The Labor Market Impacts of America's First Paid Maternity Leave Policy

Brenden Timpe [†]

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Abstract

This paper provides new evidence on the effect of a national expansion of paid maternity leave on the labor-market outcomes of women in the United States. I develop an identification strategy that exploits the staggered expansion of paid leave through short-term disability insurance in the 1960s and 1970s. The policy expanded leave-taking among new mothers but also precipitated a decrease in hourly wages, employment, and family income among women of child-bearing age. The results suggest that even modestly generous, widespread expansions of paid family leave policies have the potential to widen gender gaps in the labor market.

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[†]University of Nebraska-Lincoln Department of Economics. Email: btimpe@unl.edu. Portions of this paper were previously circulated as part of "The Long-Run Effects of America's First Paid Maternity Leave Policy." Thanks to Martha Bailey, John Bound, Gábor Kézdi, Sarah Miller, Mel Stephens, Charlie Brown, Helen Levy, Sara LaLumia, Tanya Byker, Valentina Duque, Melanie Wasserman, Chad Syverson, Ariel Binder, Dhiren Patki, Pieter de Vlieger, Shuqiao Sun, Connor Cole, Jacob Bastian, Bryan Stuart, Mike Zabek, Avery Calkins, Amelia Hawkins, Parag Mahajan, Margaret Triyana, Daniel Tannenbaum, Yifan Gong, and seminar participants at the University of Michigan, the U.S. Census Bureau, the National Tax Association, the Society of Economics of the Household, the Population Association of America, the University of Kansas, the Kansas Health Economics Conference, the Barcelona GSE Summer Forum, the EALE SOLE AASLE World Conference, the University of Nebraska-Omaha, and the UF/FSU/USF seminar series for helpful comments and discussions. This research was supported in part by an NIA training grant to the Population Studies Center at the University of Michigan (T32 AG000221). Declarations of interest: none.

1 Introduction

Policies that provide paid leave to new parents have become widespread and increasingly generous in most high-income countries in recent decades. The United States has long been an exception, and benefits for American parents remain relatively meager (OECD, 2018; Goldin, Kerr and Olivetti, 2020). However, even among U.S. policymakers, there is evidence of growing momentum behind the idea of universal access to parental leave (Sholar, 2016; Konish, 2018; Nova, 2021; Peck, 2022). Proponents of expanded paid leave often argue that the flexibility provided by paid-leave policies will encourage mothers to stay connected to the workforce and help close the gender gap in wages and occupational status (The White House Council of Economic Advisers, 2014; Mathur et al., 2018).

Despite substantial interest from policymakers and the public, the academic literature has struggled to develop definitive evidence on the labor-market implications of parental leave policies. The bulk of the existing empirical evidence draws on further expansions of the generous allotments enjoyed by parents in Europe, Canada, and other developed countries (e.g., Baker and Milligan, 2008*a*; Lalive et al., 2014; Dahl et al., 2016), or from variation in benefit *generosity* in California and a handful of U.S. states where access to paid leave has long been nearly universal (Rossin-Slater, Ruhm and Waldfogel, 2013; Byker, 2016; Baum and Ruhm, 2016; Bailey et al., 2019; Bana, Bedard and Rossin-Slater, 2020). However, much less is known about the implications of a policy that alters American parents' access to paid leave on the *extensive* margin. Furthermore, while parental leave is designed primarily to benefit mothers and fathers, these policies may have broader labor-market impacts, including effects on non-parents. But the lack of plausibly exogenous variation in access to parental leave, especially in the United States, has limited researchers' ability to gain traction on these questions. In fact, a recent literature review concluded that "no obvious consensus on the labor market impact of parental leave rights and benefits emerges from the empirical literature" (Olivetti and Petrongolo, 2017).

This paper provides new evidence on the effect of parental leave on women's labor-market prospects by exploiting a little-studied interaction between U.S. disability policy and anti-discrimination statutes that expanded access to paid maternity leave to millions of American women in the 1960s and 1970s. My research design draws on long-standing, cross-state variation in the availability of short-term disability insurance (STDI). STDI, which was originally designed to provide income insurance for temporarily disabled workers, became a common source of paid maternity leave benefits when a series of state and federal anti-discrimination laws required them to cover childbirth as a disability. For new mothers, this meant gaining access to a stream of benefits for 6-12 weeks at a rate of one-half to two-thirds of usual wages – an amount comparable to most present-day proposals for universal paid leave in the United States. In effect, the enactment of these anti-discrimination laws expanded paid maternity leave benefits to millions of American women – and disproportionately so in states where wider STDI coverage gave the policy more "bite."

Drawing on historical records, I reconstruct the timeline by which STDI maternity benefits became accessible to women across the United States. I then use an event-study approach to examine the effect of this widespread expansion of paid leave on the labor-market outcomes of American women. My research design compares the outcomes of individuals before and after the expansion of maternity benefits, and across states that adopted the policy at different times and in different intensities. The key identifying assumption behind this design is that access to STDI maternity benefits was not correlated with other determinants of my outcomes of interest. Consistent with this assumption, I show that the expansion of STDI maternity benefits did not coincide with potential confounding factors such as the expansion of Head Start and kindergarten programs and unilateral divorce laws (Pei, Pischke and Schwandt, 2018).

I first show that the policy substantially expanded access to and the use of short-term leave after childbirth. I draw on retrospective data from the Survey of Income and Program Participation (SIPP), which provides the most detailed measurement available on mothers' short-run labor supply responses to the establishment of paid leave. I estimate that the benefits were claimed by roughly half of eligible mothers, and that beneficiaries took nearly four additional weeks of leave after the birth of a child. The increase was most pronounced for mothers with relatively high socioeconomic status, especially those with some college experience. The largest effects on leave-taking happen in the first few months after the child's birth, but for some subgroups, STDI maternity benefits led to even lengthier absences: Among first-time mothers with college experience, I find an 11 percentage-point decrease in the share returning to work before their child's first birthday.

I then turn to data from the Current Population Survey's (CPS) May extracts and Outgoing Rotation Group files to examine the broader labor-market consequences of this expansion of paid leave. While the lack of household information in these data preclude analysis of parents' behavior, they offer high-quality information on labor-market outcomes, including hourly wages. In the wake of the enactment of maternity benefits, I find that hourly wages among women of "childbearing age," defined here as ages 18-45, fell by roughly 5-6 log points, even as wages for men of the same age group held steady. These wage decreases were persistent, remaining statistically distinguishable from 0 for nearly a decade following the reform. These effects cannot be explained by changes in occupation, and although I find evidence of a reduction in employment among women in this age group, there is no evidence that differential selection into the labor force can explain the deterioration in women's wages.

While these effects may seem surprising at first blush, they are consistent with literature examining the labor-market implications of mandated benefits (Summers, 1989; Gruber, 1994).

To the extent that firms saw paid leave benefits as costly – as qualitative evidence suggests they did – shifts in labor demand would tend to reduce women's hourly wages, employment, or both. My results suggest that any labor-supply response that may have occurred was too small to prevent a decrease in employment among women. While the direct cost of STDI maternity benefits was modest, the wage effects I observe are well within the magnitudes suggested by the literature on the costs of training and turnover (Manning, 2011; Boushey and Glynn, 2012; Bartel et al., 2014; Kuhn and Yu, 2021; Jaeger and Heining, 2022).

