The Effects of Pregnancy-Discrimination Laws The Case of the Pregnancy Discrimination Act of 1978

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Abstract

Pregnancy discrimination is a common form of discrimination faced by women in the labor force. Nonetheless, before the Pregnancy Discrimination Act (PDA) of 1978, it was not forbidden by existing federal anti-discrimination laws: firms who provided job-protected sick-leave to male workers, often fired their female counterparts upon pregnancy. However, the effects of the employment protection component of the PDA have not yet been studied. In this paper, we first calibrate a matching model to find that (i) the effect of the legislation on employment is unambiguously negative unless it significantly raises the firing costs for discriminating employers, (ii) conditional on being strongly implemented, the law could increase women's employment, but only if the degree of discrimination is not too high. We then examine the actual effects of the PDA empirically exploiting quasi-experimental variation, granted by US states' staggered enactment of similar policies. Difference-in-differences types of analyses, based on individual-level survey data, show that the PDA had negative effects on employment of fertile-age women. Evidence of null effects on proxies of job dismissals suggests that the PDA was not effective in sufficiently raising costs of firing discrimination. We finally document a muted response of women's wages, likely due to prior equal pay legislation. This may have exacerbated the negative effect on employment, limiting one margin of adjustment.

1 Introduction

Despite the efforts by policymakers and stakeholders to combat gender discrimination in the labor market, pregnancy discrimination – the "discrimination of a woman as a result of pregnancy, childbirth, or a [related] medical condition"¹ – is still today a common form of discrimination against women in the labor force. It can affect various aspects of employment, from hiring and pay to promotion opportunities, yet the most frequently reported case is unjust discharge on the basis of pregnancy (McCann and Tomaskovic-Devey, 2021).

Strikingly, while legislation to curb gender discrimination had already been in place at a federal level since the 1960s, with the Civil Rights Act (CRA) of 1964, pregnancy discrimination was only explicitly recognized as a form of gender discrimination in 1978 with the approval of the Pregnancy Discrimination Act (PDA). Prior to it, it was thus legal and common for employers to discriminate employees on the basis of their pregnancy, a practice that even the US Supreme Court upheld as not violating gender-discrimination laws in two notorious cases². In particular, the PDA of 1978 addressed this form of discrimination, by mandating equal *treatment* of pregnant women and men with 'comparable' temporary disabilities. In practice, it required employers to provide "light duty, modified tasks, alternative assignments, disability leave, or leave without pay", if they did so for temporarily disabled employees, and forbade discrimination in hiring, firing, promotion, and pay on the basis of pregnancy³.

Despite the intended positive goal, the effect of pregnancy-discrimination regulation, such as the PDA, on fertile-age women's labor market outcomes is *ex-ante* ambiguous. On the one hand, such legislation should reduce discriminatory firings of pregnant women, positively affecting their employment rates. On the other hand, discriminating employers might respond by shifting the discrimination on the hiring margin, also *de jure* forbidden, but much more difficult to enforce, with a negative effect on employment. Moreover, this type of policies may have important distributional effects: women who are not pregnant nor planning to have children may also be affected by the reductions in hiring, but not benefit from stronger employment protection. The effect of pregnancy-discrimination laws on the employment and wage dynamics of women in fertile age is thus an open question.

This paper tries to answer this question by taking both a theoretical and empirical approach. We use a simple matching model of the labor market with exogenous wages and fertility to capture the effect of the legislation on the unemployment rate for non-pregnant women. After calibrating the model's parameters to match the economic setting of the late

 $^{^1\}mathrm{As}$ defined by the US Equal Employment Opportunity Commision (EEOC) at www.eeoc.gov/laws/guidance/fact-sheet-pregnancy-discrimination.

 $^{^2 \}mathrm{These}$ are Geduldig v. Aiello, 417 U.S. 484 (1974) and General Electric v. Gilbert (1976), 429 U.S. 125.

³U.S. Equal Employment Opportunity Commission Guidelines (1997).

1970s, we analyze two potential scenarios, determined by the value of a parameter governing the extent of discrimination present in the market before the PDA was enacted. In both of them, the extent to which the legislation punishes discriminating behavior (and is enforced) is crucial. We find that imperfect implementation or mild sanctions are detrimental for women employment, causing an increase in unemployment rate without any effective increase in the protection of employed pregnant women. Conditional on the legislation being effectively implemented, we find that it is still possible, given the calibrated values of the parameters, that the law causes an overall increase in unemployment. However, we also find that another more optimistic scenario, in which the final effect on unemployment is negative, is possible.

With this theoretical framework in mind, we then move to the empirical analysis. We identify the causal effects of interest exploiting variation in timing of treatment generated by the fact that certain US states enacting legislation similar to the PDA prior to 1978. While data limitations at the moment do not allow us to exploit the full extent of identifying variation coming from this staggered adoption of pregnancy-discrimination policies, we can leverage on the fact that at the passage of the PDA in 1978, some US states became 'treated' for the first time, while others had already been treated for some years. We estimate this "Difference-in-differences in Reverse" estimator (Kim and Lee, 2019) using individual-level survey data from the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) to measure employment status, hiring and firing, wages, and fertility matched with a novel dataset that we compiled gathering detailed information on US states' pregnancy-discrimination policies passed prior to 1978. Under the assumption that trends of states treated first in 1978 would have been parallel, had they been treated earlier, to those of states that adopted these policies earlier, we find evidence that the enactment of the PDA led to a substantial and statistically significant decline in the likelihood of employment among women in fertile age, by 4.6 percentage points. Moreover, results using proxies of layoffs and quits of pregnant workers show that these did not decline in response to the PDA. With the lenses of the theoretical framework, this suggests that in practice there was not a strong enough implementation of the law, generating a decrease of female employment, and no significant decrease in dismissals for pregnant women.

When analyzing hourly wages, our results are more sensitive to the specification we use, but generally indicate smaller responses. We interpret this as legitimating the model's hypothesis that the prevailing institutional environment, and in particular the effect of Equal Pay Act (EPA) of 1963, which required that men and women holding the same position in a firm be paid an equal wage, may have limited substantial wage adjustments. Wage rigidities are a potential justification of the significant decline in employment we observe, as the reduced ability of wages to react could have exacerbated the policy's impact on unemployment rates. We show that this conjecture is supported empirically: if we restrict the attention to fertile-age women employed in female-dominated sectors, where the EPA is likely to pose a constraint, we detect significant negative effects on women's wages and a much smaller response of employment.

Finally, we do not find any significant effect of the law on fertility, which on the one hand appears to justify our assumption of exogenous pregnancy in the theoretical framework, and on the other hand could reflect the fact that, in line with our previous results, women do not respond to the formally higher job protection, but rationally take into account the fact that the rate of dismissals remains unchanged.

Our paper relates to the large literature on the rights and benefits of pregnant individuals in the labor market and their effects on employment and socioeconomic outcomes of women. This literature mainly focused on the effects of maternity leave, generally paid leave, and mandated health benefits (Waldfogel, 1998; Baum II, 2003; Baker and Milligan, 2008; Dahl et al., 2016; Bana, Bedard, and Rossin-Slater, 2020; Flores, Gayle, and Hincapié, 2023; Bailey, Byker, et al., 2024). In particular, the seminal work of Gruber (1994) on mandated health benefits and the more recent work of Timpe (2024) on the first paid maternity leave programs in the US, both use policy variations generated by the enactment of the PDA of 1978, and are thus closely related to our work. We contribute to these studies and to the larger body of literature about treatment of pregnancy in employment being the first to study a different, but central component of these legislation: employment protection of pregnant workers, which we isolate by looking at a different set of US states' policies prior to 1978. We show that mandating employment protection of pregnant workers, even without paid leave or mandated health benefits, can have sizeable effects on women's employment outcomes. We document that the PDA worsened employment outcomes of fertility-age women the PDA worsened employment outcomes of fertility-age women, likely due to its inability to significantly increase the costs of firing pregnant workers. These unintended consequences had been initially hypothesized in Law (Posner, 1989), but never formalized and verified empirically. Part of these findings mirror earlier evidence found by Acemoglu and Angrist (2001), who study the American with Disabilities Act, that mandated employment protection and special accommodations for worker with disabilities.

In this regard, our work also speaks to the earlier but still steadily growing strand of literature about gender discrimination in the labor market and legislative attempts to reduce gender gaps (these include works such as Zabalza and Tzannatos, 1985; Neumark and Stock, 2006; Doepke, Tertilt, and Voena, 2012; see Blau and Kahn, 2017 and Goldin, 2023 for recent comprehensive reviews). In this body of work, the recent work of Bailey, Helgerman, and Stuart (2024) about the effects of the Equal Pay Act of 1963 is particularly close to our setting and also uses a similar strategy. In this paper, we build on their evidence and contribute to this literature showing how the Equal Pay Act of 1963 mediated the effects of the Pregnancy Discrimination Act on employment and wages.

The remainder of this paper proceeds as follows: section 2 describes the provisions

of the Pregnancy Discrimination Act of 1978, the context in which it was adopted, and earlier comparable policies adopted by US states; in section 3 we present the theoretical framework and discuss the *ex-ante* possible effects of the PDA; in section 4 we outline the empirical strategy and describe the data we use to estimate it in section 5; section 6 presents the main results and some interesting evidence on the mediating role of the EPA of 1963, and finally section 7 concludes.

2 Background

In this section, we provide a brief overview of the Pregnancy Discrimination Act of 1978, the context in which it was passed, and of comparable legislation that was passed by some US states before 1978.

2.1 The Pregnancy Discrimination Act of 1978

The Pregnancy Discrimination Act of October 31, 1978, stands as a pivotal moment in U.S. legislative history, amending Title VII of the Civil Rights Act of 1964 to address discrimination based on pregnancy⁴. Prior to its passage, controversial Supreme Court decisions, such as Geduldig v. Aiello, 417 U.S. 484 (1974) and General Electric v. Gilbert (1976), 429 U.S. 125, had excluded pregnancy from the scope of sex discrimination protection, leaving a legislative gap that the PDA aimed to fill. The main rationale behind these Supreme Court decisions was that firms' temporary disability plans could exclude pregnancy as both men and women, who can both be *non-pregnant*, benefit from the exclusion of this condition from those covered by the plan, while only women would benefit from its inclusion⁵. This judicial stance highlighted the need for statutory protections, as the Supreme Court hesitated to extend protection through judicial action, leaving Congress, which at the time had a Democratic super-majority, and the Carter administration to address the issue legislatively. This met support by a coalition of civil rights and women's movements, labor unions, at a time when the labor market was witnessing stable growth in female employment levels, especially compared to the almost constant male ones, and rapid growth in nominal wages, as seen in figure A1.

The core provision of the PDA mandated that pregnancy be treated as a temporary disability by employers, applying to establishments with 15 or more employees⁶. De jure,

⁴https://www.eeoc.gov/statutes/pregnancy-discrimination-act-1978. The Civil Rights Act of 1964 forbids employment discrimination on the basis of "race, color, religion, sex, or national origin" (Public Law 88–352, 78 Stat. 241, enacted July 2, 1964).

⁵This is clearly expressed in footnote 20 of the majority opinion in Geduldig v. Aiello (1974) "The program divides potential recipients into two groups – pregnant women and non-pregnant persons. While the first group is exclusively female, the second includes members of both sexes. The fiscal and actuarial benefits of the program thus accrue to members of both sexes" https://supreme.justia.com/cases/federal/us/417/484/#F20.

 $^{^{6}}$ The text of the Act reads that "women affected by pregnancy, childbirth, or related medical conditions

this forbade employment discrimination in all aspects of employment, including: (i) hiring or the job application and selection process; (ii) firing from a job, reduction of hours, layoff, or termination of employment; (iii) pay, job assignments, or promotions; and (iv) training, employee benefits, or any other term or condition of employment⁷ De facto, as in analogous contexts⁸, other than extensions of employer-provided health insurance benefits⁹, the PDA effectively aimed at establishing a form of employment protection for pregnant workers at the federal level, with discrimination at the hiring stage being much harder to detect and prove in a court of law.

