The Role of Pregnancy in Gender Discrimination
Evidence from the Pregnancy Discrimination Act of 1978

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Abstract

Gender discrimination remains a persistent issue in labor markets and identifying its underlying drivers is crucial to guide policy solutions. Fertility-related concerns are a clear candidate, but isolating their role is hindered by the fact that pregnancy discrimination is typically addressed by broader gender-discrimination policies. However, this was not the case before the passage of the Pregnancy Discrimination Act (PDA) of 1978: employers could not discriminate on the basis of gender due to existing federal laws, but could legally dismiss workers if pregnant. 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 and hiring 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 that pre-existing equal pay legislation shaped the effects of the PDA by limiting the response of women's wages. 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 cases[2]. 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 pregnancy[3].

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 1970s, we analyze two potential scenarios, determined by the value of a parameter governing

[1]As defined by the US Equal Employment Opportunity Commision (EEOC) (source here).

[2]These are Geduldig v. Aiello, 417 U.S. 484 (1974) and General Electric v. Gilbert (1976), 429 U.S. 125.

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

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) and from the Panel Study of Income Dynamics (PSID) 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, met by a similar decrease in hirings. Moreover, results using proxies of layoffs of pregnant workers, we show that these did not decline in response to the PDA. With the lenses of our 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. Moreover, we find evidence of distributional effects: the negative effects on hirings and employment are found also among fertile-age women who did not end up having any children and so never benefited from the PDA.

When analyzing hourly wages, our results 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 less 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 gender discrimination in the labor market (Givati and Troiano, 2012; Becker, 1971; Thomas, 2020; Fernández-Kranz and Rodríguez-Planas, 2021; He, Li, and Han, 2023; Bamieh and Ziegler, 2023) and legislative attempts to reduce it (Zabalza and Tzannatos, 1985; Neumark and Stock, 2006; Doepke, Tertilt, and Voena, 2012; Passaro, Kojima, and Pakzad-Hurson, 2023; Bailey, Helgerman, and Stuart, 2024; see Blau and Kahn, 2017 and Goldin, 2023 for recent comprehensive reviews). A first set of studies, mostly audit studies such as Becker (1971) and He, Li, and Han (2023), document that employers statistically discriminate in hiring and promotion based on potential future fertility and family responsibilities. Consistently, a second set of studies show that policies that increase costs of employees' fertility for their employer, such as mandated benefits (Thomas, 2020; Fernández-Kranz and Rodríguez-Planas, 2021; Timpe, 2024), negatively affect hiring and promotion of fertile-age women. However, all these papers focus on settings in which one margin of response of the employer to employee's fertility, firing, is always forbidden (or at least limited). To the best of our knowledge, we are the first to study how the *introduction* of limits to employers' ability to fire pregnant workers, posed by pregnancy discrimination laws, affected employment outcomes of fertile-age women. Moreover, we explicitly consider how pregnancy discrimination laws interact with earlier gender discrimination laws. In particular, we build on the the recent work of Bailey, Helgerman, and Stuart (2024) on the effects of the Equal Pay Act (EPA) of 1963. We show that wage rigidities posed by earlier equal pay legislation were important in shaping the response to the PDA of 1978: where the EPA constrained the response of wages, women's employment fell substantially more.

In studying the PDA of 1978, we also add evidence on the effects of this important legislative step. All papers studying it, except Mukhopadhyay (2012), do not focus on the provisions of the Act *per se*, but rather on the mandated health Gruber (1994) and paid leave benefits (Stearns, 2015; Timpe, 2024) generated by the interaction of the Act and prior firm and state policies. We instead focus on what is arguably the intrinsic component of the Act – employment protection for pregnant workers – and study its effects on employment outcomes of fertile-age women, without restricting the attention only to recent mothers as in Mukhopadhyay (2012). 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.

In this regard, we also relate to the broader literature on employer responses to employment protection. The general finding of this literature is that increases in firing costs can have adverse consequences on employment outcomes, such as higher use of temporary workers (Autor, 2003) and reduced hiring (Acemoglu and Angrist, 2001; Kugler and Saint-Paul, 2004; Autor, Donohue III, and Schwab, 2006; Martins, 2009; Sestito and Viviano, 2018; Ichino et al., 2023), but these effects are modest. Similar to Acemoglu and Angrist (2001), studying the 1991 Americans with Disabilities Act, we also consider an employment protection policy that applies only to a subgroup of workers. However, a key distinction is that for the case of the PDA of 1978, the covered group is defined by a temporary condition, pregnancy, that is not perfectly foreseeable at the hiring stage. In this different setting, we find that higher firing costs triggered sizeable reductions in hiring of fertile-age women, but no substantial reductions in firing of pregnant women, which we attribute to insufficient enforcement. Consistent with the fact that the protected subgroup is not identifiable *ex-ante*, we find that these negative effects on hirings applied also to women who did not have children, even if they were never actually covered by the PDA.

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 5 we outline the empirical strategy and describe the data we use to estimate it in section 4; 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 which are key to our identification strategy.

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 to address discrimination based on pregnancy[4]. Already in the 1960s, legislators had addressed issues of gender discrimination in the labor market with the Equal Pay Act (EPA) of 1963 and Title VII of the Civil Rights Act of 1964. The former forbade

[4] Full text available here.

gender-based wage discrimination, the latter extended this protection to all aspects of employment and also to discrimination based on race, color, religion and national origin[5]. However, neither of these Acts *explicitly* recognized pregnancy discrimination as a form of gender discrimination, leaving a legislative gap that led to a contentious situation. On the one hand, the US Supreme Court reiterated in two famous rulings that pregnancy was excluded from the scope of gender discrimination protection[6]. The main rationale was that groups divided by gender and by pregnancy status did not entirely overlap as both men and women can be in the *non-pregnant* group[7]. On the other hand, some US states independently strengthened the employment protection of pregnant workers, as detailed in the following section. Moreover, claims to expanding these protections 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. This situation pushed Congress, which at the time had a Democratic super-majority, and the Carter administration to address the issue legislatively at the federal level, passing the PDA in 1978.

The core provision of the PDA mandated that pregnancy be treated as a temporary disability by employers, applying to establishments with 15 or more employees[8]. *De jure*, 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 employment[9] *De facto*, as in analogous contexts[10], other than extensions of employer-provided health insurance benefits[11], 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 disabilities[12].

[5]Public Law 88–352, 78 Stat. 241, enacted July 2, 1964

[6]See Geduldig v. Aiello, 417 U.S. 484 (1974) and General Electric v. Gilbert (1976), 429 U.S. 125.

[7]This 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" (see here).

[8]The text of the Act reads that "women affected by pregnancy, childbirth, or related medical conditions 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".

[9]See the EEOC guidelines at this link.

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

[11]This 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.

[12]Indeed, 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

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

As anticipated in the previous paragraph, 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 the level of employment protection of pregnant workers that was then required by the PDA[13].

Of these 21 states, ten enacted policies that simply mandated equality of treatment of pregnancy and temporary disabilities, as the PDA[14]. Six states implemented policies that, in addition to forbidding pregnancy discrimination, required employers to provide job-protected unpaid leaves of reasonable length[15]. Finally, five states further required the leave to be paid since they mandated equal treatment of pregnancy and had state-level universal STDI policies[16]. 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. We show this variation, in types of policy adopted and in timing of adoption, which is key for our identification strategy in figures A4 and A4.

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).

[13]These 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.

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

[15]These 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.

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

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 economy[17]. 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 children[18]. 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 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 settings[19], 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

[17]We 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.

[18]Derivations are available upon request.

[19]See for instance Acemoglu and Angrist (2001).

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

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

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. We allow K to assume a continuum of values, to account for the possibility of imperfect enforcement of the law[20].

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

$$r\Pi^k = y - w - c + \mu(\Pi^n - \Pi^k) + \delta(\Pi^v - \Pi^k) \tag{2}$$

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 workplace[21]. Even if the firm decides not to dismiss the worker because of pregnancy, there can be other exogenous factors causing a separation,

[20] One 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 and legal expenses D. Changes in both D and p cause changes in K.

[21] We 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.

at rate δ[22]. Finally, the flow value of a vacancy is

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

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, $\theta \equiv V/U$. We assume that the matching function takes the form

$$M(V,U) = V^{\gamma}U^{1-\gamma} \tag{4}$$

Notice that (3) implicitly makes the simplifying assumption that only non-pregnant 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

$$\Pi^n = \frac{v}{m(\theta)} \tag{5}$$

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

$$r\Pi^n = y - w + q(\max\{\Pi^k, -K\} - \Pi^n) - \delta\Pi^n \tag{6}$$

and

$$r\Pi^k = y - w - c + \mu(\Pi^n - \Pi^k) - \delta\Pi^k \tag{7}$$

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,

[22] We could allow for the fact that the firm might still incur legal costs with probability $\alpha \in [0,1]$ when the separation is exogenous. We would then have

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

We instead assume that 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 assumption simplifies the analysis without significantly altering the main qualitative results. A positive 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 when the legislator can rely on an effective judicial system ($\alpha = 0$).

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.