A final exercise provides suggestive evidence that the effects on women's labor-market outcomes were indeed driven by employers' perceptions of the cost of providing maternity leave to their female workers. If the availability of STDI maternity benefits led to shifts in demand for female labor, we would expect these effects to be strongest in positions where employees' absence or turnover is most costly to the firm. I test this theoretical prediction by constructing three occupation-level measures of the cost of employee absence or turnover: an employer survey that collected "adjustment costs" from replacing a worker (Hudomiet, 2015), an estimate of the return to working long hours from Cortés and Pan (2018), and Census occupation codes for professional and managerial workers. For each of these measures, I find that wage effects are largest in occupations with the largest expected firm-side cost of paid leave.

This paper contributes to several strands of the academic literature. First, while previous work has acknowledged the role of STDI as a source of maternity benefits (Stearns, 2015; Rossin-Slater, 2018), to my knowledge this is the first paper to trace out the history of the anti-discrimination laws that ushered in these benefits. This historical variation in the timing and intensity of the expansion lends itself to a new identification strategy that provides new traction in the effort to study paid leave's effect on the U.S. labor market. An advantage of this variation is that the expansion of benefits was broadly applied, triggered widespread takeup, and affected a population that in most cases had no previous source of maternity benefits.

This paper also contributes to the literature on the labor-market consequences of parental leave. A substantial body of evidence suggests there is demand for more generous leave policies among new mothers and, to a lesser extent, fathers (Rossin-Slater, Ruhm and Waldfogel, 2013; Baum and Ruhm, 2016; Byker, 2016; Persson and Rossin-Slater, 2019). Much of this literature studies parents' behavior in countries such as Europe and Canada that have generous and long-standing policies regarding paid leave, child care, and other support for parents (Baker and Milligan, 2008*a*; Lalive and Zweimüller, 2009; Ekberg, Eriksson and Friebel, 2013; Bergemann and Riphahn, 2023). Evidence from the United States, where benefits tend to be smaller and access is less common, has been more difficult to marshal. However, recent work in U.S. settings has broken ground by combining state policy expansions and administrative data in creative ways that allow for regression kink (Bana, Bedard and Rossin-Slater, 2020) and difference-in-difference designs

(Bailey et al., 2019). I provide new evidence from a novel and policy-relevant setting that, for most mothers, represented an expansion of paid leave along the extensive margin.

In addition, these results contribute to the literature on family policy and the persistence of gender gaps in the labor market (Goldin, 2014; Blau and Kahn, 2017). Recent literature has documented the central role played by parenthood and the structure of work (Goldin, 2014; Cortés and Pan, 2018; Kleven, Landais and Søgaard, 2019). Parental leave and other family policies have been billed as tools to even the playing field by promoting flexibility in the workplace and labor-force attachment for mothers. The bulk of the empirical evidence on these policies comes from European settings, where allotments of leave, child care, and other amenities are relatively generous, and in this context expansions of family benefits often have small or even negative impacts on women's labor-market outcomes (Havnes and Mogstad, 2011; Ginja, Karimi and Xiao, 2020; Fernández-Kranz and Rodríguez-Planas, 2021; Kleven et al., 2021). This paper contributes to this literature by studying a modest expansion of paid leave in a setting with relatively few policies designed to support working parents. While the estimates in this paper are not directly informative of the welfare implications of parental leave, they suggest that paid leave allotments will not necessarily narrow gender gaps in the labor market.

Finally, this paper is related to a broader literature on the interaction between policy mandates and the labor market (Summers, 1989). This literature suggests that general equilibrium effects are an important factor in understanding the economic consequences of publicly provided or mandated programs such as unemployment insurance (Johnston, 2021), disability insurance (Kim and Rhee, 2018; Hawkins and Simola, 2021; Aizawa, Kim and Rhee, 2020; Prinz and Ravesteijn, 2020), workers' compensation (Fishback and Kantor, 1995; Cabral, Cui and Dworsky, 2022), and paid sick leave (Pichler and Ziebarth, 2020; Maclean, Pichler and Ziebarth, 2021). In a closely related paper, Gruber (1994) finds that firms alter their labor inputs in response to a *health* insurance coverage mandate. In section 3.2 and Appendix D, I argue that some of this paper's findings can be seen as an expansion and reintepretation of those results.

2 America's first paid maternity leave policy

The United States is an outlier among developed nations when it comes to parental leave. Roughly 60 percent of workers are eligible for unpaid, job-protected leave through the federal Family and Medical Leave Act (Klerman, Daley and Pozniak, 2012). However, no national policy guarantees *paid* time off to care for a newborn. Paid family leave benefits, which can be used by parents and other caregivers who wish to take a leave of absence to bond with a newborn child, are now widely available in a handful of states that have enacted laws in the last 20 years, but large gaps in coverage remain: As of 2019, only 19 percent of U.S. workers, including 22 percent of full-time workers, reported having access to an employer-sponsored benefit explicitly devoted to providing paid family leave (U.S. Bureau of Labor Statistics, 2019, Table 31).

In the absence of a national policy, parents stitch together income from a variety of alternative sources to finance paid leave. While many parents draw on sick leave or vacation time during bonding leave, STDI has also become an important source of benefits that often provides a longer-lasting stream of income. While far from universally available, STDI policies are much more common than formal paid leave benefits, with about 40 percent of workers receiving access to an employer-sponsored policy in 2019 (U.S. Bureau of Labor Statistics, 2019, Table 16) and a much larger share covered in states with mandatory or publicly run STDI programs. Altogether, about 54 percent of American workers say they could take paid time off to care for a newborn child (Boyens, Karpman and Smalligan, 2022).[1] Using data on births from 2001-2010, Goldin, Kerr and Olivetti (2020) report that 64 percent of college graduate mothers and 36 percent of non-college graduates received some paid leave after childbirth, although benefits lasted less than 10 weeks for the vast majority of both groups and less than 6 weeks for most women without a college degree.

STDI differs from family leave programs in that it was originally designed as wage insurance for workers who suffered a non-employment-related injury or illness, not a benefit for new parents. These plans do not generally pay benefits to fathers, adoptive parents, or other caregivers who did not physically give birth. However, for mothers who are eligible, STDI otherwise serves much the same purpose as family leave policies, providing a stream of income for a number of weeks upon the birth of a child. The state-by-state process by which these STDI policies became a source of paid maternity benefits is detailed in the following section.

2.1 The role of state disability and anti-discrimination policy

The U.S. short-term disability insurance industry emerged in the mid-19th century in response to demand for a source of income replacement for temporarily disabled workers (Faulkner, 1940). While access varied widely across states and industries, by 1954 STDI policies covered about 48 percent of workers in states without coverage mandates (Price, 1986). However, in 1942, Rhode Island created the Cash Sickness Compensation System with the goal of offering wage replacement that nearly all workers could draw on in the case of an illness or injury. California, New Jersey, and New York followed suit by making STDI coverage universal in the next few years, while Hawaii and Puerto Rico adopted their own programs in the 1960s (Kamerman, Kahn and Kingston, 1983;

[1]This figure may understate access because 28 percent of workers in the Urban Institute survey analyzed by Boyens, Karpman and Smalligan (2022) were not sure whether they had access to benefits. Similarly, a 2010 survey of California workers by Applebaum and Milkman (2011) found that less than half were aware of that state's Paid Family Leave program, even 6 years after its enactment and in a sample in which all respondents had experienced a birth or other covered event, suggesting that lack of information may be a barrier to take-up (Currie, 2004). To the extent that workers were unaware of the policy reform studied in this paper, we would expect to find muted effects of STDI on labor supply.