Finally, it is important to note that the Act did not introduce special protections for pregnant workers but rather insisted on *equal* treatment relative to temporary disabilities¹⁰. Hence, it granted employment protection of pregnant women only in firms that protected employment of temporarily disabled workers (usually, via job-protected unpaid leaves). Thus, what we identify is actually the effect of this *equal protection mandate* and not of actual employment protection of *all* pregnant workers. For the sake of brevity, we will refer at the effects of the PDA as the effects of mandated employment protection of pregnant women, but it is important to keep this difference in mind.

2.2 US States' Legislation on Pregnancy Discrimination Before 1978

The fact that pregnancy discrimination was a salient issue in the 1960s and 1970s US context is clearly exemplified by the various legislative attempts and notorious court cases that discussed and aimed at limiting this phenomenon. Indeed, in the early 1970s while at the federal level the US Supreme Court upheld that pregnancy was not covered by gender-discrimination laws, numerous states independently strengthened protection of pregnant workers. By the time that the PDA was enacted, 21 US states already had some type of regulation in place that disciplined the treatment of pregnancy in the workplace. While these policies varied both in their provisions and mode of passage (via acts of legislation, administrative rulings, and states Supreme Courts' decisions), they all mandated at least

shall be treated the same for all employment-related purposes, including receipt of benefits under fringe benefit programs, as other persons not so affected but similar in their ability or inability to work".

⁷See the EEOC guidelines to Pregnancy Discrimination and Pregnancy-Related Disability Discrimination at https://www.eeoc.gov/pregnancy-discrimination.

⁸For instance, disability discrimination and coverage of the American with Disabilities Act (ADA) (Acemoglu and Angrist, 2001).

⁹This aspect of the Act has been extensively studied since the seminal work of Gruber (1994) and only applied to establishment with certain insurance plans in place.

¹⁰Indeed, a large body of literature in Law studied a different type of *unintended consequence* of the PDA, separate from the one we consider here. Namely, the fact that mandating equal treatment could hinder employers' and states' attempts at providing special protections and benefits to pregnant workers, that are not granted to other temporarily disabled workers (Remmers, 1989). The most notorious instance of this conflict is represented by the US Supreme Court case California Federal S. & L. Assn. v. Guerra, 479 U.S. 272 (1987) about the California Fair Employment and Housing Act 12945(b)(2), which required employers to provide leave and reinstatement to employees disabled by pregnancy.

the level of employment protection of pregnant workers that was then required by the PDA¹¹.

Of these 21 states, ten enacted policies that simply mandated equality of treatment of pregnancy and temporary disabilities, as the PDA¹². Six states implemented policies that, in addition to forbidding pregnancy discrimination, required employers to provide job-protected unpaid leaves of reasonable length¹³. Finally, five states further required the leave to be paid since they mandated equal treatment of pregnancy and had state-level universal STDI policies¹⁴. The remaining 30 states did not adopt any comparable policy and thus aligned, regarding the pregnancy-discrimination component, when the PDA was approved at the Federal level in late 1978. This variation is key for our identification strategy, as we outline in section 4, and is visually represented in figure A3.

3 Theoretical Framework

To capture the effect of the PDA on the employment of women, we use a simple job protection model in a labor market matching setting (see for example Mortensen and Pissarides (1999)). This class of models is particularly useful to analyze the effects generated by legislation which protects a class of workers on the broader labor market.

There is a continuum N of identical women workers in the economy¹⁵. As in Xiao (2023), at this stage we take pregnancy decisions as exogenous and assume that, in each period (unless they are already pregnant or have recently had a child), a fraction q of women becomes pregnant. This assumption appears to be supported by the data (see section 6.2), but we have also explored an alternative version of the model which focuses more on the employee's decision to have children¹⁶. We define an employee's "pregnancy status" as the time period in which a worker is pregnant or recently gave birth and may therefore need to (1) be on leave from work and (2) receive some special accommodations

¹¹These states policies do not entirely coincide with those considered in Timpe (2024), whose focus is on paid maternity leave and thus on policies that provided this either by themselves or in conjunction with states' short-term disability insurance (STDI) policies. Nonetheless, we are very grateful to Brenden Timpe for sharing his notes on US states policies prior to 1978.

¹²These are: Pennsylvania in 1973; Alaska, Iowa, South Dakota, and Wisconsin in 1975; Illinois in 1976; Maryland, Michigan, Minnesota, and District of Columbia in 1977.

¹³These are: Colorado and Massachusetts in 1972; Connecticut and Washington in 1973; Kansas in 1974; and Montana in 1975. See for instance an extract from the original text of the Montana Maternity Leave (MCA, § Ch. 26 41-2602) in figure A2, where it is apparent that these types of regulations also include the same provisions of the PDA in terms of employment protection.

¹⁴These are: Rhode Island in 1942; New Jersey in 1961; Hawaii in 1973; California and New York in 1977.

¹⁵We choose to focus the model on the labor market for women in fertile age and not explicitly represent young men or older women, because we believe both are unlikely substitutes: the former due to the highly segregated nature of the market at the time and because men were close to full employment, and the latter because reasonably they had different skills, and were mostly either already employed or out of the labor force.

¹⁶Derivations are available upon request.

if working, in accordance with the provisions of the PDA.

Upon observing a pregnancy, the employer decides whether to keep the worker or terminate the employment and open a new vacancy (endogenous separations). This means that the PDA, by strengthening employment protection, can, at least *a priori*, directly serve as an instrument to increase women employment, even without taking into consideration the possibility of suing for discrimination in hiring, which as we know from data in similar settings¹⁷, is a much less effective threat against discriminating behaviors.

Throughout our theoretical analysis, we maintain the assumption of fixed wages. This is consistent with existing literature on employment protection in contexts where the institutional framework limits the ability of firms and workers to adjust wages in response to policy changes (see for instance the review in Cahuc, Carcillo, and Zylberberg (2014)). Notably, by 1978, when the PDA was approved, the Equal Pay Act of 1963 was already in effect, mandating employers to pay equal wages for men and women employed in the same position. It is then plausible that men's wages acted as a constraint on the flexibility of overall wage adjustments, particularly in sectors where men comprised a significant portion of the workforce (see Acemoglu and Angrist (2001), for related considerations in a different setting). In our empirical analysis, we show that this assumption appears largely supported by the data, with certain exceptions that we comment and study more in detail below (section 6.4).

We focus the analysis on firms for which the PDA is binding (i.e. they have protection in place for temporarily disabled workers), so that the firing and replacing of pregnant women legally constitutes discriminating behavior after the law is passed. We also allow for exogenous separations that can occur at rate δ , independently of pregnancy. In every period, a job in a firm can be filled by a non-pregnant worker, by a pregnant worker or be vacant. The flow value of a job filled by a non-pregnant worker is

$$r\Pi^{n} = y - w + q(\max\{\Pi^{k}, \Pi^{v} - K\} - \Pi^{n}) + \delta(\Pi^{v} - \Pi^{n})$$
(1)

where w is the wage, Π^k is the value of keeping the pregnant worker, Π^v is the value of a vacancy and K is the cost associated with firing a pregnant worker. These costs may arise to some extent even in the absence of the PDA, due to organizational, legal, and administrative expenses associated with dismissals. However, we view the PDA an exogenous shifter of K: in a scenario of full compliance, firms would not have the ability to terminate pregnant employees except for non-pregnancy-related reasons. This would be captured by a substantial increase in K. In reality, we also account for instances where K assumes lower or intermediate values, to account for the possibility of imperfect enforcement of the law¹⁸.

¹⁷See for instance Acemoglu and Angrist (2001).

¹⁸One way to see this is to express K as the *expected* cost of firing a pregnant worker, K = pD, given by the product of the probability p of being found guilty in a court of law and the amount of damages

Next, we have to characterize the flow value accruing to firms which decide not to fire pregnant workers. This is given by

$$r\Pi^k = y - w - c + \mu(\Pi^n - \Pi^k) + \delta(\Pi^v - \alpha K - \Pi^k)$$
⁽²⁾

where c represents the organizational costs that are required to temporarily replace the worker while on maternity leave, to provide light-duty, modified tasks to accommodate pregnancy or any temporary disability related to it as required by the law (see U.S. Equal Employment Opportunity Commission Guidelines 1997), or even a taste parameter causing discrimination à la Becker (1971). We denote μ the rate at which pregnant workers "exit" their pregnancy status. That is, at any given period, the fraction of pregnant workers who gave birth, are ready to go back to work if they took maternity leave, and do not need any special accommodation at the workplace¹⁹. Note that, even if the firm decides not to dismiss the worker because of pregnancy, there could be other factors causing a separation. Here we allow for the fact that the firm could still incur some legal costs with probability $\alpha \in [0, 1]$, in situations where it may be difficult to determine the reasons for the dismissal. Finally, the flow value of a vacancy is

$$r\Pi^{v} = -v + m(\theta)(\Pi^{n} - \Pi^{v})$$
(3)

where v is a vacancy cost (e.g. costs of opening a position and looking for candidates) and $m(\theta)$ is the rate at which vacant jobs are filled as a function of labor market tightness, $\theta \equiv V/U$. Notice that (3) implicitly makes the simplifying assumption that only nonpregnant women match with open vacancies. While legislation as the PDA also forbids discrimination of pregnant women at the hiring stage, this is known to be extremely hard to enforce (see for instance, Acemoglu and Angrist, 2001). In any case, we plan to check the robustness of our results when we explicitly consider discrimination in hiring, in an expanded version of this model.

Free entry implies that $\Pi^v = 0$. Then, from Equation (3), we have

$$\Pi^n = \frac{v}{m(\theta)} \tag{4}$$

Moreover, we can rewrite (1) and (2) as

$$r\Pi^{n} = y - w + q(\max\{\Pi^{k}, -K\} - \Pi^{n}) - \delta\Pi^{n}$$
(5)

and legal expenses D. Changes in both D and p cause changes in K.

¹⁹We take μ as exogenous and fixed in this setting, but technological changes and organizational improvements in the workplace can certainly influence this parameter. For instance, the introduction of the baby formula, with its positive effects on female employment, studied by Albanesi and Olivetti (2016), or the introduction of work-from-home can be conceptualized as increases in μ .

and

$$r\Pi^{k} = y - w - c + \mu(\Pi^{n} - \Pi^{k}) + \delta(-\alpha K - \Pi^{k})$$

$$\tag{6}$$

We are interested in analyzing the effect of an intensification of the legislation protecting pregnant women, which is modeled as an increase in K. Note first that, in the absence of legislation, discrimination only occurs if, when K = 0, $\Pi^k < 0$. Assuming that this is the case, depending on the value of the parameters, we may have an equilibrium in which discriminating employers fire pregnant workers and one in which they do not. Intuitively, when K is low, employers might prefer incurring the firing costs and replacing the worker, while when K is high they might choose to keep the worker employed. If effective, the PDA should take the labor market from the first to the second equilibrium. As we discuss in detail below, the effect of the policy on women's unemployment rate is ambiguous.

In what follows, unless specified, we make the following two assumptions.

Assumption 1 (Relevance of the PDA). In the absence of legislation, there would be discrimination of pregnant women in the labor market. In particular, when K = 0, $\Pi^k < 0$, or, in terms of the parameters,

$$c > \frac{(y-w)(q+r+\delta+\mu)}{q+r+\delta}$$

This is a natural assumption: if this were not the case, there would not have been the need to introduce the legislation in the first place. The interpretation of the condition on c is that the taste-discrimination parameter or organizational cost of not dismissing pregnant workers is relatively high, or perceived high by the employer relative to the share of surplus that the firm appropriates.

Assumption 2 (No mistrials). A court of law can establish with reasonable accuracy whether a separation was illegal (i.e. on the basis of a pregnancy) or due to other factors. That is, $\alpha \approx 0$.

This second assumption simplifies the analysis without significantly altering the main qualitative results. As we explain more in detail below, a higher α would mechanically exacerbate the negative effects of employment protection on overall female employment, since non-discriminating firms would internalize the risk of being wrongly accused of discriminatory firing and thus further reduce the number of hires. We instead show here that a negative effect of the PDA on hirings is present even in the benchmark when the legislator can rely on an effective judicial system ($\alpha = 0$).

The first step of the analysis, then, is to characterize the threshold \overline{K} at which the PDA becomes binding in terms of the model's fundamentals. At this level of K, the firm is indifferent between firing and keeping the worker, i.e. we must have

$$\Pi^k = \Pi^v - \overline{K} \quad \Rightarrow \quad \Pi^k = -\overline{K}$$

After some calculations we get

$$\overline{K} = -\frac{(y-w)(q+r+\delta+\mu) - c(q+r+\delta)}{(r+\delta)(q+r+\delta+\mu)}$$

which is positive under Assumption 1.

