Fertility-Based Discrimination

Evidence from the Pregnancy Discrimination Act of 1978*

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Abstract

Identifying the role of expected fertility as a driver of gender discrimination is crucial to guide policy solutions, but is hindered by the fact that pregnancy discrimination is typically addressed by broader gender-discrimination policies. This was not the case in the U.S., where before the 1978 Pregnancy Discrimination Act (PDA) pregnancy constituted a legal exception to existing gender discrimination laws. Exploiting states' staggered adoption of laws similar to the PDA, we use difference-in-differences estimates based on survey data to show that the PDA reduced employment and hiring of women of fertile age, while having no detectable effect on dismissals. Our estimates imply that nearly 30% of the gender employment gap at the time was due to potential fertility. A simple search-and-matching model explains these results: by raising firing costs for discriminating employers, the PDA discouraged hiring but was not enforced strongly enough to prevent dismissals of pregnant employees. Finally, we show that pre-existing equal pay laws limited wage adjustments, exacerbating the negative effect on employment.

Keywords: Pregnancy Discrimination Act, gender gap, fertility, labor market, employment protection
JEL Codes: J16, J13, J78, K31, N32

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1 Introduction

Despite policymakers' efforts to combat gender discrimination, it remains a persistent issue in the labor market (Goldin, 2024; Blau, 2025). To effectively guide regulation, it is crucial to identify the underlying drivers of such discrimination. While gender differentials in employment and earnings nowadays predominantly emerge around motherhood (Gallen, 2023; Kleven, Landais, and Leite-Mariante, 2024), the widespread documentation of motherhood penalties alone cannot definitively establish that employers' responses to fertility are a main source of gender discrimination. First, these gaps could be explained by supply-side responses. Second, by definition, motherhood penalties cannot capture employers' responses to *potential* fertility, which affect all women of childbearing age, irrespective of their actual fertility choices. Thus, to identify how fertility-related concerns drive demand-side discrimination, alternative empirical strategies are needed. One possible approach is to study the impact of anti-discrimination regulation on women's labor market outcomes through the response of the employers. However, a major challenge in empirically identifying the contribution of pregnancy discrimination arises from the fact that pregnancy discrimination is typically addressed by broader gender discrimination laws, that is, policies that sanction behaviors that are discriminatory on the basis of gender, including realized fertility. Strikingly, this was not the case in the United States until the passage of the Pregnancy Discrimination Act (PDA) in 1978. Legislation to curb gender discrimination in employment had already been in place at a federal level since the 1960s, with the Civil Rights Act (CRA) of 1964. However, before the PDA, pregnancy was effectively treated as an *exception* to these gender discrimination policies[1], allowing employers to legally dismiss pregnant workers. The PDA sought to address this gap by requiring that pregnant employees be treated the same as other workers with comparable needs in all employment matters, including hiring, pay, promotion, and discharge. Effectively, it constrained employers' ability to condition employment outcomes on realized fertility, forcing them to anticipate the effects and likelihood of future pregnancies of young female employees at the hiring stage.

In this paper, we exploit the introduction of the PDA as a natural experiment to isolate and quantify the role of employers' fertility-related concerns in driving gender discrimination. Moreover, we measure the impact of pregnancy-discrimination policies on women's labor market outcomes, which is *ex ante* ambiguous. We use a simple search-and-matching framework to show this and shed light on the mechanisms that shape the response to such policies. Based on the fact that, especially at the time, anti-discrimination policies were mostly enforceable on

[1]The U.S. Supreme Court explicitly held that discrimination based on pregnancy did not constitute gender discrimination in landmark cases (see, Geduldig v. Aiello, 417 U.S. 484 (1974) and General Elec. Co. v. Gilbert, 429 U.S. 125 (1976)).

the firing margin (Acemoglu and Angrist, 2001), we conceptualize the PDA as strengthening the employment protection of pregnant workers. Indeed, the policy effectively increased the expected costs of firing pregnant employees, in an amount proportional to the probability of litigation after separation and the fines if found guilty. Firms employing fertile-age women and providing accommodations for male workers face a trade-off: either incur accommodation costs to retain pregnant employees or risk penalties for discriminatory dismissal. We thus expect to observe a reduction in firing of pregnant workers, but only if the penalties and enforcement are high enough to overcome the accommodation costs. Moreover, compared to the situation before the PDA, we have an increase in the expected costs of employing, and, consequently, a reduction in hiring of women of fertile age. The reduction will be stronger where employers perceive greater fertility-related costs. Note that, while also *de jure* forbidden, discrimination on the hiring margin is much harder to detect and punish, leaving employers more space to adjust on this margin. Which of these two channels dominates will determine whether a pregnancy-discrimination policy negatively or positively affect employment of fertile-age women. We focus on the potential negative response on the hiring margin as the other margin, wages, is expected to respond less given pre-existing anti-discrimination laws which imposed equal pay requirements. By tying female wages to those of male co-workers in similar positions, these laws likely constrained the adjustments of women's wages. The overall effect of the PDA on the employment and wage dynamics of women of fertile age is thus an empirical question.

With this theoretical framework in mind, we empirically identify the causal effects of interest exploiting quasi-experimental variation generated by certain US states enacting policies similar to the PDA prior to 1978. While the PDA was a federal policy, leaving no untreated state, the fact that some states passed comparable or more protective legislation allows us to use them as a comparison group, whose treatment status did not change around 1978. We leverage this variation to estimate the effects of the PDA via a difference-in-differences (DID) estimator. We implement this combining a novel dataset on all US states' pregnancy-discrimination policies passed prior to 1978, with 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). The survey data allows us to measure employment status, hiring and firing, wages, and fertility. Under the assumption that, absent the PDA, trends of states treated first in 1978, which we label *PDA-treated*, would have been parallel to those of states that adopted these policies earlier, labeled *PDA-control*, we find evidence that the PDA led to a substantial and statistically significant decline in the likelihood of employment among women of fertile age. Our preferred estimate, based on the CPS ASEC, show a decline of 4.5 percentage points, or 6.6% in relative terms. We offer evidence that this decline was driven by a comparable decline in hirings and no change in

the rate of firing workers upon pregnancy. 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 in employment of fertile-age women, 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 directly benefited from the PDA.

When analyzing hourly wages, another possible margin of response by employers, our results generally indicate smaller effects. We interpret this as legitimating the 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 to be paid an equal wage, may have limited substantial wage adjustments. We support this empirically showing that, in female-dominated industries, where the EPA posed weaker constraints, wages declined while employment did not. And *vice versa* in other industries. This also suggests that wage rigidities are a potential justification of the sizeable decline in employment we observe: the reduced ability of wages to react could have exacerbated the policy's impact on employment rates.

Furthermore, 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, but, 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 that the rate of dismissals upon pregnancy remains unchanged.

Finally, we conduct a simple back-of-the-envelope exercise to quantify the role of discrimination based on expected fertility in driving the overall gender employment gap in this period. The exercise rests on the idea that the PDA, by increasing the expected costs of dismissing pregnant workers, leads employers to adjust their hiring behavior. Consequently, the observed change in employment probability for fertile-age women reflects the portion of job loss driven by fertility-related concerns. By comparing this change to the contemporaneous gender employment gap—which serves as a proxy for overall gender discrimination—we can estimate the share attributable specifically to expected fertility. Specifically, we estimate a gender employment gap for fertile-age individuals equal to 15.9 percentage points, which we compare to our preferred estimate of the effect of the PDA on fertile-age women employment, equal to -4.6 percentage points. This implies that 28.3% of the gender employment gap after the PDA was explained by employers' discrimination based on expected fertility. Moreover, if we restrict attention to fertile-age childless women, for which the overall gender employment gap was much smaller (3.0 percentage points), we find that 80.0% of it is explained by the response to the policy (-2.4 percentage points). This further suggests that, even where obstacles to

women advancement in the labor force have been removed or diminished, a substantial part of the gap that is left to close must be tackled addressing employers' response to expected fertility, especially on margins that are harder to regulate and enforce.

Our paper relates to the large literature on gender discrimination in the labor market (Goldin and Rouse, 2000; Givati and Troiano, 2012; Azmat and Petrongolo, 2014; S. O. Becker, Fernandes, and Weichselbaumer, 2019; Thomas, 2020; Fernández-Kranz and Rodríguez-Planas, 2021; Bamieh and Ziegler, 2023; He, S. X. Li, and Han, 2023; Q. Li and D. Xiao, 2025; Maloney and Neumark, 2025) 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). However, we still lack precise evidence on the determinants of gender discrimination and, in particular, the role played by fertility. A small set of recent correspondence studies (S. O. Becker, Fernandes, and Weichselbaumer, 2019; He, S. X. Li, and Han, 2023; Q. Li and D. Xiao, 2025) documents that employers statistically discriminate in hiring based on expected fertility and family responsibilities. Consistently, a second set of studies shows that mandated benefits that increase costs of employees' fertility for employers (Thomas, 2020; Fernández-Kranz and Rodríguez-Planas, 2021; Timpe, 2024) negatively affect hiring and promotion of fertile-age women. While all these papers study settings in which employers are forbidden to condition employment on realized fertility, we leverage the unique quasi-experiment offered by the PDA of 1978 to study the effects of introducing such prohibition and isolate the contribution of expected fertility to overall gender discrimination. To the best of our knowledge, we are the first to provide causal estimates that quantify what share of observed gender gaps is explained by fertility-related discrimination.

In studying the PDA of 1978, we also add evidence on the effects of this important legislative step. The papers that studied it so far 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 health benefits, can have sizeable effects on women's employment outcomes. Moreover, we document how the PDA interacted with earlier gender discrimination laws. Building on 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 the EPA shaped the response to the PDA of 1978: where the EPA constrained adjustments on the wage margin, women's

employment fell substantially more.

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), who examined the 1991 Americans with Disabilities Act, we consider an employment protection policy that applies only to a subgroup of workers, with a key distinction: 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 4 we describe the data we use and we outline the empirical strategy in section 5; section 6 presents the main results and some interesting evidence on the mediating role of the EPA of 1963 and section 7 uses the estimated effects to quantify the role of fertility-related discrimination. Finally, section 8 concludes.

2 Background

In this section, we provide a brief overview of the history of the Pregnancy Discrimination Act of 1978, and of comparable state policies that were passed 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[2]. Already in the 1960s, legislators had addressed issues of gender discrimination in the labor market with the

[2]Full text available here.

Equal Pay Act (EPA) of 1963 and Title VII of the Civil Rights Act of 1964. The former forbade 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[3]. 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.

In 1973, the EEOC tried to address this gap by promulgating guidelines specifying that discrimination on the basis of pregnancy constituted gender discrimination and was prohibited by Title VII[4]. While lower federal courts initially accepted this view (Wolkinson and Liberson, 1982), the US Supreme Court ruled against this interpretation. In two famous cases concerning the issue of disability benefits for pregnancy, Geduldig v. Aiello (1974) and General Electric v. Gilbert (1976), the court ruled that pregnancy-based distinctions were *not* a gender-based classification (Pedriana, 2009). 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[5]. The view of the Supreme Court, binding across all federal courts, was thus that pregnancy constituted an exception to Title VII[6].

These decisions by the Supreme Court generated significant controversies that ignited claims to expanding protections for pregnant workers, led by a coalition of civil rights and women's movements, labor unions. These groups view the Supreme Court decisions as fundamentally undermining protections against gender discrimination mandated by Title VII of the CRA. This is made evident by a statement by Susan Ross, on behalf of the Coalition to End Discrimination Against Pregnant Workers: "Employers routinely fire pregnant workers, refuse to hire them, strip them of seniority rights, and deny them sick leave and medical benefits given other workers. [...] Together, they add up to one basic fact: employers use women's role of childbearer as the central justification of and support for discrimination against women workers. Thus, discrimination against women workers cannot be eradicated unless the root discrimination, based on pregnancy and childbirth, is also eliminated"[7]. Moreover, this

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

[4]U.S. Equal Employment Opportunity Commission, "Guidelines on Discrimination Based on Sex," 29 CFR 1604.10 L1973.

[5]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).

[6]Note that in Cleveland Board of Education v. LaFleur (1974), the Supreme Court ruled against public-school policies that forced pregnant teachers to leave their positions for five months around childbirth. However, they decided based on the Due Process Clause, rather than on equal protection grounds. As Pedriana (2009) states, "*LaFleur* did not directly address whether pregnancy discrimination was tantamount to sex discrimination."

[7]Hearings on S. 995 before the Subcommittee on Labor of the Senate Committee on Human Resources, 95 Cong., 1 Sess. (1977)

was at a time when the labor market was witnessing stable growth in female employment levels, particularly of women of fertile-age and especially compared to the almost constant male ones(Blau, 2025), 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 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 hiring, firing, pay, and promotions[9] *De facto*, as in analogous contexts[10], other than extensions of employer-provided health insurance benefits[11], the PDA was effectively 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. Indeed, among the pregnancy-discrimination cases pursued after the PDA, only those concerning discharge exhibit a consistently high rate of decisions favoring workers (Edwards, 1996). Moreover, some court cases that followed the PDA show that employers attempted 'creative' solutions to 'mask' dismissal of workers upon pregnancy. This suggests that employers were aware of sanctions placed by PDA on unjust dismissals[12].

Finally, the Act did not introduce special protections for pregnant workers but rather insisted on *equal* treatment relative to temporary disabilities[13]. 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.

[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 [...] as other persons not so affected but similar in their ability or inability to work".

[9]See the EEOC guidelines here.

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

[11]This aspect of the Act has been studied by the seminal work of Gruber (1994) and only applied to establishment with certain insurance plans in place.

[12]See Tamini v. Howard Johnson (1987) and EEOC v. Service News Co. (1991).

[13]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 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).

2.2 US States' Legislation on Pregnancy Discrimination Before 1978

In the same years in which, at the federal level, the Supreme Court ruled against interpreting pregnancy discrimination as gender discrimination, leading to the passage of the PDA, 19 US states and the District of Columbia had already adopted policies concerning the treatment of pregnant workers. These policies varied both in their provisions and mode of passage – via acts of legislation, administrative rulings, and states Supreme Courts' decisions – but most of them mandated at least the level of employment protection of pregnant workers that was then required by the PDA[14].

Of these 20 states, ten enacted policies that simply mandated equality of treatment of pregnancy and temporary disabilities, as the PDA[15]. Six states implemented policies that, strengthened employment protection even more, requiring employers to provide job-protected unpaid leaves of reasonable length[16]. Importantly, Wolkinson and Liberson (1982) states that these policies continued to be enforced and intended as valid by arbitrators in the respective states, even after the Supreme Court rulings of 1974 and 1976. The timeline of these policies is summarized in Table A1. Finally, five states amended their short-term temporary disability insurance (STDI) policies to include pregnancy in the set of qualifying conditions[17]. Leave under state STDI is not job-protected. Hence, we do not consider these states as 'treated' before the PDA[18].

The remaining 31 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 A2 and A3.

[14]These states policies do not 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.

[15]These are: Pennsylvania in 1973; New York in 1974, Alaska, Iowa, South Dakota, and Wisconsin in 1975; Maryland, Minnesota, Oregon and the District of Columbia in 1977.

[16]These are: Colorado, Kansas, Massachusetts and Washington in 1972; Connecticut in 1973; Montana in 1975.

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

[18]We also exclude New York from the analyses, as it adopts a STDI amendment for pregnancy, likely affecting employment, after passing a pregnancy-discrimination policy in 1974 and before 1978.

3 Theoretical Framework

To capture the effect of the PDA on female employment, we use a simple job protection model in a labor market matching setting (see, for example, Mortensen and Pissarides, 1999). There is a continuum N of identical female workers in the economy[19] and free entry of firms. Motivated by our main empirical results (see section 6.2), which suggest that fertility decisions did not react to the policy, and following P. Xiao (2023), we assume that, in each period (unless they are already pregnant or have recently had a child), a fraction q of women becomes pregnant. 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. 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[20], is a much less effective threat against discriminating behaviors.