Wisensale, 2001). While data on STDI coverage at the state level is not available during this time frame, Figure 1a plots estimates of coverage among working women based on the 1976 Survey of Income and Education (see Section 3 for details on these calculations). The figure shows that access to STDI varied widely across states, from as low as 29% in South Dakota to 54% in Ohio and virtually universal in the five states with long-standing disability insurance policies.

This pre-existing variation in coverage became particularly consequential in the 1970s, when a series of state laws banned workplace discrimination on the basis of pregnancy. The staggered expansion of these laws is shown in Figure 1b. While these laws came in a variety of forms – including acts of the legislature in Montana in 1972 and Maryland in 1977, administrative rulings in Kansas in 1975 and Illinois in 1976, and state supreme court decisions such as those in Iowa in 1975 and New York in 1976 – the end result was similar: group STDI plans could no longer exclude childbirth as a covered disability. When Congress approved the Pregnancy Discrimination Act of 1978, this policy was imposed on the rest of the nation, effectively creating America's first paid maternity leave policy.[2]

The STDI maternity benefits provided to women were relatively modest by the standards of most OECD countries. In states with universal STDI, minimum standards were set by law and covered between one-half and two-thirds of usual weekly wages and lasted between 6 and 12 weeks.[3] In the private sector, STDI plans varied based on firm policy, but could include either full or partial wage replacement and lasted 7.5 weeks on average for a pregnancy without complications (Gladstone, Williams and Belous, 1985, Table 1). While the anti-discrimination laws offered no formal guarantee that a mother's job would be protected, they did require that women on maternity leave receive treatment *equal* to that afforded to others who were absent due to a disability. This formulation could cut both ways: While it afforded "soft" job protection to women at firms that allowed disability leave, it technically did not preclude employers from uniformly revoking the right to disability leave for all workers.

While the push for these anti-discrimination laws was successful in many states – and in Congress – they inspired substantial opposition from the business community. After Maryland's legislature voted to pass its anti-discrimination bill in 1977, the state's Chamber of Commerce launched an "urgent" campaign to convince the governor to veto it, arguing that "costs to employers would rise substantially" (Rousmaniere, 1977). Ardie Epranian, a representative of the AVX Corporation, warned members of Congress in a hearing on the federal Pregnancy Discrimination Act of 1978 that the "real cost is the hidden increase in claims incidence and additional time lost that would be the inevitable consequence... It is rather easy to envision the abuses and extra time lost that can occur." Similarly, a representative of the Electronic Industries Association cited fig-

[2] A detailed description of the enactment of state anti-discrimination laws is provided in Appendix A.

[3] See Appendix Table A2 for information on private and state-mandated STDI benefits as of the late 1970s.

ures from a recent Supreme Court decision, *General Electric Co. v. Gilbert*, that had sided against a woman who sought disability benefits for pregnancy:

> "Other costs associated with this legislation, and I think that some of these have been overlooked, are productivity costs. Employee replacements for women on pregnancy leaves are not as productive as experienced workers. We feel that providing disability benefits will result in longer leaves... It costs money to screen and hire new employees, and as the *Gilbert* case points out, 40 to 50 percent of females on pregnancy leaves do not return" (U.S. House of Representatives, 1977).

The legislative debate demonstrates that the salient firm-side costs of maternity leave included not only the pecuniary cost of the benefits, but also the cost of employee absence and turnover. While the magnitude of these costs is difficult to quantify, the literature on turnover costs suggests they can be substantial (Manning, 2011; Boushey and Glynn, 2012; Bartel et al., 2014; Kuhn and Yu, 2021; Jaeger and Heining, 2022). Furthermore, the impact of these costs on women's labor-market outcomes may be magnified by factors such as taste-based discrimination or the availability of substitute groups of workers who could not make use of the benefits. I discuss these considerations further in section 2.3.

The decision to treat pregnancy as a disability was not unique to the U.S. setting. For example, Canada's legal history includes a similar debate about whether denying benefits to pregnant women constituted sex discrimination (Trzcinski and Alpert, 1994). At the urging of the Royal Commission on the Status of Women (Bird et al., 1970), the country established 15 weeks of paid maternity benefits in 1971 as part of its unemployment insurance program, which provides benefits to sick workers. However, United States' adoption of STDI as a mechanism to fund paid leave is unique in that its benefits are similar to many proposals of current-day U.S. federal and state policymakers. While the Trump Administration proposed offering 6 weeks of paid leave through the unemployment insurance programs (Konish, 2018), the Biden Administration advocated 12 weeks Nova (2021). Lawmakers in Congress have called for benefits of similar duration (Peck, 2022). Among the minority of states that have enacted their own parental leave programs, the vast majority provide 12 weeks of benefits with a progressive benefit schedule, a duration that is very similar to the STDI policies available in the 1970s (Bipartisan Policy Center, 2023).

2.2 The enactment of STDI maternity coverage

The extent to which this expansion of access to paid leave benefits translated to changes in behavior is explored in Figure 2a, which illustrates the variation in maternity benefit receipt over time that was created by the enactment of anti-discrimination laws in two states with available data, California and New York. The figure plots STDI pregnancy claims as a share of births to residents of each state. With the exception of complications from childbirth, neither state provided STDI benefits to new mothers before pregnancy coverage was extended in 1977. However, the reform led to a sharp increase in benefit receipt, leveling off at roughly 25-30 percent of births or about half of working mothers.[4] While this take-up rate is relatively low by the standards of many other OECD countries, it represents relatively healthy usage by U.S. standards, where the share of eligible individuals receiving benefits often falls well below 50 percent for some populations, even for programs such as disability benefits or health insurance for children (Currie, 2004). As recently as 2014, despite decades of gains among working mothers and a more generous slate of benefits than those available under STDI, California's Paid Family Leave benefits were claimed by only 47 percent of eligible mothers (Bana, Bedard and Rossin-Slater, 2018).

Figure 2b illustrates the differing "bite" that the anti-discrimination laws had across states. The figure displays the share of mothers, by month relative to childbirth, who report receiving STDI benefits in the 1984-1989 panels of the Survey of Income and Program Participation (SIPP). Benefit receipt spikes for women in both states in the months around the birth of a child, but it is much higher in universal-STDI states (solid line) than among women in all other states (dashed line).[5] These differences in the take-up of STDI benefits over time and across states provide *prima facie* evidence of the importance of STDI in the growth of maternity leave among American women.

2.3 Expected effects of paid maternity leave

Discussions of the provision of paid parental leave often focus on its implications for mothers and fathers, the time they spend at home caring for a new child, and their likelihood of returning to a job. A robust theoretical and empirical literature suggests that these policies lead unambiguously to an increase in the use and length of maternity leave (Klerman and Leibowitz, 1997; Waldfogel, 1999; Baum, 2003; Han and Waldfogel, 2003; Baker and Milligan, 2008*b*; Han, Ruhm and Waldfogel, 2009; Baum and Ruhm, 2016; Rossin-Slater, Ruhm and Waldfogel, 2013; Byker, 2016). This literature is consistent with the descriptive evidence in Figure 2, and in the following section I formally test the possibility that the expansion of STDI maternity benefits affected mothers' short-term labor-market outcomes.