We can picture how the decision of the employer changes with K as follows.

Figure 1: Illustration of firms' response to legislation. To the left of the threshold \overline{K} firms choose to pay -K and fire the worker, to the right of \overline{K} they choose to keep the worker.



Since our focus is on the potentially unintended consequences of the PDA on nonpregnant women, we analyze the effect of the policy on the unemployment rate among women who are not pregnant²⁰, u^n .

Consider first the case where $K < \overline{K}$. This is the situation where the employers prefer to pay the firing costs (e.g. incur the risk of litigation) and dismiss pregnant workers. We have

$$\begin{cases} \dot{u}^{n} = \delta(1 - u^{n} - u^{c}) + \mu u^{c} - qu^{n} - m(\theta)\theta u^{n} \\ \dot{u}^{c} = q(1 - u^{n} - u^{c}) + qu^{n} - \mu u^{c} \\ \dot{u}^{n} = \dot{u}^{c} = 0 \end{cases}$$

The first equation tells us the rate of change of unemployment among non-pregnant workers. The inflow in each period is due to δ non-pregnant workers who are dismissed, and μ , the rate at which pregnant women 'exit' the pregnancy status. The outflow is made of $\theta m(\theta)$ workers who are hired in each period and q workers who become pregnant. The second equation specifies the rate of change of unemployment among pregnant women. Each period, we have an inflow of q pregnant employed women who are dismissed by the

 $^{^{20}}$ We extend the analysis to the overall unemployment rate for women in the appendix.

discriminating employers and q unemployed women who become pregnant. The outflow is due to μ workers exiting the pregnancy status. Finally, the last equation summarizes the steady state conditions.

Solving the system, we obtain an expression for the rate of unemployment of nonpregnant women

$$u_f^n = \frac{(q+\delta)\mu}{(q+\delta+m(\theta)\theta)(q+\mu)} \tag{7}$$

This expression confirms the intuition that u_f^n increases with the rate of job destruction, which is proportional to $q + \delta$ and decrease with the rate of job creation, $m(\theta)\theta$. As long as we remain in the region with $K < \overline{K}$, the strengthening of employment protection does not have any impact on job destruction but it does affect job creation, as we now show.

To study the effect of a change in K on $m(\theta)\theta$, we can solve (5) for Π^n and get

$$\Pi^n = \frac{y - w - qK}{r + q + \delta}$$

Using (4), we have

$$\frac{v}{m(\theta)} = \frac{y - w - qK}{r + q + \delta} \tag{8}$$

Taking the differential with respect to θ and K we obtain

$$\frac{d\theta}{dK} = \frac{q}{v(r+q+\delta)} \cdot \frac{m(\theta)^2}{m'(\theta)} < 0$$

Thus, an increase in K causes a reduction in the tightness of the market and therefore, given the standard assumptions on the matching function, a reduction in $m(\theta)\theta$. In other words, in this region an increase in K does not affect job destruction but only the rate at which firms post new vacancies, thereby unambiguously increasing unemployment.

Next, we consider the case where $K > \overline{K}$. Here, following similar steps as above, we can derive the unemployment rate among non-pregnant women as

$$u_k^n = \frac{(1+q)\delta\mu}{q^2\delta + (\delta + m(\theta)\theta)\mu + q\delta(1+m(\theta)\theta + \mu)}$$
(9)

As long as δ is small and μ sufficiently large, it is possible to show that $u_k^n < u_f^n$ for any given θ . This intuitively reflects the fact that, as employment protection becomes binding, a smaller number of pregnant women get fired. This reduces unemployment among *non*-pregnant women, because it stops the influx of those workers who were previously pregnant, lost the job and are now looking for new employment (which is indeed governed by μ). However, as we discussed above, we have to compare this positive effect of an increase in K to the potential effect on market tightness. In particular, when $K > \overline{K}$ we have

$$\frac{v}{m(\theta)} = \frac{(y-w)(r+\mu+q+\delta) - q(c+\alpha\delta K)}{(r+\delta)(q+r+\mu+\delta)}$$

and

$$\frac{d\theta}{dK} = \frac{\alpha q\delta}{v(r+\delta)(r+\mu+q+\delta)} \cdot \frac{m(\theta)^2}{m'(\theta)} \le 0$$

When we are above the threshold, \overline{K} , an increase in K determines a decrease in market tightness. It is important to note that this is driven by the assumption that the firm may have to bear some legal costs for dismissing workers even when this is not on the basis of pregnancy. If this effect is negligible, i.e. $\alpha \approx 0$, - which we can interpret as the case where a court of law can effectively determine whether the dismissal was discriminatory or not - we get that an intensification of employment protection when $K > \overline{K}$ has no effect on job creation.

Finally, we look at the more interesting case where the policy brings us from below to above the threshold. In this case a local analysis is not viable, as we could have nonmonotonic responses of the unemployment rate to changes in K. We already commented that, for reasonable values of the parameters, $u_k^n < u_f^n$ at each given level of market tightness θ . We then have to determine the effect of the change in K on $m(\theta)\theta$. It turns out that the overall effect could be positive or negative, depending on the values of the parameters and the initial value of K, as shown in the following figure.

Figure 2: Effect of the PDA on the unemployment rate of non-pregnant women as a function of K for different values of c



Notes: We assume a Cob-Douglas matching function $M(V,U) = V^{\gamma}U^{1-\gamma}$. The baseline values of the parameters are y = 1, r = 0.075, $\delta = 0.02$, q = 0.01, $\mu = 0.54$, w = 0.984, $\gamma = 0.5$, $\alpha = 0$, v = 0.5. See appendix A.1 for details on calibration. We express K as fraction of average monthly production. In the left-hand graph we assume a value of c = 0.3, in the right-hand graph c = 0.6.

The figure shows that indeed the unemployment rate is discontinuous in K around the threshold \overline{K} . If we take the initial (pre-PDA) value of K to be close to 0, then the left-hand graph shows that a sufficiently large increase in K until after the threshold would reduce unemployment. Conversely, in the right-hand graph we see that a similar intervention would determine an increase in unemployment.

The critical factor is the value of the discrimination parameter c. If c takes low-tointermediate values, then a strong legislative intervention, taking the economy from a situation with $K < \overline{K}$ to $K > \overline{K}$, would yield a beneficial effect on women employment. However, when c is relatively high, the in tervention could worsen unemployment among non-pregnant women.

All these effects are reinforced by the inability of wages to adjust. With flexible wages, we would likely observe a reduction in wage and smoother reactions of unemployment to the policy. An empirical confirmation of these considerations is presented in section 6.4.

Notice that throughout the analysis we focused on the effect of the policy on the employment of non-pregnant women. In the appendix (section A.2), we also look at the effect on the total women's unemployment rate. The results remain qualitatively similar for reasonable values for the parameters. We still find that the policy has a negative effect on women's unemployment for values $K < \overline{K}$, and that it can have an ambiguous effect the total unemployment rate u when we go from $K < \overline{K}$ to $K > \overline{K}$. The only difference is that now we are of course internalizing the *direct* effect of the protection of working pregnant women, so the set of parameter values c for which the policy has an overall positive effect expands.

We thus conclude that the net effect of the policy on u^n is ambiguous, and turn to the empirical analysis to see (1) whether the PDA's provisions and implementation were strong enough to bring the labor market from below to above the threshold \overline{K} , and (2) if successful in achieving a $K > \overline{K}$ identify which of the two regimes described in figure 2 realized in practice.

4 Empirical Strategy

The aim of this paper is to document how the Pregnancy Discrimination Act of 1978 affected employment and wage outcomes of women in fertile ages. Since this law was a federal act of legislation the policy does not provide us with a natural comparison group that can be used as a reasonable counterfactual absent the policy change. Obviously, a simple before versus after comparison around the enactment of the PDA of 1978 would not allow us to separate its effects from those of concurrent changes affecting the outcomes of interest. In a period of steep growth in female employment and wages, as shown in figure A1, and also hit by numerous recessions, this simple comparison would not reveal much²¹.

²¹The provisions of the Act would indeed naturally point to a clean quasi-experiment, as the Act only applied to establishments employing 15 or more employees. Unfortunately, we cannot use this threshold for a regression discontinuity design due to data limitations. The CPS ASEC survey that we use, described in detail in section 5.2, only records establishment size starting from 1988 and even then, it groups firms

Nonetheless, as we detailed in section 2, some US states enacted legislation comparable to the PDA of 1978. This generates useful variation in the *timing* of treatment, that we exploit with our identification strategy. Specifically, we rely on the fact that the treatment status of 'early-adopting' states did not change in 1978²², while other states were first treated with the passage of the PDA.

Specifically, the first set of states consists of Connecticut, Massachusetts, New Jersey, and Pennsylvania. The second set, those first treated with the PDA of 1978, includes Alabama, Arkansas, Florida, Georgia, Indiana, Kentucky, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. To avoid confusion, we will not define these states as Treated and Control states, but rather call the former Stayers, as their treatment status stays the same throughout the observation period, and the latter Switchers, as their treatment status changes after 1978. This is consistent with the nomenclature of Tazhitdinova and Vazquez-Bare (2023). We display these sets of states in figure A4. Our choice of states for the two groups was mainly guided by two factors. First, we chose as stayers the states that were treated no later than 1973. This ensures that we observe 6 years before the enactment of the PDA. A sufficiently long pre-period is even more important in this setting than in traditional settings where difference-in-differences types of estimators are applied, given that the period before switchers are treated grants us cross-state variation in treatment status. Second, we chose, among the potential set of stayers and switchers, those that in the CPS ASEC data were not grouped with states that were treated at some point strictly between 1973 and 1978. Unfortunately, the CPS ASEC dataset groups numerous states together in years between 1968 and 1976, close to our policy variations. We are thus forced to exclude subgroups of states whose treatment status is heterogeneous²³. While this is definitely not the ideal way to select comparison

of less than 25 employees together, exactly encompassing the 15-employees threshold established by the Act. Similar surveys that we considered, such as the National Longitudinal Surveys (NLS) and Panel Study of Income Dynamics (PSID), share similar limitations and have smaller sample sizes. Furthermore, establishment-level datasets, such as the Annual Survey of Manufacturers do not report employment breakdowns by gender for the 1970s and 1980s (surprisingly given the availability of such information for the 1930s) and could also not be used.

 $^{^{22}}$ Indeed, regulation and court decisions that affected the four states that we use as reference (Connecticut, Massachusetts, New Jersey, and Pennsylvania) had broader applicability than the PDA of 1978, so that the policy regime regarding employment protection of pregnant women in these four states really did not change in 1978. In New Jersey, state legislation did not have an establishment-size threshold (N.J.S. § 43:21-29). Similarly the Pennsylvania Supreme Court decision that mandated employment protection of pregnant women applied to establishments of all sizes (Cerra v. East Stroudsburg Area School District). Instead, state legislation in Connecticut (Pub. Act No. 73-647, Conn. Gen. Stat. §§ 46a-60(a)(7)) and Massachusetts (Mass. Gen. Laws Ch. 149 § 105D) applied to all establishments of at least three and six employees, respectively. Furthermore, as discussed in section 2, note that all these states, except Pennsylvania, mandated some type of job-protected pregnancy leave on top of forbidding pregnancy discrimination. Since this is constant throughout the period we study, after the enactment of these states' legislation, these differences are not problematic for our identification strategy and are captured by time-invariant differences across states.

²³This data limitation also prevents us from exploiting other quasi-experiments generated by the staggered adoption of pregnancy-discrimination legislation. Unfortunately, as noted in section 2, most of these legislative efforts took place exactly in the early 1970s, when a lot of states are not separately

groups, visual examination of raw trends of average outcomes in these states reassures us that these groups are sufficiently comparable in this period. We discuss these potential issues and robustness to the choice of stayer states along with the discussion of results in section 6.