Throughout our theoretical analysis, we maintain the assumption of fixed wages. Notably, by 1978, when the PDA was approved, the Equal Pay Act of 1963 was already in effect, requiring 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 6.3).

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

[19]We focus the model on the labor market for women of fertile age and do not explicitly represent young men or older women, as neither group was a likely substitute in the context of the U.S. labor market in the late seventies. Young men are excluded due to the highly segregated nature of the labor market at the time, as well as the fact that they were close to full employment. Older women, by contrast, likely had different skills and were, for the most part, either already employed or out of the labor force.

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

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[21].

Next, we have to specify 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 G. S. 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[22]. Even if the firm decides not to dismiss the worker because of pregnancy, there can be other exogenous factors causing a separation, at rate δ[23]. Finally, the flow value

[21] 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.

[22] 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.

[23] 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

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 assumption that only non-pregnant women match with open vacancies. This is motivated by the fact that, 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).

A formal treatment of the model is provided in Appendix A.1. Here, we present the intuition and key mechanisms. Given this set-up, we analyze the effect on women's labor market outcomes of an intensification of legislation protecting pregnant workers, modeled as an increase in K—that is, the legal cost of firing a pregnant employee. For each level of K, when a worker becomes pregnant, employers face a trade-off: they can either keep the worker employed and incur the accommodation cost c, or fire her and face replacement costs (posting a vacancy and waiting for it to be filled), along with the legal repercussions, i.e., paying K.

There must then exist a threshold $\overline{K}$ (characterized in Equation 10, and shown in Figure 6) such that, when $K < \overline{K}$, employers prefer to incur the firing costs and replace the worker, while when $K > \overline{K}$, they choose to keep her employed. We say that in the former situation the labor market is in the "firing regime", while in the latter the labor market is in the "keeping regime". If effective, the PDA should lead to a large enough increase in K to shift the market from the first to the second equilibrium.

This framework highlights two possible outcomes for the equilibrium unemployment rate as K changes. If the PDA fails to raise K above the threshold $\overline{K}$ (a case we refer to as a *weak enforcement* of the policy), the effect on employment for women of fertile age is unambiguously negative. Discriminating firms now face higher costs when firing pregnant employees but still choose to do so. The policy thus makes it more expensive to employ women of fertile age, and firms respond by reducing hirings.

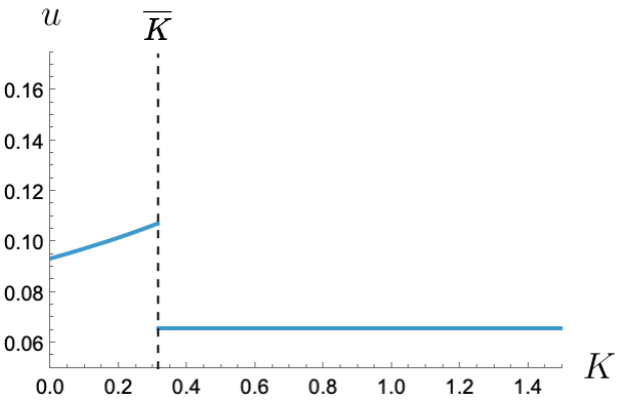
In the second scenario, the PDA does push K above $\overline{K}$. At that point, employers prefer to keep pregnant workers rather than pay the cost of dismissal, which leads to fewer separations. But since firms now bear the cost of accommodations, they also scale back new hires of fertile

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 ($\alpha = 0$).

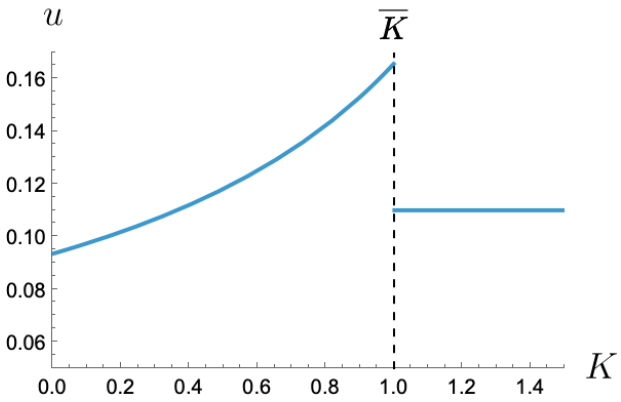
age women. In practice, by limiting the ability to fire pregnant women *ex post*, the policy increases the cost of hiring all women of fertile age *ex ante*, independently of whether they'll actually have children. In this case, the overall effect on female unemployment is ambiguous and depends on the value of the parameters, as Figure 1 illustrates.

Figure 1: Effect of the PDA on the unemployment rate of women as a function of K for different values of c

(a) Decrease in unemployment



(b) Increase in unemployment



Notes: We assume a Cobb-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 $c = 0.7$.

A key factor in the analysis is the value of the discrimination parameter c. This parameter not only affects the threshold $\overline{K}$—which increases with c—but also shapes the overall impact of the policy on unemployment. When c is low to moderate, a strong legislative intervention that shifts the economy from a state where $K < \overline{K}$ to one where $K > \overline{K}$ can improve employment outcomes for women (left-hand graph in Figure 1). In this case, the reduction in hiring—driven by employers' anticipation of potential pregnancies—is relatively mild compared to the reduction in firings due to the increased employment protection. By contrast, when c is high, the same policy intervention could increase unemployment among non-pregnant women (right-hand graph in Figure 1).

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 6.3.

We thus conclude that the net effect of the policy on u is theoretically ambiguous, and turn to the empirical analysis to determine (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 1 realized in practice.

4 Data

This section describes the data on pregnancy-discrimination policies, the basis of our identification strategy, and on employment and fertility outcomes, obtained from individual-level surveys.

4.1 Data on Pregnancy-Discrimination Policies

To identify the effects of the PDA, we must determine which states passed comparable or more protective policies before 1978. To do this, we carefully studied the provisions of the PDA and their interpretation in the Law literature[24]. Moreover, we collected and codified information on all pre-1978 state policies regarding the employment of pregnant women, which we discussed in section 2.2. We compiled this data using both primary and secondary sources, guided by policy reports from the National Partnership for Women and Families, the US Census Bureau, and the Department of Labor (see here). We obtained the original text for almost all of these state laws, administrative rulings and court decisions. This was mostly done consulting legal databases. In particular, the HeinOnline database on Session Laws, which provided us with specific information on the date of enactment, exact provisions and amendments of these policies. For administrative rulings, this was done contacting the relevant state archives and libraries. For the few case in which it was not possible to recover the original text, 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. The complete list of these states and their state policies can be found in Table A1.

4.2 Individual-level Data

Our main analyses are based on two individual-level surveys, the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) and the Panel Study of Income

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

Dynamics (PSID)[25]. The former is an addendum to the March CPS, repeated yearly for a large cross-section of individuals and collecting fairly rich information on employment and earnings. The PSID instead has a smaller sample, but it provides more detailed geographical information on respondents and, by following individuals across years, on changes in employment status.

CPS ASEC data: The CPS ASEC is a cross-sectional, nationally representative supplement to the March CPS that, since 1962, collects detailed annual information on individuals' labor-force status, earnings, and demographic characteristics. Our main variable of interest to capture the response to the PDA is employment. The CPS ASEC records employment status in two ways: asking respondents whether they were *ever* employed in the previous year and whether they were employed in previous week. We use the former as our main outcome, as it is less noisy, and check robustness to using the latter. Note that this variable captures employment *vs non-employment*. Not unemployment as we do not condition on being in the labor force. This indeed the margin that was relevant in the US at the time: women were either employed or out of the labor force, particularly after childbirth. Next, we combine the information on employment in the previous year and week to define a proxy of hiring. We defined hiring as the event in which an individual reports being employed in the previous week, but not employed in the preceding year. Since the CPS ASEC survey was run in March, this variable can at most capture hiring events that happened in the first three months of each year, being clearly a strong underestimate. We chose to include it anyway since it is the best proxy of hiring that we can build with this survey. The PSID, described below, offers a better proxy of hiring which we thus prioritize. 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. To measure wages, we combine information from multiple survey questions. We compute hourly wages by dividing the annual total income from wage earnings for the previous year by the total number of hours worked in the previous year. The latter is estimated multiplying the total number of weeks worked in the previous year by the usual number of hours worked per week in that year. This variable is only available since the 1976 survey, thus referred to year 1975. From survey questions on the presence and age of children we constructed a proxy of fertility. We coded a woman as pregnant in the previous year if she reports having a child less than one year old in March of the current year, which thus encompasses all births from April of the previous year to March of the current one.

Furthermore, we take from the survey a number of other variables on other outcomes, for robustness and individual-level covariates, such as marital status, age, race, education,

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

location information, and industry of employment[26]. Finally, we assign to each variable the year to which it is referred. That is, for variables referred to the previous year, we assign their values to this year and not the survey year in which they were recorded. This way we can measure the correct dynamic of the effects.

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.

PSID data: We strengthen the analysis by replicating our main results with another individual-level survey available before and after 1978, the PSID (Pfeffer, Daumler, and Friedman, 2023). This is a longitudinal household survey that follows the same families and their offspring from 1968 onward, providing annual data on employment, income, fertility, and geography.

The definition of most variables is analogous to the CPS ASEC data, with a few exception. In particular, by tracking individuals for multiple years, the PSID provides a more precise measure of hiring, knowing employment status each year for the entire year, and not just in the previous week as in the CPS ASEC. We instead cannot look at hourly wages as it is not possible to reconstruct this information from available variables. But we can add more on other dimensions. 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, the PSID reports also the respondents' childhood state of residence, allowing us to address concerns of selective migration.

Note that, in the analyses below, we exploit the cross-sectional dimension of the PSID and we make use of all the respondents in the PSID, including the so-called 'nonsample persons'[27], to maximize sample size.

Additional data: We exploit three other data sources to enrich our analyses. We use data from Basic Monthly CPS to obtain a more frequent measure of employment and hiring, which we use to assess trends around the enactment of the PDA and check robustness. We also use data from the Census. Specifically, we use the 1% form 1 state sample of the 1970 census to measure more precisely the share of women employed in each industry, and the 1% and 5%

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

[27]These are PSID respondents that were not part of the original 1968 sample, but joined the survey later by entering the household of the original sample families. Since the PSID provided survey weights for these respondents only starting in 1997, we computed survey weights for the previous years extending their methodology (Berglund and Heeringa, 2015).

samples of, respectively the 1960 and 1980 censuses for additional robustness checks. Last, we leverage the 1976 Survey of Income and Education (SIE) (United States. Bureau of the Census, 1992), a large supplement of the CPS, to construct measures of the availability of health and temporary disability insurance by industry.

Main Sample and Summary Statistics Our main sample consists of women of fertile age. We define this as women age 18 to 35, to capture fertility patterns at the time, displayed in Figure A5[28].

The second restriction we impose on our sample is dictated by our identification strategy, described in section 5 and based on states' early adoption of pregnancy-discrimination policies, and by the detail of geographical information in the CPS. Unfortunately, in the CPS respondents' state of residence is often grouped with multiple states 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. In particular, we retain data from 1973 to 1984, to have enough years before and after the enactment of the PDA in 1978. We then 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, Illinois, Indiana, Kentucky, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. Instead, the early-adopters, used as reference group, are Connecticut, Massachusetts, and Pennsylvania. Finally, we further restricted our main sample to fertile-age women, the group that is directly affected by the policy[29]. While the PSID does not group states of residence, for consistency and comparability, all the main results are based on these two groups of states. We show however that results are robust to using all possible states with the PSID in section 6.5.

Table 1 reports sample sizes and summary statistics based on the CPS ASEC and PSID data for the variables discussed in the previous section and for the main sample. As seen

[28]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.

[29]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).

from the panel (a) of this table, which is based on the CPS ASEC, primary education is virtually universal in this time period. College education is much less frequent but growing. 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. Looking at employment outcomes for women aged 18–35, employment has been rising over time, driven entirely by private-sector jobs. This higher employment rate translates into an increased average number of weeks worked per year, while the usual number of hours worked each week has remained essentially unchanged. Nominal wages over the period also exhibit steady growth, in line with the high inflation rates throughout the decade[30]. Finally, the annual likelihood of pregnancy among women aged 18–35 was about 9% before the PDA and 8% after it—meaning just under one in ten women in these cohorts became pregnant in a given year. Similar patterns are found in panel (b), which relies on the PSID, although with some discrepancies between the two sources. In particular, the growth in employment rates detected with CPS ASEC is not found in the PSID. In any case, as long as these survey differences are homogeneous within a survey across states, they do not constitute an issue for the validity of our results.

5 Empirical Strategy

The aim of this paper is to identify the effects of the PDA on fertile-age women employment outcomes to isolate and quantify the role fertility-related concerns in driving gender discrimination by the employers. Since the PDA was a federal law, its provisions applied in all states, leaving no untreated comparison group to be used as a reasonable counterfactual. Nonetheless, as detailed in section 2, multiple US states enacted policies comparable to the PDA. This generates useful variation in the *timing* of treatment for a quasi-experiment: these states were 'treated' before the passage of the PDA and can thus serve as comparison for those that were only treated by the PDA in 1978. We label these two groups of states as *PDA-control* and *PDA-treated*, respectively, and exploit the time and state variation with a difference-in-differences (DID) estimator. Intuitively, this is similar to the basic two-periods two-groups DID setting, with the difference that the comparison group – PDA-control states – is not a *pure control* group, but is rather always treated throughout the observation window. By not changing treatment status around 1978, PDA-control states gives us an estimates of how PDA-treated states' outcomes would have changes without the PDA. To do this, we need to impose a stricter identification assumption, which we discuss below, after introducing the

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

main specification[31].

Quasi-experimental states: For our main empirical strategy, we chose as PDA-control states those that were treated no later than 1973, to observe enough years before the enactment of the PDA. Out of these states, since our main results will be based on the CPS ASEC data, we dropped those that ere grouped by the CPS with states treated after 1978. Similarly, within the states first treated by the PDA, we kept as PDA-treated only those either separately identified or grouped with states all first treated in 1978. This ensures that we can attribute each respondent to the correct treatment group. PDA-treated states are Alabama, Arkansas, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Mississippi, North Carolina, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. PDA-control states instead consist of Connecticut, Massachusetts, and Pennsylvania. We display these sets of states in Figure A4. As shown in the map, the two groups of states are quite spatially separated. Nonetheless, 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 states in section 6.5.

Specification: We implement the DID estimator using a usual two-way fixed effects (TWFE) specification, estimated on the subgroup that is directly affected by the PDA, women aged 18 to 35. That is, we estimate the following regression

$$y_{i,s,t} = \beta T_s \times P_t + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t} \tag{5}$$

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. T_s is a dummy that takes value one if state s is a PDA-treated state and zero if it is a PDA-control state. P_t is a dummy equal to one for post-PDA years, from 1979 onward, and zero otherwise. We start post-periods from the first full-treatment year, 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, 1973-1984 for our baseline specification, and groups of states for those

[31]Another approach could be using the pre-PDA staggered adoption of these state policies. However, as explained in section 4.2, most states are grouped in the CPS until 1976 and almost all PDA-control states are not identifiable separately from some PDA-treated states or other PDA-controls treated in a different year, which makes this strategy unfeasible. The PSID distinguishes all states but state samples are too small to credibly estimate the effects of these early treatments. Besides these reasons, as we are interested in studying the PDA of 1978, we must ultimately rely on a comparison between newly-treated and already-treated states.

that are grouped[32]. Finally, standard errors are robust to heteroskedasticity[33]. The inclusion of year fixed effects helps us separating the effect of the PDA from concurrent factors affecting employment outcomes of women, such as the positive growth in fertile-age women employment and wages observed at the national level, but also recessions that hit the US economy around the passage of the PDA[34].