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

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

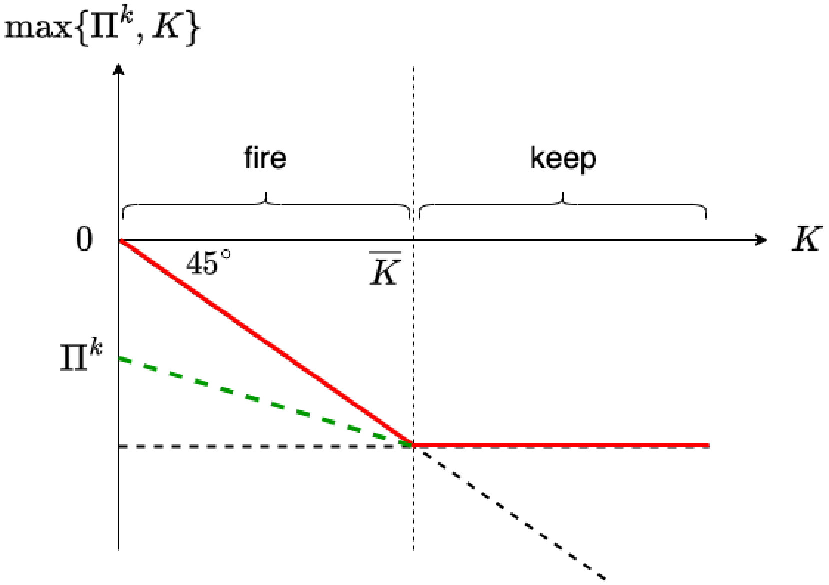
After some calculations we get

$$\overline{K} = -\frac{(y-w)(q+r+\delta+\mu) - c(q+r+\delta)}{(r+\delta)(q+r+\delta+\mu)} \tag{8}$$

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.



The free entry condition allows us to solve the system of equations (1)-(3) for the equilibrium level of market tightness, for values of K below and above the threshold, $\overline{K}$. In particular, for $K < \overline{K}$

$$\theta_f = \left(\frac{y - w - qK}{v(q + r + \delta)}\right)^{\frac{1}{1-\gamma}} \tag{9}$$

Note that θ_f depends negatively on K: discriminating employers internalize the increase in the expected legal costs when they fire pregnant workers. To the left of $\overline{K}$, this does not reduce the extent of discrimination in the market, but it does negatively affect the number of hirings, thus reducing market tightness.

For $K > \overline{K}$, we have

$$\theta_k = \left(\frac{(y - w)(q + r + \delta + \mu) - cq}{v(r + \delta)(q + r + \delta + \mu)}\right)^{\frac{1}{1-\gamma}} \tag{10}$$

We now characterize the relationship between the unemployment rate and the market tightness. Once again, we perform the analysis for the two regimes separately.

There is a total of N women in the labor force, which grows at rate $n \equiv \dot{N}/N$. Such rate takes into account the fact that non-pregnant women enter the labor force (passing through unemployment), while unemployed pregnant women exit the labor force at rate φ[23]. We need to track the evolution of four groups: non-pregnant employed workers,

[23] Formally, we assume that every instant, non-pregnant women enter the labor force through unemployment at rate φ_1, while pregnant unemployed women leave the labor force at rate φ_0. Assuming that population grows at rate n, and denoting O women who are out of the labor force, we have

$$n = \varphi_1 \frac{O}{N} - \varphi_0 \frac{U^c}{N}$$

L^n, pregnant employed workers, L^c, non-pregnant unemployed workers, U^n, pregnant unemployed workers, U^c. Of course,

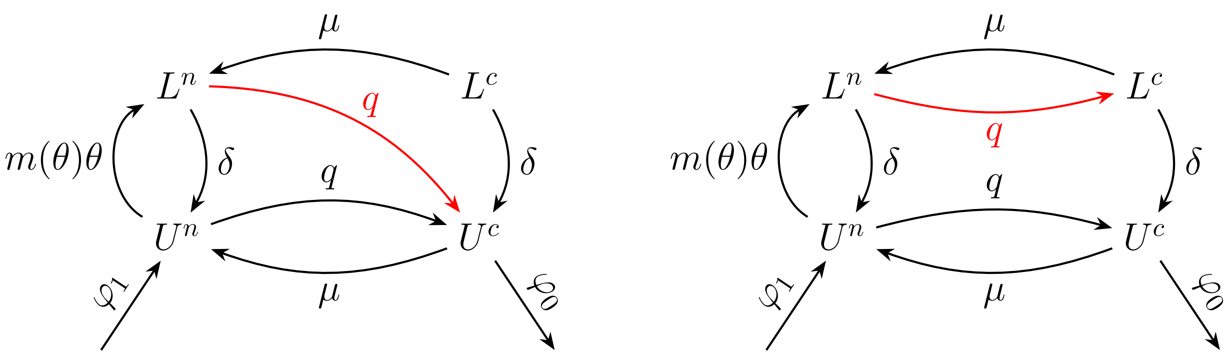
$$L^n + L^c + U^n + U^c = N$$

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 the following laws of motion

$$\begin{cases} \dot{l}^n + l^n n & = m(\theta)\theta u^n + \mu l^c - (q+\delta) l^n \\ \dot{l}^c + l^c n & = -(\mu + \delta) l^c \\ \dot{u}^n + u^n n & = \delta l^n + (\mu + \varphi) u^c + n - (m(\theta)\theta + q) u^n \\ \dot{u}^c + u^c n & = q l^n + q u^n + \delta l^c - (\mu + \varphi) u^c \end{cases} \tag{11}$$

where we use the notation $x \equiv X/N$ to denote the variables normalized by the size of the (female) labor force. The first equation describes the evolution of L^n: at every instant, a flow $m(\theta)\theta$ of non-pregnant unemployed women are hired, while employed women separate from the employer at rate $q + \delta$. Finally, an inflow of μ women re-enter L^n from L^c. We can similarly characterize the remaining equations following Figure 2a.

Figure 2: Flows of workers in the two regimes



(a) Firing regime

(b) Keeping regime

Imposing the steady state condition:

$$\dot{l}^n = \dot{l}^c = \dot{u}^n = \dot{u}^c = 0$$

we solve the system for u^n and u^c, the sum of which gives us the female unemployment

We can then express $\varphi_1 \frac{O}{N} = \varphi_0 \frac{U^c}{N} + n$. Calling $\varphi \equiv \varphi_0$, we can write the flow equations as in (11).

rate in the firing regime: $u_f \equiv u_f^n + u_f^c$

$$u_f = \frac{q + \frac{(n+q+\delta)(n+\mu+\phi)}{n+q+\delta+\theta_f^{\gamma}}}{n+q+\mu+\phi} \tag{12}$$

Note that, since θ_f, characterized in equation (9) is decreasing in K, we have that an increase in K determines an increase in unemployment. The intuition is that the increase in firing costs is not strong enough to deter firing of pregnant women, but are still internalized by discriminating firms who reduce hirings.

We now turn to the case where $K > \overline{K}$. The laws of motion are

$$\begin{cases} \dot{l}^n + l^n n &= m(\theta)\theta u^n + \mu l^c - (q+\delta)l^n \\ \dot{l}^c + l^c n &= q l^n - (\mu+\delta)l^c \\ \dot{u}^n + u^n n &= \delta l^n + (\mu+\varphi)u^c + n - (m(\theta)\theta + q)u^n \\ \dot{u}^c + u^c n &= q u^n + \delta l^c - (\mu+\varphi)u^c \end{cases} \tag{13}$$

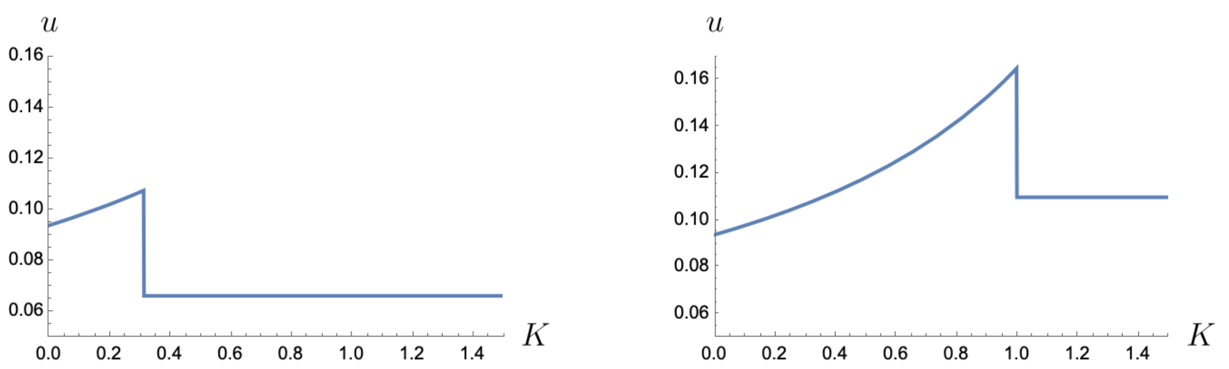
The flows are described in Figure 2b.

System (13) allows us to solve for the unemployment rate $u_k \equiv u_k^n + u_k^c$ in the keeping regime as a function of θ. Since θ_k, characterized in equation (10) is constant in K, in this region further increases of K have no effect on the unemployment rate.

The last question we need to address is what happens to the unemployment rate as a change in K brings us from the firing to the keeping regime. It turns out that the answer depends on the value of the parameters, as Figure 3 illustrates.

Figure 3: Effect of the PDA on the unemployment rate of women as a function of K for different values of c

(a) Negative effect (b) Positive effect



Notes: We assume a Cob-Douglas matching function $M(V,U) = V^{\gamma}U^{1-\gamma}$. The baseline values of the parameters are $y = 1$, $r = 0.075$, $\delta = 0.02$, $q = 0.01$, $\mu = 0.54$, $w = 0.98$, $\gamma = 0.5$, $v = 0.5$, $n = 0.001$ and $\varphi = 0.05$. 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 $c = 0.7$.

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-to-intermediate 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.

We thus conclude that the net effect of the policy on u 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 3 realized in practice.

4 Data

This section describes the data on states' pregnancy-discrimination policies, which is the basis of our identification strategy, and on employment outcomes, which we obtain from individual-level surveys.

