evidence that employers engaged in statistical discrimination based on paid maternity leave usage. While our approach is different, our finding is consistent with recent work that shows that firms statistically discriminate against workers based on observable characteristics (e.g., Altonji and Pierret, 2001; Ichino and Moretti, 2009). This practice is economically efficient if there is valid relationship between the worker characteristic that is easily observed by the firm and the underlying characteristic that is difficult to observe, such in the case of higher educational attainment serving as an indicator of higher unobservable ability (Altonji and Pierret, 2001).

However, statistical discrimination for decisions relating to hiring, promotion, or wage setting on the basis of protected characteristics, such as race, gender, sex, national origin, religion, and age, is illegal and is prohibited by the Civil Rights Act of 1964. Furthermore, the Pregnancy Discrimination Act of 1978 amended the Civil Rights Act to include protection for women who are pregnant or affected by pregnancy-related conditions. While statistical discrimination based on maternity leave appears objectionable on legal grounds, devising legal consequences is beyond the scope of the present endeavor. Instead, we conduct a supplemental analysis to assess the empirical validity of whether maternity leave users are less committed than non-users by comparing the wage growth of former users to non-users at subsequent employers.

To conduct this supplemental analysis, we rely on asymmetry across employers in terms of prior maternity leave usage. The literature on employer learning finds support for asymmetric learning across employers in terms of employee ability (Gibbons and Katz, 1991; Kahn, 2008). We make a weaker assumption that only requires that information on past policy usage is not transferred to subsequent employers. We
compare initial wage growth in new employment spells between women who used paid leave at their past employer and those who did not and we find no significant difference in wage growth (available upon request). Assuming that commitment is captured in initial wage growth, we do not find empirical support for a valid connection between usage and job commitment. Although this is only weak evidence that maternity leave users do not have systematically lower commitment than non-users, exploiting informational asymmetries across employers is likely to be a fruitful way of evaluating whether statistical discrimination based on leave usage was efficient or whether it represented implicit biases against mothers, or caregivers more generally (e.g., Williams 2000).

Looking at the paper’s main result from a policy perspective, the passage of FMLA appears to have decreased the quality of the screen to the point that utilization of paid leave is no longer an effective way of statistically discriminating, or differentiating workers. While there has been criticism surrounding the limited direct impact of FMLA, this paper shows the possible indirect effect of the Act. Past research by Aghion and Hermanin (1990), which shows that under certain conditions mandating maternity benefits is socially optimal because it prevents workers with a low risk of usage from signaling their type at time of hire, suggests that FMLA could directly limit the screening role of unpaid leave. However, instead of framing the passage of FMLA as an event that directly limited a worker’s ability to signal, we use FMLA as marking a change in attitudes surrounding utilization of maternity leave benefits. Hence, we demonstrate the indirect effect of FMLA by providing support for the empirical implications of our screening model.

5 CONCLUSION

This paper develops a model in which firms decide whether to use participation in work-family policies to infer a worker’s level of commitment in the context of
asymmetric information. The model implies that firms are more likely to use this screening mechanism when both the costs of direct monitoring are the quality of the screen are high. We evaluate these propositions by examining the effect of using paid maternity leave on wage growth using the NLSY79. We proxy for differences in monitoring costs with job autonomy and use the passage of FMLA as an exogenous change in the screen’s quality. We find that the wage growth penalty due to maternity leave usage is concentrated among workers with high job autonomy prior to the passage of FMLA. This pattern of wage growth penalties provides evidence that employers used maternity leave to screen for worker commitment and cannot be fully explained by human capital depreciation.

Our results are robust to several sensitivity tests, including an alternative proxy for monitoring costs, modeling the effect of usage on wage levels as well as wage growth, restricting the age window to prime childbearing years, and finally, controlling for episodes of unpaid leave. The paper’s findings have implications for policy-makers, including the discovery of an indirect effect of FMLA on attitudes towards paid leave taking that had an important impact on the ability of firms to use paid leave to differentiate workers.