The parameter of interest in equation (5) is the coefficient β. This parameter identifies the average treatment effect on the PDA-treated (ATT) under a parallel trends assumption. We require that, absent the PDA, outcomes of individuals in PDA-treated and PDA-control states would have evolved in parallel around 1978. In words, this is the usual parallel trends assumption. However, as the PDA-control states were treated before 1978, this assumption implicitly requires that, throughout the period that we use for our quasi-experiment, 1973-1984, PDA-control states are not 'affected' by any dynamic treatment effect. Intuitively, as we use PDA-control states to proxy for the time trend that PDA-treated states would have followed *absent the PDA*, a growing treatment effect in PDA-control states would lead us to overestimate the time effects and thus underestimate the true ATT, *vice versa* with a decaying treatment effect[35].

As usual, the parallel trends assumption described above is fundamentally untestable, as we do not observe the untreated potential outcome for PDA-treated states after 1978. However, we can assess its plausibility looking at trends of PDA-treated and PDA-control before 1978. Observing parallel trends before 1978, would reduce concerns over the validity of this assumption. This is discussed in detail in section 6.5, where we present estimates from the following event-study version of specification (5),

$$y_{i,s,t} = \sum_{l \neq 1977} \beta_l T_s \times \mathbf{1}\{t = l\} + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t} \tag{6}$$

where we omit the first post-PDA year as a reference point and $\mathbf{1}\{t = l\}$ are year indicators. Coefficients β_l for $l \in \{1973, 1976\}$ inform us on the plausibility of the parallel trends assumption[36] and those for $l \in \{1978, 1979\}$ show us the dynamic of the treatment

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

[33]We do not cluster at the level of our policy variation, the state, as we only have 12 clusters. Both with the CPS and with the PSID, estimated confidence intervals based on robust standard errors are wider than those based on clustering by state. We thus adopt the more conservative option.

[34]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.

[35]Note that we can allow for some adjustment period, where the treatment effect is changing for PDA-control states, so long as this period ends before the start of our quasi-experiment time window.

[36]In a usual DID, with an untreated control group, parallel pre-trends tell us that, within the pre-period, the evolution of the control group outcomes would have been a valid proxy for the effect of time on the treated

effect for the PDA-treated group. Notice that, despite both groups being treated, we do not need to assume homogeneous treatment effects to identify the ATT. Nor we need to assume that there is no dynamic treatment effect for the PDA-treated. However, seeing a fairly constant effect for the PDA-treated would reassure us that the assumption we are imposing on the treatment effect for the PDA-control states is not unrealistic.

Furthermore, like in any other DID setting, we make two implicit assumptions: *(i)* absence of spillovers across PDA-treated and PDA-control group, which we will address in section 6.5, and *(ii)* that PDA-control states are not treated as well in 1978. The ensure that the second assumption holds we carefully studied the provisions of each policy passed before 1978 and only used as PDA-control states those whose policies were equally or more protective than the PDA. For instance, we included Massachusetts, whose 1972 policy did not just forbid employers from discriminatorily fire pregnant workers, but provided more by requiring employers to grant job-protected pregnancy leaves. We did not include New Jersey, whose 1961 policy required treating pregnancy as any other temporary disabilities *for disability insurance purposes*, but not for other aspects of employment. However, enforcement in PDA-control states could still have varied around 1978. Despite evidence that the pre-1978 state policies were indeed enforced by the respective state agencies Wolkinson and Liberson (1982), as discussed in section 2.2, it could be that additional enforcement by the EEOC at the federal level after 1978 increased the 'intensity' of treatment in PDA-control states. While we cannot exclude that this happened, we note that this would bias our results towards zero. Our estimates can thus be thought as a lower bound of the true effects of the PDA on the PDA-treated states.

Finally, we show that our results are robust to alternative identification strategies that relax the parallel trends assumption, via triple differences, or exploit other sources of variation. The details about these strategies are provided in section 6.5 and in the respective appendixes.

6 Effects of the PDA

We begin with a descriptive analysis of employment trends. Next, we present our main estimates of the effects of the PDA and discuss how the EPA of 1963 shaped the response. We then provide support for the robustness of these results and use the identified effects to quantify the role of fertility-related discrimination.

group. In this case, both time and potential dynamic treatment effects affect the outcome trend in the control. Hence, parallel pre-trends do not imply same time effects in the pre-period, but only in the very special and unlikely case of time effects in the PDA-treated group matching the sum of time and dynamic treatment effects in the PDA-control group. Thus, it is not unjustified to interpret the pre-trends test as usual.

6.1 Descriptive Evidence

Before we discuss our main result, we show descriptive evidence on the trends in employment of fertile-age women in PDA-treated and PDA-control states around 1978. Using the monthly CPS survey, Figure 2 plots the share of female respondents 18 to 35 that are currently employed for the two groups of states separately. We group estimates by quarter to reduce noise.

PDA-treated and PDA-control states display steadily increasing employment levels, as expected given the context, and generally parallel trends throughout this period, with a visible break when the PDA is passed. PDA-treated states, whose shares where constantly above those of PDA-control states before the PDA, experience a sharp decline right after the PDA and then keep trending on parallel but lower levels with respect to PDA-control states. While this evidence is reassuring, we do not interpret it as causal as it does not account for the set of fixed effects and controls that we include in our main DID specification, whose results we discuss next.

6.2 Difference-in-Differences Estimates

Table 2 shows our main DID estimates of the effect of the PDA on the outcomes of interest for fertile-age women (β in equation (5)), using the CPS ASEC data. Column (1) shows that the PDA reduced the likelihood of employment by 4.5 percentage points, a reduction that is significant at the 1% level. This is a sizeable and economically meaningful reduction considering that about 69% of women of childbearing age were employed in the pre-PDA period, amounting to a 6.6% reduction in relative terms. Table 3 shows the analogous estimates based on the PSID data. Also with this data we detect a significant reduction in employment due to the PDA, equal to 8.6 percentage points[37] and 13.1% in relative terms. Such a strong response already suggests that fertility-related concerns are indeed a key driver of gender discrimination.

Next, we examine how the PDA affected hiring, looking at the estimate in columns (2) of the respective tables. While with CPS ASEC we do not find significant evidence of a reduction in hiring, the PSID shows that it did indeed decrease significantly, by a substantial 9.9 percentage points. This discrepancy is not surprising: as explained in section 4.2, our proxy of hiring based on the CPS ASEC can only capture hires between January and March of each year and is thus likely to underestimate changes in hiring. By tracking individuals over multiple years, the PSID captures year-to-year changes in employment status and is

[37] As noted in section 4.2, the PSID has a much smaller sample size, so it is reasonable to expect slightly different and noisier estimates.

thus better suited to measure the effect on this outcome[38]. While this decrease in hires was expected, due to the costs posed by stronger employment protection, the effect on firings is less predictable as compliance with the policy depends on the perceived firing costs after the PDA. Columns (3) of the two tables show that both with the CPS ASEC and with the PSID we do not detect any significant reduction in firing of pregnant workers. With the former survey, we know the precise reason for non-employment and can thus isolate dismissals, with the PSID instead we need to focus on job-loss more broadly. This may explain why the magnitude is larger in the latter. Being also based on a substantially smaller sample, we prefer not to draw conclusions based on this estimate alone.

Since the PDA is a peculiar type of employment protection policies, whose protected group is defined by a temporary condition: pregnancy. And given that this is not foreseeable, especially by employers when hiring, they may discriminate against all women of fertile age, irrespectively of their fertility intentions. For this reason, contrarily to usual employment protection, we expect the 'cost' of this policy to fall outside of the protected class. We test this using PSID data, where we know respondents total fertility. In line with expectations, as columns (3) to (6) of Table A3 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.

We then focus on a second margin of adjustments to the PDA: hourly wages, an outcome that we can only study with the CPS ASEC. To facilitate interpretation, we transform this variable as a z-score. The results, displayed in column (4) of Table 2, show a small and insignificant reduction in wages, equal to -0.02 standard deviations. Overall, employers adjusted primarily on employment, via less hiring, rather than on wages. This finding is consistent with wage frictions limiting this margin of response. In the next section, we test this claim empirically focusing on frictions posed by equal pay requirements.

Finally, we look at whether fertility responded to the PDA. In principle, if women believe that, by providing higher protection, the PDA lowers the costs of having children, fertility should increase. However, both the CPS ASEC and the PSID data show a precise null effect on this outcome (as seen from columns (5) and (4) of Tables 2 and 3, respectively). This could be either due to fertility decisions being exogenous to labor market policies, at least in the short run, or to women realizing that the PDA did not actually strengthen their employment protection and thus change their costs of having children.

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. This signals that the perceived costs of

[38]In Table A2, we report the effects on hiring based on the short panels of the monthly CPS which confirms that the effect was indeed negative and significant

employees' fertility are an important factor in shaping employers' choices and that, raising the costs of them acting upon realized fertility, pushes them to discriminate fertile-age women more broadly at the hiring stage. 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 7 the PDA was enacted, as the threshold $\overline{K}$ was likely not passed.

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

In the previous section, 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 before the PDA, required all employers to pay the same wages for workers employed in the same position, regardless of their gender[39]. Thus, under this Act, any adjustment of female wages of would have necessarily triggered a similar change in wages of male workers employed in the same position and establishment (if any). Clearly, the extent to which this constraint is binding is tightly linked to the gender mix of 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 only by women, then the EPA would place no limits to wage adjustments for that position. We thus expect wage responses to the PDA to be stronger in places where mostly women are employed.

Ideally, we could test this 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.

We compute the share of women employed in each industry in PDA-treated states before the PDA, to avoid capturing endogenous industry sorting in response to the Act, and divide industries in two groups: *(i)* female-dominated industries (share female $\geq$75%), and *(ii)* other industries (share female $<$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

[39]Full text available here.

industries as business and legal services, food stores, telecommunications and manufacturing. We then estimated our main specification, equation (5), 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 employment dummy into a dummy for being employed in the first group of industries and one for being employed in the second group of industries.

Table 4 displays results from this exercise. Consistent with the EPA limiting wage adjustments, we see that the PDA is associated with a stronger and significant decrease in hourly wages in female-dominated industries. Where the EPA posed a weaker constraint, as seen in columns (2) and (5), wages responded to the PDA instead of employment. The effect is indeed insignificant and very small even in relative terms. We observe the opposite in more balanced industries, whose coefficients are shown in columns (3) and (6). Likelihood of employment in these industries decreased by about 3.6%, as opposed to about 8.0% 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.

6.4 Parallel Trends

In the previous sections, we described the changes associated with the enactment of the PDA. To interpret them causally, we require a parallel trends assumption. In this section, we provide evidence that supports this assumption.

We start by looking at the raw trends in employment, showed in section 6.1 for the monthly CPS, for the CPS ASEC and the PSID. Figure A6 confirms what seen before: PDA-treated states were trending on consistently higher employment levels before 1978 and not after. This is especially visible with the CPS ASEC, while the PSID yearly estimates, as expected, are much noisier. With the CPS ASEC data it appears that the relative decline in employment for PDA-treated states comes from them curbing the increasing trends prior to 1978, contrarily to the steep increase witnessed in PDA-control states. This is not surprising, as said before, fertile-age women participation rates were steadily growing in this period (Blau and Kahn, 2017). Nonetheless it is reassuring that the effect does not seem to be driven by the PDA-control states using the monthly CPS or the PSID and, even more, that the gap in employment between PDA-treated and PDA-control states widens after the passage of the states' policies, in 1973, and closes with the PDA in 1978. This is seen extending the time window of the analysis in Figure A7.

Next, we evaluate this more formally including all the controls and fixed effects of our main specification, estimating the event-study equation 6). The results for employment are

shown in Figure 3 and match what presented so far: the PDA generated a strong decline in employment in PDA-treated states relative to PDA-control ones. Additionally, the figures confirm that there is no evidence of significantly different pre-trends with the CPS ASEC, which reassures us about the validity of our identification assumption. The estimated lead coefficients for the PSID are less stable, but at least there does not seem to be a continuing difference in trends around 1978, if anything a reversal. We exploit this occasion to also evaluate the dynamic of the treatment effects. Estimates of lag coefficients with CPS ASEC are not constant, but oscillate around the DID coefficient of -4.5 percentage points, suggesting no clear growing or decreasing dynamic. The PSID estimates instead are very stable in the post period.

Appendix Figures A8 and A9 show the estimated event studies for the other outcomes. Unsurprisingly, yearly coefficients for hiring and firing upon pregnancy are very noisy. Still they do not suggest strong differences in trends before the PDA or systematic patterns of dynamic effects after it. Event-study estimates for hourly wages and pregnancy instead are generally flat, in line with the null aggregate responses of these outcomes.

6.5 Robustness and Mechanisms

We now describe the results of a series of robustness checks that reassure us that our results are indeed explained by fertility-related concerns and are not driven by any idiosyncratic design choices.

Fertility: The mechanism we posit is that the PDA will push employers to anticipate the accommodation costs of employees' fertility and thus reduce their hiring and employment of fertile-age women. If this is true, we would expect employers to discriminate more against women that, based on their observable characteristics, are more likely to have children in the next few years. The first obvious characteristic is age. To test this, we separately estimate the DID specification (5) within each 4-year age bin and plot the estimated effect against the observed fertility rate in each bin. The results are displayed in Figure 4. In line with the theorized mechanism, the negative effect on employment is only detected for those age groups whose fertility rate is positive and effects are stronger when fertility is higher. At the same time, this pattern is not found for men.

Beyond age, a number of observable characteristics predict fertility. For instance, we find evidence of stronger effects for black and less educated women, whose fertility rates at the time were significantly higher[40]. These estimates of heterogeneous effects are shown in

[40]Based on the CPS ASEC data, in PDA-treated states before 1978, black women's fertility rates were 6.1% higher than white women's ones. Fertility rates of women with no college education were 22.0% higher.

Figure A10. We generalize this considering a number of predictors of fertility. Specifically, we leverage the fact that, with the PSID, we can observe respondents' fertility in future years to create a dummy for having a child in the next 1 to 2 years and use LASSO to identify the strongest predictors of this outcome in that period of time, including the covariates of our main specification and various interactions between them. We do this separately for men and women. We then use the estimated coefficients on the set of variables selected by LASSO in the CPS ASEC to predict each respondents probability of having a child in the next 1 to 2 years and divide them in two groups: low predicted fertility, if below median, and high predicted fertility otherwise[41]. We separately estimate the DID coefficients in these two groups, for men and women, and plot the results in Figure A11. Again, estimates are in line with expected fertility driving the effects.

Affected groups: We now document that the effects we presented are indeed driven by the PDA and not by broader contemporaneous differences in trends between PDA-treated and PDA-control states. To do this, we estimate the DID specification for two groups that should not be directly affected by the policy: women above fertile age, 40 to 60, and men, 18 to 60. We plot the estimated coefficients and 95% confidence intervals in Figure A12. It is reassuring that these two groups do not match the pattern of effects that we find for women of fertile age, who are the only ones displaying a significant reduction in employment and in hiring. Thus, their employment rates were not affected by the PDA. Men do show a significant decline in hourly wages, but this poses less of a concern since this was not a main margin of response to the PDA. Overall, these estimates reassure us that our results are not driven by broader differential trends across states. Moreover, they confirm that employers responded by decreasing their employment of women of fertile age, without substituting them, at least to the same extent, with older women or men. As discussed before, this is consistent with occupational segregation and change in skills across generations making these groups imperfect substitutes.