4.1 Data on Pregnancy-Discrimination Policies

In order to construct our measure of treatment, the enactment of employment protection of pregnant workers, we carefully studied the provisions of the PDA and their interpretation in the Law literature[24]. Moreover, we collected and codified information on all pregnancy-discrimination 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, 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

[24]See for instance Weissmann (1983), Siegel (1985), Remmers (1989), and Habig (2007).

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). Together, these data allowed us to reconstruct the steps that preceded the passage of the PDA of 1978, described above in section 2.2, and to build a viable reference group for states first 'treated' in 1978, as detailed below in section 5.

4.2 Employment Data

We use two individual-level survey datasets, the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) and the Panel Study of Income Dynamics (PSID)[25]. The former is an addendum to the March CPS, repeated yearly for a cross-section of individuals. We use this data for all our main analyses, given its large sample size and fairly rich information on employment and earnings. The PSID instead provides less details on employment and has a smaller sample, but it is a yearly *panel* and it has more detailed geographical information on respondents. For this reason, we use it both for robustness and additional analyses.

CPS ASEC data: We consider two main measures of employment. First, an extensive margin measure of employment, whether the respondent worked at any time during the previous year. Second, an intensive margin measure of employment, the number of weeks employed in the previous year, conditional on working. 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. We then construct a measure of hiring using a specific feature of the CPS ASEC survey. Namely, that respondents are asked about their employment status and other outcomes in the previous year and in the week preceding the survey. Thus, even if it is not a panel, it allows us to build a proxy of hiring using changes in employment status. Specifically, we defined hiring as the event in which an individual, who was *never* employed in the previous year, reports to be working in the previous week. For firings, we rely on answers to a question asked to respondents who were absent from work in the previous week. We coded firing as the event in which a respondent answered that the reason for the absence from work was a layoff. 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. From survey questions, we also constructed a proxy of fertility using information on the of all respondent's children less than 5 at the time of the survey (March). We coded a woman as

[25] Pfeffer, Daumler, and Friedman (2023).

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. Finally, we obtained from the survey other individual-level characteristics, such as marital status, age, race, education, location information, and industry of employment[26].

Our identification strategy, described in section 5, relies on states' staggered adoption of pregnancy-discrimination policies before the PDA of 1978. For this reason, information on respondents' state of residence is crucial. Unfortunately, in the CPS most states are grouped together until 1976. It is often the case that states that passed pregnancy-discrimination policies prior to 1978 are grouped together with states that did not. Hence, we cannot precisely determine which respondents were exposed to pregnancy-discrimination policies if they appear to be from a group of states that passed the policy in different years. This limits the extent of policy variation that we can exploit with the CPS ASEC data and constrains the choice of states that we can use. We discuss this extensively in section 5, but the main implication is that: (*i*) it does not allow us to use early adoptions as quasi-experiments, and (*ii*) even using the policy variation granted by some states being only treated in 1978, with others being already treated, we cannot use all states. In particular, we retain data from 1973 to 1984, to have six years before and after the enactment of the PDA in 1978. Since some states are only separately identified from 1976, we drop all those that are grouped together in a group with states treated only in 1978 and states treated before that. Moreover, we keep only states exposed to a pregnancy-discrimination law either in 1978, with the PDA, or before 1973. This way, for early-adopters, exposure is fixed throughout the observation period, and for all late-adopters it changes in 1978. In practice, this leaves us with the following late-adopting states: Alabama, Arkansas, Florida, Georgia, Indiana, Kentucky, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. Instead, the early-adopters, used as reference group, are Connecticut, Massachusetts, New Jersey, and Pennsylvania. Finally, we further restricted our main sample to fertile-age women, the group that is directly affected by the policy[27]. We thus restrict the sample to women age 18 to 35, the age range where fertility is significantly more likely, as clearly seen in figure A6. As a sort of placebo exercise, we also examine the effects on individuals who should not be directly affected by the policy: older women (age 40 to 60) and all men between 18 and 60 years old.

PSID data: We strengthen the analysis replicating our main results with another commonly used individual-level survey, available for this time period, the PSID. This is important not only to show that our results are not sensitive to the survey we use, but also

[26]We use the 1950 Census Bureau industrial classification system.

[27]Results 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).

to show that they are robust to using: (i) a more precise measure of hiring, since the panel structure of the PSID allows us to define hiring as above but knowing the employment status in the entire year and not just in the previous week, and (ii) our identification strategy with all possible states, given they are always separately identified in the PSID[28]. Moreover, while the PSID lacks information to proxy hourly wages, it complements what we can learn from the CPS on other dimensions. In particular, we can observe women's total fertility and test whether effects are found for both women with and without children, at the end of the fertility cycle. Moreover, it reports also the respondents' childhood state of residence, allowing us to address concerns of selective migration.

4.3 Summary Statistics

Table 1 reports sample sizes and summary statistics based on the CPS ASEC survey for the variables discussed in the previous section. We split the sample in the three subgroups of interest, fertile-age women and the potential comparison groups, and report statistics 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. Increasing likelihood employment mechanically 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 period[29]. 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[30].

[28]Unfortunately, the much smaller sample size of the PSID does not allow us to study early-adoptions as separate quasi-experiments as the treated group would have too few observations.

[29]The cumulative inflation rate from 1972 to 1982 was 130.9% (source here).

[30]The same statistic, unreported, is virtually 0 for women of ages 40 to 60.

5 Empirical Strategy

The aim of this paper is to document how policies that strengthen employment protection of pregnant workers, such as the PDA 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[31]. Nonetheless, as we detailed in section 2, some US states enacted legislation comparable to the PDA of 1978, which produces variation in the *timing* of treatment. As explained in section 4.2, data limitations do not allow us to use the full extent of this variation in a staggered adoption type of design, but we can still exploit the fact that the treatment status of 'early-adopting' states did not change in 1978[32], while other states were first treated with the passage of the PDA. 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).

Our choice of states for the two groups was mainly dictated 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. Second, we had to exclude those states that the CPS ASEC grouped in subgroups of heterogeneous treatment timing. That is, subgroups where some states were treated earlier and other later, whose exposure to treatment cannot be precisely assigned using the CPS data. *Stayer* states thus consist of Connecticut, Massachusetts, New Jersey, and Pennsylvania. *Switcher* states instead are Alabama, Arkansas, Florida, Georgia, Indiana, Kentucky, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. We display these sets of states in figure A5. While this is definitely not the ideal way to select comparison 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 in section 6.

Comparing outcomes in switcher and stayer states before and after the PDA of 1978 is intuitively very similar to a standard difference-in-differences design, with the exception that the reference group is *treated* throughout the period. Hence, we pass from a pre-period with different treatment status across groups, to a post-period with both groups treated, or with *universally adoption* of the treatment (Tazhitdinova and Vazquez-Bare, 2023). The estimator we adopt is thus what Kim and Lee (2019) name 'Difference-in-differences

[31]We cannot exploit the discontinuity at 15 employees, under which establishments are not subject to the PDA, as we are not aware of any dataset that covers establishments of that size for this time period.

[32]Indeed, policies adopted prior to 1978 in the four states that we use as reference (Connecticut, Massachusetts, New Jersey, and Pennsylvania) all had broader coverage than the PDA. Thus, the policy regime regarding employment protection of pregnant women in these four states did not change in 1978.

in Reverse' (DDR). In practice, this is implemented with a usual two-way fixed effects (TWFE) specification, estimated on the subgroup that should be directly affected by this law, women aged 18 to 35. That is, we estimate the following regression

$$y_{i,s,t} = \beta SW_s \times P_t + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t} \tag{14}$$

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 grouped[33]. Finally, standard errors are robust to heteroskedasticity[34]. 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 PDA[35].

The parameter of interest in equation (14) 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 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-time[36]. 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

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

[34]We do not cluster at the level of our policy variation, the state, as we only have 12 clusters.

[35]Three 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.

[36]See 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.

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.

As usual, the parallel trends assumption described above is fundamentally untestable. This is because we do not observe the treated potential outcome for switchers before the policy change. However, we can assess its plausibility looking at *post*-PDA trends of switchers and stayers. Observing parallel trends after 1978, when both groups are treated, would reduce concerns over the validity of this assumption. This is discussed in detail in section 6.3, where we present estimates from the following event-study version of specification (14)

$$y_{i,s,t} = \sum_{l \neq 1979} \beta_l SW_s \times \mathbf{1}\{t = l\} + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t} \tag{15}$$

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.

Finally, we exploit the fact that pregnancy-discrimination policies should not directly affect older women and men to relax the identification assumption adding a further difference to equation (14). Specifically, we use older women and men as a plausible control group within each state to also control for state-by-year fixed effects. We believe that this approach is justified by the fact that substitution channels should not be first-order as men are close to full employment and older women are generally either employed or out of the labor force, with virtually zero re-entry rates after exit, and have different skills. Still, if these policies increase overall costs for firms, these subgroups could be indirectly affected by downsizing effects. For this reason, we prefer equation (14) as our main specification, but we also report results from this triple-DID specification. The technical details about this specification and its counterpart for equation (15) are discussed in appendix A.2.

6 Results

We begin with a descriptive analysis of the data, showing how our outcomes of interest trended around 1978 in the two groups of states. Next, we estimate the baseline model and provide support for the robustness of these results. We then discuss results from event-study specifications of these models and finally conclude with an exercise to deepen the role of wage rigidities, due to the EPA 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 4, 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, using the CPS ASEC data.

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 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 regime. Looking at the intensive margin measure of employment in panel (b), the number of weeks employed in the year conditional on working, we see that raw trends are generally parallel and exhibit a similar gap before and after 1978. This suggests that the relative decline in employment levels seen in panel (a) is not met by a similar decrease in the intensive margin, with switchers working on average about 2 weeks less throughout the period.