The estimator we adopt is what Kim and Lee (2019) name 'Difference-in-differences in Reverse' (DDR). As in a standard difference-in-differences estimator, we compare the switchers' and stayers' outcomes before and after the enactment of the PDA. In practice, we identify the parameter of interest, the average effect of mandating employment protection of pregnant women, using a usual two-way fixed effects (TWFE) specification on our repeated cross-section of individuals from these states. We estimate this on the subgroup of the population that is potentially directly affected by this law, women aged 18 to 35. The group of women that is significantly more likely to experience pregnancy, as clearly seen in figure A5. That is, we estimate the following regression

$$y_{i,s,t} = \beta SW_s \times P_t + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t}$$
(10)

where $y_{i,s,t}$ is the outcome of interest for a woman age *i* of age between 18 and 35, residing in state *s*, in year *t*. SW_s is a dummy that takes value one if state *s* is a switcher state and zero if it is a stayer state. P_t is a dummy equal to one for post-PDA years, from 1979 onwards, and zero otherwise. We start post-periods from 1979 since the PDA was approved on October 31, 1978. $X_{i,s,t}$ is a vector of individual-level characteristics, including a polynomial in age, marital status, race, education, and residency in a metropolitan area. θ_s are state fixed effects, and δ_t are year fixed effects. To be precise, *s* indexes states for states that are separately identified in CPS ASEC throughout the observation period, from 1973, and groups of states for those that are grouped²⁴. Finally, standard errors are robust to heteroskedasticity²⁵. The inclusion of year fixed effects helps us separating the effect of the pregnancy-discrimination legislation from concurrent factors affecting employment outcomes of women, such as the positive growth in female employment and wages observed at the national level, but also recessions that hit the US economy around the passage of the PDA²⁶.

The parameter of interest in equation (10) is the coefficient β . This parameter identifies the average treatment effect on the switchers (as in a normal difference-in-differences setting it would identify the average treatment effect on the treated (ATT)) under a parallel

observable in our data. However, we are confident that data not affected by this limitation (for example the EEO-1 establishment-level data for which we are applying again) will significantly strengthen our analyses and allow us to expand our current project on several fronts.

²⁴These groups are: Alabama - Mississippi, North Carolina - South Carolina - Georgia, Kentucky - Tennessee, and Arkansas - Louisiana - Oklahoma.

 $^{^{25}}$ We do not cluster at the level of our policy variation, the state, as we only have 12 clusters.

²⁶Three recessions were particularly close to this period. The Oil Embargo Recession, from November 1973 to March 1975 and the two early 1980s recessions, the first one from January 1980 to July 1980 and the second one from July 1981 to November 1982.

trends assumption. Specifically, we require that outcomes of individuals in switcher and stayer states would have evolved in parallel, had both groups been 'treated' with pregnancy-discrimination laws before and after 1978. This assumption is similar to, but stronger than the parallel trends assumption needed to identify the ATT in standard difference-in-differences setting with equal treatment status at baseline. In our case, we further require that treatment effects are not dynamic. That is, they do not vary with respect to event-time²⁷. Intuitively, this is because we use the trend in outcomes of the stayers group to construct the counterfactual outcome of the switchers, had they been treated in the pre-period. If for instance the treatment effect was growing in time relative to the beginning of treatment, we would underestimate the treatment effect, erroneously attributing part of it to time trends. Notice however that, despite both groups being treated, we do not need to assume an assumption of homogeneous treatment effects. As long as they are constant, an instantaneous shift upon treatment, they can be heterogeneous across switchers and stayers. Of course, as in the usual difference-in-differences setting, if they are homogeneous, then β identifies the average treatment effect (ATE) for the population.

We then augment this strategy relying on the fact that the policy is expected to affect mainly women in fertile ages who are pregnant or may be pregnant in the future. Instead, the policy should not directly affect older women and men, if not through substitution channels, that we deem much less strong. With men close to full employment and strong segregation across occupations, we do not expect substitution effects in employment outcomes to be first order. Older women are also likely not a close substitute, given that they are generally either employed or out of the labor force, with virtually zero re-entry rates after exit, and have different skills. We exploit this characteristic of pregnancydiscrimination policies to strengthen our empirical strategy adding a further difference: the one between outcomes of women aged 18 to 35 and of women 40 to 60 and men of ages 18 to 60. We thus do a triple-difference-in-differences strategy, in reverse. Let $I_{i,s,t}$ be an indicator taking value of one if the individual is a woman aged 18 to 35 and zero if she/he belongs to one of the other two groups. We augment specification (10) estimating

$$y_{i,s,t} = \beta SW_s \times P_t \times I_{i,s,t} + \alpha SW_s \times I_{i,s,t} + \nu P_t \times I_{i,s,t} + \rho I_{i,s,t} + \Gamma X_{i,s,t} + \eta_{s,t} + \varepsilon_{i,s,t}$$
(11)

where $\eta_{s,t}$ are state-by-year fixed effects and all other terms are as described above. Notice that the interaction between the switcher and post-PDA indicators is subsumed by state-by-year fixed effects. If pregnancy-discrimination laws affect only women aged 18 to 35, and not the other included individuals, then β in (11) identifies the parameter of

²⁷See Tazhitdinova and Vazquez-Bare (2023) for a formal proof of this and a discussion of the only limit (and unrealistic) case in which treatment effects are dynamic but estimates are not biased.

interest under a less stringent parallel trends assumption. With the inclusion of a control group within each state, we can include in the specification state-by-year fixed effects and linearly control for all factors that vary across time and states and affect outcomes of fertile-age women and the control group in the same way. For instance, this would capture the effect of state-specific trends in labor market conditions. We thus only require that the within-state difference between outcomes of treated and control individuals would have evolved in parallel in switcher and stayer states, had they both been treated before 1978. This is less stringent than the parallel trends presented above, but relies also on the assumption that the control individuals are not affected by the enactment of the PDA. Given that the latter might be violated, we include both specifications and, for the triple-difference one, multiple choices of control groups when we present our results.

As usual, the parallel trends assumptions described above are fundamentally untestable. This is because we do not observe the treated potential outcome for switchers before the policy change. However, we can assess their plausibility looking at *post*-PDA trends of switchers and stayers, to check if after the policy both groups trended in parallel, when they were both treated. This is discussed in detail in section 6.3, where we present estimates from the following event-study version of specification (10)

$$y_{i,s,t} = \sum_{l \neq 1979} \beta_l SW_s \times \mathbf{1}\{t = l\} + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t}$$
(12)

where we omit the first post-PDA year as a reference point and $\mathbf{1}\{t = l\}$ are year indicators. Coefficients β_l for $l \in \{1980, 1984\}$ inform us on the plausibility of the parallel trends assumption and those for $l \in \{1973, 1978\}$ show us the dynamic of the treatment effect. Thus, the latter also inform us on the plausibility of the constant-treatment effect assumption.

We similarly augment specification (11) as follows

$$y_{i,s,t} = \sum_{l \neq 1979} \beta_l SW_s \times \mathbf{1}\{t = l\} \times I_{i,s,t} + \alpha SW_s \times I_{i,s,t}$$

$$+ \sum_l \nu_l \mathbf{1}\{t = l\} \times I_{i,s,t} + \rho I_{i,s,t} + \Gamma X_{i,s,t} + \eta_{s,t} + \varepsilon_{i,s,t}$$
(13)

where terms are as described above.

5 Data

This section describes the raw data used in this paper as well as the data

5.1 Data on Pregnancy-Discrimination Policies

In order to construct our measure of treatment, the enactment of employment protection of pregnant workers, we carefully studies the provisions of the PDA and their interpretation in the Law literature²⁸. Moreover, we collected and codified information on all pregnancydiscrimination policies passed in US states prior to the enactment of the PDA in 1978, which we discussed in section 2.2. Our search was guided by work on similar policies (Gruber, 1994; Timpe, 2024), that had already listed and classified some of these policies, when that also included mandated health benefits and paid maternity leave. We complemented these lists using both primary and secondary sources, mostly surveying the large literature in Law on pregnancy discrimination, employment protection and maternity leave, and policy reports from the National Partnership for Women and Families and the US Census Bureau. When possible, we obtained the original text of these state laws, administrative rulings and court decisions. Otherwise, we collected information on their provisions and coverage from journal articles about them. In particular, we obtained these data from Stucke (1945), Dowd (1985), Gardin and Richwald (1986), and O'Brien and Madek (1988).

5.2 Employment Data

We use individual-level survey data from the CPS ASEC. This survey is an addendum to the March CPS, repeated yearly for a large cross-section of individuals, that has richer information on employment and earnings. Moreover, most survey questions are referred to the previous calendar year. While this may add noise due to imperfect recollection, compared to the usual CPS questions referred to the previous week, it offers a more comprehensive view of employment statuses and actual labor supply, compared to information referred to a single week.

We retain data from 1973 to 1984, to have six years before the enactment of the PDA on October 31, 1978, and six years after this event. We then restrict the dataset to the switcher and stayer states described in section 4. The switcher states are Alabama, Arkansas, Florida, Georgia, Indiana, Kentucky, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. Instead, the stayers are Connecticut, Massachusetts, New Jersey, and Pennsylvania. As mentioned above, due to limitations of the CPS dataset, we cannot separately identify three groups of states: Alabama - Mississippi, North Carolina - South Carolina - Georgia, Kentucky - Tennessee, and Arkansas - Louisiana - Oklahoma. For this reason, we did not include other switcher and stayer states, that were grouped together in our data. Finally, we further restricted our sample to women of age 18 to 35, the group that is directly affected by the policy, and older women (age 40 to 60) and all men between 18 and 60 years old, who constitute our control groups that should not be

 $^{^{28}}$ See for instance Weissmann (1983), Siegel (1985), Remmers (1989), and Habig (2007).

directly affected by the policy 29 .

We consider two main employment outcomes. First, an extensive margin measure of employment, whether the respondent worked at any time during the previous year. While this question was only available in certain years, we are able to thoroughly reconstruct it from other variables that are available throughout the period³⁰. Second, an intensive margin measure of employment, the number of weeks employed in the previous year. This information is only available from 1976, so we can only use four pre-PDA years (1975-1978) for analyses using this variables and others derived from it. Regarding wage outcomes, the ASEC survey does not record directly the hourly or weekly pay rate. Instead, it provides the annual total income from wage earnings for the previous year. We thus derive the hourly wage dividing this number by the total number of weeks worked in the previous year and by the usual number of hours worked per week in that year. While this derived variable will inevitably incorporate some noise, it represents our best pay rate measure that is not affected by changes in extensive or intensive margin variations in the respondent's labor supply.

In our analysis, two aspects of particular importance are hiring and firing. As argued in section 3, we posit that legislation targeting pregnancy discrimination is *de facto* akin to increasing the costs associated with firing pregnant employees, while its impact on hiring is expected to be much weaker due to challenges in enforcement. Although the CPS ASEC survey lacks direct questions on hiring and firing, we derived approximate measures from supplementary questions. In practice, we defined hiring as the event in which an individual, who was not employed in the previous year, reports to be working in the previous week. Conversely, we define firing as the case in which an individual was employed in the preceding year, reports not to be working in the past week, and specifies job loss as the reason for not working (distinct from voluntary resignation)³¹. Similarly, we characterize quitting for individuals who reported not working in the previous week due to voluntarily leaving their prior job and who were employed in the previous year. Thus, hiring is only defined on the sample of workers who were *never* employed in the previous year, and firing and quitting are defined only on the sample of workers who were employed *at some point* in the previous year.

In our theoretical framework, described in section 3, we take pregnancy as an exogenous phenomenon, we thus want to verify whether this is consistent with the data, looking as

²⁹Results are robust to alternative, reasonable, definitions of our sample. For instance, excluding 1978 as it is a year of partial treatment, excluding more distant states such as Florida and Texas, and restricting the sample to married individuals as in Gruber (1994).

³⁰These variables are: class of worker last year (only asked if employed at some point in the previous year), weeks worked last year (intervalled, set to zero if never worked), reason for not working last year (only asked if the respondent never worked in the previous year).

³¹We construct also a second measure of firing obtained from another variable: whether a respondent was absent from work in the previous week due to a layoff. Unfortunately, neither this measure, nor the previous one identify clearly the timing of the layoff. We thus consider both in an attempt to use as much information as we can from the CPS ASEC data.

pregnancy as an outcome of our analysis. Again, the CPS ASEC survey does not record whether a respondent was pregnant, but it records the age of all children less than 5 at the time of the survey (March). We use this information to construct a measure of fertility. Namely, we identify a woman as pregnant in the previous year if she reports having a child less than one year old in March of the current year, which thus encompasses all births from April of the previous year to March of the current one. We then use the variables on layoffs and quits, constructed as per the previous paragraph, to get suggestive evidence on the direct effects of pregnancy-discrimination laws on employment protection of pregnant workers.