Given that older women and men were not directly affected by the PDA, one could relax the parallel trend assumption imposed by our identification strategy using them as a within-state control group in a triple-DID specification. We describe this specification in detail in Appendix A.3. We do not use this as our baseline specification because, *ex ante* it is not sure that these groups would not be affected indirectly. Nonetheless, we report these estimates in Table A4 to show that indeed the effects we detected are robust to the inclusion of state-by-year fixed effects.

[41] More precise information about this measure and this test can be found in Appendix A.4.

Other components of the PDA: As detailed in section 2.1, the PDA mandated equal treatment in *all* aspects of employment. Justified by the difficulties of enforcing equal treatment on the hiring margin, especially at the time, we claim that this mandate generally meant stronger employment protection for pregnant employees. However, for some employers who, before the PDA, were providing other benefits, such as health insurance or temporary disability insurance (TDI), to temporarily disabled employees but *not* pregnant ones, the PDA also meant extending these benefits to the latter. As mentioned before, the interactions between the PDA and employer's or states' policies regarding these benefits have already been studied: Gruber (1994) focused on health insurance and Timpe (2024) on paid leave via TDI. Extending these benefits generally meant higher costs of employing women of fertile age, decreasing the labor demand, as we showed for employment protection, but could also trigger a positive supply-side response, if workers value these benefits, as in the usual mandated benefits framework. Gruber (1994) indeed shows that the two responses lead to a decrease in wages for this subgroup and no change in their employment levels, while Timpe (2024) shows that, for paid leave via TDI, there was also a decline in employment, driven especially by states that had universal TDI before the PDA[42].

One could be concerned that employers' responses to these costly expansions was so strong that it alone led to the significant decline in employment, while the employment protection component played a minor role. We now show that this was not the case with two exercises. First, we show that employment effects were negative and large both where health and TDI benefits were common and where rare. To do this, we leverage information about the frequency of employer-provided health insurance from the 1976 SIE to rank industries by the share of employees covered by these benefits[43].

In Figure A15a, we decompose the effect on employment across industries grouped by decile of health insurance coverage. If effects were driven simply by these benefits, we should expect them to grow monotonically across deciles. Instead, we observe a noisy but quite uniform effect across these groups of industries and, if anything evidence of null effects among the industries with high benefit availability. When we look at wages, in Figure A15b, we see that there is a more visible pattern: little to no change for the first three deciles and then a growing negative effect, up to more than 0.1 standard deviations. This is in line with the effects identified and described by Gruber (1994) and Timpe (2024). In the particular cases

[42]These states are California, Hawaii, New Jersey, New York, and Rhode Island. They are not part of our analyses both because of the interaction with TDI policies and, in the cases of California and New York, because they adopted different policies regarding employment of pregnant women at various points in our time window, 1973-1984, making it difficult to precisely classify their treatment status.

[43]Timpe (2024) uses this measure to construct a proxy of TDI availability, relying on the assumption that availability of TDI correlates positively with that of health insurance, but is rarer. Following this argument, our measure also ranks industries by the share of employees covered by TDI.

in which the PDA expanded health and TDI benefits for many pregnant workers, women of fertile age adjusted their labor supply and, in the new equilibrium, had lower wages but similar or slightly lower employment levels. In general, however, the uniform decline in employment signals that it was the more pervasive, employment protection, component of the PDA that drove the effects we captured and described so far.

As a second check, we exploit some variation in the policies adopted by the PDA-control states. Specifically, Massachusetts was the only state that, before the PDA, mandated employment protection for pregnant workers without also requiring equal treatment in all other aspects of employment. Thus, in this state, employer-provided health and TDI benefits were expanded in 1978 as in PDA-treated states, while employment protection was expanded in 1972. We then replicate our estimates of equation 5 using only Massachusetts as a control state and the same set of PDA-treated states. Using the 1976 SIE, we first confirm that benefits were similarly frequent in Massachusetts, where 44.5% of respondents had employer-provided health insurance, and in PDA-treated states, where 43.4% had it. Unless expansion of these benefits has strongly heterogeneous effects across states, this DID would isolate the effect of the PDA that is due to employment protection and not to these other components. Estimates, reported in Table A5, show that the effect on employment and hiring is similar to our baseline estimates, although noisier because we now have only one state in the control group.

Overall, these results confirm that the broader labor market effects of the PDA were indeed driven by employers responding to the newly imposed firing costs for pregnant workers. However, one should not discount the important role played by adjustments also from the supply side, in the instances where the PDA also provided substantial access to health and TDI benefits, as documented by Gruber (1994) and Timpe (2024).

Additional outcomes: We complement the evidence shown in section 6.2 considering additional outcomes that could have been affected by the PDA. Estimates, reported in Table A6 columns (1) to (3), show the PDA not only reduced fertile-age women's likelihood of being employed, but also shifted them more into part-time occupations. Consequently, we observe significant reductions on the number of weekly hours, but not on the number of weeks employed, conditional on employment. It thus appear that the PDA also affected employment on the intensive margin. To the extent that accommodation costs for full-time positions are generally higher, these patterns are consistent with the mechanisms outlined by our theoretical framework in section 3. Next, as a robustness check we document in column (4) that very similar, or even larger, effects are found if we use employment status in the week preceding the March CPS survey, instead of the status in the year preceding the survey.

Additionally, we do not detect significant effects on a proxy for quitting after childbirth[44] and on marriage rates, respectively in column (5) and (6).

Treated and control states: To show that our findings are not driven by idiosyncratic design choices, we first document that the estimated negative effect on employment is not driven by any particular state, or state-group with the CPS. Figure A13 shows that the estimated coefficient is remarkably robust, both with the CPS ASEC and the PSID, to dropping each state from the sample, one at a time. Next we show that the effects are similar if we change the inclusion criteria for the quasi-experiment. With the PSID, where states are separately identified, we replicate our analyses using all possible PDA-treated and PDA-control states, including in the latter also those treated until 1975, to increase the number of control states. The new set of states is shown in Figure A14. We report the estimates in columns (1) and (2) of Table A7, which are indeed robust to this alternative definition of the quasi-experiment.

Additional robustness checks: We conclude this section by showing that our effects are robust to controlling for contemporaneous policies that may affect employment of fertile-age women, to allowing for selective migration, and to an alternative identification strategy that does not rely on using early-treated states as control. To deal with the first concern, we add data on state variation in Head Start grants (from Bailey, Sun, and Timpe, 2021), in enactment of job-protection policies via exception to the employment-at-will doctrine (from Autor, 2003), and in adoption of unilateral divorce laws (UDL) (from Voena, 2015). We estimate specification (5) controlling for each of these policies and plotting the resulting DID coefficient in Figure A16. Our estimates are robust to the inclusion of these controls.

To allow for selective migration, we rely on the PSID asking respondents their state of residence during childhood. For our sample of women 18 to 35, this is thus orthogonal to our policy variation. We then replicate our main estimates defining treatment based on this variable instead of the current state of residence. Results, displayed in columns (3) and (4) of Table A7, show that estimated effects remain negative, albeit smaller and noisier. This is not surprising given that childhood state of residence is a noisy measure of state of residence at the time of treatment and random noise in our treatment indicator would exactly bias the coefficient downward and reduce estimates' precision.

Finally, our identification strategy requires us to assume that the early-treated PDA-

[44]Similar to our proxy of firing after childbirth, we use CPS questions regarding reasons for not being employed in the week preceding the survey. Also in this case, this proxy will capture only a fraction of the actual job separation events that happen throughout the year. This explains that relatively low mean for outcome that was not infrequent at the time.

control states had no dynamic treatment effects, at least after 1973. To reduce concerns that our results are biased due to a violation of this assumption, we show that strong negative employment effects are found even without using the early-treated states as a control group. We exploit the fact that only women that where *at risk* of pregnancy were actually affected by the policy, as documented above. Using the 1960 to 1980 waves of the census, we estimate a difference-in-differences specification comparing, within states, the likelihood of employment for women with high *versus* low predicted fertility before and after 1978. As before, we estimate predicted fertility using the prediction model ran on the PSID data, and plugging in the coefficients using the covariates' values from the census. We do this using our main sample of interest, women age 18 to 35[45]. The exact specification is described in Appendix A.4 and the results are displayed in Table A8. Estimates, shown separately for PDA-treated, PDA-control, and all states, support our conclusions: the PDA had strong negative effects on the employment levels of women who, based on observables, were more likely to have children in the near future, and whose employment became more costly because of the PDA.

7 Quantifying Fertility-Related Discrimination

In section 6.2, we documented strong reductions in the probability of being employed and hired for fertile-age women after the enactment of the PDA. These results already suggest that fertility-related concerns must be a relevant factor in shaping employers' decisions and discriminatory behaviors. Thus, a key driver of gender discrimination. To attempt to measure the *relative* importance of this determinant, in this section, we conduct a simple back-of-the-envelope exercise. The logic behind this exercise is that the PDA, by first imposing costs on the choice of employers to fire workers upon pregnancy, forces them to anticipate these costs and change their hiring decisions. The effect of the PDA on fertile-age women employment probability thus captures the reduction in employment that is due to fertility-related concerns from the employers. By benchmarking this effect against the gender employment gap at the time, which proxies for the overall amount of gender discrimination, we obtain an estimate of the role of expected fertility in driving gender discrimination.

We thus start by estimating the gender employment gap that provides the correct comparison for our estimated effects. Since these are treatment on the treated estimates, we compute the gender employment gap restricting the sample to CPS ASEC respondents in PDA-treated states, and after the PDA is enacted. Moreover, to parse out the gap from

[45]If we used all women age 18 to 60, we would end up capturing not the effect of the PDA but rather the broader upward trend in employment of women of childbearing age that characterized the 1960s to 1980s period. This pattern is visible in Figure A1a and has been documented extensively in the literature (Blau, 2025; Goldin, 2024).

differences in observed determinants of employment, we control for the same individual-level covariates that we include in our main specification (5). The gap is then estimated regressing a dummy for being employed on a female indicator and this vector of controls. Restricting the sample to individuals 18 to 35, as we do for our main estimates. Our regression yields a gender employment gap among fertile-age individuals of 15.7 percentage points, which we contrast with our estimated –4.6 percentage points effect of the PDA on fertile-age women's employment. We plot these estimates in the first bar of Figure 5. This comparison suggests that employers' fertility-related discrimination accounts for 29.3% of the gender employment gap following the PDA.

Building on the aggregate back-of-the-envelope results, we next explore how this discrimination varies between mothers and non-mothers. Employers reviewing applications from current mothers may already infer their post-childbirth labor-force attachment, whereas the impacts of future pregnancies remain more ambiguous for non-mothers. This likely increases the importance of fertility-related concerns for non-mothers. Moreover, every pregnancy entails short term costs—leave, temporary replacements, and accommodations—while only a first birth gives rise to lasting expectations of reduced long-term commitment and family-care needs. Under the PDA, firms can no longer sidestep these costs: they must absorb the short-term costs for all hires and, for non-mothers, the long-term costs as well, thereby raising the relative cost of hiring non-mothers. To examine this heterogeneity, we replicate the back-of-the-envelope calculation separately for respondents with and without children at the time of the survey. The results, displayed in the latter two bars of Figure 5, confirm our expectations. While the gender employment gap is substantially larger for mothers and smaller for non-mothers—reflecting the high exit rates of mothers after childbirth at the time—a markedly greater share of the gap among non-mothers is attributable to expected-fertility concerns. Specifically, 17.9% of the gap for fertile-age mothers and a striking 78.1% for non-mothers can be explained by the PDA response. These findings align with Gallen (2023), who shows that productivity differentials account for only a small portion of the gender *wage* gap among non-mothers, but a much larger share for mothers. Our quasi-experimental design, by raising employers' costs of conditioning employment on realized fertility, thus provides causal evidence that, at that time, concerns over expected future fertility—rather than actual productivity—drove most of the gender employment gap among non-mothers.

8 Conclusion

In this paper, we exploit a unique policy experiment—the introduction of the Pregnancy Discrimination Act of 1978—to assess the extent to which employers' fertility-related concerns

contribute to gender discrimination in employment. By isolating the impact of the PDA on hiring and firing decisions, we both evaluate the law's labor-market effects and, critically, use it to quantify the role of expected fertility in shaping gender gaps.

Conceptually, 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 lower employment among fertile-age 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 decline in employment 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.

Furthermore, 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.

Finally, our back-of-the-envelope exercise then quantifies the quantitative importance of expected-fertility discrimination: the 4.6-point employment decline for fertile-age women represents approximately 29.3 percent of the contemporaneous 15.7-point overall gender employment gap, and fully 78.1 percent of the much smaller 3.2-point gap faced by childless women. These findings demonstrate that anticipated fertility costs are a central driver of gender disparities, and that prohibiting pregnancy discrimination alone—absent robust enforcement or broader measures to address employers' fertility-cost expectations—is unlikely to close the gender employment gap.

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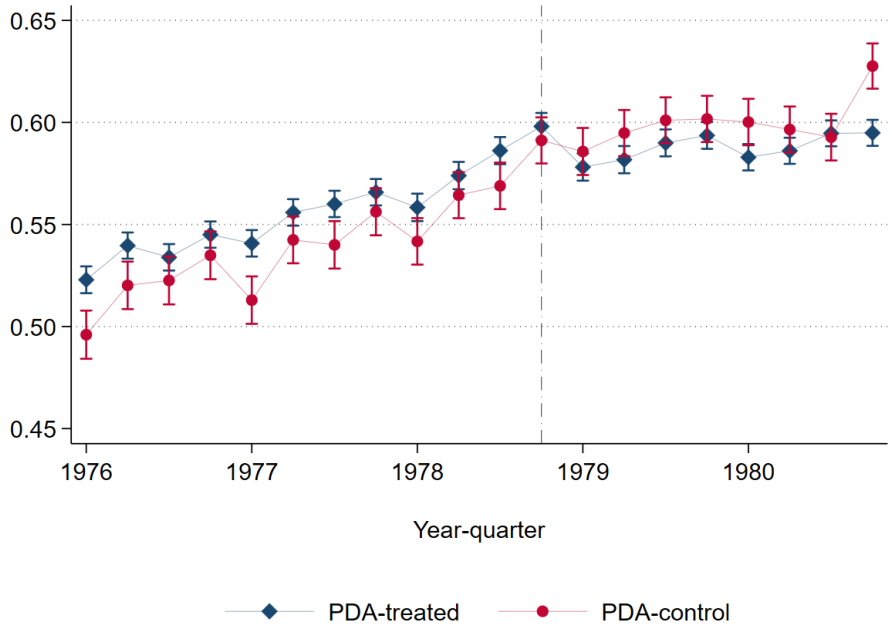
Main Figures & Tables

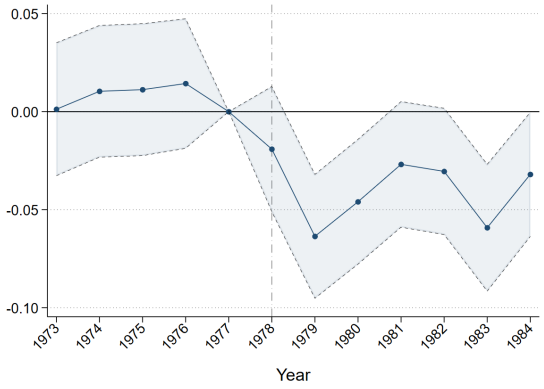
Figure 2: Raw Trends in Employment



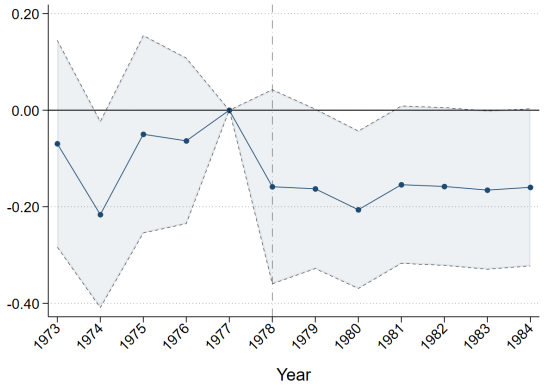
Notes: The sample is restricted to women of age 18 to 35 in PDA-treated and PDA-control states between 1976 and 1980, from the basic monthly CPS. Plotted are the average values of the dummy for being employed for each group of states in the year indicated on the x-axis. Estimates are computed using CPS individual sample weights. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure 3: Event-Study Estimates: Employment