Next, we examine how changes in hiring and firings contribute to the observed closure of the gap in employment levels. In panel (c), we plot the mean of our proxy for hiring. While this measure is less precise and noisier than our measure of employment, here too we see that hiring rates are higher in switcher stated and that raw trends are quite parallel in the post-period. Moreover, the gap seems to shrink overall after 1978. Panel (d) instead displays a different picture: firing rates for pregnant women are very similar both in trends and in levels in all years, with no visible change around 1978. This already hints that the PDA affected employment of fertile-age women more through the hiring channel, rather than the firing one, something that we discuss more formally in the next section.

Finally, we plot the trends for nominal hourly wages, panel (e), and fertility rates, panel (f). In both cases, trends are roughly parallel during the entire period, with no clear shift around the passage of the PDA. Fertile-age women wages in stayer states remain constantly higher, while the opposite is true for pregnancies. We interpret this as a first signal of no strong responses of wages and fertility to the PDA[37]. For nominal wages, this is in line with wage rigidities, especially due to the EPA of 1963, playing a role. We expand on this in section 6.4. For pregnancies, the lack of changes is consistent with their exogenous nature, at least for the time period under consideration, posited in section 3, but could also be due to the PDA not impacting fertility decisions substantially.

[37]All results on wages us nominal wages, but the same pattern holds using real wages, deflating nominal values by the region-specific CPI obtained from the Bureau of Labor Statistics.

Overall, we interpret these raw trends 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 formally study the effects of the PDA using the identification strategy discussed in section 5.

In table 2, we report estimates from specification (14), using our main outcomes of interest from the CPS ASEC data. We report the coefficient on the interaction between the *Switcher State* and *Post-PDA* dummies, which captures the effect of the PDA, under the DDR assumption of parallel trends discussed above, on outcomes of fertile-age women. In column (1), we look at the effect on employment. This shows that the PDA reduced the likelihood of employment 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 period[38], amounting to a 6.6% reduction in the share employed in relative terms. In column (2), we examine how the PDA affected employment on the intensive margin, looking at the number of weeks employed in a year. Estimates indicate that the PDA affected employment levels only at the extensive margin: conditional on being employed, the DDR estimate on this intensive margin measure is small and insignificant, in line with evidence from the raw data trends.

Despite the data limitations of the CPS ASEC survey, that dictated our choice of switcher and stayer states, the negative effect on employment reported in this table is highly robust. First, it is not driven by any particular state. In figure A7, 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. Second, we find a similar, and even larger result using the PSID data. This is seen in column (1) of table A1, where we estimate equation (14) using the same set of switcher and stayer states.

To understand the source of this negative effect on employment, we then examine the effect on hiring, looking at column (3) of table 2. As said above, the proxy of hiring obtained with the CPS data is inevitably noisy, as it is based on the employment status in the week preceding the survey. Nonetheless, the coefficient is negative, albeit small and insignificant. Reassuringly, using PSID data, where we can compare year-on-year employment status changes to construct a better proxy of hiring, we do see a significant and large decrease in hirings of fertile-age women. The estimated coefficient, reported in

[38]The 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.

column (2) of table A1, indeed matches the decrease in employment. While this decrease in hirings was expected, due to the costs posed by stronger employment protection, the effect on firings is less predictable. As discussed in section 3, discriminatory dismissals of pregnant women may not necessarily decrease, unless firing costs are sufficiently high. Indeed, column (4) of table 2 shows a null effect on this outcome, as the raw trends examined in the previous section already suggested. Thus, we interpret the observed decrease in employment, hirings, but not firings, as evidence that the PDA did not raise firing costs sufficiently to reduce layoffs of pregnant workers and offset the reduction in hiring of fertile-age women.

As mentioned while discussing the literature, pregnancy-discrimination laws are a peculiar type of employment protection policies because the protected group is defined by a temporary condition: pregnancy. Since this is not foreseeable, especially by the employer, at the hiring stage, employers cannot distinguish fertile-age women that will or will not have children in the future and be thus covered by the policy. For this reason, contrarily to usual employment protection, we expect the 'cost' of this policy to fall outside of the protected class. Namely, we expect employment and hiring rates of all women of childbearing age to be negatively hit by the PDA. We test this using PSID data, where we know respondents total fertility. In line with expectations, as columns (3) to (6) of table A1 show, the PDA had a strong negative effect on all fertile-age women, even those that never had children and thus were never really protected by the PDA.

Having documented this sizeable negative effect on employment, we turn to studying how wages were affected. In our theoretical framework, we assumed fully rigid wages. Wages would thus not adjust and alleviate the decrease in employment. At a first glance, this may seem to contradicted by the estimated coefficient reported in column (5). This shows a small but significant decrease in hourly wages associated to the PDA, which amounts to 0.125\$[39]. 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\$). While wages were certainly not fully rigid in practice, we interpret these findings as ruling out sizeable adjustments on this margin, 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 and find that the coefficient reported here masks substantial heterogeneity. Finally, note that these changes are not necessarily

[39]Visual inspection of figures 4e and 5d 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.

causal effects since they are measured conditional on employment, which is an endogenous policy response.

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). Looking at column (6) of table 2, we see that there was not a fertility response to the PDA, with a precisely estimated null effect[40]. The evidence is thus consistent with pregnancy being exogenous[41].

In sum, 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 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 been not high 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 3 the PDA was enacted, as the threshold $\overline{K}$ was likely not passed.

6.3 Validity of the Identification Strategy and Robustness

In the previous section, we described the changes associated with the enactment of the PDA. To interpret them causally, we require a specific parallel trends assumption. In this section, we provide evidence that supports this assumption and discuss some additional robustness exercises.

Compared to a traditional difference-in-differences application, 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 specification (15), those after 1978, which should be insignificantly different from zero. Instead, we will empirically examine the validity of the latter looking at the dynamics of lead coefficients, that should be roughly constant.

Figure 5 plots estimates from equation (15) for our main outcomes of interest. For

[40] This null effect is also found when restricting the sample to employed women, which is of course an endogenous condition. It would have been reasonable to see a response from them as, by providing employment protection, the PDA should effectively lower the costs associated with pregnancies, but this is not observed.

[41] While certainly suggestive of the exogeneity of pregnancy, we do not take this as conclusive evidence. 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 setting, that fertility does respond to strengthened employment protection when the change is substantial.

employment, on the extensive margin, 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 PDA[42]. Again, on the intensive margin, shown in panel (b), estimates are less precise but do not show differential trends before the PDA, nor after its passage.

Having in mind panel (a) of figure 4, one may worry that the negative coefficient estimated in table 2 and the event-study estimates shown here are not really capturing a negative effect of the PDA on employment, but rather the concurrent and unrelated growth in fertile-age women's employment levels witnessed in stayer states. Indeed, we need to assume that, without the PDA of 1978, switcher states would have also seen employment levels increase around 1978 as it did for stayer states. To provide further support for this parallel trends assumption, we exploit the fact that we can expand the observation window and look at employment trends also around the introduction of pregnancy-discrimination policies in some *stayer* states[43]. We do this in figure A8a, both with CPS and PSID data. While trends are different across the two data sources, they both clearly show that most of the gap between switchers and stayers opens when only the stayers are treated, after 1973, and closes when switchers also are, as the PDA is passed at the end of 1978. 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.

We complement evidence on the main measure of employment studying our proxies of hirings and firings of pregnant women in this dynamic setting. Estimates, plotted in panels (c) and (d), show that post-trends are never significantly different and the difference is generally close to zero. While for firings there is clearly no change before versus after 1978, for hirings levels the difference was generally positive in the pre-period, but very noisy, and turns to an almost constant zero afterwards. 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 hirings. In any case, these plots remain broadly consistent with the main effects discussed so far and with the implication of our model.

Regarding hourly wages, panel (e) confirms what we argued in the previous two sections:

[42]Anticipatory behavior in our setting would bias the coefficient towards zero. Indeed, untabulated estimates obtained removing year 1978 are similar but slightly larger in magnitude.

[43]We need to exclude New Jersey as there the policy is introduced in 1961, before both CPS and PSID start.

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 (5) in table 2, which we therefore do not take as evidence of a strong negative effect of the PDA on wages of fertile-age 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. Next, we examine the null effect on pregnancies documented in section 6.2, which is confirmed also in this dynamic specification. Estimates are all small in magnitude and oscillating around zero.

Finally, we provide three additional robustness exercises using PSID data. First, to show that the results are not driven by the specific choice of states, dictated by limitations of the CPS ASEC data, we re-estimate equation (14), using all possible stayer and switcher states. That is, we only exclude the early-adopters that pass a policy after 1973, the start of our observation window. This set of states is shown in figure A9 and the resulting estimates for the likelihood of employment and hiring are reported in the first two columns of table A2. Both columns confirm a significant negative effect of the PDA on employment and hiring of fertile-age women. Second, to mitigate concerns of respondents migrating as a function of pregnancy-discrimination policies, we assign treatment status based on the childhood state of residence instead of the current one. Estimated coefficients, displayed in columns (3) and (4), of the same table show that this is not driving the results. Estimates are less precise, but remain negative and similar in magnitude. Third, we relax the assumption of parallel trends exploiting a third difference. As detailed in section 5 and appendix A.2, we use a triple-difference estimator using women age 40 to 60, whose fertility rates are virtually zero as seen from figure A6, and men age 18 to 60. Estimates, reported in table A3, show a robust negative effect on employment and a similar, small but negative effect on hiring. The coefficient for the number of weeks employed and for the hourly wage instead remain small, and the latter turns insignificant, confirming no detected sizeable change on these margins. While informative, we do not think this specification should be preferred to our baseline one as these other subgroups of individuals are also potentially affected by the policy, in indirect ways. Indeed, if we estimate equation (14) using, separately, older women and men instead of fertile-age women, we see a small but significant negative effect on their likelihood of employment too. Reassuringly, both of these to estimates are much smaller than the one for fertile-age women, suggesting that we are really capturing something specific to this subgroup. Plausibly, the effect of the PDA. Instead, we think that a possible interpretation for these small decreases is that firing costs imposed by the PDA push firms to downsize, affecting their broader workforce beyond fertile-age women.