The implications for parents' subsequent labor-market outcomes are less clear. Parental leave may promote attachment to the labor force and retention of firm-specific human capital, increas-

[4]Eligibility requirements for STDI benefits are minimal in California and New York, suggesting that the share of eligible mothers can be approximated by the share of New York and California women with a child age 0 who report working for pay in the previous year to the March CPS. This figure hovered between 40 and 50 percent during the late 1970s and early 1980s, suggesting a take-up rate among eligible women of about 50 percent.

[5]Additional estimates of take-up by demographic group are provided in Figure A1.

ing earnings in the long run (Klerman and Leibowitz, 1997). Alternatively, these policies' stream of income and job security may provide an opportunity to explore entrepreneurial opportunities (Gottlieb, Townsend and Xu, 2022). On the other hand, the extended time away from work may also harm labor-market prospects through mechanisms such as signaling or deterioration of human capital. Much of the evidence on this question comes from Canada and countries in Europe with relatively long policy histories and detailed administrative records. This research often finds that more generous benefits lead to very small negative or null long-run effects on mothers' careers (Lalive and Zweimüller, 2009; Lalive et al., 2014; Schönberg and Ludsteck, 2014; Dahl et al., 2016; Canaan, 2019; Kleven et al., 2021; Bergemann and Riphahn, 2023). However, evidence on reforms in Canada (Baker and Milligan, 2008*a*), Germany (Frodermann, Wrohlich and Zucco, 2020; Bergemann and Riphahn, 2023) and Great Britain (Stearns, 2018) suggests that relatively short leave allotments that incentivize job retention can lead to long-run labor-market gains for mothers. The importance of these institutional details underscores the need for additional evidence from the United States, where labor-market institutions are different and family policies are less generous. While evidence suggests that recent U.S. paid leave expansions have increased mothers' labor-force participation (Rossin-Slater, Ruhm and Waldfogel, 2013; Byker, 2016) and even job retention (Baum and Ruhm, 2016; Bana, Bedard and Rossin-Slater, 2020) early in their children's lives, Bailey et al. (2019) find that California's 6-week extension of paid leave in 2004 led to a *decrease* in employment and earnings for eligible mothers, even 6 to 10 years after giving birth.

However, a complete examination of the theoretical consequences of parental leave policies would consider effects beyond those on mothers and fathers. For example, the historical evidence discussed in section 2.1 suggests leave policies may have implications for firms' hiring and other personnel decisions (Ginja, Karimi and Xiao, 2020; Goldin, Kerr and Olivetti, 2020). Because mothers are more likely to make use of parental leave, these policies may also reinforce traditional social norms or create "glass ceiling" effects that reduce gender equality (Angelov, Johansson and Lindahl, 2016; Thomas, 2018; To, 2018; Xiao, 2021). Furthermore, to the extent that firms use imperfect screening mechanisms or rely on co-workers to minimize the costs of employee absences, the effects of paid leave may spill over onto other workers, affecting the wages, employment, and career trajectories of non-parents (Brenøe et al., 2018; Gallen, 2019; Ginja, Karimi and Xiao, 2020).

Evidence on these mechanisms from the United States has relied on state-level, intensive-margin expansions of paid leave in California and New Jersey (Das and Polachek, 2015) and the FMLA's expansion of unpaid leave to eligible women at large firms (Waldfogel, 1999). Much less is known about the implications of the type of a reform that would bestow a modest amount of paid leave benefits to workers who currently enjoy few such benefits. This paper contributes to the literature by outlining exactly this type of setting, and the cross-state and timing variation in the expansion of STDI maternity benefits may provide more traction to study broad effects on the labor market.

3 Data and empirical strategy

I draw on several sources of data to study the impact of STDI paid maternity benefits on the U.S. labor market. First, to study the impact of STDI maternity benefits on the short-run labor-market outcomes of American mothers, I construct a sample of women from the 1984-1989 SIPP. The SIPP's longitudinal data provides detailed information on labor-market activity and receipt of income from a variety of sources, including STDI. Most importantly, the 1984 and 1985 panels include a module on fertility and migration that asks women about their labor supply just prior to and after their first childbirth.[6] I use this module to construct a balanced panel of monthly labor supply for each mother, from 9 months before childbirth to 12 months after. The panel provides the year and month of childbirth, and in my preferred specifications I proxy for state of residence at childbirth with the mother's state of birth.[7] Summary statistics for this sample are shown in column 1 of Table 1. The average mother gave birth for the first time at just under 24 years of age, and about two-thirds were employed at some point during their pregnancy, consistent with aggregate trends during this period (Goldin and Mitchell, 2017).

I complement the data from the SIPP with the analysis of data from the Current Population Survey (CPS) May Extracts from 1969-1978 and the Outgoing Rotation Groups (ORG) from 1979-1987. The strength of these data is that they provide a high-quality measure of hourly wages beginning in 1973, and employment back to 1969. The drawback is that these surveys did not collect information on children under age 14 until the 1980s, so I cannot observe anything related to fertility or the presence of young children. For this reason, I use these data for my analysis of the effect of STDI maternity benefits on all women. An additional complication in the early CPS data is that some states are consolidated into groups. After dropping Hawaii and Connecticut from the sample because the date of their reform coincides with the beginning of the sample containing hourly wages, I am left with 27 states or state groups.[8] Following Lemieux (2006), I use the wage reports of both hourly and salaried workers, dropping imputed values and observations with an

[6] The survey asks three questions of importance. First, in what year and month did the woman give birth to her first child? Second, did she work during this first pregnancy? And finally, if she did work, when did she stop working before the birth and when, if ever, did she return?

[7] This choice avoids concerns about endogenous migration after childbirth and delivers qualitatively similar estimates to those that use contemporaneous state of residence or the limited migration history available. In Appendix C.1, I use data on children born just before the 1970 or 1980 decennial Census to study effects on leave-taking for all births, including higher-parity births.

[8] The state groups are detailed in Appendix Table A3.

hourly wage less than \$1 or greater than \$100 in 1979 dollars. To study labor supply, I use the CPS definition of employment and a measure of hours worked in the previous week. I also limit the sample to women of "child-bearing age" – defined here as ages 18-45 – and men of the same age range. Columns 2 and 3 of Table 1 show summary statistics for women and men, respectively; women are substantially less likely to be employed and the raw hourly wage gap is just under 70%.

Finally, to measure exposure to STDI maternity benefits, I require information on the availability of government-, employer-, or union-provided STDI and the enactment of state- or federal-level pregnancy discrimination laws. My hand-collected information on the timing of anti-discrimination laws is summarized in Appendix A. In cases where I am able to distinguish the date of *enactment* of the laws from the date of *passage*, I code the date of treatment as the former when studying mothers' take-up of paid leave and the latter when studying broader labor-market responses. Measuring access to STDI is more difficult because little information exists at the sub-national level prior to the 1980s. As a result, I rely on the 1976 Survey of Income and Education (SIE), a large-scale supplement to the CPS that collected information on employment status and access to employer- or union-provided *health* insurance. I then take advantage of the fact that employer offers of STDI are highly correlated with – although rarer than – offers of health insurance to impute STDI coverage (Levy, 2004). The procedure I use to impute access to STDI is discussed further in section 3.1 and Appendix A.1.

Several other public sources of data are used to operationalize and test my research design. These data are described further in the sections that follow.