(a) CPS ASEC



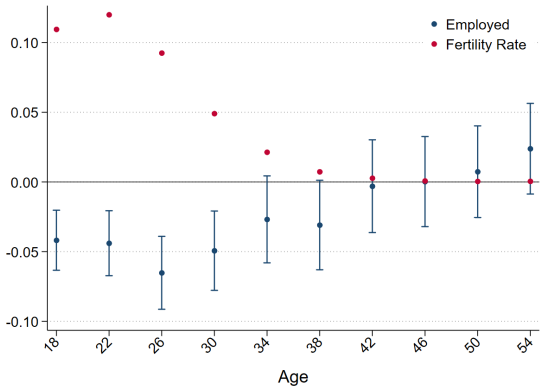
(b) PSID



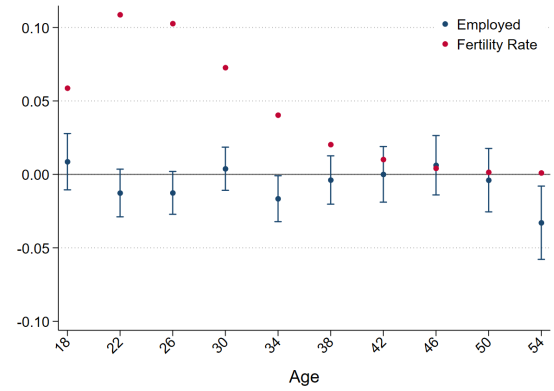
Notes: The sample consists of all female respondents of age 18 to 35 in PDA-treated and PDA-control states, between 1973 and 1984 from CPS ASEC, in panel (a), and PSID, in panel (b). Estimates are computed using each survey's individual sample weights. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure 4: Effect on Employment and Fertility Rate by Age Group, CPS ASEC data

(a) Women

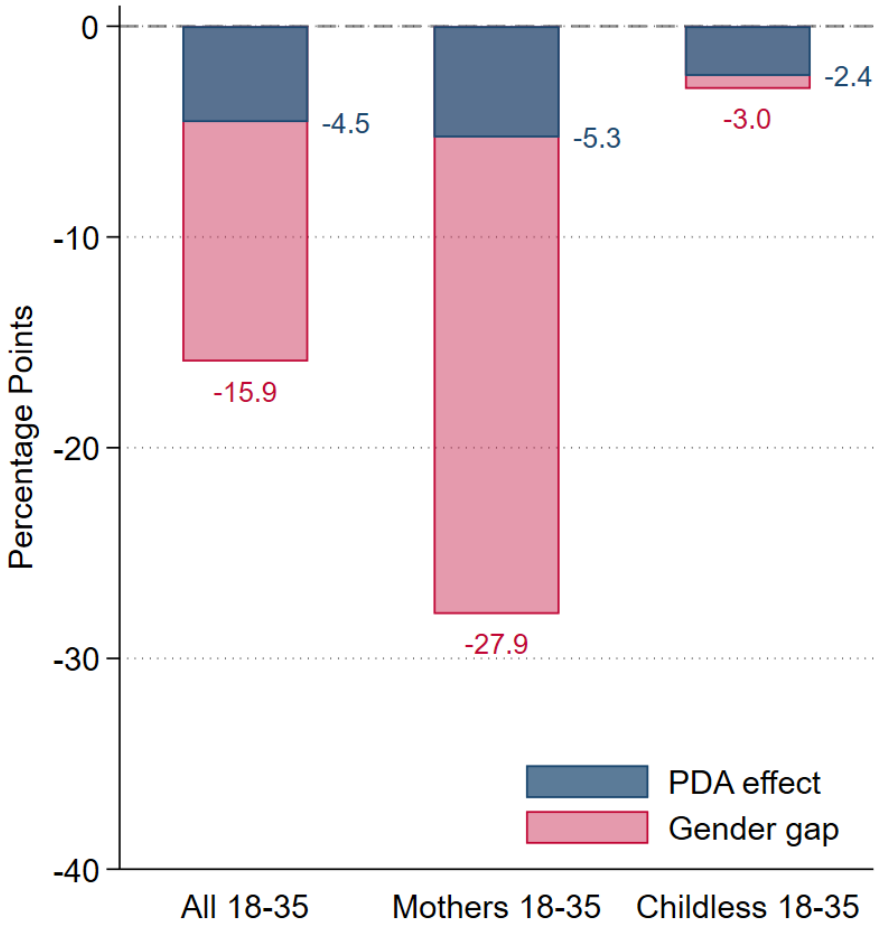


(b) Men



Notes: The sample is restricted to individuals of age 18 to 60 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC. Estimates are computed using ASEC individual sample weights. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. The fertility rate is the average share of respondents having a child at that age. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure 5: Effects on Employment and Gender Employment Gaps



Notes: The sample is restricted to women of age 18 to 35. The effect of the PDA on employment probability (in blue) is estimated using CPS ASEC data, restricted to respondents in PDA-treated and PDA-control states between 1973 and 1984, using specification (5). The gender employment gap was estimated using CPS ASEC data, restricted to respondents in PDA-treated states between 1979 and 1984. It was estimated regressing a dummy for being employed on a dummy for gender, including a vector of individual level covariates (age, age squared, education, ethnicity, marriage, metro-area status). In the second bar, both the effect of the PDA and the gender employment gap were estimated restricting the sample to respondents with at least one child already born at the time of the survey. In the third bar, to those with no child born at the time of the survey.

Table 1: Summary Statistics, CPS ASEC and PSID data

	1973-1978				1979-1984			
Variable	Mean	Std.Dev.	Med.	Obs.	Mean	Std.Dev.	Med.	Obs.
Panel A. CPS ASEC								
Education: Primary	0.99	0.09	1	54625	0.99	0.079	1	60592
Education: High School	0.79	0.41	1	54625	0.83	0.37	1	60592
Education: College	0.32	0.47	0	54625	0.38	0.48	0	60592
White	0.84	0.36	1	54625	0.83	0.38	1	60592
Black	0.15	0.35	0	54625	0.16	0.37	0	60592
Married	0.65	0.48	1	54625	0.58	0.49	1	60592
Metropolitan area resident	0.38	0.49	0	54625	0.39	0.49	0	60592
Employed	0.69	0.46	1	54625	0.74	0.44	1	60592
Employed: Private	0.54	0.5	1	54625	0.59	0.49	1	60592
Employed: Government	0.13	0.33	0	54625	0.12	0.32	0	60592
Employed: Self	0.019	0.14	0	54625	0.026	0.16	0	60592
Weeks employed	38.5	16.6	49	26157	40.3	16	52	44592
Weekly hours	35.3	10.6	40	26135	35.3	10.7	40	44564
Annual wage income	3255.5	3930.7	1603	54625	6009.3	6905.4	4000	60592
Hourly wage income	3.69	3.04	3.21	25226	5.5	3.91	4.79	43148
Pregnant	0.09	0.29	0	54625	0.085	0.28	0	60592
Panel B. PSID								
Education: Primary	1	0.054	1	7433	1	0.025	1	11810
Education: High School	0.76	0.43	1	7433	0.8	0.4	1	11810
Education: College	0.28	0.45	0	7433	0.34	0.48	0	11810
White	0.73	0.45	1	9945	0.73	0.44	1	11961
Black	0.14	0.34	0	9945	0.17	0.38	0	11961
Married	0.74	0.44	1	9945	0.69	0.46	1	11961
Employed	0.67	0.47	1	2716	0.6	0.49	1	11859
Annual wage income	1186.6	2946.6	0	9945	5119.7	6620.8	1300	10937
Pregnant	0.081	0.27	0	9945	0.088	0.28	0	11961

Notes: The sample is restricted to observations in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC (panel a) and PSID (panel b). All statistics are computed using 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 with the CPS ASEC and as having given birth in the current year with the PSID.

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

Dep. Variable	(1) Employed	(2) Hired	(3) Pregnant & Fired	(4) Hourly Wage	(5) Pregnant
PDA-treated x Post-PDA	-0.045***	-0.001	0.004	-0.021	0.000
	(0.007)	(0.007)	(0.004)	(0.018)	(0.004)
Observations	115,217	34,395	10,441	68,374	115,217
Pre-PDA Mean	0.686	0.0873	0.00440	3.549	0.0901
R-squared	0.073	0.016	0.007	0.154	0.045
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: The sample is restricted to female respondents age 18 to 35 in PDA-treated and PDA-control 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: Difference-in-Differences Estimates, PSID data

	(1) Employed	(2) Hired	(3) Pregnant & Lost Job	(4) Pregnant
PDA-treated x Post-PDA	-0.086***	-0.099**	-0.122	-0.005
	(0.030)	(0.042)	(0.191)	(0.011)
Observations	14,479	8,639	1,289	20,250
Pre-PDA Mean	0.659	0.533	0.261	0.0815
R-squared	0.113	0.115	0.084	0.035
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: The sample is restricted to female respondents age 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from the PSID. Estimates are computed using 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 year, and 0 if not employed in the current year. Lost Job is a dummy equal to 1 if an individual is not employed in the current year and was employed in the previous one, 0 if still employed in the current year. Pregnancy is defined as a dummy for having a child born in the current year. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

	Hourly Wage in Industry			Employed in Industry		
	All	$\geq 75\%$	$< 75\%$	All	$\geq 75\%$	$< 75\%$
	(1)	(2)	(3)	(4)	(5)	(6)
PDA-treated x Post-PDA	-0.021	-0.060**	-0.001	-0.045***	-0.008	-0.037***
	(0.018)	(0.030)	(0.023)	(0.007)	(0.007)	(0.008)
Observations	68,374	22,442	45,921	115,217	115,217	115,217
Pre-PDA Mean	3.549	3.191	3.725	0.686	0.221	0.465
R-squared	0.154	0.154	0.151	0.073	0.012	0.048
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 PDA-treated and PDA-control 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 (5) 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. The hourly wage is expressed as a z-score. The hourly wage 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 Model derivations

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

$$\Pi^n = \frac{v}{m(\theta)} \tag{7}$$

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

$$r\Pi^n = y - w + q(\max\{\Pi^k, -K\} - \Pi^n) - \delta\Pi^n \tag{8}$$

and

$$r\Pi^k = y - w - c + \mu(\Pi^n - \Pi^k) - \delta\Pi^k \tag{9}$$

We make the following assumption.

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

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

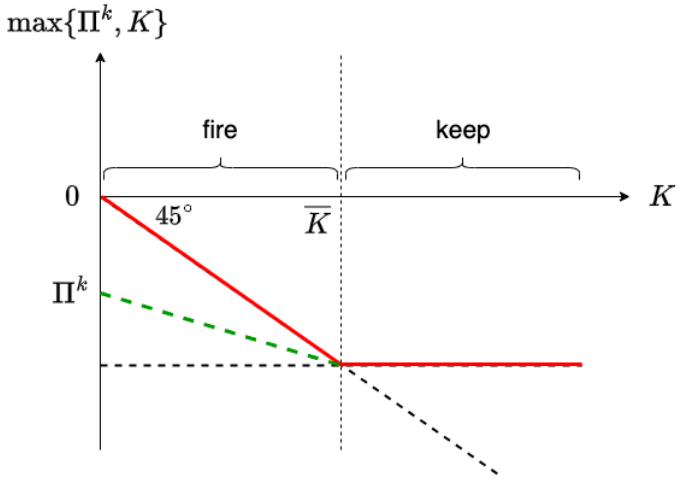
After some calculations we get

$$\overline{K} = \frac{c(q+r+\delta) - (y-w)(q+r+\delta+\mu)}{(r+\delta)(q+r+\delta+\mu)} \tag{10}$$

which is positive under Assumption 1.

We can illustrate how the employer's decision changes with K as follows:

Figure 6: 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 $K < \overline{K}$

$$\theta_f = \left(\frac{y - w - qK}{v(q + r + \delta)} \right)^{\frac{1}{1-\gamma}} \tag{11}$$

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

$$\theta_k = \left(\frac{(y - w)(q + r + \delta + \mu) - cq}{v(r + \delta)(q + r + \delta + \mu)} \right)^{\frac{1}{1-\gamma}} \tag{12}$$

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[46]. We need

[46]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

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

to track the evolution of four groups: non-pregnant employed workers, L^n, pregnant employed workers, L^c, non-pregnant unemployed workers, U^n, pregnant unemployed workers, U^c. Of course,

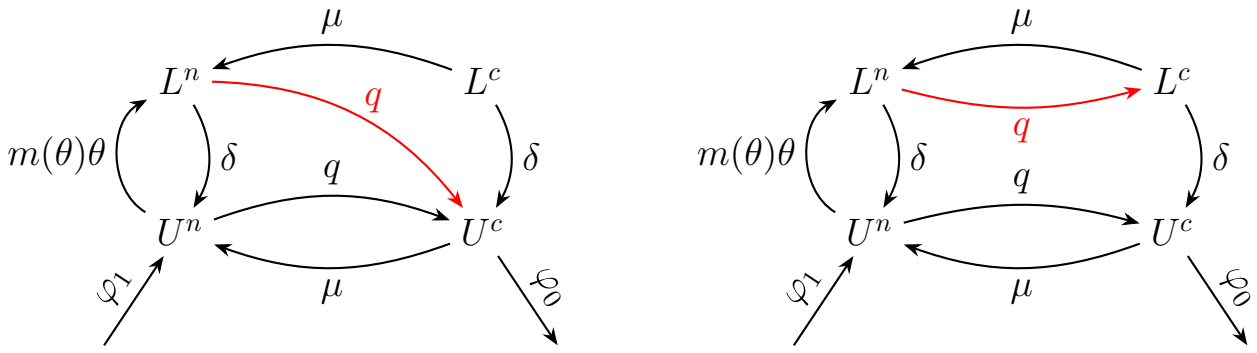
$$L^n + L^c + U^n + U^c = N$$

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

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

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 7a.

Figure 7: 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 rate

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 (13).

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

$$u_f = \frac{q + \frac{(n+q+\delta)(n+\mu+\phi)}{n+q+\delta+\theta_f^\gamma}}{n+q+\mu+\phi} \tag{14}$$

Note that, since θ_f, characterized in equation (11) 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

$$\begin{cases} \dot{l}^n + l^n n &= m(\theta)\theta u^n + \mu l^c - (q+\delta) l^n \\ \dot{l}^c + l^c n &= q l^n - (\mu+\delta) l^c \\ \dot{u}^n + u^n n &= \delta l^n + (\mu+\varphi) u^c + n - (m(\theta)\theta + q) u^n \\ \dot{u}^c + u^c n &= q u^n + \delta l^c - (\mu+\varphi) u^c \end{cases} \tag{15}$$

The flows are described in Figure 7b.

System (15) 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 (12) is constant in K, in this region further increases of K have no effect on the unemployment rate.

The last case—where the policy raises the firing cost from below to above the threshold—is analyzed in the main body (see Figure 1).

A.2 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 PDA-treated 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[47], 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

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

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 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 (11), we get

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

Substituting this into (14) we obtain the unemployment rate in terms of fundamentals and can match it to 0.091, which is the female unemployment rate. This yields a wage rate of $w \approx 0.98$.

A.3 Triple Difference Estimator

As said in section 5, we augment the baseline DID specification of equation 5 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. Indeed, as documented in section 6.5, we do not detect signifcant employment effects for these groups. 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 (5) estimating

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

where $\eta_{s,t}$ are state-by-year fixed effects and all other terms are as described above. Notice that the interaction between the PDA-treated 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 PDA-treated and PDA-control 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.