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 gender[44]. 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. 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. 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.

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 data, 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 industry[45].

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 (14), 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

[44] Full text available here.

[45] While 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.

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 3 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.

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 and hiring 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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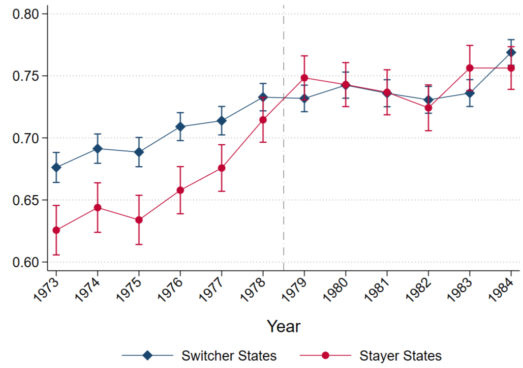
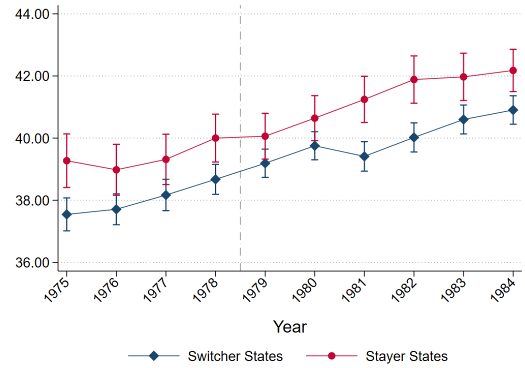
Main Figures & Tables

Figure 4: Raw Trends in Average Outcomes

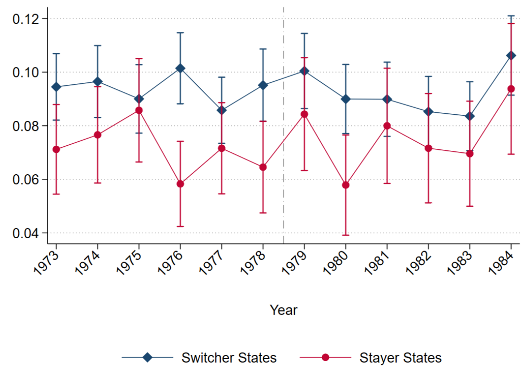
(a) Share Employed



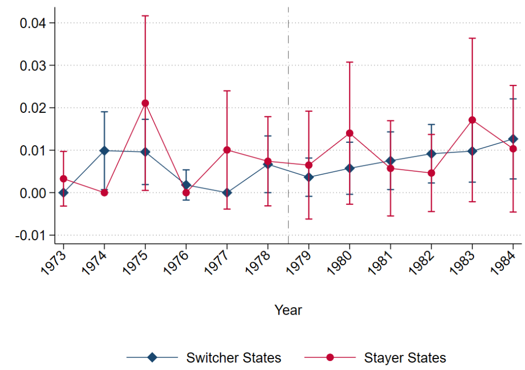
(b) Number of Weeks Employed



(c) Share Hired

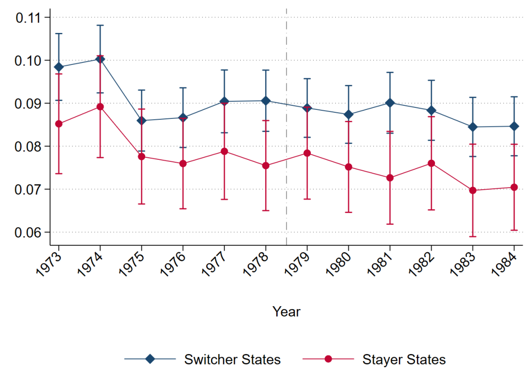


(d) Share Fired Among Pregnant



(e) Hourly Wage ($)

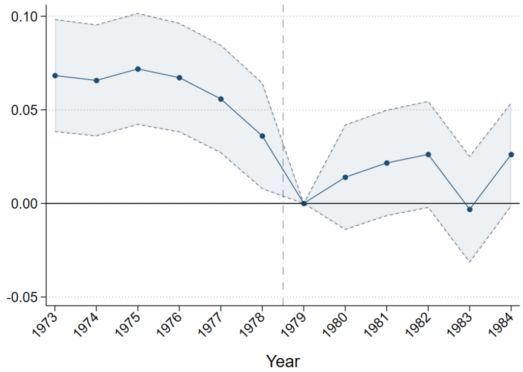
(f) Share Pregnant



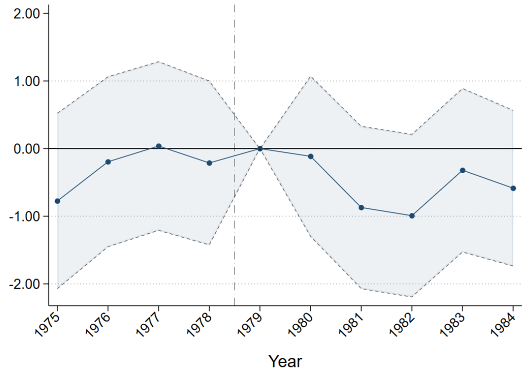
Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984, from the CPS ASEC. Plotted are the average values of the outcome for each group of states in the year indicated on the x-axis. Estimates are computed using ASEC individual sample weights. Wage 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.

Figure 5: Event Study, Main Outcomes

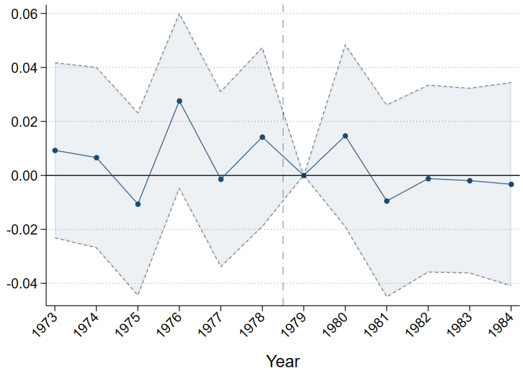
(a) Share Employed



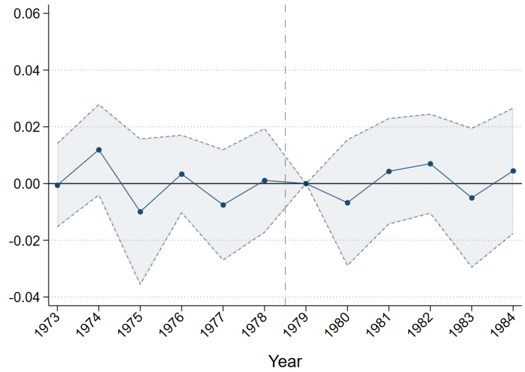
(b) Number of Weeks Employed



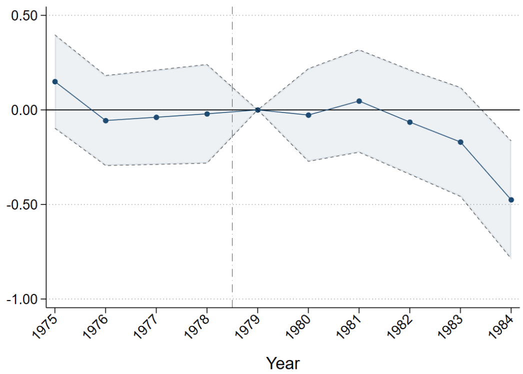
(c) Share Hired



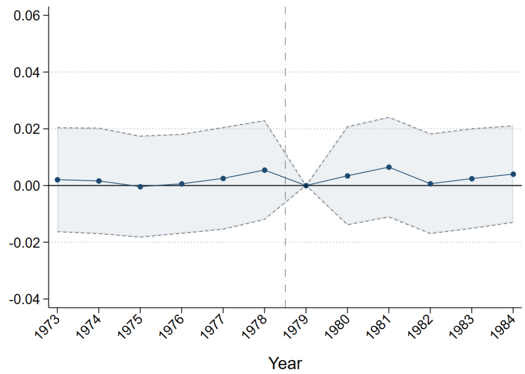
(d) Share Fired Among Pregnant



(e) Hourly Wage ($)



(f) Share Pregnant



Notes: The sample is restricted to women of age 18 to 35 in switcher and stayer states between 1973 and 1984, from the CPS ASEC. Plotted are estimates of the β_l coefficients in specification (15). The first post-treatment year of switcher states, 1979, is omitted as reference point. Estimates are computed 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.

Table 1: Summary Statistics, CPS ASEC data

	1973-1978				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

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984, from the CPS ASEC. 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.