Our findings have several limitations. First, age and time effects may be confounded in the analysis because additional cohorts are not added to the NLSY79 over time. The potential concern is that women who use maternity leave early in their career may suffer a larger penalty than those who use later in their career due to greater human capital depreciation. While we address this in a robustness check by limiting the sample to women in their prime childbearing years, future research could potentially address this using an alternative dataset. In addition, there could be other factors that occur over this time period besides the passage of FMLA. Finally, the sample used is restricted to women working in permanent, full-time employment spells and to women who return to their pre-birth employer
following maternity leave. While this sample selection limits our ability to examine the effect of maternity leave taking on all female workers, we believe that this restriction biases our estimate downward because the women facing larger penalties are more likely to exit the labor force or work in more temporary work arrangements. Hence, despite these limitations, we believe our results provide evidence consistent with screening. Future research should explore alternative ways of implementing and testing our paper’s propositions, particularly in regard to the quality of the screening technology.

One may expect that employers would be least likely to use maternity leave as a screening device relative to other work-family policies, such as flexible scheduling or telecommuting, because taking leave for the birth or adoption of a child is the most “justifiable” policy. If this is the case, then we can conclude that other work-family policies are likely providing firms with a means to screen workers because we find evidence of screening for maternity leave usage. While taking maternity leave is no longer associated with a wage penalty, our findings indicate that work-family policies provide an avenue for firms to assess workers and, thus, firms may currently engage in statistical discrimination based on usage of other policies.

If firms engage in screening based on policy usage, then subsequent career consequences will continue to act as a barrier to participation. Our paper’s findings are consistent with evidence that workers are hesitant to use these policies, even when they are available. The results from our paper imply that HR decision-makers who want to promote work-life balance need to be aware of how usage is perceived in jobs with high monitoring costs, such as management and professional occupations. In particular, they should consider undertaking actions to emphasize the business case for work-family policies if they want to increase utilization. Indeed, HR departments at some firms are beginning to move in this direction by re-framing policies as business necessities rather than using the nomenclature of work-family. By emphasizing
the potential for cost reductions and increased productivity, this reframing of the policies makes participation a less informative indicator of a worker’s commitment level. Future work can apply the empirical implications of our paper’s model to other work-family policies to evaluate if participants continue to be cast as less committed workers.
References


Table 1: Descriptive Statistics

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<th>Mean</th>
<th>St.Dev</th>
</tr>
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<td>Real Wage Growth: $\ln\left(\frac{w_t}{w_{t-1}}\right)$</td>
<td>0.037</td>
</tr>
<tr>
<td>Real Wage Growth By Usage:</td>
<td></td>
</tr>
<tr>
<td>Never Used</td>
<td>0.038</td>
</tr>
<tr>
<td>Used Leave</td>
<td>0.038</td>
</tr>
<tr>
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<td>0.038</td>
</tr>
<tr>
<td>After Usage</td>
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<td>Age</td>
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</table>

** The growth rate after usage is significantly different than before at the 1% level

$^a$ White-Collar includes professionals, managers, sales, and clerical occupations.

$^b$ Job autonomy measured by response to O*NET question: “Workers on this job plan their work with little supervision,” on a scale of 1 (strongly disagree) to 5 (strongly agree), averaged at one-digit SOC code.
Table 2: Effect of Maternity Leave Usage on Wage Growth

<table>
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<tr>
<th>VARIABLES</th>
<th>All Years 1</th>
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<th>Pre-FMLA 3</th>
<th>Post-FMLA 4</th>
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<td>(0.013)</td>
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<td>0.2546*</td>
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<td>(0.005)</td>
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<td></td>
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<td>(0.005)</td>
<td>(0.008)</td>
</tr>
<tr>
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<td>-0.0053+</td>
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<td>(0.003)</td>
<td>(0.010)</td>
<td>(0.008)</td>
</tr>
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<td>Labor Market Exp²</td>
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<td>0.0000</td>
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<td>(0.000)</td>
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Included Controls

| Access to Leave         | Yes | Yes | Yes | Yes |
| Industry               | Yes | Yes | Yes | Yes |
| Marital Status         | Yes | Yes | Yes | Yes |
| Education Dummies      | Yes | Yes | Yes | Yes |
| Hours                  | Yes | Yes | Yes | Yes |
| Year Dummies           | Yes | Yes | Yes | Yes |