3.1 Empirical strategy

My main empirical approach relies on the following event-study specification that makes use of both the variation in timing of anti-discrimination laws and the differential "bite" of these laws in states with more and less access to STDI:

$$y_{ist} = \sum_{k \neq -1} \tau_k \, STDI_s \, \mathbb{1}\{k = t - T_s^*\} + X_{ist}'\beta + \delta_s + \theta_{r(s)t} + \varepsilon_{ist} \tag{1}$$

where y_{ist} is the outcome of interest, defined for individual i in state s at time t, and T_s^* is the date the pregnancy anti-discrimination law was enacted in state s. The specification includes state fixed effects δ_s that adjust for time-invariant determinants of the outcome that may vary across states, Census-region-by-year fixed effects $\theta_{r(s)t}$ that adjust flexibly for differential trends by region of the country, and controls X_{ist} for age, race, and – where specified – other features of the policy and macroeconomic environment that are discussed below.

The key variable $STDI_s$ is designed to capture state-level variation in female workers' access to STDI – i.e., the "dosage" of the treatment – without contamination from firm responses to the policy.[9] Unfortunately, data on STDI coverage at the sub-national level is not available in the 1970s or earlier. My preferred estimate of $STDI_s$ relies on the fact that, as documented by Levy (2004) and demonstrated in Appendix A.1, employer offers of STDI are strongly correlated with offers of other benefits, including health insurance. The 1976 SIE allows me to observe the share of workers in each state that receive health insurance through an employer or union. I use the SIE to estimate $STDI_s$ as:

$$STDI_s = \frac{\sum_i \omega_{is}(HI_{is} - \gamma)Emp_{is}}{\sum_i \omega_{is}Emp_{is}} \tag{2}$$

where ω_{is} is a sampling weight for person i in state s, Emp_{is} is an indicator for being employed, and HI_{is} is an indicator for having health coverage through an employer or union. The parameter γ is chosen so that average of $HI_{is} - \gamma$ among among all workers age 18-64 in states without universal STDI coverage is 0.486, the share reported by Price (1986). I calculate $STDI_s$ using only women age 18-45.[10] I set $STDI_s$ to 1 in universal states (Wisensale, 2001).[11]

The parameters of interest from equation (1), τ_k, can be interpreted as the causal effect of paid leave under the key assumption that the enactment of STDI maternity benefits is the *only* reason that outcome y_{ist} is correlated with my treatment variables. Confounders of this assumption could come in two general forms. First, if conditional on the covariates, trends in y_{ist} differed across states, estimates of τ_k for $k \geq 0$ may not represent the causal effect of STDI maternity benefits. Second, a break in the trend in unobserved determinants of outcome y_{ist}, if correlated with the enactment of paid leave through STDI, would lead me to erroneously attribute the changes in the

[9] I would ideally examine these firm responses directly. For example, firms could have responded by dropping STDI for all workers because anti-discrimination laws did not require them to offer STDI; rather, they required only that firms treat women and men equally if they decided to offer STDI. Unfortunately, data on STDI offers at the sub-national level is unavailable during my sample period. However, the national data plotted in Appendix Figure A2 suggests that few firms dropped STDI coverage in the wake of the anti-discrimination laws; a test of the null hypothesis that there was no trend break in 1979, the year the national Pregnancy Discrimination Act took effect, results in a p-value of 0.27. The statistical power of this test is limited, but to the extent that some firms in non-universal states dropped coverage, my estimated effects on women's wages would likely be attenuated.

[10] While this approach approach takes advantage of the substantial cross-sectional variation in the "bite" of pregnancy discrimination laws, it also imposes a linear relationship between exposure and my outcomes of interest. In Appendix C.2, I report more flexible estimates that allow for non-linearity in the dose-response. I also report estimates using an alternative measure of $STDI_s$ that relies on industry-level estimates of coverage from the BLS National Compensation Survey, linked to individual-level data from the 1970 Census. These results are discussed in Appendix C.3.

[11] Appendix A examines the predictive power of this measure by comparing $STDI_s$ to STDI benefit take-up in the 1984-1989 SIPP. Despite the small samples and well-known issues of under-reporting in the SIPP (Meyer, Mok and Sullivan, 2015), I find a correlation of 0.45 between $STDI_s$ and take-up among mothers in non-universal STDI states, suggesting this measure meaningfully captures the cross-sectional variation in STDI access. The correlation rises to 0.82 if I include the five universal STDI states.

outcome to STDI maternity benefits.[12]

My flexible event-study specification provides a built-in placebo test of the former assumption. To the extent that violations of the parallel trends assumption are present even before the advent of STDI maternity benefits, they would be likely to appear in the form of estimates of τ_k for pre-reform periods that are significantly different from 0. In some instances, I do see evidence of pre-trends, suggesting that states with different timing or intensity of exposure to STDI maternity benefits were trending differently even prior to their enactment. Motivated by this pattern, and following Dobkin et al. (2018), I explore the sensitivity of my estimates to the addition of a linear pre-trend term to equation 1. Specifically, I drop event-time indicators $\mathbb{1}\{k = t - T_s^*\}$ for $k < 0$ and replace them with $STDI_s \times k$. This modification allows us to interpret τ_k as a causal effect under the alternative assumption that the linear pre-trend observed prior to the reform is informative of the counterfactual path of women's employment after STDI maternity benefits were adopted.

On the other hand, the latter potential confounder – the presence of shocks to women's labor-market outcomes that are correlated with the expansion of STDI maternity benefits – is fundamentally untestable. I will discuss this assumption further and provide some suggestive evidence of its validity in section 3.2.

In addition to my main event-study specification, I use a restricted version of equation 1 to estimate summary measures of the impact of STDI maternity benefits:

$$y_{ist} = \tau_{SR} STDI_s \mathbb{1}\{0 \leq k \leq 4\} + \tau_{LR} STDI_s \mathbb{1}\{5 \leq k \leq 9\} + X'_{ist}\beta + \delta_s + \theta_{r(s)t} + \varepsilon_{ist} \tag{3}$$

where τ_{SR} represents the "short run" effect in the first five years after the expansion of paid leave benefits, and τ_{LR} represents the "long-run" effect in the second five years after the reform.[13] This specification increases statistical power and provides a concise summary measure under the assumption that any level shift in outcome y_{ist} in periods $k \geq 0$ relative to periods $k < 0$ is due solely to the effect of STDI maternity benefits. As in the event-study specification in equation 1, in some instances I test the sensitivity of this assumption by adding a linear term in event time interacted with exposure to STDI for pre-reform periods $k < 0$.

[12] A growing literature on two-way fixed effect models in settings with staggered program adoption highlights the sensitivity of such tests (Goodman-Bacon, 2021; Sun and Abraham, 2021). In Appendix C.3, I consider an alternative estimator proposed by Sun and Abraham (2021) and show that it delivers results similar to my standard event-study design, suggesting that heterogeneous treatment effects do not substantially alter the conclusions drawn from estimates using equation 1.

[13] The specification also includes indicators for periods outside the balanced window of event time, interacted with $STDI_s$. I omit these terms for brevity. These terms ensure my estimates of τ_{SR} and τ_{LR} are not affected by compositional changes in the extremes of event time.

3.2 Internal validity of the research design

My identification strategy relies on the assumption that no unobserved determinant of labor-market outcomes is correlated with the expansion of STDI maternity benefits. While an evaluation of my estimates of τ_k for $k < 0$ from equation 1 may provide suggestive evidence of the validity of the design, they cannot speak to the possible existence of shocks to women's labor-market outcomes that may be correlated with the expansions of paid leave through STDI.