Table 2: Difference-in-Differences in Reverse Estimates, CPS ASEC data

	(1) Employed	(2) Weeks Employed	(3) Hired	(4) Pregnant and Fired	(5) Hourly Wage ($)	(6) Pregnant
Switcher State x Post-PDA	-0.046***	-0.212	-0.008	0.001	-0.126**	0.001
	(0.006)	(0.294)	(0.007)	(0.004)	(0.061)	(0.004)
Observations	111,643	68,527	33,480	9,955	66,246	111,643
Pre-PDA Mean	0.679	38.14	0.0879	0.00476	3.498	0.0892
R-squared	0.073	0.070	0.016	0.007	0.153	0.044
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is restricted to female respondents age 18 to 35 in switcher and stayer states between 1973 and 1984, from the CPS ASEC. Estimates are computed using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. Hired is a dummy equal to 1 if the respondent was not employed in the previous year and is employed in the previous week, and 0 if not employed in the previous week. Fired is a dummy equal to 1 if an individual absent from work in the previous week is absent due to a layoff and 0 otherwise. 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. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Changes in Hourly Wage and Employment, Heterogeneity by Share Female in Industry, CPS ASEC data

	Hourly Wage in Industry			Employed in Industry		
	Any (1)	$\geq 75\%$ (2)	$< 75\%$ (3)	Any (4)	$\geq 75\%$ (5)	$< 75\%$ (6)
Switcher State x Post-PDA	-0.126**	-0.231*	-0.104	-0.046***	-0.004	-0.042***
	(0.061)	(0.129)	(0.069)	(0.006)	(0.005)	(0.007)
Observations	66,246	13,180	53,057	111,643	111,643	111,643
Pre-PDA Mean	3.498	3.364	3.533	0.679	0.133	0.547
R-squared	0.153	0.196	0.146	0.073	0.006	0.054
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	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, from the CPS ASEC. Columns (1) to (3) further restrict the sample to employed individuals and estimate our baseline specification (14) 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 computed 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 11 parameters: r, δ, y, q, μ, w, c, γ, v, n, φ. We calibrate 10 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
μ	0.54
w	0.98
γ	0.5
v	0.5
n	0.001
φ	0.05

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 after[46], 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. In particular, we set $v = 0.5$, 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).

The growth rate of the female labor force $n = 0.001$ and the rate $\varphi = 0.05$ are computed using the data in the Current Population Report. In particular, for n we use the data reported in footnote 17, page 4 of the document. It is more difficult of obtain information about the number of pregnant unemployed women who give up on job search and leave the labor force. To calibrate φ, we thus use the data in Table 8 at page 14 of the document

[46] See for instance this document by the Census Bureau.

as a proxy to at least partly capture the extent of this phenomenon. Between 1971-1975, 84.7% of women who did not work during pregnancy were still not working after 12 months. If we take this as an (upper bound) estimate of the expected number of exits from the LF of non-working pregnant women over 12 months, we get an exit rate of 0.07 per month. Since this number might include women who were already out of the labor force when they became pregnant, it might be an over-estimate of the rate of *unemployed* (but still in the LF at the time of pregnancy) women who leave the LF. To address the problem, we also compute the analogous value for "Women who worked during pregnancy". These workers are less likely to leave the labor force in the time immediately following the pregnancy, so it provides a lower estimate for the same rate, 0.05. We repeat the computation for the years 1976-1980 and 1981-1985 and take the overall average of these rates over time to get $\varphi = 0.05$.

The remaining parameter to calibrate is the wage, w. Under our assumption that pre-PDA, $K = 0$, to find w we can match our predicted unemployment rate u with its empirical counterpart. Plugging $K = 0$ into (9), we get

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

Substituting this into (12) we obtain the unemployment rate in terms of fundamentals and can match it to 0.094, which is the female unemployment rate. This yields a wage rate of $w \approx 0.98$.

A.2 Triple Difference Estimator

As said in section 5, we augment the baseline DDR specification of equation 14 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. We exploit this characteristic of pregnancy-discrimination 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 (14) estimating

$$y_{i,s,t} = \beta SW_s \times P_t \times I_{i,s,t} + \alpha SW_s \times I_{i,s,t} + \nu P_t \times I_{i,s,t} + \rho I_{i,s,t} + \Gamma X_{i,s,t} + \eta_{s,t} + \varepsilon_{i,s,t} \quad (16)$$

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 (16) identifies the parameter of 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.

We similarly augment specification (16) as follows

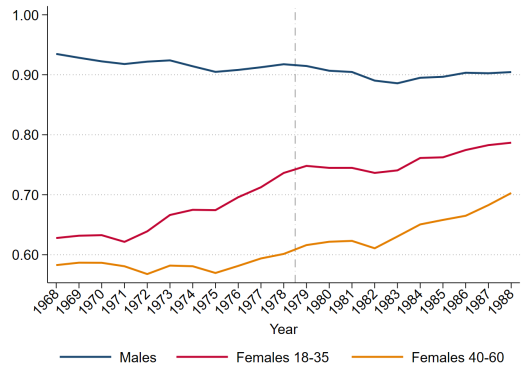
$$\begin{aligned} y_{i,s,t} = &\sum_{l \neq 1979} \beta_l SW_s \times \mathbf{1}\{t = l\} \times I_{i,s,t} + \alpha SW_s \times I_{i,s,t} \\ &+ \sum_{l} \nu_l \mathbf{1}\{t = l\} \times I_{i,s,t} + \rho I_{i,s,t} + \Gamma X_{i,s,t} + \eta_{s,t} + \varepsilon_{i,s,t} \end{aligned} \tag{17}$$

where terms are as described above.

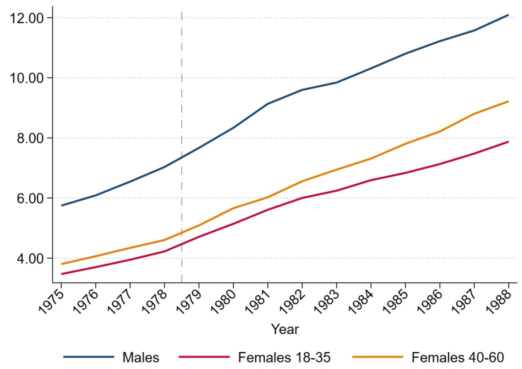
A.3 Additional Figures

Figure A1: National Trends around 1978

(a) Share Employed



(b) Hourly Wage ($)



Notes: The sample consists of all CPS ASEC respondents of age 18 to 60. Estimates 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.

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

CHAPTER 26—MATERNITY LEAVE

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. 320, L. 1975.

Title of Act

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

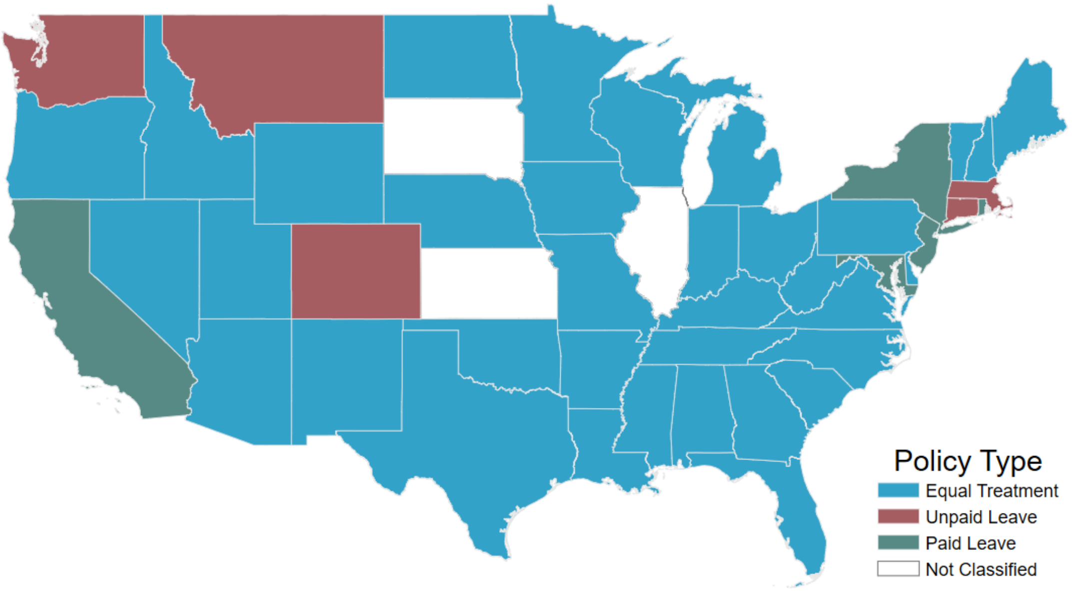
(e) to require that an employee take a mandatory maternity leave for an unreasonable length of time.

(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.

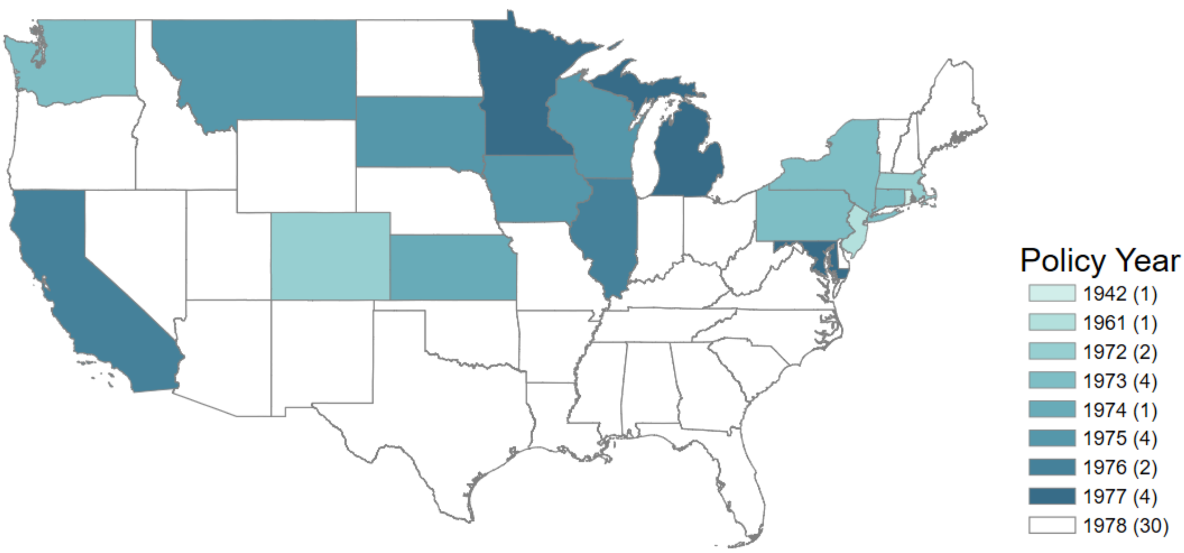
Source: Labor, 3 Mont. Code Ann. 1 (1977).