Finally, we use other information present available from the survey to strengthen our analysis by including individual-level characteristics, such as marital status, age, race, education, and location information, and to study potential heterogeneous effects by industry of employment³². Moreover, we replicate results on employment, firing and hiring, using the Panel Study of Income Dynamics (PSID). This survey does not contain information on wages and has a much smaller sample, but allows us to make sure that our main employment results are not sensitive to the survey data we use³³.

5.3 Summary Statistics

Table 1 reports sample sizes and summary statistics for the variables discussed in the previous section. We divide the sample in the three subgroups of interest and report statistics separately for pre- and post-PDA years, pooling across all US states included in the sample.

As seen from the table, primary education is virtually universal in this time period. College education is much less frequent but growing and on similar levels for men and women, with young women particularly close to the men's educational levels. This is all in line with evidence showing gender gaps narrowing in many dimensions in the 1970s and 1980s (Blau and Kahn, 2017). The vast majority of our sample is white and mostly married, while slightly less than 40% resides in a metropolitan area. As expected, the share of married individuals is lower for younger women than older ones and decreases in the post-PDA period, with a generally increasing age at first marriage.

Looking at employment outcomes, we notice that employment is more frequent for younger than older women. A finding that is consistent with both exits from the labor force after pregnancies and with younger cohorts being generally more attached to the labor market. In any case, employment is rising for both groups, while stable for males, who are close to full employment, at 90%. The rise in female employment is entirely attributable to private employment, with the share of females employed in government positions or selfemployed changing little in these 12 years. Increasing likelihood employment mechanically

 $^{^{32}\}mathrm{We}$ use the 1950 Census Bureau industrial classification system.

³³Untabulated results based on PSID data are available upon request.

translates into a higher number of weeks worked for the average female respondent, but not in the usual number of weekly working hours. Nominal wages in the period also exhibit a steady growth, more so for females than for males, something expected given the high inflation rates throughout this period³⁴. Finally, the likelihood of pregnancy for women age 18 to 35 amounted to 9% before the PDA and 8% after it. That is, slightly less than 10% of all women age 18 to 35 became pregnant in a given year in this period. The same statistic, unreported, is effectively 0 for women of ages 40 to 60.

6 Results

We begin by providing a descriptive analysis of the data, showing how our outcomes of interest where evolving around 1978 in the two groups of states. Next, we estimate the baseline model and the augmented version including a further comparison group. We then discuss results from event-study specifications of these models and finally conclude with an exercise to deepen the role of wage rigities, due to the Equal Pay Act of 1963, in shaping the effects of the PDA.

6.1 Descriptive Evidence

As a first piece evidence, we show how our main outcomes of interest evolved around the passage of the PDA in switcher and stayer states. We do this in figure 3, where we plot raw trends of mean outcomes for women 18 to 35 years old in the two groups for each year in the observation period.

For the likelihood of employment, we can clearly see in panel (a) that the young women in the two groups were experiencing significantly different levels of employment before the passage of the PDA. In the period when only stayer states had pregnancy-discrimination laws in place, employment levels were much higher in switcher states, with a fairly constant gap of about 5 percentage points. This gap narrows and then disappears completely at the time of enactment of the PDA: while both groups were growing before 1978, in line with national trends, growth in switcher states stops in 1978, resulting in stayer states catching up with them. Reassuringly for our identification strategy, raw trends seem parallel and even coincident in the post-PDA period, when both groups are under the same policy regime³⁵. We see a similar dynamic in panel (b), looking at the average number of weeks

 $^{^{34}{\}rm The}$ cumulative inflation rate from 1972 to 1982 was 130.9%, source: https://fred.stlouisfed.org/series/FPCPITOTLZGUSA.

³⁵The trends do seem to diverge in the last two years, 1983 and 1984. This is something we see in the other panels too. This pattern could be due to different paths of recovery from the early 1980s recessions. Given that these differences are not significant, and that all results replicate trimming the observation window to end in 1982, we decided to keep a longer and symmetric window around the passage of the PDA. In any case, all these results showing the robustness to the alternative observation window are available upon request.

employed, for which unfortunately we only have data since 1975.

To interpret gap closure in employment rates as evidence of a negative effect of the PDA, one needs to assume that, without it, switcher states would have also seen employment increase around 1978 as it did for stayer states. To provide further support for this parallel trends assumption, we prolonged the observation window in order to observe two stayer states (Pennsylvania and Connecticut) before their pregnancy-discrimination policy was passed in 1973. As we can see from figure A6, trends in employment for these two states are parallel to those of switcher states until 1973, see a negative shift exactly after their law is passed, and then return to the parallel increasing trend, although on lower levels. This reassures that we are indeed capturing the effect of pregnancy-discrimination policies on treated states and not contemporaneous variation affecting states whose treatment status is not changing.

The trajectories of hourly wages, displayed in panel (c) are roughly parallel and upwardsloping throughout the period, with young women in stayer states constantly displaying higher wages in this period. These raw trends already suggest that there was not a major response of wages to the enactment of the PDA. This would be in line with the role of wage rigidities, especially attributable to the Equal Pay Act of 1963, discussed in sections 2 and 3. As for pregnancies, they also do not seem responsive to the PDA. As seen in panel (d), there is a persistent, albeit never significant, gap in the likelihood of pregnancy between the two groups. This outcome is quite noisy, both due to its construction and to its relatively infrequent nature (on a yearly basis), but visual inspection certainly does not point to trends evolving very differently in the two groups. Again, the suggested unresponsiveness of pregnancy to the PDA is consistent with its exogenous nature, at least for the time period under consideration, posited in section 3.

Finally, we also examined the raw trends of measures of hiring, firing and quits. As detailed in section 5.2, the outcomes we use are proxies for these dimensions that are directly and indirectly affected by the PDA and are well-defined only on a subset of cases. Thus, the sample size on which these estimates are based is very small, only previously unemployed individuals for hirings, and only employed pregnant women for layoffs and quits of pregnant workers. Results are therefore noisy. With this caveat in mind, we still report these estimates as they provide some suggestive evidence on these margins of adjustment. Figure A7 displays these raw trends. The overall picture that emerges is that post-trends are relatively parallel, although with considerable noise. In the pre-PDA period instead there are visible differences between the groups. Both in levels, compared to the post-period, and in trends. For instance, in panel (a) we can see that the gap in hiring narrows with the passage of the PDA, with stayer states getting closer to the level of hiring of stayer states.

Overall, we interpret this evidence based on the raw data as reassuring for the validity of the identification strategy and as transparently pointing at employment effects of pregnancy-discrimination legislation. With this in mind, we proceed by formally applying our quasi-experimental strategy and discuss its results in the following section.

6.2 Empirical Results

Building on the descriptive evidence presented in the previous section, we now conduct a formal analysis of the effects of the PDA by applying the identification strategies presented in section 4.

In table 2, we report estimates for the effect of pregnancy-discrimination legislation on the likelihood of employment, our main outcome of interest. In column (1), we report estimates of specification (10) and in columns (2) to (4) of specification (13), using respectively only women aged 40 to 60, only males 18 to 60, and both groups as unaffected control groups. The difference-in-differences in reverse strategy shows that the PDA reduced the likelihood of employment among women in fertile age by 4.6 percentage points, a reduction that is significant at the 1% level. This is a sizeable and economically meaningful reduction considering that about 70% of young women in switcher states were employed in the pre-PDA period³⁶, amounting to a 6.6% reduction in the share employed in relative terms. This finding is confirmed also using the triple-difference-in-differences strategy and is very robust to the choice of the unaffected control group, as seen comparing column (1) with the other three columns. The negative effect observed for our extensive margin measure of employment is also observed when we look at the number of weeks worked during the year. We do so in table A1, whose columns all display a negative effect of the PDA on weeks worked: a decrease of about 1.9 weeks on average when using our baseline DDR strategy and a lower, but stable, decrease of about 0.8 weeks when including state-by-year fixed effects. As a robustness check, we test whether this negative employment effect is driven by any particular state in our sample. In figure A8, we plot the estimated DDR coefficient dropping one state-group at a time. As it clear from this figure, the estimated negative effect is very robust, both to excluding switcher and stayer states.

When instead we look at hourly wages we find that results are sensitive to the specification we use, but generally point at much smaller and less robust variations. Table 3 reports these results. Using a specification (10), we obtain a small but significant decrease in hourly wages associated to the PDA, which amounts to 0.125³⁷. This is a 3.6% relative decrease compared to the average hourly wage of fertile-age women in switcher states prior to 1979 (3.48\$). Using the second identification strategy, we obtain estimates ranging

³⁶The average treatment effect we identify with this estimator is identified for the switchers and for the period before switchers are treated. See Kim and Lee (2019) and Tazhitdinova and Vazquez-Bare (2023) for a formal discussion.

 $^{^{37}}$ Visual inspection of figures 3c and 4c suggest that this decrease is entirely driven by diverging trends years after the enactment of the PDA, something also discussed in section 6.1. We see this as a further reason not to over-interpret these estimates, but rather focus on their overall magnitude.

from an insignificant decrease of 0.128\$, when older women are used as the unaffected control group, to a slightly significant increase of 0.154\$, when men are used³⁸. While this lack of robustness is likely attributable to inevitable noise in the way our wage measure is constructed, we interpret these findings as ruling out sizeable adjustments on the side of wages, especially compared to the evident reduction in employment. One of the constraints that limited these adjustments, as discussed in sections 2 and 3, may have been the Equal Pay Act of 1963. Under this regulation, wages of female workers could not move separately from those of male workers employed in the same establishment and position, creating a nominal rigidity on the wages of young female workers. We deepen the discussion on this in section 6.4, looking at heterogeneous effects on wages with respect to the share of women employed in different industries. Finally, note that these changes are not necessarily causal effects since they are measured conditional on employment, which is an endogenous policy response.

Through the lenses of our theoretical framework, described in section 3, we interpret the observed decrease in employment and the smaller responses of wages as evidence that the PDA did not raise firing costs sufficiently to reduce layoffs of pregnant workers and offset the reduction in hiring of young women³⁹ and that nominal rigidities limited the scope of wages for alleviating this decrease in employment.

In line with this interpretation, we see that the PDA generally had a negative effect on young women's likelihood of being hired. Table A2 shows that the passage of the PDA is associated with an estimated decrease in the probability of being hired ranging from -0.8 percentage points to -1.9 percentage points. These effects are imprecisely estimated due to the noisy measure of hiring that we obtained from our data, but relatively robust to the choice of the specification. While these number may seem low, the average of this outcome in the pre-PDA period was 9.4%, for young women in switcher states. Hence, the estimated decrease in the probability of being hired ranges from -8.5% to -20%.

We then look at pregnancy as an outcome, to examine whether it reacts to the policy (something that would contradict the exogenous pregnancy assumption we made in section 3), and outcomes of pregnant women, to study firing and quitting around the enactment of the PDA. Notice that we only estimated specification (10) for these outcomes, as pregnancy is not an occurring event for the two unaffected control groups, post-fertility age women and men. First, looking at column (1) of table A3, we see that there was not

³⁸We do not want to over-interpret this increase, given this lack of robustness. However, we do note that an observed increase in wages would be consistent with the dynamic of a moral hazard problem with imperfect compliance with the PDA, where wages act as an opportunity cost of pregnancy and this puts upward pressure on wages of female employees. We formally examined these dynamics in a previous version of the model, whose derivations are available upon request.