Observations 26979
Number of ID 4211

R² 0.015

Notes: Regression estimated by applying Fixed Effects estimation to equation 2. Coefficient on Usage is the estimated effect of maternity leave on wage growth. Job autonomy measured by response to O*NET question: “Workers on this job plan their work with little supervision,” on a scale of 1 (strongly disagree) to 5 (strongly agree), averaged at one-digit SOC code. Robust standard errors are in parentheses.
Table 3: Effect of Maternity Leave Usage on Wage Growth - By Job Type

<table>
<thead>
<tr>
<th>VARIABLES</th>
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Included Controls

- Access to Leave: Yes
- Industry: Yes
- Marital Status: Yes
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- Observations: 11236, 8169, 4487, 3087
- Number of ID: 2815, 2357, 1444, 1100
- $R^2$: 0.029, 0.024, 0.017, 0.015

Notes: Regression estimated by applying Fixed Effects estimation to equation 2. Coefficient on Usage is the estimated effect of maternity leave on wage growth. White-Collar includes professionals, managers, sales, and clerical occupations. Robust standard errors are in parentheses.
Table 4: Usage Penalty vs. Motherhood Penalty

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Included Controls

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</thead>
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<tr>
<td></td>
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<td>White-Collar</td>
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<tr>
<td></td>
<td>Fixed Effect</td>
<td>Fixed Effect</td>
<td>Fixed Effect</td>
<td>Fixed Effect</td>
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<tr>
<td>Access to Leave</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
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<td>Industry</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Marital Status</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Education Dummies</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Hours</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Year Dummies</td>
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<td>Yes</td>
<td>Yes</td>
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<td>Observations</td>
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<td>19405</td>
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<tr>
<td>Number of ID</td>
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<td>3428</td>
<td>2815</td>
<td>2357</td>
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<tr>
<td>(R^2)</td>
<td>0.015</td>
<td>0.025</td>
<td>0.030</td>
<td>0.024</td>
</tr>
</tbody>
</table>

Fixed Effect Estimation, Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1

Notes: Regression estimated by applying OLS estimation to equation 2. Birth, No Usage indicates that the women had a child, but did not take paid maternity leave. The omitted category are women who did not have a birth and did not use paid maternity leave. Robust standard errors are in parentheses.
APPENDIX

Workers’ Strategies

Firm Adopts Monitor-Only Strategy: First, we solve the worker’s problem for when the firm adopts the monitor-only strategy. Using equation 1, we get the following expressions for utility based on participation in the work-family policy \( (p_i = \{0, 1\}) \) given \( w_1, w_2^M \), and the worker’s value of \( \theta \) because the firm directly learns the worker’s type via monitoring.\(^{13}\) To ensure that workers enter the labor market, we assume that the utility from \( w_1 \) minus the cost of work without participation \((p = 0)\) exceeds the worker’s reservation utility. For the case of \( \theta = \theta_L \),

\[
U(p_i : w_1, w_2^M) = \begin{cases} 
2w_1 - 2c(1, \theta_L) & \text{if } p_i = 1 \\
2w_1 - 2c(0, \theta_L) & \text{if } p_i = 0.
\end{cases} \tag{A-1}
\]

where \( w_2^M \) is the wage paid in job 2 when monitoring-only is used. Because monitoring is used, the firm will never promote a worker of type \( \theta_L \), therefore, \( U(1, \theta_L) \) exceeds \( U(0, \theta_L) \) due to the properties for \( c(p, \theta) \). Similarly, for \( \theta = \theta_H \) we get the following expressions for utility:

\[
U(p_i : w_1, w_2^M) = \begin{cases} 
w_1 + w_2^M - 2c(1, \theta_H) & \text{if } p_i = 1 \\
w_1 + w_2^M - 2c(0, \theta_H) & \text{if } p_i = 0.
\end{cases} \tag{A-2}
\]

Again, because the firm directly learns the worker’s type via monitoring and because of the properties for \( c(p, \theta) \), \( U(1, \theta_H) \) exceeds \( U(0, \theta_H) \). Therefore, both types of workers participate in the policy when monitoring-only is used, which results in \( \phi_L = 1 \) and \( \phi_H = 1 \).