Figure A3: Type of Policy Adopted



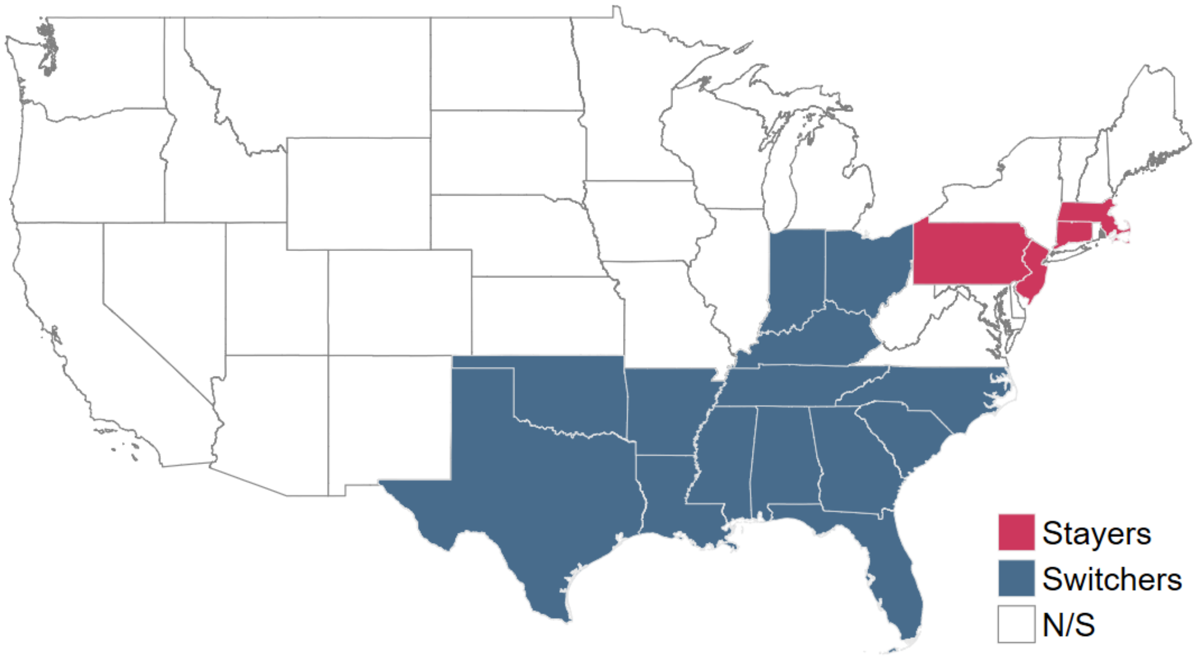
Notes: This map displays the type of policy adopted. States that adopted an equal treatment policy include also all states that adopted it only when the PDA was passed in 1978.

Figure A4: Timing of Pregnancy Discrimination Legislation



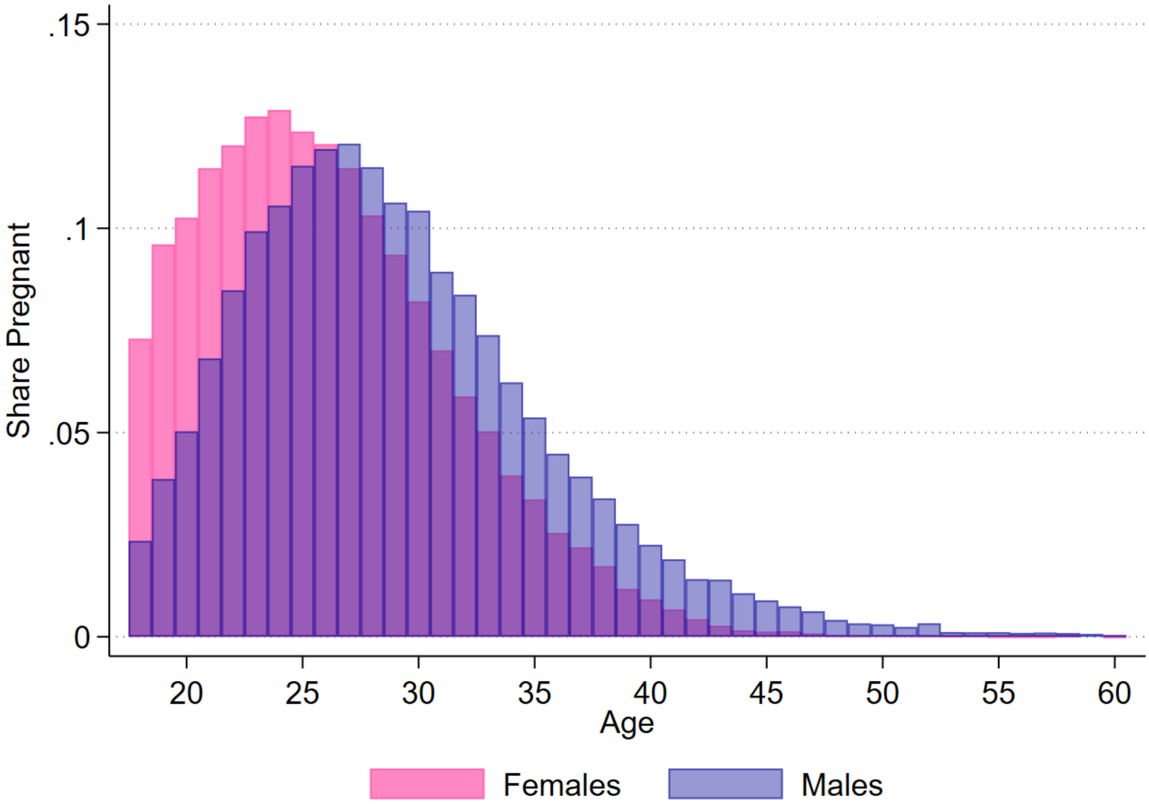
Notes: This map displays the year of passage of each state's pregnancy-discrimination policy. States that only enacted such a policy in 1978, with the passage of the PDA at the federal level are displayed in white.

Figure A5: Switcher and Stayer States, Main Analysis



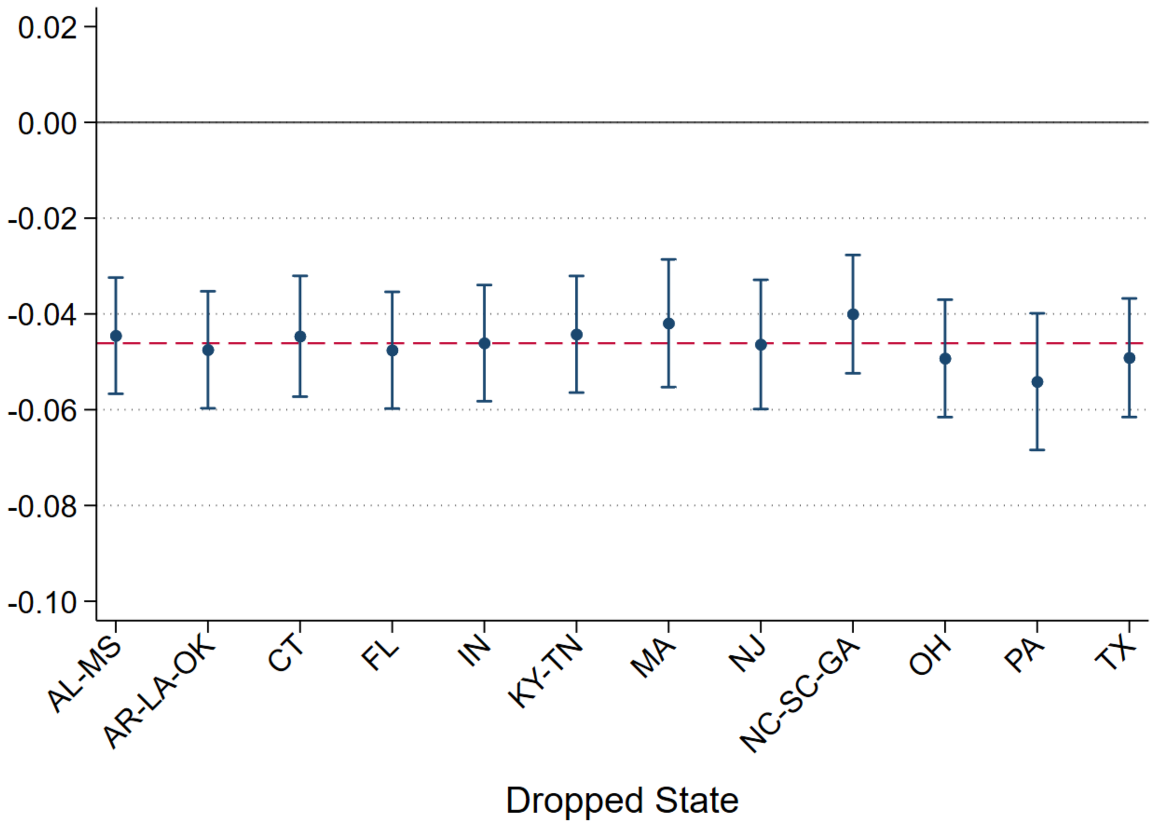
Notes: This map displays the set of switcher and stayer states used in the main analyses based on CPS data.