³⁹That, in the pre-PDA period, the economy was in a scenario where employers frequently preferred firing and substituting, rather than keeping, pregnant employees is clearly documented in policy reports, see for instance National Partnership for Women & Families (2003), and proved by the demand for regulation protecting employment of pregnant workers, such as the PDA, in the first place.

a fertility response to the PDA, with a precisely estimated null effect⁴⁰. The evidence is thus consistent with pregnancy being exogenous⁴¹. In the next two columns, we consider the effect of the PDA on the likelihood of layoff for pregnant women, using the two proxies described in section 5.2. As these estimates are based on a subset of pregnant women, they are estimated on a very small sample, so we once again stress that we only take these as suggestive evidence and use them to complement the analysis. With this in mind, both estimates suggest that the PDA was not effective in reducing firing discrimination, possibly due to insufficiently high penalties for those found violating the Act, or to insufficient enforcement. Using one of the two measures, we actually see an increase in the likelihood of firing. But again, we do not want to risk over-interpreting these estimates based on small samples, and simply see this as evidence against strong reductions in firings of pregnant employees. Finally, one could expect the employment protection legislation to indirectly affect quits. Indeed, the historically frequent quits of female workers during their pregnancy could be also due to anticipation of a layoff from a discriminating employer, where legislation such as the PDA does not apply. If this is true, the perception of a more protected employment position could lead to fewer quits among pregnant women. Nonetheless, also on this margin, we do not detect significant reductions. The estimate reported in column (4) of table A3 is positive, but small and close to zero.

In summary, these estimates are consistent with the dynamics highlighted in section 3. In particular, they imply that the PDA overall had negative effects on employment of fertility-age women and more muted effects on their wages. Given that we do not observe reductions in layoffs and quits of pregnant women, our interpretation is that the Act did not raise the cost of firing pregnant workers sufficiently to change this behavior and offset the reduction in hiring. In the context of the model, the increase in K must have not been enough to pass the threshold \overline{K} , otherwise job destruction rates would have also been significantly affected. For completeness, notice that, given the information we have, we cannot identify in which of the two scenarios described in figure 2 the PDA was enacted, as the threshold \overline{K} was not passed.

⁴⁰This null effect is also found when restricting the sample to employed women, which is of course an endogenous condition. It could have been reasonable to see a response from them as, by providing employment protection, the PDA should effectively lower the costs associated with pregnancies. However, we do not see this in the setting we are considering and this reassures us on the validity of the exogenous pregnancy assumption that we impose.

⁴¹While certainly suggestive of the exogeneity of pregnancy, we do not take this as conclusive evidence, given that, as we argue, the PDA was not effective in increasing employment protection of pregnant workers. An alternative explanation of the lack of response of fertility to a formal increase in job protection is that women internalize that the legislation does not generate an effective decrease in the rate of dismissals. Prifti and Vuri (2013) found, in a historically and geographically different settings, that fertility does respond to strengthened employment protection when the latter is substantial.

6.3 Validity of the Identification Strategy

In the previous section, we described the changes associated with the enactment of the PDA. To interpret them causally, we require a parallel trend assumption, which needs to hold conditionally on the covariates and fixed effects included. To assess the plausibility of this assumption, we now discuss estimates of event-study specifications (12) and (13). Remember that, compared to a traditional quasi-experiment with two groups and a preperiod and a post-period, here one group is always treated, the stayer states and one becomes treated after October 31, 1978. Hence, for our identification strategy to be valid we need both a parallel trend assumption and a time-invariant treatment effect assumption. We will assess the plausibility of the former looking at estimated lags of treatment effect coefficients in specifications (12) and (13), those after 1978, which should be insignificantly different from zero. Instead, we will empirically examine the validity of the latter looking at the dynamic of lead coefficients, that should be roughly constant.

Figure 4 plots estimates from equation (12) for our main outcomes of interest. For employment, extensive margin and number of weeks employed, estimates show a relatively stable positive difference in the pre-PDA period and a near-zero, non-significantly different trend after the enactment of the PDA. We interpret this as evidence in favor of both our identification assumptions, parallel trends and constant treatment effect. Indeed, notice that the only year in which the estimated treatment effect is visibly different is 1978, which is a year of partial treatment. This may also probably incorporate some anticipatory behavior from employers, given the salience of the PDA⁴².

Regarding hourly wages, panel (c) of figure 4 confirms what we argued in the previous two sections: effects on wages are small in magnitude and generally insignificant. Moreover, while the difference in post-trends in hourly wages is also very stable around zero until 1983, it seem to diverge from this year onwards. This is indeed what translates into the significantly negative estimate of column (1) in table 3, which we therefore do not take as evidence of a significant negative effect of the PDA on wages of young women. Overall, this picture is consistent with a scenario in which there are wage rigidities that prevent adjustment on this margin, as posited in section 3, something that likely exacerbated the negative effect on employment. We then examine the null effect on pregnancies documented in section 6.2, which is confirmed also in this dynamic specification. Estimates are certainly very noisy, due to the nature of this proxy, but are reassuringly all small in magnitude and oscillating around zero.

We complement evidence on these main outcomes studying our proxies of hires, layoffs and quits in this dynamic setting. Estimates, plotted in panels (a) to (d) of figure A9,

⁴²Anticipatory behavior in our setting would bias the coefficient towards zero. Indeed, estimates obtained removing year 1978 are similar but slightly larger in magnitude. To be conservative, and lacking exact evidence on anticipatory behavior, we decided to keep year 1978 in all specifications to be conservative, but results without this year are available upon request.

show that post-trends are never significantly different and the difference is generally close to zero. However, estimated treatment effects in the pre-PDA period are very unstable. On the one hand, this is certainly due to noise and imprecision due to the way these variables are derived, lacking exact measures on them in our data. On the other hand, we remark the importance of being cautious in interpreting these effects, especially since we cannot claim that the constant-treatment effect assumption is supported, particularly for hiring and the measure of layoffs reported in panel (b). In any case, these plots remain broadly consistent with the main effects discussed so far and with the implication of our model.

Finally, in figure A10, we show that these dynamics are robust to the choice of specification. In panels (a) to (d), we plot the main outcomes, excluding the pregnancy-related ones that do not apply, using men and older women as unaffected control groups, and find visibly similar patterns.

6.4 Wage Rigidities: The Role of the Equal Pay Act of 1963

In section 6.2, we claimed that the lack of strong responses in young female employees' wages may be due to rigidities imposed by legislation forbidding discrimination in pay. The Equal Pay Act of 1963, enacted long before the PDA, essentially required all employers to pay the same wages for workers employed in the same position, regardless of their gender⁴³. Thus, under this Act, any movement of wages of young female workers would have necessarily triggered a similar change in wages of male workers employed in the same position and establishment (if any). This limited the possibility of wage adjustments given that: (i) any increase in fertile-age women's wages would cost more for a firm that needs to grant the same increase to all other comparable workers, and (ii) any decrease would face opposition from other male employees, whose wages would also have to be lowered. Clearly, the extent to which this constraint is binding is tightly linked to the share of women employed in a given position at each establishment. In particular, the higher this share, the less the EPA will limit adjustments in wages. For instance, if a position at a given firm is occupied by only young women, then the EPA would place no limits to movements of wages for that position. In the opposite scenario, where the position is occupied by only men except one woman, movements of that woman's wage would be extremely costly or infeasible for the firm. We thus expect wage responses to the PDA to be stronger in places where mostly women are employed and more muted where mostly men are employed.

 $^{^{43}\}mathrm{The}$ original text of the Act read ""[N]o employer having employees subject to any provisions of this section shall discriminate, within any establishment in which such employees are employed, between employees on the basis of sex by paying wages to employees in such establishment at a rate less than the rate at which he pays wages to employees of the opposite sex in such establishment for equal work on jobs". Full text available at: https://www.govtrack.us/congress/bills/88/s1409/text.

Ideally, we could test for the role of the EPA-related constraints in mediating effects of the PDA by exploring heterogeneous effects on wages with respect to the share of women employed by position for each establishment. Unfortunately, lacking establishment-level data⁴⁴ this exercise is not feasible. Instead, we use detailed information on industry of employment from the CPS ASEC data to conduct a similar test. Assuming that the share of women employed in an industry is a good proxy of the share of women employed at a given position and establishment within that industry, we test whether the effect of the PDA on hourly wages is heterogeneous with respect to the fraction of women in the industry⁴⁵.

Specifically, we compute the share of women employed in each industry in switcher states before the enactment of the PDA, to avoid capturing endogenous industry sorting in response to the Act, and divide industries in two groups: (i) industries where at least 75% of the workforce is female, and (ii) industries where this fraction is lower than 75%. Examples of industries in the first group are hospitals and medical services, apparel and accessories, and domestic services. In the second one we find industries as business and legal services, food stores, telecommunications and manufacturing. We then estimated our main specification, equation (10), separately for workers employed in these two groups of industries, to look at differences in wages. To look at responses of the likelihood of employment in these two groups of industries, we decomposed the main employment dummy into a dummy for being employed in the first group of industries and one for being employed in the second group of industries.

Table 4 displays results from this exercise. Consistent with the EPA limiting the wage adjustments, we see that the PDA is associated with a stronger and significant decrease in hourly wages in female-dominated industries. In places where the EPA posed a weaker constraints on movements in wages, as seen in columns (2) and (5), wages of fertile-age women actually decreased in response to the PDA to smooth the negative effects on employment, which is indeed insignificant and very small even in relative terms. Likelihood of employment in these industries decreased by about 3%, as opposed to about 8% in the other group of industries. This is thus consistent with the theorized role of EPA constraints and the hypothesized response of wages in places where these constraints were not binding, as discussed in section 3. As argued above, we therefore expect the employment response to be more muted in these sectors.

 $^{^{44}\}mathrm{Such}$ as the EEO-1 dataset for which we are in the process of applying again.

⁴⁵While this only a proxy of the measure we would ideally exploit, industry information is relatively precise in the CPS ASEC data, with almost 150 different industry codes.

7 Conclusion

This paper studies how pregnancy-discrimination laws affect employment and wage dynamics of women of fertile age, examining the case of the Pregnancy Discrimination Act of 1978. This type of pregnancy-discrimination legislation aims to address discrimination against women, mandating equal treatment for pregnant employees and temporarily disabled employees. Formally, the PDA constituted a strengthening of employment protection of pregnant women. However, the effectiveness of the legislation in protecting pregnant workers and its impact on overall female employment is not clear *ex ante*, as the relative difficulty to enforce measures against discrimination in hiring compared to discrimination in firing, might have led discriminating employers to shift discrimination to the less effectively regulated margin.

We first showed with a theoretical model, calibrated to the economic setting of the late 1970s, that the legislation's efficacy depends heavily on enforcement and the extent to which it deters discriminatory behavior. Imperfect implementation or mild sanctions could lead to increased unemployment among women without effectively protecting pregnant employees. This would have an unambiguous negative effect on employment of fertile-age women. Instead, if the policy is sufficiently enforced, so that it deters firing of pregnant women, the model indicates the possibility of either a positive or negative overall effect on employment. We then examined the actual outcome of the policy in our empirical analysis.

Leveraging early adoption of similar pregnancy-discrimination laws by some US states, our empirical analysis shows that employment of fertile-age women decreased substantially in response to the policy. Yet, this was not accompanied by an observed decrease in firing rates of pregnant women. This suggests that enforcement of the PDA was weak, leading to the unambiguous negative employment effects predicted by the model.

The effects of the policy on hourly wages, as captured by our analysis, are less significant and robust, indicating smaller responses overall. This suggests that prevailing institutional factors, such as the Equal Pay Act of 1963, may have constrained significant wage adjustments. The observed decline in employment might have indeed been exacerbated by wage rigidities, limiting the ability of wages to react and thus causing a sharper increase of unemployment rates. This is indeed confirmed by our heterogeneity analysis with respect to the share of women employed in an industry. Female-dominated industries, where equal pay constraints are less binding, displayed significant lower wages after the passage of the policy and a much more muted response of employment.

Finally, we found no significant effect of the PDA on fertility rates. One the one hand, this is consistent with our maintained assumption that fertility is exogenous. On the other hand, this would also be the outcome in a setting in which fertility rates are responsive to variations in employment protection, but workers internalize the fact that the PDA did not result in an effective strengthening of protections for pregnant women, as our findings seem to suggest.