I begin by constructing a state-year panel that measures access to a range of policies that may affect labor-market or family outcomes. The first such policy is access to early childhood education, which expanded greatly during the 1960s and 1970s via Head Start and state-level kindergarten funding programs (Cascio, 2009; Wikle and Wilson, 2020; Bailey, Sun and Timpe, 2021). Second, I examine access to federally funded programs that provided health and infant care to mothers and their children (Bailey, 2012). The third potential confounder is access to federally funded family planning programs, which have been found to impact fertility (Bailey, 2012). The fourth potentially confounding factor is the expansion of state unilateral divorce laws (Gruber, 2004; Wolfers, 2006). Finally, I assemble information on state-level, gender-specific minimum wage laws using data from Vaghul and Zipperer (2016) and Derenoncourt and Montialoux (2021).[14] Each of these policies saw considerable changes during the 1960s and 1970s and may have played a role in women's labor-market decisions. If correlated with the timing of the expansion to STDI maternity benefits, a sharp break in measures of exposure to these policies would be cause for concern that my estimates are driven by factors unrelated to paid leave (Pei, Pischke and Schwandt, 2018). In addition, I use birth records from National Center for Health Statistics (2015) to evaluate potential changes in fertility. An effect on fertility might be viewed as a treatment effect of the expansion of paid leave rather than a confounding factor. However, given the focus of this paper on the general equilibrium impacts of the policy, we might also interpret a change in fertility as a signal that composition effects play an important role in any changes observed in the labor market.

The results are shown in Figure 3. Figure 3a shows no evidence of a sharp, coinciding change in access to early education programs. My measure of Head Start access delivers a precisely estimated 0, which is unsurprising since the vast majority of programs were rolled out by the end of the 1960s (Bailey, Sun and Timpe, 2021). Meanwhile, kindergarten programs trend slightly downward, but the estimates are never statistically distinguishable from 0. Crucially, there is no evidence of a trend break that coincides with access to STDI maternity benefits.

Figures 3b, 3c, 3d, and 3e evaluate the possibility that STDI maternity benefits were correlated with grants for programs to support child health and infant care, grants for family planning,

[14] For each set of policies, I construct a state-year panel that tracks exposure to each of these policies and regress the appropriate indicator on equation 1. Further details on sample construction are available in Appendix A.2.

unilateral divorce laws, and state-specific minimum wages, respectively. None of the figures show evidence of a sharp change in policy that coincided with the enactment of STDI maternity benefits. Finally, Figure 3f shows little change in child-bearing in the years around the enactment of the policy.[15] Overall, the results in this section suggest little reason to think some of the most likely confounders are driving my estimates of effects on female labor-force outcomes.[16]

A final consideration is the extent to which the labor-market effects I explore can be attributed to other components of the Pregnancy Discrimination Act and its state-level predecessors. Gruber (1994) highlights the effect of these laws on health insurance coverage for the medical costs of childbirth. He argues that firms shifted the cost of the health insurance component of the mandate onto workers – and especially women – in the form of lower wages.

While these two channels are difficult to disentangle with certainty – employer offers of health insurance and STDI are highly correlated but unobserved in my data – there are several reasons to interpret the estimates in this paper as primarily the effects of maternity leave. First, in addition to the variation in timing of the anti-discrimination laws, my results rely on long-standing *cross-sectional* variation in access to STDI that stems from state-level differences in disability policy and industrial mix. This variation does not have a similar impact on medical coverage. Second, while the timing of STDI maternity benefits and the health insurance reform studied by Gruber (1994) often coincided, a handful of states adopted these policies at *different* times. In the appendix, I replicate Table 4 of Gruber (1994) and show that the wage effects are driven by states where STDI maternity benefits were adopted in concert with enhanced health insurance benefits, but were absent where health insurance benefits were adopted alone. Finally, even if we allow for some portion of the wage effects to be driven by health insurance costs, these estimates may still be interpreted as informative about an expansion of paid maternity leave since such policies generally entail the continuation of benefits such as health insurance along with wage replacement and job protection.[17]

[15]This null result is consistent with a literature that generally finds very small or no effect of maternity benefits on fertility (e.g., Baker and Milligan, 2014; Dahl et al., 2016). The notable exceptions in the literature exploit variation from much more generous policies that provide benefits for 18 months or or more and take place in settings with more comprehensive policies in place to support working parents or encourage fertility (Lalive and Zweimüller, 2009; Malkova, 2018; Raute, 2019).

[16]In Appendix section A.3 I report additional estimates from an exercise that tests for systematic relationships between state characteristics and the timing of the expansion of anti-pregnancy discrimination laws. I find little evidence that the timing of these state-level laws was correlated with state characteristics as measured in the 1960 Census.

[17]For example, the U.S. Family and Medical Leave Act (FMLA) requires employers to continue providing group health insurance coverage to workers while absent on leave.

4 Results

4.1 Short-run impacts on mothers

I first examine the effect of access to STDI maternity benefits on short-run leave-taking using retrospective data from the SIPP. Table 2 examines the effect on labor supply at various times relative to the birth of her first child. Each cell reports an estimate of τ_{SR} from equation 3.

Panel A of Table 2 tests whether the expansion of STDI maternity benefits altered the share of women who worked at any time during pregnancy. I interpret this as a test for STDI-induced selection into the labor force prior to childbirth. I find no evidence that first-time mothers with access to paid leave were more likely to work during pregnancy. In fact, I estimate a statistically insignificant 2.8 percentage-point *decrease* in the share working during pregnancy, and the 95% confidence interval rules out increases larger than 5 percentage points or 7%. Similarly, I see no statistically significant changes for several sizeable subgroups – white mothers, married mothers, and mothers with and without some college education.[18] These results provide little support for business groups' concern (see discussion in section 2.1) that the availability of paid leave would attract less-committed workers to the labor force.

Panel B of Table 2 examines the impact of STDI maternity benefits on work at different points during pregnancy and the child's first year of life. In these analyses, I restrict my sample to women who worked at any point during pregnancy. This restriction is made for two reasons. First, the SIPP did not measure post-childbirth labor-force attachment for women who did not work during pregnancy, so I cannot conduct a similar analysis for the full sample of women. Second, the restriction is also justified by the lack of evidence of selection into the workforce prior to pregnancy (see panel A) and the fact that women who did not work at all during pregnancy were unlikely to be eligible for STDI benefits.

The results show that there is no evidence of effects on that STDI maternity benefits altered the labor-market activity of mothers in the second or third trimester. However, during the first three months postpartum, access to STDI maternity benefits leads to a sharp and statistically robust decrease in the amount of time spent at work. This decrease is most pronounced for relatively advantaged groups – it is larger for white and married women, small and indistinguishable from 0 for women with no more than a high school education, and particularly large for women who attended at least some college.

A more detailed picture can be found in Figure 4, where I plot estimates of τ_{SR} by single month for all mothers who worked during childbirth. Here we see the largest drop in labor supply in months 1-3 postpartum. To get a sense of the impact on time spent at home in the aggregate, we

[18] I see no significant effects for nonwhite or unmarried mothers either, but these groups are so small that the 95% confidence intervals also cannot rule out implausibly large effects in either direction.

can simply sum the coefficients from months 0 through 5, the primary period during which women take maternity leave. This sum amounts to an intent-to-treat effect of -0.35 months, or about 1.5 extra weeks spent at home relative to the counterfactual. To get an estimate of the treatment effect on mothers who received STDI, I scale these figures by 0.4, my best estimate of the effect of the expansion of STDI maternity benefits on maternity benefit receipt.[19] This exercise suggests that women who received STDI benefits took about 3.8 weeks extra away from work on average.