A.4 Predicted Fertility

To confirm that the results are indeed driven by the PDA increasing the expected costs of employees' fertility, triggering a negative labor demand response, we show that the effects are much stronger for women who are more likely to become pregnant, based on their observable characteristics. We do this by estimating a prediction model, with future realized fertility as outcome, and isolating the variables that result as strongest predictors of it.

Specifically, we take advantage of the panel structure of the PSID, where we know individuals' realized fertility at the time of the survey but also in future years. We thus define a dummy variable that takes value 1 if the respondents will have children in the next few years[48] and 0 otherwise. We define this variable both for women and men. Next, we fit a penalized logit model using a LASSO estimator, regressing this outcome on many potential predictors of future fertility. The included regressors are a polynomial in age, flexibly interacted with a dummy for age below 40, race dummies, Census region dummies, dummies for being married, for the number of children and for educational achievement, years since the most recent childbirth and the age at first childbirth. Additionally, we interact age with dummies for being married, being childless, and race dummies. Finally, we also interact race dummies with indicators for being married and being childless. The LASSO model selected

[48]We define this as in the next 2 years, but results are robust to alternative time windows.

the following variables for women: age, age below 40, the dummy for having one child, age at first childbirth, years since the most recent childbirth, the dummy for achieving college education, and the interaction between being married and white. For men, age, age below 40, age squared, the dummy for having one child, age at first childbirth, years since the most recent childbirth, the dummy for being married, and the interaction between being married and white. Finally, we used the estimated coefficient for these variables to predict future fertility for men and women in the CPS and Census data by simply multiplying them by the values of the respective variables for each individual in these datasets. This way, we defined having *High Predicted Fertility* as having an above-median predicted probability of having children in the next few years.

The estimates displayed in Figure A11 are based on the following augmented version of equation 5:

$$y_{i,s,t} = \beta_1 T_s \times P_t \times HighPF_i + \beta_2 T_s \times P_t \times (1 - HighPF_i) + \rho HighPF_i \tag{17}$$

$$+ \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t} \tag{18}$$

where $HighPF_i$ is the indicator for having high predicted fertility, constructed as described above, and the remaining variables are defined as in the rest of the analyses.

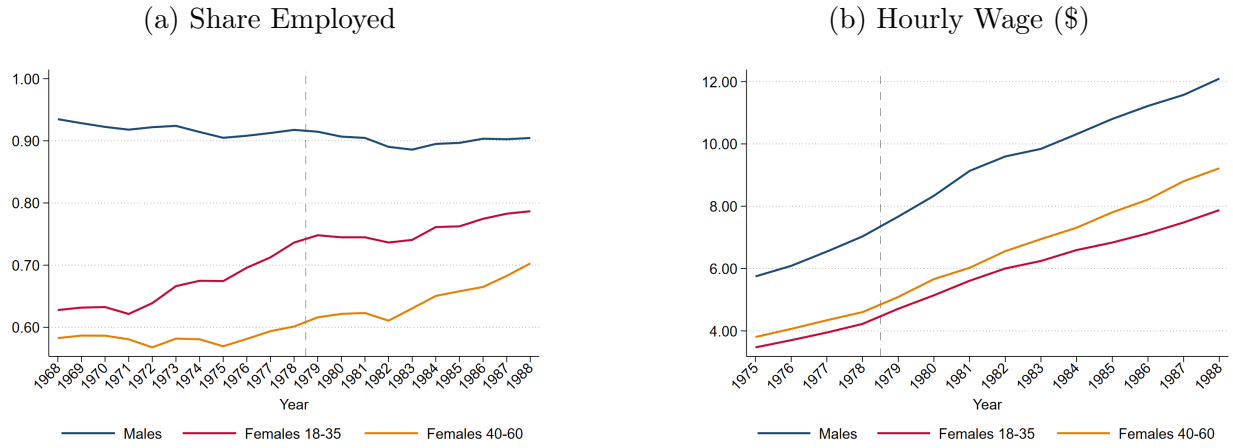
Instead, the estimates reported in Table A8, are based on the following specification:

$$y_{i,s,t} = \beta P_t \times HighPF_i + \rho HighPF_i + \Gamma X_{i,s,t} + \theta_s + \delta_t + \varepsilon_{i,s,t} \tag{19}$$

with terms defined as above. As this strategy purposely avoids making across-state comparisons, it only includes an interaction of the post-PDA dummy with the high predicted fertility dummy.

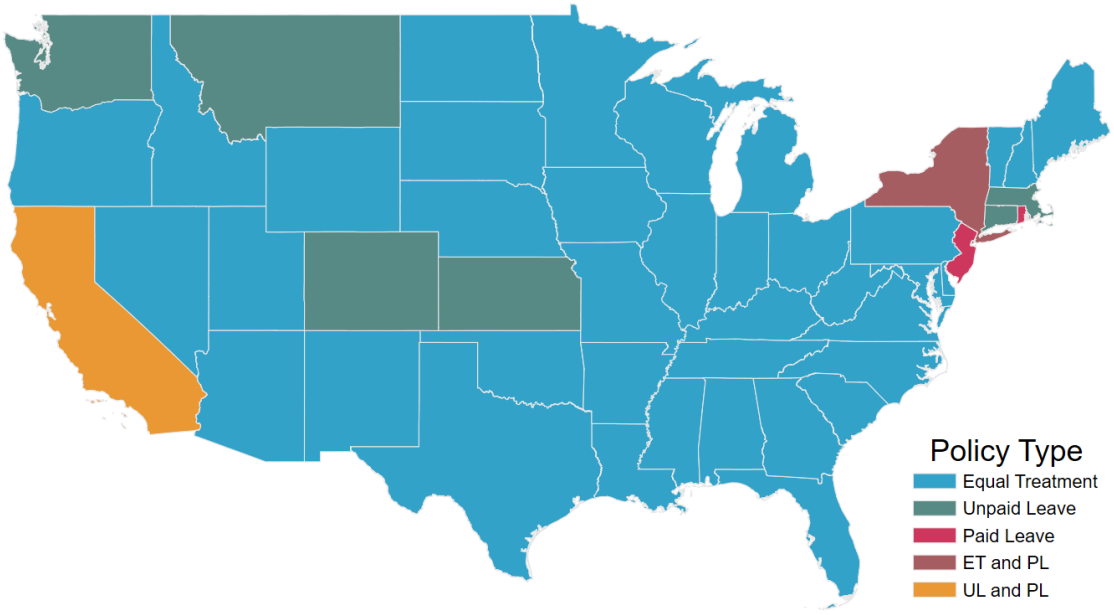
A.5 Additional Figures

Figure A1: National Trends around 1978



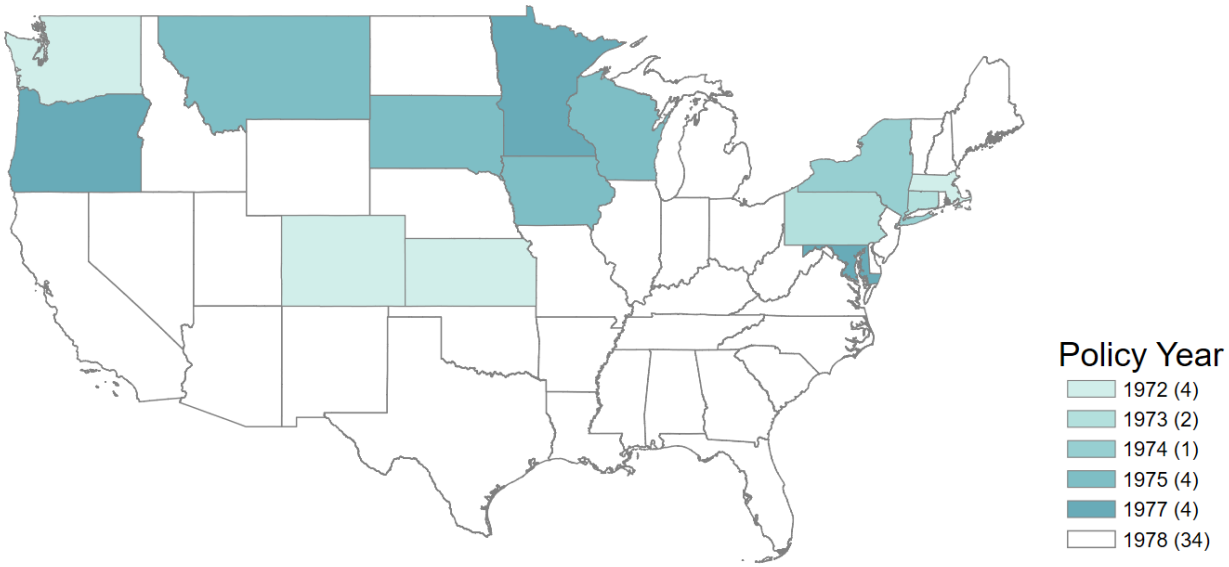
Notes: The sample consists of all CPS ASEC respondents of age 18 to 60. Estimates are computed using ASEC individual sample weights. Wage income is measured in US dollars and is pre-tax. Hourly wage income is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working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

Figure A2: Type of Policy Adopted



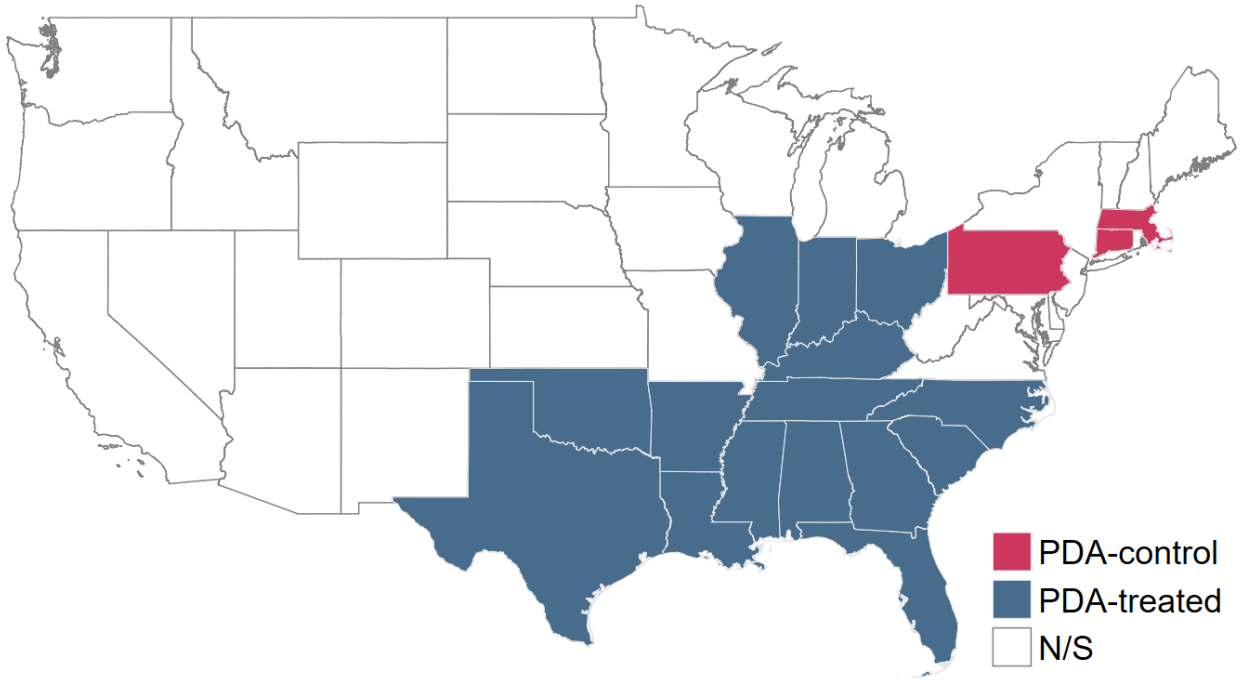
Notes: This map displays the type of policy adopted. States that adopted an equal treatment policy include also all states that adopted it only when the PDA was passed in 1978.

Figure A3: Timing of Pregnancy Discrimination Legislation



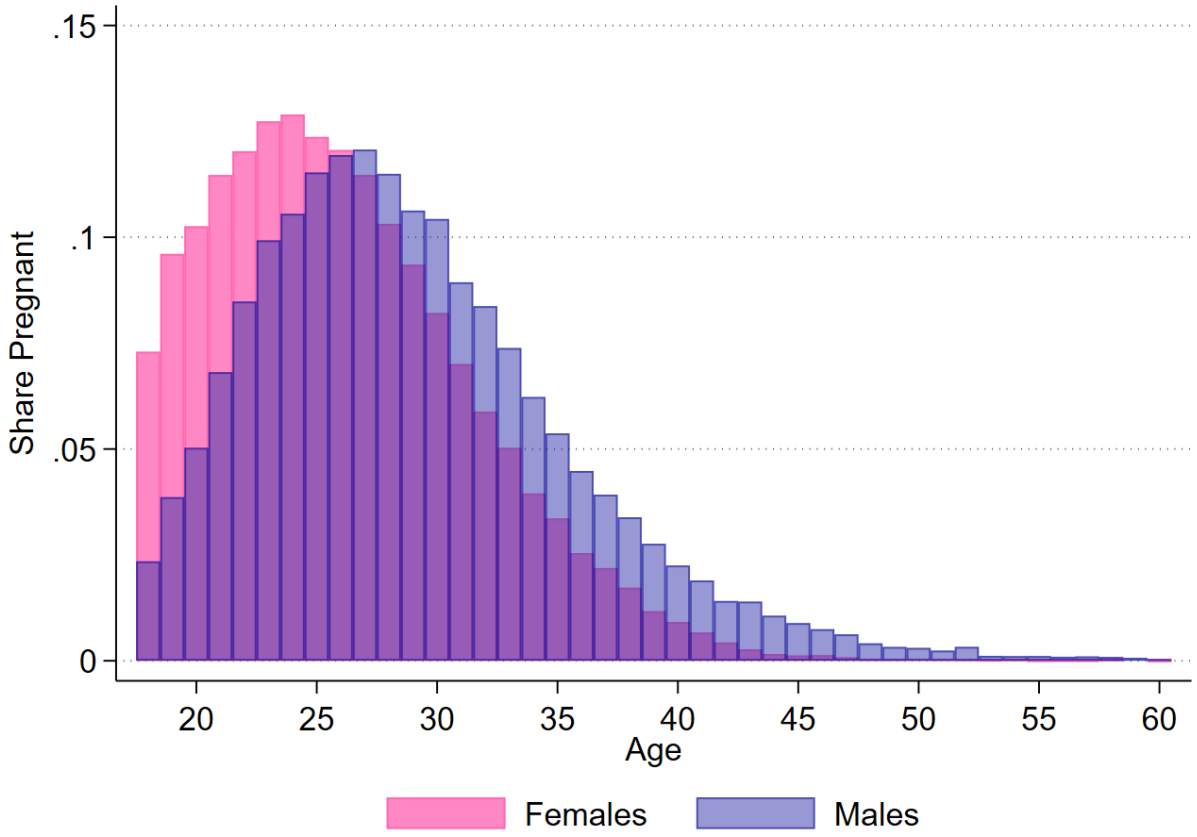
Notes: This map displays the year of passage of each state's pregnancy-discrimination policy. States that only enacted such a policy in 1978, with the passage of the PDA at the federal level are displayed in white. Michigan passed a state pregnancy-discrimination law in 1978, it is also displayed in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

Figure A4: PDA-treated and PDA-control States, Main Analysis



Notes: This map displays the set of PDA-treated and PDA-control states used in the main analyses. N/S stands for 'not in sample'.