There are different natural next steps to further our understanding of the impact of the legislation, which we are already taking or plan to take in the near future. Access to firm-level data is essential to allow us to focus the analysis on those firms that already had some form of accommodations in place for temporarily disabled workers, and were thus directly affected by the PDA and to measure employment and wages more precisely. To this regard, we are in the process of obtaining access to establishment-level staffing records from the EEOC, the EEO-1 records, to examine the effects on female employment at the firm level. From a theoretical standpoint, it is of primary importance to endogenize wages, especially to explain the responses of unemployment in those sectors in which women were the majority and the constraints on wage adjustments described above were less likely to be binding. Importantly, the combined increased protection of pregnant workers due to the PDA and the equal pay requirements of the Equal Pay Act might have led employers to adjust both their labor force composition, so that the PDA might have affected gender segregation across industries and occupations, and also the other types of accommodations provided to other temporarily disabled workers. While these next steps will surely allow us to strengthen our analysis and better examine the validity of our claims, the current evidence clearly points to the fact that, while positively motivated, the PDA of 1978 was not able to achieve its goals of reducing pregnancy discrimination and strengthening women's position in the labor market, likely because of weak enforcement.

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Main Figures & Tables



Figure 3: Raw Trends in Average Outcomes

Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984. Plotted are the average values of the outcome for each group of states in the year indicated on the x-axis. Estimates are computes using ASEC individual sample weights. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Pregnancy is defined as reporting, in March of the following year, having a child younger than one.



Figure 4: Event Study, Main Outcomes

Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984. Plotted are estimates of the β_l coefficients in specification (12). The first post-treatment year of switcher states, 1979, is omitted as reference point. Estimates are computes using ASEC individual sample weights. The shaded blue areas display 95% confidence intervals based on heteroskedasticity-robust standard errors. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Pregnancy is defined as reporting, in March of the following year, having a child younger than one.

	<u>1973-1978</u> 1979-1984							
Variable	Mean	Std.Dev.	Med.	Obs.	Mean	Std.Dev.	Med.	Obs.
Panel A. Females 18-35								
Education: Primary	. 0.99	0.09	1.00	52734	0.99	0.08	1.00	58909
Education: High School	0.79	0.41	1.00	52734	0.83	0.37	1.00	58909
Education: College	0.32	0.47	0.00	52734	0.37	0.48	0.00	58909
White	0.85	0.36	1.00	52734	0.83	0.38	1.00	58909
Black	0.14	0.35	0.00	52734	0.16	0.37	0.00	58909
Married	0.65	0.48	1.00	52734	0.58	0.49	1.00	58909
Metropolitan area resident	0.38	0.49	0.00	52734	0.39	0.49	0.00	58909
Employed	0.69	0.46	1.00	52734	0.74	0.44	1.00	58909
Employed: Private	0.54	0.50	1.00	52734	0.59	0.49	1.00	58909
Employed: Government	0.13	0.33	0.00	52734	0.12	0.32	0.00	58909
Employed: Self	0.02	0.13	0.00	52734	0.03	0.16	0.00	58909
Weeks employed	26.90	22.50	27.00	36126	29.90	22.40	39.00	58909
Weekly hours	35.40	10.70	40.00	25129	35.30	10.80	40.00	43398
Annual wage income	3212.50	3874.70	1593.00	52734	6021.60	6900.30	4000.00	58909
Hourly wage income	3.67	3.02	3.19	24252	5.49	3.95	4.77	42032
Pregnant	0.09	0.28	0.00	52734	0.08	0.28	0.00	58909
Panel B. Females 40-60								
Education: Primary	0.96	0.19	1.00	44381	0.97	0.16	1.00	41781
Education: High School	0.60	0.49	1.00	44381	0.67	0.47	1.00	41781
Education: College	0.17	0.38	0.00	44381	0.22	0.41	0.00	41781
White	0.88	0.33	1.00	44381	0.87	0.34	1.00	41781
Black	0.12	0.32	0.00	44381	0.12	0.33	0.00	41781
Married	0.75	0.43	1.00	44381	0.73	0.44	1.00	41781
Metropolitan area resident	0.37	0.48	0.00	44381	0.40	0.49	0.00	41781
Employed	0.58	0.49	1.00	44381	0.61	0.49	1.00	41781
Employed: Private	0.41	0.49	0.00	44381	0.42	0.49	0.00	41781
Employed: Government	0.12	0.32	0.00	44381	0.12	0.33	0.00	41781
Employed: Self	0.04	0.18	0.00	44381	0.05	0.21	0.00	41781
Weeks employed	25.50	24.20	26.00	29580	27.30	24.20	36.00	41781
Weekly hours	35.40	11.70	40.00	16989	35.80	11.40	40.00	25324
Annual wage income	3107.60	4315.70	468.00	44381	5623.20	7590.60	2000.00	41781
Hourly wage income	3.92	2.88	3.37	15592	5.95	4.24	5.01	23393
Panel C. Males 18-60								
Education: Primary	0.97	0.17	1.00	95184	0.98	0.14	1.00	103130
Education: High School	0.71	0.45	1.00	95184	0.76	0.43	1.00	103130
Education: College	0.33	0.47	0.00	95184	0.37	0.48	0.00	103130
White	0.88	0.33	1.00	95184	0.86	0.34	1.00	103130
Black	0.12	0.32	0.00	95184	0.12	0.33	0.00	103130
Married	0.72	0.45	1.00	95184	0.66	0.47	1.00	103130
Metropolitan area resident	0.39	0.49	0.00	95184	0.40	0.49	0.00	103130
Employed	0.91	0.28	1.00	95184	0.90	0.30	1.00	103130
Employed: Private	0.69	0.46	1.00	95184	0.67	0.47	1.00	103130
Employed: Government	0.12	0.33	0.00	95184	0.11	0.32	0.00	103130
Employed: Self	0.10	0.30	0.00	95184	0.11	0.31	0.00	103130
Weeks employed	42.10	17.30	52.00	64313	41.40	18.00	52.00	103130
Weekly hours	42.90	10.40	40.00	58569	42.40	10.40	40.00	93015
Annual wage income	9649.80	8404.30	8970.00	95184	14705.30	13809.70	12500.00	103130
Hourly wage income	6.05	4.00	5.34	53896	8.83	5.89	7.69	85285

 Table 1: Summary Statistics

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984. All statistics are computed using ASEC individual sample weights. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Pregnancy is defined as reporting, in March of the following year, having a child younger than one.

1	1 1			
	(1)	(2)	(3)	(4)
Switcher State x Post-PDA x Treated Individual		-0.032***	-0.041^{***}	-0.038***
		(0.010)	(0.007)	(0.007)
Switcher State x Treated Individual		0.043^{***}	0.053^{***}	0.048^{***}
		(0.007)	(0.005)	(0.005)
Post-PDA x Treated Individual		0.039^{***}	0.098^{***}	0.080^{***}
		(0.008)	(0.006)	(0.006)
Treated Individual		-0.125***	-0.275***	0.011^{**}
		(0.008)	(0.004)	(0.005)
Switcher State x Post-PDA	-0.046***			
	(0.006)			
Observations	111,643	200,082	309,957	398,396
R-squared	0.073	0.087	0.093	0.132
State FE	\checkmark	\checkmark	\checkmark	\checkmark
Year FE	\checkmark	\checkmark	\checkmark	\checkmark
State-Year FE		\checkmark	\checkmark	\checkmark
Sample	F 18-35	F 18-35	F 18-35	F 18-35
-		F 40-60		F 40-60
			M 18-60	M 18-60

Table 2: Effects on Probability of Employment

Dependent Variable: Employed

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984. Column (1) estimates our baseline specification (10) using the dependent variable reported on top of the table. Columns (2) to (4) instead estimate the triple-difference specification (11) using this dependent variable. In column (1), the sample is further restricted to women of age 18 to 35. In columns (2) and (4) we also include women of age 40 to 60, and in columns (3) and (4) also men of age 18 to 60. Estimates are computes using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

		0		
	(1)	(2)	(3)	(4)
Switcher State x Post-PDA x Treated Individual		-0.128	0.154^{*}	0.081
		(0.098)	(0.084)	(0.078)
Switcher State x Treated Individual		0.021	0.091	0.073
		(0.068)	(0.057)	(0.054)
Post-PDA x Treated Individual		-0.152*	-1.206^{***}	-0.971^{***}
		(0.082)	(0.070)	(0.065)
Treated Individual		0.981^{***}	-1.247***	2.502^{***}
		(0.080)	(0.049)	(0.056)
Switcher State x Post-PDA	-0.126**			
	(0.061)			
Observations	66,246	106,801	205,465	245,982
R-squared	0.153	0.158	0.299	0.293
State FE	\checkmark	\checkmark	\checkmark	\checkmark
Year FE	\checkmark	\checkmark	\checkmark	\checkmark
State-Year FE		\checkmark	\checkmark	\checkmark
Sample	F 18-35	F 18-35	F 18-35	F 18-35
-		F 40-60		F 40-60
			M 18-60	M 18-60

Table 3: Changes in Hourly Wage

Dependent Variable: Hourly Wage

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984. Column (1) estimates our baseline specification (10) using the dependent variable reported on top of the table. Columns (2) to (4) instead estimate the triple-difference specification (11) using this dependent variable. In column (1), the sample is further restricted to women of age 18 to 35. In columns (2) and (4) we also include women of age 40 to 60, and in columns (3) and (4) also men of age 18 to 60. Estimates are computes using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4: Changes in Hourly Wage and Employment, Heterogeneity by Share Female in Industry

Dep. Variable	Hourly Wage in Industry			Employed in Industry			
Share Women	$\begin{array}{c} \text{Any} \\ (1) \end{array}$	$\geq 75\%$ (2)	<75% (3)	Any (4)	$\geq 75\%$ (5)	<75% (6)	
Switcher State x Post-PDA	-0.126^{**} (0.061)	-0.231^{*} (0.129)	-0.104 (0.069)	-0.046^{***} (0.006)	-0.004 (0.005)	-0.042*** (0.007)	
Observations Pre-PDA Mean	$66,246 \\ 3.498$	$13,180 \\ 3.364$	53,057 3.533	$111,\!643 \\ 0.679$	$111,\!643 \\ 0.133$	$111,\!643 \\ 0.547$	
R-squared	0.153	0.196	0.146	0.073	0.006	0.054	
State FE Year FE	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	

Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984. Columns (1) to (3) further restrict the sample to employed individuals and estimate our baseline specification (10) using hourly wage as dependent variable. In columns (2) and (3), the specification is estimated using only respondents employed in industries with a share of women employed included in the interval reported at the top of the table. Columns (4) uses a dummy for employment as outcome. Column (5) and (6) use a dummy equal to one if the respondent is employed in an industry with a share of women employed included in the interval reported at the top of the table, and zero otherwise. Estimates are computes using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

APPENDIX

A.1 Calibration

In the model we have 10 parameters: $r, \delta, y, q, \mu, w, c, \gamma, \alpha, v$. We calibrate 9 of them and leave the taste/cost parameter c free to analyze the two possible scenarios in the paper. The calibrated values are in the table below.

Parameter	Value
y	1
r	0.075
δ	0.02
q	0.01
$\mid \mu$	0.54
w	0.984
γ	0.5
α	0
v	0.5

We first normalize the average monthly production to y = 1, and set $\gamma = 0.5$. We calibrate the monthly interest rate r using historical data from the Social Security Administration. In particular, we set r equal to the average monthly interest rate in the years 1975-1978.

To calibrate the exogenous separation rate δ , we use the benchmark tenure level for male workers in 1978 from Sekscenski (1979), as it is meant to capture the probability of separation independent of pregnancy.

To calibrate q we use the average number of children by age 35 in switcher states pre-PDA.

For the calibration of μ we used as reference the 12 weeks required by the Family and Medical Leave Act (FMLA) of 1993 as a reasonable minimum amount of leave time that employers had to guarantee workers. Since there is evidence that before FMLA most women were working until late into the pregnancy and going back to work early after⁴⁶, we shorten the time to 8 weeks.

The cost of posting a vacancy v cannot be directly observed in the data, but we can look at previous estimates in the literature and determine its relationship with the wage wusing our knowledge of the unemployment rate among non-pregnant women before PDA was approved. In particular, we observe from the data the share unemployed in the week preceding the survey among women 18 to 35 who were not pregnant the year before in switcher states, which was 8.7%. Under our assumption that pre-PDA, K = 0, we can match our unemployment rate u_f^n with its empirical counterpart and obtain a relationship between w and v. Indeed, plugging K = 0 in (8)

$$\frac{v}{m(\theta)} = \frac{y-w}{r+q+\delta}$$

⁴⁶See for instance this document by the Census Bureau.