Firm Adopts Screening Strategy: Next, we solve for the workers’ strategies when the firm screens via policy usage, i.e. firm infers that all participants are of low commitment and monitors non-participants. The expressions for utility based on participation in the work-family policy \( (p_i = \{0, 1\}) \) given \( w_1, w_2^S \), and \( \theta \) are as follows:

\(^{13}\)All workers are eligible for the policy in this model, which corresponds to the case for most work-family policies. For paid maternity leave, workers qualify through giving birth or through adoption; however, we do not model this decision.
\[
U(p_i : w_1, w_2^S) = \begin{cases} 
2w_1 - 2c(1, \theta) & \text{if } p_i = 1 \\
2w_1 - 2c(0, \theta_L) & \text{if } p_i = 0, \theta = \theta_L \\
w_1 + w_2^S - 2c(0, \theta_H) & \text{if } p_i = 0, \theta = \theta_H 
\end{cases} 
\tag{A-3}
\]

For the case when \( \theta = \theta_L \), \( U(1, \theta_L) \) is greater than \( U(0, \theta_L) \) because \( c_p(p, \theta) < 0 \). Therefore, workers with low commitment will always participate regardless of the firm’s strategy: \( \phi_L = 1 \). The condition of interest is in regards to the decision by high-commitment types of whether to participate in the work-family policy. Namely, participation in the policy will serve as a screening mechanism if \( U(0, \theta_H) > U(1, \theta_H) \), which is satisfied when

\[
\frac{w_2^S - w_1}{2} > c(0, \theta_H) - c(1, \theta_H). \tag{A-4}
\]

Therefore, \( \theta_H \)-types will only decide to not participate if the compensation associated with the promotion is high enough. If equation A-4 is satisfied with equality, then the probability that a \( \theta_H \)-type uses the policy is \( \phi_H \in (0, 1) \); if the left-hand side of A-4 is less than the right-hand side, then \( \phi_H = 1 \).

Thus, we have shown that the workers’ optimal strategies result in \( 0 \leq \phi_H \leq \phi_L = 1 \). In addition, the workers’ strategies imply that wages are independent of policy usage in the monitoring-only scenario because both types participate. However, for the screening strategy, total wages of users \( (2w_1) \) are less than wages of non-users \( (w_1(2-\lambda\phi_H)+w_2(1-\phi_H)) \) when condition A-4 is satisfied. Notice that this differential can be written in terms of differences in wage growth rates.

**Relative Profitability of Strategies**

Now that we have solved the worker’s problem conditional on the firm’s strategy, we will determine the relative profitability of the firm’s two strategies. Notice that we do not have to solve for the optimal choice of \( w_1 \) or \( w_2^S \) because we are only interested in how the relative profitability changes with key cost parameters in the model. First, we’ll compare the profits from the combination strategy when the screen is perfect (i.e. \( \phi_H = 0, \phi_L = 1 \)) to the profits when the monitor-only strategy is used.

The firm’s profits under the screening strategy when A-4 is satisfied are:

\[
\Pi^S = (2 - \lambda)h + \lambda g(\theta_H)h - ((2 - \lambda)w_1 + \lambda w_2^S) - \lambda m \tag{A-5}
\]
where $\lambda$ is the fraction of high-commitment types, $m$ is the cost of monitoring the continuum of workers, and $w_2^S$ is the wage for job 2 when the screening strategy is used. The monitoring-only strategy yields the following profits:

$$\Pi^M = (2 - \lambda)h + \lambda g(\theta_H)h - ((2 - \lambda)w_1 + \lambda w_2^M) - m,$$  

(A-6)

where $w_2^M$ is the wage for job 2 when the monitoring-only strategy is used. Taking the difference between equations A-5 and A-6, we have:

$$\Pi^S - \Pi^M = (1 - \lambda)m - \lambda (w_2^S - w_2^M),$$  

(A-7)

We can clearly see that the relative profitability of the combination strategy is increasing in the monitoring cost $m$, which gives us Proposition 1 in the text. The firm will screen as long as monitoring costs are high enough: if $m > \frac{\lambda}{1-\lambda} [w_2^S - w_2^M]$.