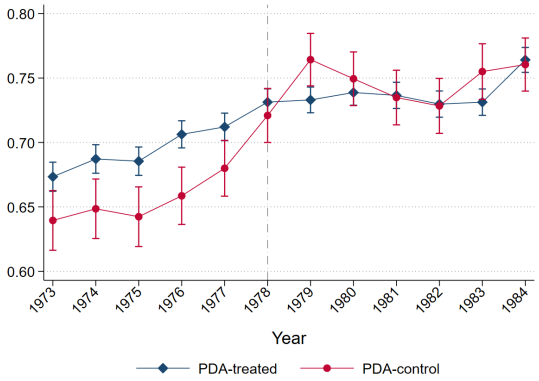
Figure A5: Share Pregnant by Age



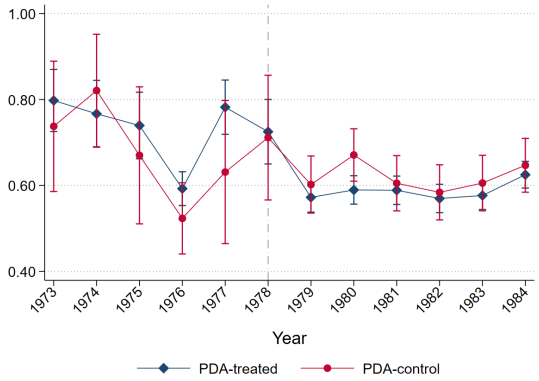
Notes: The sample consists of all CPS ASEC respondents of age 18 to 60, between 1973 and 1984. Estimates are computed using ASEC individual sample weights. Pregnancy for male and female respondents is defined as reporting having a child age 0 in March of the following year. For males the value is thus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

Figure A6: Raw Trends in Employment

(a) CPS ASEC



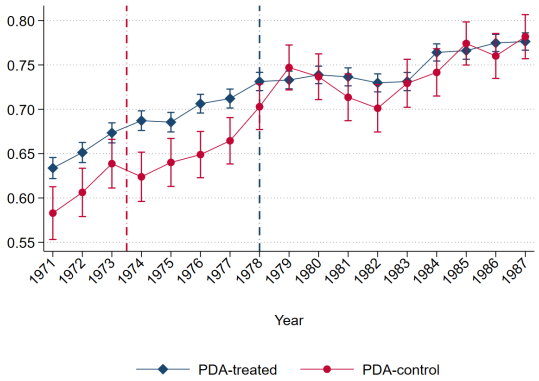
(b) PSID



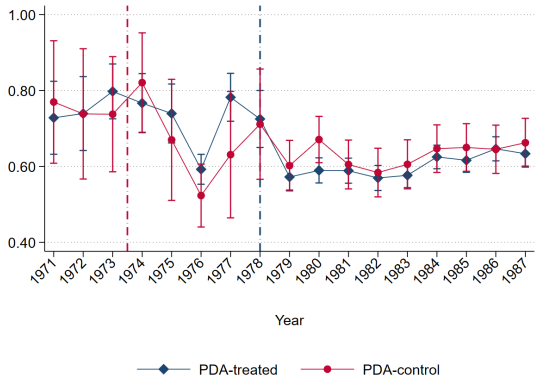
Notes: The sample consists of all female respondents of age 18 to 35 in PDA-treated and PDA-control states, between 1973 and 1984 from CPS ASEC, in panel (a), and PSID, in panel (b). Estimates are computed using each survey's individual sample weights. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure A7: Raw Trends in Employment, Extended Time Window

(a) CPS ASEC



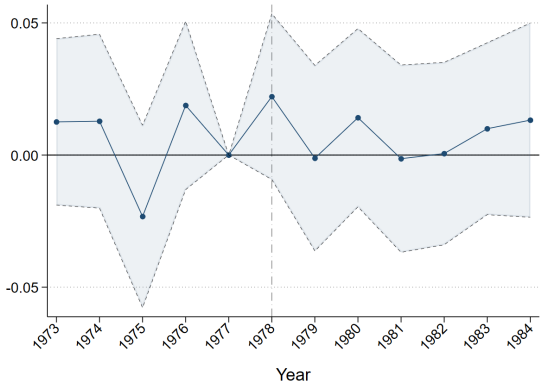
(b) PSID



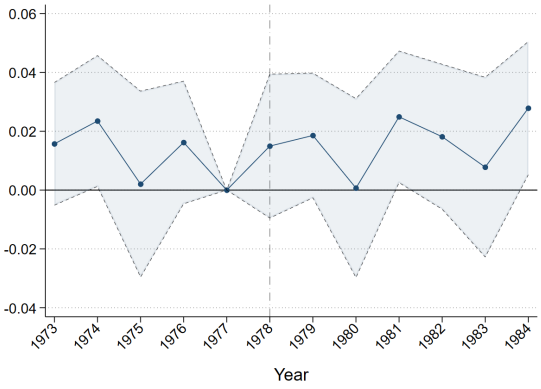
Notes: The sample consists of all female respondents of age 18 to 35, between 1971 and 1987 from CPS ASEC, in panel (a), and PSID, in panel (b). Estimates are computed using each survey's individual sample weights. The red (blue) vertical dashed line indicates the time at which PDA-control (PDA-treated)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

Figure A8: Event-Study Estimates, CPS ASEC data

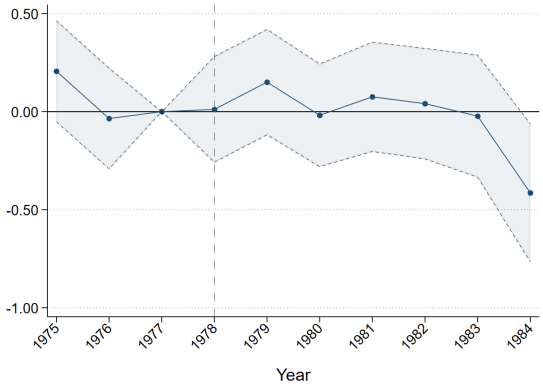
(a) Share Hired



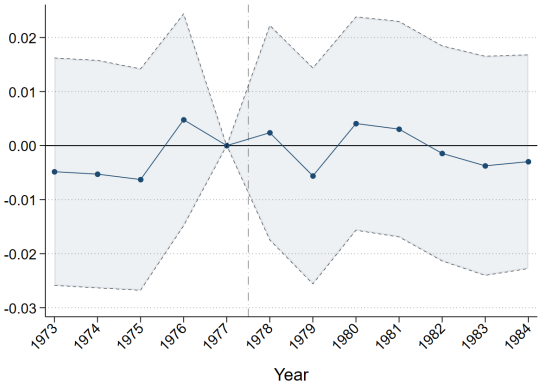
(b) Share Fired Among Pregnant



(c) Hourly Wage ($)



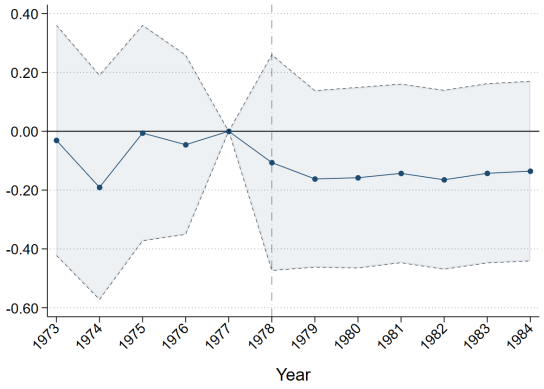
(d) Share Pregnant



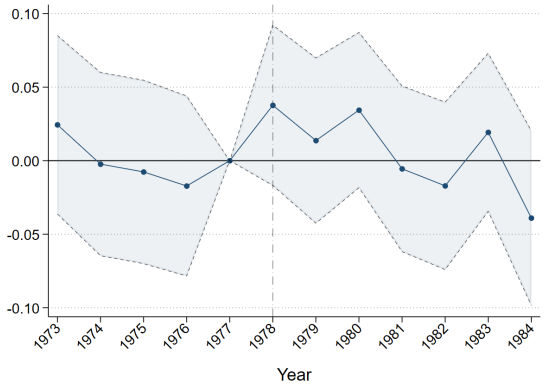
Notes: The sample is restricted to women of age 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC. Plotted are estimates of the β_l coefficients in specification (6). Estimates are computed using ASEC individual sample weights. Hired is a dummy equal to 1 if the 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. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure A9: Event-Study Estimates, PSID data

(a) Share Hired



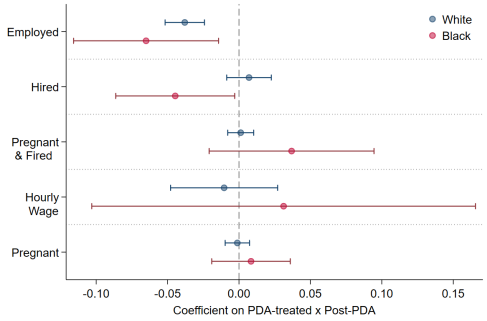
(b) Share Pregnant



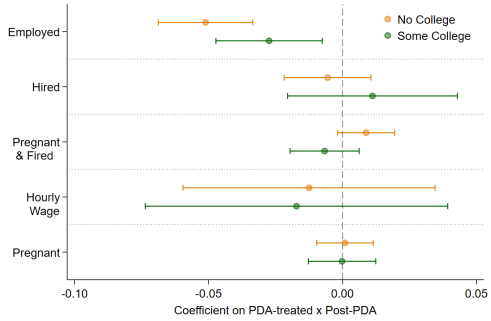
Notes: The sample is restricted to women of age 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from the PSID. Plotted are estimates of the β_l coefficients in specification (6). Estimates are computed using individual sample weights. Employed is a dummy equal to 1 if the respondent 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 year, and 0 if not employed in the current year. Pregnancy is defined as a dummy for having a child born in the current year. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure A10: Heterogeneous Effects, CPS ASEC data

(a) Effects by Race

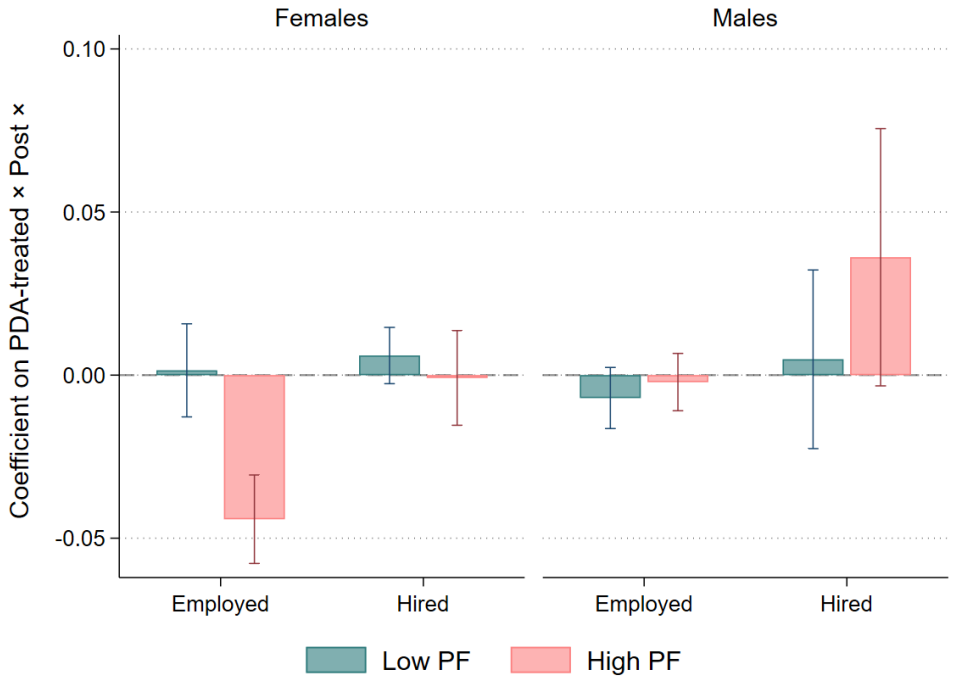


(b) Effects by Education



Notes: The sample is restricted to women of age 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC. Estimates are computed using ASEC individual sample weights. 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. Estimates are computed estimating equation (5) restricting the sample to the set of individuals described in the figures' labels. 95% confidence intervals are based on heteroskedasticity robust standard errors.

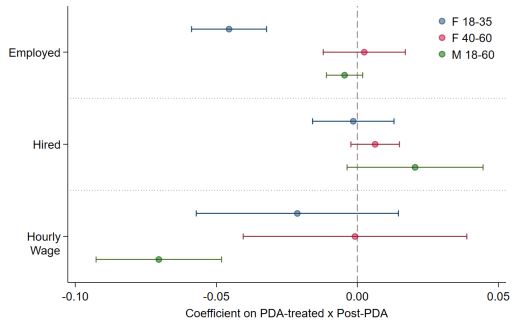
Figure A11: Effects by Predicted Fertility, CPS ASEC



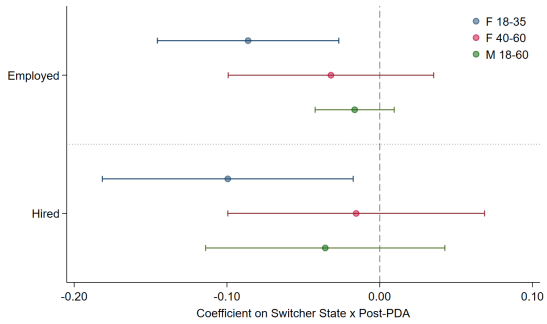
Notes: The sample is restricted to respondents age 18 to 60 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC. Estimates are computed using ASEC individual sample weights. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. Hired is a dummy 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. PF stands for predicted fertility, which is computed as detailed in section 6.5. Bars display the estimated coefficients on the interactions between the *PDA-treated* and the *Post-PDA* indicators and, separately, a dummy for having below-median PF (green) and above-median PF (orange). The regression includes all double and triple interactions between the *PDA-treated* and *Post-PDA* indicators and the dummy for above-median PF, and this dummy alone. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure A12: Effects for Women 40-60 and Men 18-60

(a) CPS ASEC



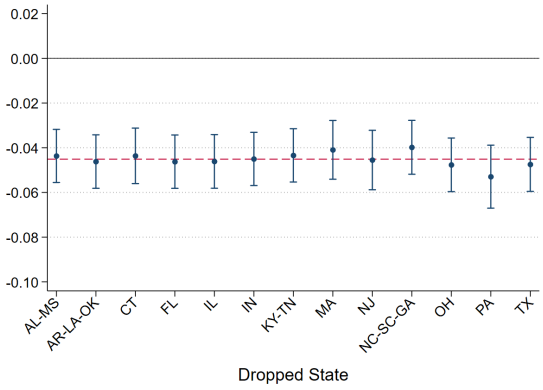
(b) PSID



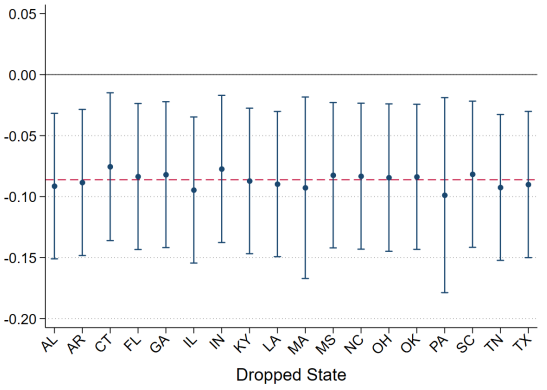
Notes: The sample is restricted to respondents age 18 to 60 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC. Plotted are the estimates from equation (5), separately for each subgroup. Estimates are computed using ASEC individual sample weights. Employed is a dummy equal 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. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure A13: DID Estimates for Employment, Robustness to Set of States