Using $m(\theta) = 1/\theta^{1-\gamma}$, we can write

$$m(\theta)\theta = \left(\frac{v(r+q+\delta)}{y-w}\right)^{-\frac{\gamma}{1-\gamma}}$$

Substituting this into equation (7) we get

$$u_f^n = \frac{(q+\delta)\mu}{(q+\delta+\left(\frac{v(r+q+\delta)}{y-w}\right)^{-\frac{\gamma}{1-\gamma}})(q+\mu)}$$
(14)

Setting this equal to 0.087, we obtain a non-linear relationship between v and w. We find that the values of wages w are relatively insensitive to v and close to 1, meaning that, at least according to this calibration, women workers were obtaining wages close to their marginal productivity. Setting v = 0.5, which is an average between the value found by Pissarides (2009) and the one resulting from Shimer (2005) (see for this Cahuc, Carcillo, and Zylberberg (2014)), we obtain a wage of $w \approx 0.984$.

A.2 Extension of the theoretical analysis to overall unemployment rate for women

In this section we look at the theoretical implications of the policy on women's unemployment rate u, including both pregnant and non-pregnant women in the analysis.

In this case, the pre-PDA unemployment rate is

$$u_f = \frac{q + \left(\frac{q+\delta}{q+\delta+m(\theta)\theta}\right)\mu}{q+\mu}$$

Once again, we find that it is increasing in the rate of job destruction $q + \delta$ and decreasing with the rate of job creation, $m(\theta)\theta$. We have seen how, to the left of the cutoff \overline{K} , an increase in K determines a decrease in labor market tightness θ and a corresponding decrease in $m(\theta)\theta$, without any effect on the job destruction rate. Thus, also in this case, we have that any shift of K to the left of \overline{K} has a positive effect on *overall* unemployment.

Next, we look at the rate u when we are to the right of the cutoff,

$$u_k = \frac{\delta \left(q^2 + \mu + q \left(1 + m(\theta)\theta + \mu\right)\right)}{q^2 \delta + (\delta + m(\theta)\theta)\mu + q\delta \left(1 + m(\theta)\theta + \mu\right)}$$

We have that, as long as $\delta \leq 1$, $u_k \leq u_f$, confirming that, for any given level of tightness θ , a reduction in job destruction, decreases unemployment. It is possible to show, however, that u_k reacts negatively to increases in θ . As we show in the theoretical section of the paper, an increase of K to the right of the cutoff can only have non-positive effects on θ , thus causing in an aggravation of unemployment (as long as $\alpha > 0$, if $\alpha = 0$ we have no effect on unemployment).

Finally, we have to compare what happens when we move from below to above the threshold \overline{K} . The result is *ex ante* ambiguous, and we once again rely on calibration to isolate plausible values of the parameters. The results are presented in the figures below and are qualitatively similar to the ones in 3.

Figure 5: Effect of the PDA on the unemployment rate women as a function of K for different values of c



Notes: We assume a Cob-Douglas matching function $M(V,U) = V^{\gamma}U^{1-\gamma}$. The baseline values of the parameters are y = 1, r = 0.075, $\delta = 0.02$, q = 0.01, $\mu = 0.54$, w = 0.984, $\gamma = 0.5$, $\alpha = 0$, v = 0.5. See appendix A.3 for details on calibration. We express K as fraction of average monthly production. In the left-hand graph we take a value of c = 0.3, in the right-hand graph c = 0.6.

The main qualitative insights of Section 3 still hold. The policy has a strong negative effect on unemployment when c = 0.3. When c = 0.6, instead, the effect on unemployment is positive.

A.3 Additional Figures



Figure A1: National Trends around 1978

Notes: The sample consists of all CPS ASEC respondents of age 18 to 60. Estimates are computes using ASEC individual sample weights. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours.

Figure A2: Montana Maternity Leave Act (1975), Original Text

CHAPTER 26-MATERNITY LEAVE

Section 41-2601. Definitions. 41-2602. Denial of m

41-2602. Denial of maternity leave unlawful.

41-2603. Complaint-how filed.

41-2604. Enforcement.

41-2605. Regulations. 41-2606. Individual action,

41-2601. Definitions. (1) "Commissioner" means the commissioner of labor and industry.

(2) "Employer" means any public or private employer.

History: En. 41-2601 by Sec. 1, Ch. Title of Act 320, L. 1975. An act to provide maternity leave to public and private employees.

41-2602. Denial of maternity leave unlawful. (1) It shall be unlawful for an employer or his agent:

(a) to terminate a woman's employment because of her pregnancy, or

(b) to refuse to grant to the employee a reasonable leave of absence for such pregnancy, or

(c) to deny to the employee, who is disabled as a result of pregnancy, any compensation to which she is entitled as a result of the accumulation of disability or leave benefits accrued pursuant to plans maintained by her employer; provided that the employer may require disability as a result of pregnancy to be verified by medical certification that the employee is not able to perform her employment duties, or

(d) to retaliate against any employee who files a complaint with the commissioner under the provisions of this act, or

(e) to require that an employee take a mandatory maternity leave for an unreasonable length of time.

(2) Upon signifying her intent to return at the end of her leave of absence, such employee shall be reinstated to her original job or to an equivalent position with equivalent pay and accumulated seniority, retirement, fringe benefits, and other service credits unless, in the case of a private employer, the employer's circumstances have so changed as to make it impossible or unreasonable to do so.

History: En. 41-2602 by Sec. 2, Ch. 320, L. 1975.

Source: Labor, 3 Mont. Code Ann. 1 (1977).



Figure A3: Timing of Pregnancy Discrimination Legislation

Notes: This map displays the year of passage of each state's pregnancy-discrimination policy. States that only enacted such a policy in 1978, with the passage of the PDA at the federal level are displayed in white.



Figure A4: Switcher and Stayer States, Main Analysis

Notes: This map displays the set of switcher and stayer states used in the main analyses based on CPS data.



Figure A5: Share Pregnant by Age

Notes: The sample consists of all CPS ASEC respondents of age 18 to 60, between 1973 and 1984. Estimates are computes using ASEC individual sample weights. Pregnancy for male and female respondents is defined as reporting having a child age 0 in March of the following year. For males the value is thus referred to the partner's pregnancy.



Figure A6: Raw Trends in Employment, Extended Time Window

Notes: The sample consists of all CPS ASEC respondents of age 18 to 60, between 1971 and 1987. Estimates are computes using ASEC individual sample weights.

Figure A7: Raw Trends in Average Outcomes

(a) Share Hired, among unemployed last year



(c) Share Fired (last week measure), among women pregnant last year

(b) Share Fired (last year measure), among women pregnant in that year



(d) Share Quits (last year measure), among women pregnant in that year



Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984. Plotted are the average values of the outcome for each group of states in the year indicated on the x-axis. Estimates are computes using ASEC individual sample weights. Pregnancy is defined as reporting, in March of the following year, having a child younger than one. Outcomes are constructed as detailed in section 5.2.



Figure A8: DDR Estimates for Employment, Robustnees

Notes: This plot displays the DDR coefficient on the interaction between the *Switcher* and *Post-PDA* dummies from specification (10), estimated dropping from the sample observations from one state group at a time. The omitted state group is indicated on the x-axis.

Figure A9: Event Study, Other Outcomes

(a) Share Hired, among unemployed last year



(c) Share Fired (last week measure), among women pregnant last year



Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984. Plotted are estimates of the β_l coefficients in specification (12). The first post-treatment year of switcher states, 1979, is omitted as reference point. Estimates are computes using ASEC individual sample weights. The shaded blue areas display 95% confidence intervals based on heteroskedasticity-robust standard errors. Pregnancy is defined as reporting, in March of the following year, having a child younger than one. Outcomes are constructed as detailed in section 5.2.

(b) Share Fired (last year measure), among women pregnant in that year



(d) Share Quits (last year measure), among women pregnant in that year



Figure A10: Event Study, Triple-Difference-in-Differences, Main Outcomes

Notes: The sample is restricted to women of age 18 to 35, 40 to 60 and males 18 to 60, in switcher and stayer states between 1973 and 1984. Plotted are estimates of the β_l coefficients in specification (13). The first post-treatment year of switcher states, 1979, is omitted as reference point. Estimates are computes using ASEC individual sample weights. The shaded blue areas display 95% confidence intervals based on heteroskedasticity-robust standard errors. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Outcomes are constructed as detailed in section 5.2.

A.4 Additional Tables

Dependent Variable: Weeks Employed						
	(1)	(2)	(3)	(4)		
Switcher State x Post-PDA x Treated Individual		-0.805	-0.896**	-0.872**		
		(0.529)	(0.404)	(0.397)		
Switcher State x Treated Individual		0.777^{*}	0.721^{**}	0.604^{*}		
		(0.409)	(0.314)	(0.309)		
Post-PDA x Treated Individual		1.463^{***}	4.429^{***}	3.555^{***}		
		(0.449)	(0.345)	(0.339)		
Treated Individual		-3.211***	-14.570***	1.791^{***}		
		(0.430)	(0.271)	(0.292)		
Switcher State x Post-PDA	-1.863***					
	(0.340)					
Observations	95,035	168,673	262,478	336,116		
R-squared	0.089	0.082	0.159	0.161		
State FE	\checkmark	\checkmark	\checkmark	\checkmark		
Year FE	\checkmark	\checkmark	\checkmark	\checkmark		
State-Year FE		\checkmark	\checkmark	\checkmark		
Sample	F 18-35	F 18-35	F 18-35	F 18-35		
-		F 40-60		F 40-60		
			M 18-60	M 18-60		

Table A1: Effects on Number of Weeks Employed

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984. Column (1) estimates our baseline specification (10) using the dependent variable reported on top of the table. Columns (2) to (4) instead estimate the triple-difference specification (11) using this dependent variable. In column (1), the sample is further restricted to women of age 18 to 35. In columns (2) and (4) we also include women of age 40 to 60, and in columns (3) and (4) also men of age 18 to 60. Estimates are computes using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A2: Effects on Probability of being Hired

		-		
	(1)	(2)	(3)	(4)
Switcher State x Post-PDA x Treated Individual		-0.011	-0.019	-0.012
		(0.008)	(0.013)	(0.008)
Switcher State x Treated Individual		0.018^{***}	0.043^{***}	0.025^{***}
		(0.005)	(0.010)	(0.006)
Post-PDA x Treated Individual		0.012^{*}	0.033^{***}	0.018^{***}
		(0.007)	(0.011)	(0.007)
Treated Individual		-0.028***	-0.070***	-0.035***
		(0.006)	(0.009)	(0.005)
Switcher State x Post-PDA	-0.008			
	(0.007)			
Observations	33.480	69.285	51.983	87.788
R-squared	0.016	0.029	0.026	0.034
State FE	\checkmark	\checkmark	\checkmark	\checkmark
Year FE	\checkmark	\checkmark	\checkmark	\checkmark
State-Year FE		\checkmark	\checkmark	\checkmark
Sample	F 18-35	F 18-35	F 18-35	F 18-35
*		F 40-60		F 40-60
			M 18-60	M 18-60

Dependent Variable: Hired

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984. Column (1) estimates our baseline specification (10) using the dependent variable reported on top of the table. The outcome is constructed as explained in section 5.2. Columns (2) to (4) instead estimate the triple-difference specification (11) using this dependent variable. In column (1), the sample is further restricted to women of age 18 to 35. In columns (2) and (4) we also include women of age 40 to 60, and in columns (3) and (4) also men of age 18 to 60. Estimates are computes using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

	(1)	(2)	(3)	(4)
Dependent Variable:	Pregnant	Pregnant and Fired	Pregnant and Fired	Pregnant and Quitted
Switcher State x Post-PDA	0.001	0.024^{***}	0.001	0.009
	(0.004)	(0.008)	(0.004)	(0.008)
Observations	$111,\!643$	5,504	9,955	5,504
R-squared	0.044	0.021	0.007	0.016
State FE	\checkmark	\checkmark	\checkmark	\checkmark
Year FE	\checkmark	\checkmark	\checkmark	\checkmark
State-Year FE				
Sample	F 18-35	F 18-35	F 18-35	F 18-35

Table A3: Effects on Pregnancy, Layoffs and Quits of Pregnant Workers

Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984. All columns estimate our baseline specification (10) using the dependent variable reported on top of each column. Pregnancy is defined as reporting, in March of the following year, having a child younger than one. Other outcomes are constructed as explained in section 5.2. Estimates are computes using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.