Given that STDI generally provided only between 6 and 10 weeks of wage replacement, these figures suggest the benefits induced longer leaves of roughly two-fifths to two-thirds of the full length of time allotted. These estimates are similar in magnitude to the response to more recent expansions of leave policy. For example, in an analysis of California's 2004 paid family leave expansion, Rossin-Slater, Ruhm and Waldfogel (2013) estimate that an extra 6 weeks of paid benefits led to roughly 3 extra weeks of leave for new mothers, quite similar to the magnitude of my estimate.

Appendix Figure A3 shows estimates separately for white mothers, married mothers, and mothers split by education. I see little evidence of a response to the policy among women without any college education. On the other hand, women with some college experience responded most strongly of any group. Interestingly, I see evidence of an increase in labor supply in the month *before* childbirth for women with some college education. This suggests that even though the policy did not induce women into the labor force on the extensive margin, it did have an intensive-margin impact on their attachment during pregnancy, as women responded to the incentive to stay on the job through childbirth to preserve eligibility for STDI benefits. These more-educated women were also disproportionately likely to respond to the availability of paid leave by extending their time away from work in the months following childbirth. Women with some college experience take an extra 2.7 weeks away from work (6.7 more weeks for beneficiaries) during their children's first six months of life.

It is worth noting that due to data limitations, the estimates presented above are specific to *first-time* mothers. In Appendix C.1, I use the 1970 and 1980 decennial Census to examine effects on short- and medium-run employment and find effects are similar across birth parity.

[19]This figure is calculated as follows: Data from the 1984-1989 panels of the SIPP suggests roughly 18 percent of new mothers receive STDI benefits in universal-STDI states, but only 2 percent in other states (see Appendix Figure A1). While these estimates are known to be downward biased (see Meyer, Mok and Sullivan (2015) and Figure 2a), if the ratio of these two figures represents the true ratio, then administrative data on STDI receipt among mothers from New York and California suggests 3.3 percent of women in non-STDI states received benefits in the wake of the reform, $\frac{0.02\times0.3}{0.18} = 0.033$. The difference in the share of working women covered in the two groups of states is roughly 0.65, which suggests that providing access to paid leave to women results in a change in probably of receiving STDI maternity benefits of $\frac{0.3-0.033}{0.65} \approx 0.4$.

4.2 Medium-run impacts on mothers

The retrospective SIPP data also allows me to look at mothers' labor-market responses in the medium run – up to a year after the birth of the first child. In theory, maternity leave policies may increase the labor-market attachment of mothers by facilitating an institution in which they may take extended leave without quitting a job. However, labor-force attachment may also fall for a number of reasons – if, for example, the extended leave leads to deterioration in human capital, increased discrimination from employers, or even changes in preferences.

The final two rows of Panel B of Table 2 suggest that the availability of paid leave led to labor-market absences that extended even beyond the life of the STDI maternity benefits. The small size of my sample results in noisy estimates for most groups. The exception is among women with some college experience, where I find that mothers are 12 percentage points (25%) less likely to be at work 4-6 months after childbirth and 11 percentage points (21%) less likely to work during the second six months of the child's life.[20]

4.3 The effect of paid leave on women's wages

To examine broader impacts of the expansion of paid leave on the labor market, I turn to my sample of women and men ages 18-45 from the 1973-1987 CPS May extracts and Outgoing Rotation Group files. Because I am interested in the effect of paid maternity leave policy on the labor-market prospects of *all* women of "child-bearing age," regardless of decisions related to parenthood and fertility, I do not restrict these samples on the presence of children.

A simple model of labor-market responses to mandate maternity benefits would suggest that shifting labor supply and demand curves would yield downward pressure on women's hourly wages but ambiguous effects on employment. Figure 5a provides evidence in line with this theoretical prediction: The estimates show little evidence of a pre-trend in log wages – if anything, there is a slight upward trend in some specifications that works against finding a negative effect – but a steady decrease in the wake of the reform that remains statistically significant nearly a decade later. The estimates change little with the addition of several measures of the policy and economic environment – specifically, per-capita transfers from the Earned Income Tax Credit and other sources, real state GDP, and the existence of unilateral divorce laws. Finally, given that the details of STDI policies varied considerably from state to state, we may be concerned about the possibility that heterogeneous effects have an impact on my estimates. If so, we would expect the results to change substantially in an unweighted regression (Solon, Haider and Wooldridge, 2015),

[20] In my analysis using the decennial Census in Appendix C.1, I find a medium-run decline in labor-force participation among all mothers, consistent with the decrease in employment found for mothers by Bailey et al. (2019) and by several papers studying the European context (e.g., Lalive et al., 2014).

but in fact the estimates are reassuringly stable.

By contrast, Figure 5b shows little evidence that men in the same age group saw a substantial or long-lasting deterioration in their hourly wages. The trend is flat across all specifications, offering reassurance that the effect is driven by the combination of access to STDI and the enactment of anti-discrimination policies, rather than a more general shock that affected workers of all demographic backgrounds.

Table 3 explores these results further. In my baseline specification, women's wages fell by 5.6 log points in the first five years after the reform, and 5.9 log points in the second five years, underscoring the persistence of the decrease. These estimates are not sensitive to the addition of covariates or the choice of weighting scheme. In line with the visual evidence of a pre-existing, slight upward trend in Figure 5a, the addition of a linear pre-trend term increases the point estimate slightly to 5.9 log points. By contrast, effects on men's wages are much smaller and never statistically distinguishable from 0. In Appendix Table A9, I show that effects on wages are also absent for women and men age 46-64, who presumably were unlikely to be eligible for STDI maternity benefits.

Columns 5 and 6 of Table 3 explore whether these decreases in women's wages can be attributed to changes in selection – either on the extensive margin of labor supply or into particular types of work. In column 5, I add covariates for years of education attained, as well as indicators for high school and college completion. If the availability of STDI benefits drove differential selection into the workforce by skill level, I would expect these covariates to substantially attenuate the estimates. Instead, the effect on women's wages remains strongly significant both economically and statistically, suggesting that these changes in wages cannot be attributed to a change in composition of the labor force.

Another possibility is that the expansion of paid leave shifted the occupational distribution of working women – either because women sought out jobs that would offer STDI benefits or greater flexibility, or because firms funneled women into certain jobs. It is important to note that such effects would be of interest on their own, and I examine occupational shifts further below. Nevertheless, in column 5, I show that the addition of fixed effects for detailed occupation categories do little to alter the estimates – which remain at -4.4 log points – suggesting the wage effects occur within occupations rather than due to shifting across occupations.