(a) CPS ASEC

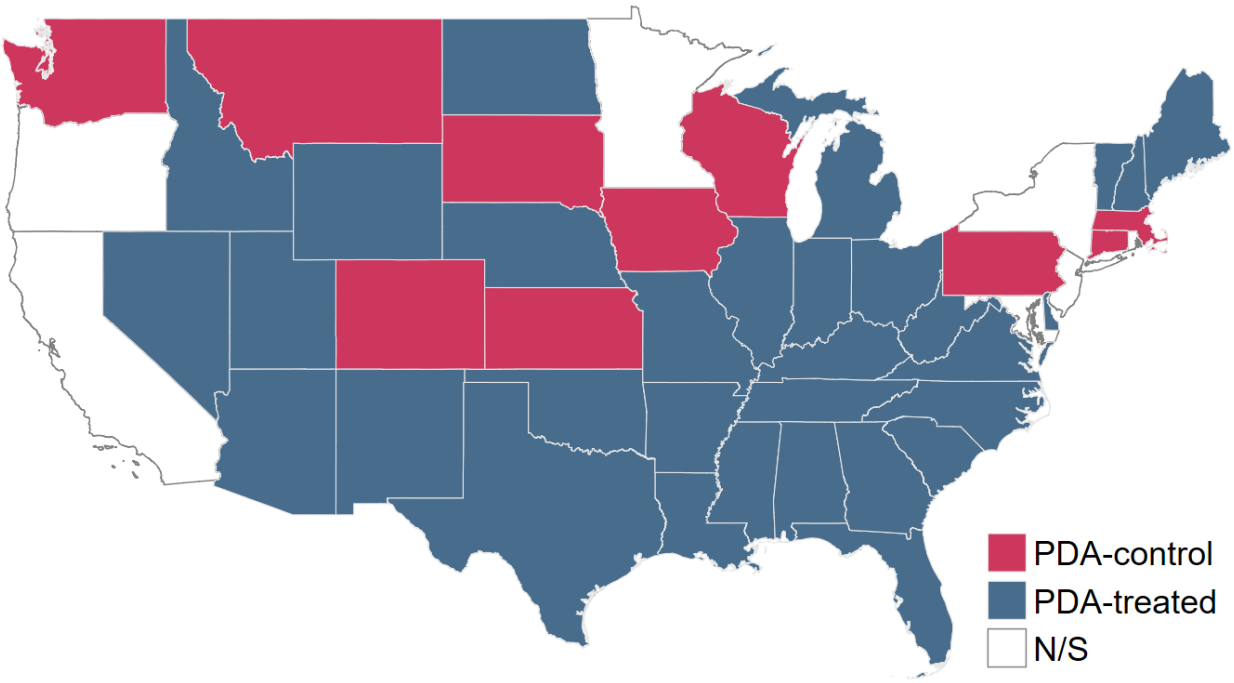


(b) PSID



Notes: This plot displays the DID coefficient on the interaction between the *PDA-treated* and *Post-PDA* dummies from specification (5), estimated dropping from the sample observations from one state group at a time. The omitted state group is indicated on the x-axis. The main sample is taken from the CPS ASEC, in panel (a), and from the PSID, in panel (b). Estimates are computed using individual-level survey weights. 95% confidence intervals are based on heteroskedasticity robust standard errors.

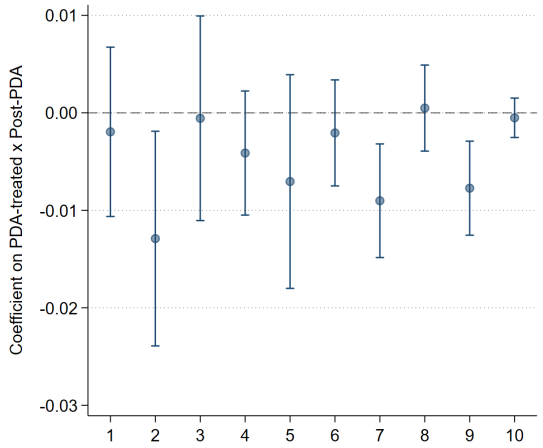
Figure A14: PDA-treated and PDA-control States, PSID All Possible States



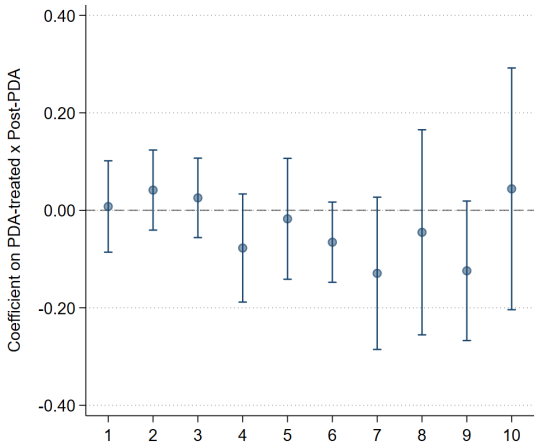
Notes: This map displays the set of PDA-treated and PDA-control states used in the robustness exercise using all possible states from PSID data.

Figure A15: Effect on Employment and Wages by Decile of Industry-level Health Insurance Coverage, CPS ASEC data

(a) Employment in Industry Decile



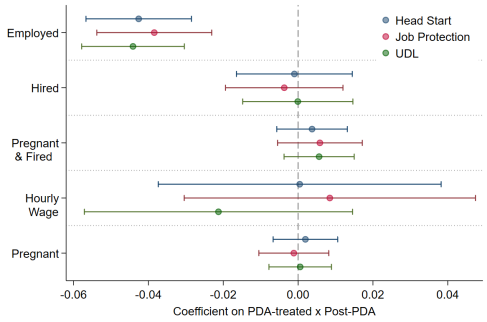
(b) Hourly Wage in Industry Decile



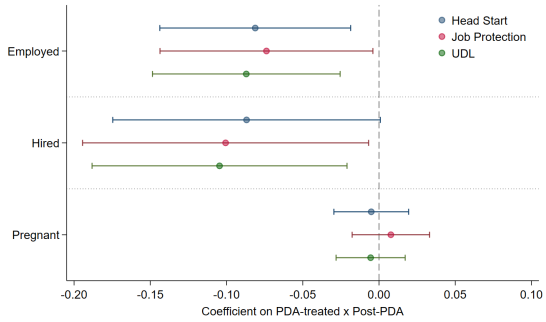
Notes: The sample is restricted to women of age 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC. Estimates are computed using ASEC individual sample weights. Employed is a dummy equal to 1 if the respondent is employed at some point in the year. The hourly wage is expressed as a z-score. The hourly wage is constructed using annual pre-tax wage income, number of weeks employed and usual weekly working hours. Industries are grouped in deciles based on their share of workers covered by own employer-provided health insurance, which is measured from the 1976 SIE. 95% confidence intervals are based on heteroskedasticity robust standard errors.

Figure A16: Robustness to Inclusion of Contemporaneous Policy Controls

(a) CPS ASEC



(b) PSID



Notes: The sample is restricted to women age 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from the CPS ASEC, in panel (a), and from the PSID, in panel (b). Plotted are the estimates from equation (5), separately for each subgroup. Estimates are computed using ASEC individual sample weights. 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. Estimates are obtained from equation (5), augmented with the inclusion of the controls indicated in the figures' labels. Contemporaneous policy controls are based on the data sources described in section 6.5. 95% confidence intervals are based on heteroskedasticity robust standard errors.

A.6 Additional Tables

Table A1: Pregnancy Discrimination Legislative Changes Prior to 1978

State	Enactment Year	Mode of Passage	Policy Mandate
Alaska	1975	Legislature	Equal Treatment
Colorado	1972	Administrative	Equal Treatment, Unpaid Leave
Connecticut	1973	Legislature	Equal Treatment, Unpaid Leave
District of Columbia	1977	Legislature	Equal Treatment
Iowa	1975	State court	Equal treatment
Kansas	1972	Administrative	Equal Treatment, Unpaid Leave
Maryland	1977	Legislature	Equal treatment
Massachusetts	1972	Legislature	Unpaid Leave
Minnesota	1977	Legislature	Equal treatment
Montana	1975	Legislature	Equal Treatment, Unpaid Leave
New York	1974	State court	Equal treatment
Oregon	1977	Legislature	Equal treatment
Pennsylvania	1973	State court	Equal treatment
South Dakota	1975	Administrative	Equal treatment
Washington	1972	Administrative	Equal Treatment, Unpaid Leave
Wisconsin	1975	State court	Equal Treatment

Notes: Details about state policies and sources are reported in sections 2.2 and 4.1. Mode of passage lists the institutional channel through which the policy was enacted: state legislature, administrative agency, or state supreme court decision. States not listed did not enact a policy comparable or more protective than the PDA before 1978. California passed an unpaid leave mandate in 1978, but it is never used in the sample since it passed an amendment to cover pregnancies via STDI in 1976. Michigan passed an equal treatment mandate in 1978 and it is thus used as a PDA-control state. New York is not used as a PDA-treated state since it also passed an amendment to cover pregnancies via STDI in 1977.

Table A2: Difference-in-Differences Estimates, Basic Monthly CPS data

	(1) Employed	(2) Hired
PDA-treated x Post-PDA	-0.029***	-0.007**
	(0.003)	(0.003)
Observations	1,040,533	244,533
Pre-PDA Mean	0.541	0.0848
R-squared	0.078	0.024
State FE	Yes	Yes
Year FE	Yes	Yes

Notes: The sample is restricted to women of age 18 to 35 in PDA-treated and PDA-control states between 1976 and 1984, from the basic monthly CPS. Employed is a dummy equal to 1 if the respondent was employed in the previous week. Hired is a dummy equal to 1 if the respondent was not employed in the previous month and is employed in the previous week, and 0 if still not employed in the previous week. The sample in column (2) is thus restricted to respondents of the rotating panel survey. Estimates are computed using CPS individual sample weights. 95% confidence intervals are based on heteroskedasticity robust standard errors. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A3: Difference-in-Differences Estimates by Motherhood Status, PSID

Women 18-35	All		With Children		Without Children	
	(1) Employed	(2) Hired	(3) Employed	(4) Hired	(5) Employed	(6) Hired
PDA-treated x Post-PDA	-0.086***	-0.099**	-0.116***	-0.095**	-0.040	-0.110
	(0.030)	(0.042)	(0.039)	(0.048)	(0.047)	(0.084)
Observations	14,479	8,639	11,526	6,963	2,953	1,676
Pre-PDA Mean	0.659	0.533	0.611	0.510	0.792	0.626
R-squared	0.113	0.115	0.083	0.102	0.239	0.224
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 PDA-treated and PDA-control states between 1976 and 1984, from the PSID. In columns (3) and (4), the sample is further restricted to respondents that have children at some point in their life. Columns (5) and (6) instead restrict the sample to respondents that never have children. Employed is a dummy equal to 1 if the respondent was employed in the previous week. Hired is a dummy equal to 1 if the respondent was not employed in the previous month and is employed in the previous week, and 0 if still not employed in the previous week. Estimates are computed using individual sample weights. 95% confidence intervals are based on heteroskedasticity robust standard errors. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A4: Triple Difference-in-Differences Estimates

Source:	CPS ASEC			PSID	
	(1) Employed	(2) Hired	(3) Hourly Wage	(4) Employed	(5) Hired
PDA-treated x Post-PDA x F 18-35	-0.043***	-0.006	0.033*	-0.071**	-0.082
	(0.008)	(0.009)	(0.017)	(0.035)	(0.054)
Observations	409,825	89,726	252,634	49,071	18,995
Pre-PDA Mean	0.686	0.0873	3.549	0.659	0.533
R-squared	0.132	0.032	0.293	0.173	0.176
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: The sample is restricted to observations in PDA-treated and PDA-control states between 1973 and 1984, from the data described in the column headers. The sample consists of three groups: women age 18 to 35, women age 40 to 60 and men age 18 to 60. Estimates are computed using individual sample weights and include the fixed effects indicated in the table. Standard errors are robust to heteroskedasticity. The specification is described in Appendix A.3 and it includes all double and triple interactions between the dummies for *PDA-treated* state, *Post-PDA* year, and being a woman age 18 to 35, along with this last dummy included independently. 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. The hourly wage is expressed as a z-score. The hourly wage 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 A5: Robustness to using only Massachusetts as PDA-control state

Source:	CPS ASEC		PSID		Basic Monthly CPS	
	(1) Employed	(2) Hired	(3) Employed	(4) Hired	(5) Employed	(6) Hired
PDA-treated x Post-PDA	-0.061***	0.005	-0.071*	-0.108	-0.025***	-0.011
	(0.011)	(0.015)	(0.041)	(0.075)	(0.005)	(0.007)
Observations	100,427	29,571	12,836	7,582	849,795	199,289
Pre-PDA Mean	0.693	0.0914	0.684	0.557	0.547	0.0875
R-squared	0.069	0.014	0.113	0.117	0.077	0.021
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is restricted to women age 18 to 35 in PDA-treated states and Massachusetts between 1973 and 1984, from the data described in the column headers. Estimates are computed using 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 A6: Difference-in-Differences Estimates on Additional Outcomes, CPS ASEC data

	(1) Employed Part-time	(2) # Weeks Employed	(3) # Weekly Hours	(4) Employed Last Week	(5) Pregnant & Quit Job	(6) Married
PDA-treated x Post-PDA	0.016*	-0.168	-0.750***	-0.050***	0.007	-0.003
	(0.008)	(0.326)	(0.226)	(0.007)	(0.005)	(0.007)
Observations	82,283	70,749	70,699	115,794	10,441	115,794
Pre-PDA Mean	0.254	38.24	35.33	0.519	0.0113	0.632
R-squared	0.034	0.068	0.034	0.055	0.008	0.224
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 PDA-treated and PDA-control 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. The outcomes of each column are defined as: (1) a dummy equal to 1 if the respondent is employed part-time and 0 if full-time, missing otherwise; (2) number of weeks employed in the year, conditional on ever being employed in that year; (3) number of usual weekly hours, conditional on ever being employed in the year; (4) a dummy equal to 1 if employed in the week preceding the March CPS survey; (5) a dummy equal to 1 if the respondent was unemployed or looking for work in the preceding week because of a quit, defined only for individuals pregnant in the previous year; (6) a dummy equal to 1 if currently married and 0 otherwise. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: 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)
PDA-treated x Post-PDA	-0.064**	-0.056	-0.047	-0.060
	(0.026)	(0.035)	(0.029)	(0.041)
Observations	19,375	11,558	15,093	8,953
Pre-PDA Mean	0.626	0.523	0.661	0.531
R-squared	0.108	0.115	0.114	0.119
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: The sample is restricted to women 18 to 35 in PDA-treated and PDA-control states between 1973 and 1984, from PSID data. The set of PDA-treated and PDA-control states in columns (1) and (2) are is illustrated in Figure A14. Columns (3) and (4) use the baseline set of PDA-treated and PDA-control states, shown in Figure A4, but assign respondents treatment status based on their childhood state of residence instead of their current state of residence. Estimates are computed using 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 A8: Difference-in-Differences High *versus* Low Predicted Fertility, 1960-1980 Census

Outcome:	Employed		
Source:	PDA-treated States	PDA-control States	All States
	(1)	(2)	(3)
Post-PDA x High PF	-0.026***	-0.053***	-0.038***
	(0.002)	(0.004)	(0.002)
High PF	0.012***	0.010**	0.008***
	(0.002)	(0.004)	(0.002)
Observations	1,169,993	341,636	2,141,577
Pre-PDA Mean	0.554	0.568	0.563
R-squared	0.109	0.141	0.118
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The sample is restricted to women age 18 to 35 from samples of the Censuses of 1960, 1970, and 1980, from the set of states described in the column headers. Estimates are computed using 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. PF stands for predicted fertility, which is computed as detailed in section 6.5. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.