Figure A6: Share Pregnant by Age



Notes: The sample consists of all CPS ASEC respondents of age 18 to 60, between 1973 and 1984. Estimates are computed 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 A7: DDR Estimates for Employment, Robustnees

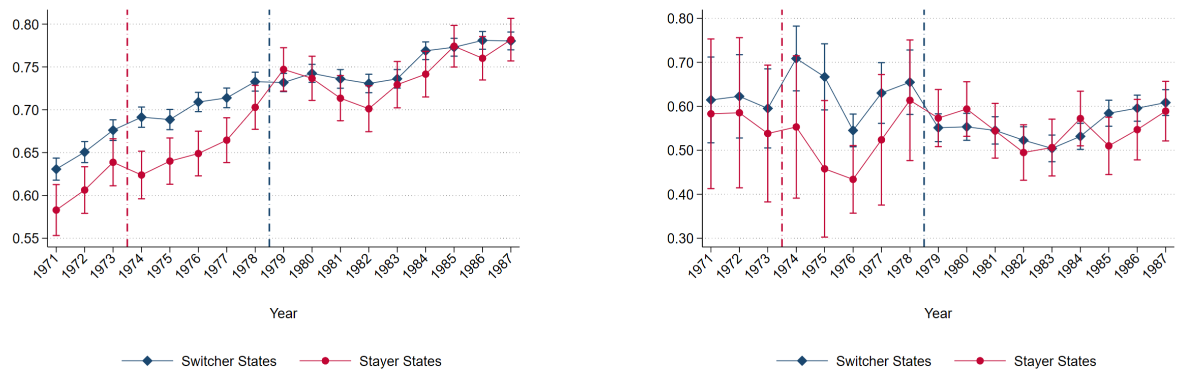


Notes: This plot displays the DDR coefficient on the interaction between the *Switcher* and *Post-PDA* dummies from specification (14), estimated dropping from the sample observations from one state group at a time. The omitted state group is indicated on the x-axis.

Figure A8: Raw Trends in Employment, Extended Time Window

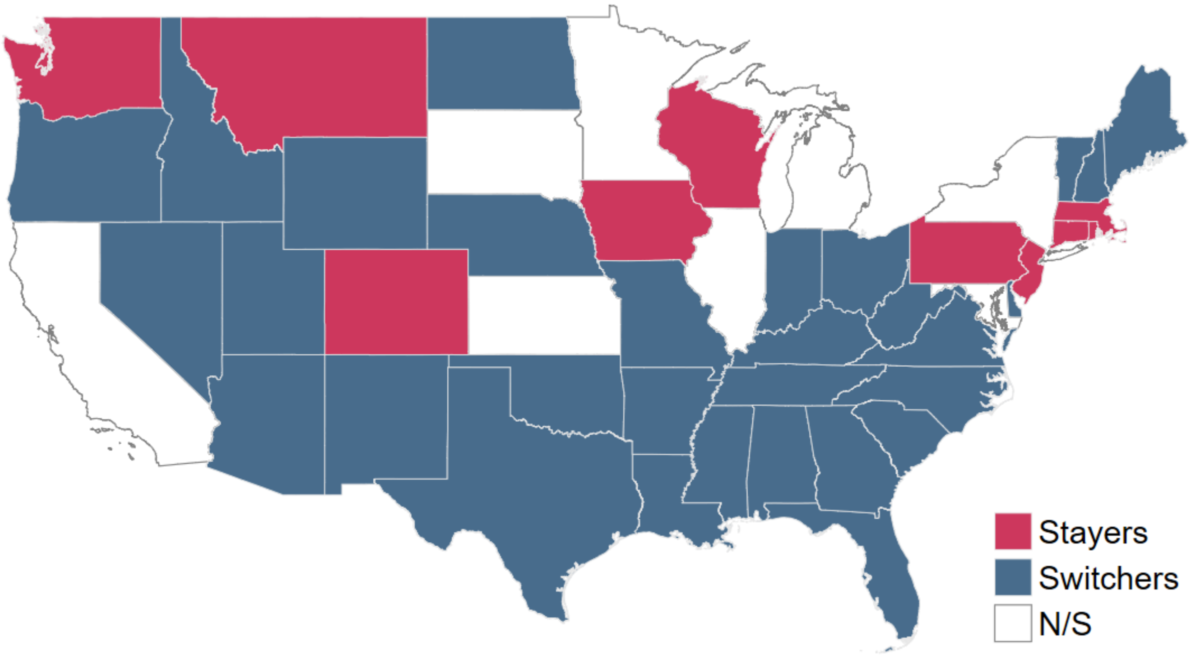
(a) CPS ASEC

(b) PSID



Notes: The sample consists of all female respondents of age 18 to 35, between 1971 and 1987 from CPS ASEC, in panel (a), and PSID, in panel (b). Estimates are computed using each survey's individual sample weights. The red (blue) vertical dashed line indicates the time at which stayer (switcher) states get treated. Stayer states include only Pennsylvania and Connecticut in panel (a), and also Massachusetts in panel (b). New Jersey is excluded since treated in 1961.

Figure A9: Switcher and Stayer States, PSID All Possible States



Notes: This map displays the set of switcher and stayer states used in the robustness exercise using all possible states from PSID data.

A.4 Additional Tables

Table A1: Difference-in-Differences in Reverse Estimates, PSID data

	All		With Children		Without Children	
	(1) Employed	(2) Hired	(3) Employed	(4) Hired	(5) Employed	(6) Hired
Switcher State x Post-PDA	-0.072**	-0.083**	-0.065*	-0.063	-0.142***	-0.184**
	(0.028)	(0.034)	(0.033)	(0.038)	(0.052)	(0.072)
Observations	14,215	8,393	11,299	6,778	2,916	1,615
Pre-PDA Mean	0.584	0.469	0.552	0.452	0.689	0.542
R-squared	0.098	0.109	0.083	0.099	0.206	0.198
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is restricted to female respondents age 18 to 35 in switcher and stayer states between 1973 and 1984, from PSID data. The sample in columns (3) and (4) is further restricted to women that had at least one child and, in columns (5) and (6), to women that did not have any child throughout their entire fertility cycle. Estimates are computed using PSID individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. Hired is a dummy equal to 1 if the respondent was not employed in the previous year and is employed in the current one, and 0 if still not employed. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Robustness to Larger Set of States and to Using Childhood State, PSID data

	All Possible States		Childhood State	
	Employed (1)	Hired (2)	Employed (3)	Hired (4)
Switcher State x Post-PDA	-0.060**	-0.069**	-0.052*	-0.055
	(0.026)	(0.031)	(0.029)	(0.035)
Observations	18,026	10,563	13,389	7,908
Pre-PDA Mean	0.575	0.486	0.575	0.454
R-squared	0.097	0.113	0.107	0.118
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984, from PSID data. The set of switcher and stayer states in columns (1) and (2) are is illustrated in figure A9. Columns (3) and (4) use the baseline set of switcher and stayer states, shown in figure A5, but assign respondents treatment status based on their childhood state of residence instead of their current state of residence. Estimates are computed using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. Hired is a dummy equal to 1 if the respondent was not employed in the previous year and is employed in the current one, and 0 if still not employed. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A3: Triple Difference Estimates, CPS ASEC data

	(1) Employed	(2) Weeks Employed	(3) Hired	(4) Hourly Wage ($)
Switcher State x Post-PDA x Treated Individual	-0.038***	0.500	-0.012	0.085
	(0.007)	(0.325)	(0.008)	(0.078)
Switcher State x Treated Individual	0.048***	-1.722***	0.025***	0.068
	(0.005)	(0.260)	(0.006)	(0.054)
Post-PDA x Treated Individual	0.080***	1.386***	0.018***	-0.973***
	(0.006)	(0.276)	(0.007)	(0.065)
Treated Individual	0.011**	0.493**	-0.035***	2.510***
	(0.005)	(0.240)	(0.005)	(0.056)
Observations	398,396	264,074	87,788	245,586
Pre-PDA Mean	0.771	43.91	0.0690	4.889
R-squared	0.132	0.110	0.034	0.294
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984, from CPS ASEC data. Estimates are computed using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. Hired is a dummy equal to 1 if the respondent was not employed in the previous year and is employed in the previous week, and 0 if not employed in the previous week. 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 A4: DDR Estimates for Older Women and Men, CPS ASEC data

	Women age 40-60				Men age 18-60			
	Employed	Weeks Employed	Hired	Hourly Wage ($)	Employed	Weeks Employed	Hired	Hourly Wage ($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Switcher State x Post-PDA	-0.013*	-0.397	0.004	0.001	-0.007**	-0.796***	0.012	-0.336***
	(0.007)	(0.299)	(0.004)	(0.077)	(0.003)	(0.138)	(0.011)	(0.055)
Observations	88,439	43,963	35,805	40,478	198,314	151,584	18,503	138,862
Pre-PDA Mean	0.574	43.85	0.0359	3.753	0.914	46.35	0.104	5.833
R-squared	0.075	0.020	0.011	0.164	0.060	0.142	0.037	0.279
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is restricted to observations in switcher and stayer states between 1973 and 1984, from CPS ASEC data. The sample is restricted to female respondents age 40 to 60 in columns (1) to (4) and male respondents age 18 to 60 in columns (5) to (8). Estimates are computed using ASEC individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. Hired is a dummy equal to 1 if the respondent was not employed in the previous year and is employed in the previous week, and 0 if not employed in the previous week. 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.