Equation A-7 compares the screening strategy to monitoring when the screening technology is perfect. Now, let’s consider the relative profitability when $\phi_H \in (0, 1)$, which occurs when $w_2^S - w_1$ satisfies equation A-4 with equality, call this $S'$.\(^{14}\) Below is the expression for profits for the screening strategy if the screening technology is not perfect:

$$\Pi^{S'} = (2 - \lambda)h + \lambda \phi_H h + (1 - \phi_H)g(\theta_H)h - [(2 - \lambda)w_1 + \lambda((1 - \phi_H)w_2 + \phi_H w_1)] - \lambda (1 - \phi_H)m,$$  

(A-8)

which is attained when $w_2^{S'}$ is the wage that satisfies equation A-4 with equality. Now subtracting equation A-6 from equation A-8, we get:

$$\Pi^{S'} - \Pi^M = \lambda \phi_H (h - g(\theta_H)h) + \lambda [w_2^M - (1 - \phi_H)w_2^{S'}] + m [1 - \lambda (1 - \phi_H)]. \quad (A-9)$$

We again can see that the relative profitability of the combination strategy is increasing in $m$. Now, we can also evaluate how the relative profitability changes with a decrease in the quality of the information captured by the screening technology, or an increase in $\phi_H$. The derivative of equation A-9 with respect to $\phi_H$ if negative if $g(\theta_H)h - h > w_2^{S'} + m$. This gives us Proposition 2 in the text. Further, we can solve for probability of usage such that the monitor-only strategy is preferable to the combination strategy by setting A-9 equal to 0. This implies that if $\phi_H$ exceeds $\phi_H^{\ast}$ then

\(^{14}\)Notice that if the left-hand-side of equation A-4 is less than the right-hand side, then $\phi_H = 1$; under this scenario, it can be verified that the monitoring-only strategy dominates the screening strategy because the latter imposes a condition on $w_2^S$, while the former does not.
the screening mechanism is too noisy and the monitoring-only strategy dominates, where \( \phi_H \) is given by:

\[
\phi_H = \lambda (w_2^S - w_2^M) - (1 - \lambda)m - \frac{(1 - \lambda)h + w_2^{S_H} + m}{h - g(\theta_H)h + w_2^{S_H}}.
\] (A-10)

which implies a lower bound on \( s \), namely \( s = \frac{\phi_L}{\phi_H} = \frac{1}{\phi_H + 1} \). Table A-1 outlines the firm’s strategy given parameters of the model, namely monitoring costs and the quality of the information captured in the screen. The most interesting scenario is when \( \theta_H \)-types are indifferent between participation in the work-family policy or where \( \phi_H \) is indeterminate. If we allow mechanisms outside the model (i.e. social norms) to resolve worker indifference such that \( \phi_H < \phi_H \), then the perfect Bayesian equilibrium strategy for the firm is to choose the combination strategy. However, if the indifference is resolved such that \( \phi_H \geq \phi_H \), then the firm’s equilibrium strategy is monitor-only.
### Table A-1: Firm’s Strategy given Parameters

<table>
<thead>
<tr>
<th>Information Quality (s)</th>
<th>$\phi_H$, $\phi_L$</th>
<th>Monitoring Costs ($m$)</th>
<th>Firm Strategy</th>
</tr>
</thead>
<tbody>
<tr>
<td>$s = 1$</td>
<td>$\phi_H = 0$, $\phi_L = 1$</td>
<td>$m \leq \frac{\lambda}{1-\lambda} (d^c - d^m)$</td>
<td>Monitor-only</td>
</tr>
<tr>
<td></td>
<td>$\phi_H = 0$, $\phi_L = 1$</td>
<td>$m &gt; \frac{\lambda}{1-\lambda} (d^c - d^m)$</td>
<td>Combination</td>
</tr>
<tr>
<td>$s &gt; \bar{s}$</td>
<td>$\phi_H &lt; \phi_H$, $\phi_L = 1$</td>
<td>$\frac{\phi_H}{\phi_H} = \frac{\lambda (w^S - w^M) - (1-\lambda)m}{h-g(\theta, h)+w^S+m}$</td>
<td>Combination</td>
</tr>
<tr>
<td>$s \leq \bar{s}$</td>
<td>$\phi_H \geq \phi_H$, $\phi_L = 1$</td>
<td>$\phi_H = \frac{\lambda (w^S - w^M) - (1-\lambda)m}{h-g(\theta, h)+w^S+m}$</td>
<td>Monitoring-only</td>
</tr>
<tr>
<td>$s = 0.5$</td>
<td>$\phi_H = 1$, $\phi_L = 1$</td>
<td>$m = -$</td>
<td>Monitoring-only</td>
</tr>
</tbody>
</table>