4.4 The effect of paid leave on women's employment

Another important factor in the interpretation of these wage effects is the labor supply response of women in this age group. Table 4 examines this question by reporting estimated effects on employment and hours worked. The event-study versions of these estimates are shown in Figure

6. The first three columns of Panel A examine effects on employment for the full sample of women age 18-45. Column 1 suggests that employment fell by about 2.6 percentage points in the first five years and 3.9 percentage points in the second five years after enactment of paid leave. However, the results in columns 2 and 3 and panel A of Figure 6 underscore the sensitivity of these results. The addition of policy variables cuts the magnitude of the estimates nearly in half. Furthermore, the event-study results suggest that female employment was growing more slowly in states that adopted more generous STDI benefits, raising the question of whether these negative estimates can be explained by a negative pre-trend. In fact, accounting for a linear pre-trend cuts the estimated effect on employment by about half in the short run and one-third in the long run (column 3 of Table 4). Columns 4 through 6 demonstrate a similar pattern for the measure of hours worked in the previous week: While point estimates are uniformly negative, they are sensitive to the specification, and in particular they tend to greatly diminish in both economic and statistical significance when I adjust for pre-existing trends.

How do we interpret these estimated effects on employment? The negative effects suggest that employers' concerns about the cost of maternity leave shifted labor demand in a way that overwhelmed any countervailing increases in women's labor supply. Meanwhile, the sensitivity of these results suggests caution in interpreting them causally. One possibility suggested by the event-study estimates in Figure 6 is that the adoption of maternity benefits led to a stark drop in female employment that slightly preceded the policy; this is plausible if firms and workers anticipated the policy, which would certainly be in line with the historical evidence presented in section 2.1. The second possibility is that the employment estimates are picking up differential, pre-existing trends. If so, the specification in column 3 may be preferable because it captures *deviations* from the linear pre-trend. Unfortunately, the data do not provide definitive evidence as to which of these stories is most credible. However, one feature of the results worth noting is that the effects are uniformly larger in the long run than in the short run. In fact, the larger long-run effects are in line with the evidence in Figure A4 showing that STDI benefits decreased mothers' labor force participation. Since a relatively small share of women give birth in a given year, we would expect this pattern to generate negative employment effects, but only with a delay as medium-run effect on mothers accumulates.

4.5 The effect of paid leave on women's family income

Given that I observe a significant decrease in women's hourly wages and employment but relatively little change in men's labor-market outcomes, a natural question is whether these effects translated to changes in family income. My sample from the CPS offers a rough measure of family income in the subset of years from 1972-1981 (excluding 1973). In this time frame, I observe a categorical measure of ranges of nominal income. I take advantage of this data by creating a series of indicator variables that take the value 1 if family income is higher than a given threshold, and 0 otherwise. I then use each of these indicators separately as the dependent variable in equation 3.[21] This approach traces out the effect of STDI maternity benefits on the entire distribution of family income.

Figure A5 plots separate estimates of τ_{SR} for each threshold available in the CPS, and shows that the deteriorating wages and employment of women age 18-45 translated into a decrease in family income that was concentrated among middle-income families. I see little effect on family income at the lowest thresholds, and the largest impact at \$15,000-\$20,000. This suggests that family income was affected most in exactly the families where women were more likely to take up STDI maternity benefits – those in the middle of the distribution. This finding aligns with the pattern that take-up of benefits was largest among women with some college experience (see Appendix Figure A1) and wage effects were larger among women with higher levels of education (see Appendix Table A7).

4.6 Mechanisms

The results above suggest that the enactment of STDI maternity benefits led to a substantial decrease in the hourly wage paid to women, and that these effects were not due to differential selection into the labor force or shifts in occupation. What, then, can explain these patterns?

One possibility, reflected both in the public debates described in section 2 and the academic literature on the economics of mandated benefits (Summers, 1989; Gruber, 1994), is that the expansion of STDI maternity benefits drew more women into the labor force, lowering wages. Even in the absence of such a large increase in the *size* of the labor force, a more nuanced explanation could involve a change in the *composition* of the workforce: The amenity value of publicly provided or mandated benefits may attract workers who are relatively less attached to the labor force or have lower levels of human capital. Finally, another possibility is that the longer leaves taken by STDI-eligible mothers results in a fundamental shift in parents' preferences for work, leading to a shift toward part-time or other less lucrative work (Kuziemko et al., 2018; Bailey et al., 2019).

While I cannot rule out these supply-side factors entirely, I argue that they are inconsistent with the bulk of the evidence on the evolution of the labor market during this time period. First, note that the null or negative effects on employment, as discussed in section 4.4, suggest that any increase in willingness to work was muted at best and dominated by other factors. Furthermore,

[21] Since my data go only through 1981, in this case τ_{SR} measures the impact only in the first three years after enactment of STDI maternity benefits, rather than the first five years, relative to the three years prior.

there is little evidence that the observable characteristics of the female labor force changed in ways that would predict lower hourly earnings. Although some specifications suggest that more-educated women saw larger employment decreases (e.g., column 3 of panels B and C of Table 4), the difference is neither statistically significant nor large enough to explain a substantial decrease in hourly earnings.[22] Nor did working women appear to move toward lower-paying, part-time work, as shown in panels C and D of Figure 6.

Another important way that STDI maternity benefits may have altered the composition of the labor force via changes in supply is through its effect on mothers and women who expect to soon become parents. We can consider two ways maternity benefits may have altered labor supply. First, to the extent that women saw paid maternity benefits as a valuable amenity, the reform may have drawn more women into the workforce *prior* to childbirth. This could lower hourly wages in the aggregate if this population of women has less experience and places more value on amenities like flexibility than incumbent workers (Goldin, 2014; Cortés and Pan, 2018). Second, since STDI maternity benefits provided an opportunity to spend more time away from work after childbirth, it may have led to a shift in preferences or otherwise altered women's willingness to continue working during a child's early years. This latter effect could have either positive or negative impacts on employment. For example, employment among parents may rise if the availability of leave encouraged job retention (Klerman et al., 1997), or it may fall if the experience serves as an information shock or discourages future career investment (Kuziemko et al., 2018; Bailey et al., 2019).

While my main analysis dataset lacks information on children in the household, and therefore limits my ability to examine mothers' labor-market activity, in the appendix I provide evidence from two alternative datasets that suggests mothers' responses in the time around childbirth are also unlikely to explain the wage effects I observe. First, I construct a sample of women with a newborn child in the household using the 1963-1988 March CPS. I focus on two outcomes: Weeks worked in the previous year, which I interpret as an indicator of work history prior to the birth of the child, and current employment. Appendix Figure A16 shows event-study estimates; since my sample sizes are small, I pool event time into two-year bins to improve precision. Panels A and B show there is little evidence of an increase in work the previous year, suggesting the benefits did little to attract workers prior to childbirth. On the other hand, the share employed *after* childbirth falls modestly. Although these estimates are noisy, the magnitudes are similar to the changes in employment I find among all women age 18-45 in Table 4.

[22] The p-value for a test of no difference between the estimates in column 3 of panels B and C are 0.31 for the short run and 0.15 for the long run. Regarding magnitudes, Table 1 shows that only 25% of women in my CPS sample attended any college. From Table 4, 66% of women with some college were employed, relative to only 53% with a high school degree or less. Taking these as baseline figures and comparing to a 4.5 percentage-point decrease in employment for college-educated women and a 2.6 percentage-point decrease for women without college experience (the long-run estimates from column 3 of Table 4), the share of the female workforce with some college fell from 29.3% to 28.9%.

These results are echoed in Appendix Table A4, which reports estimates from my second alternative sample, which is made up of mothers of newborn children age 0-3 months from the 1970 and 1980 Census. The large sample and relatively precise information on children's place and date of birth allow me to evaluate labor-market impacts with more precision, although without the benefit of annual data. Similar to the results from the March CPS, panel A shows that STDI maternity benefits did not increase labor force participation prior to childbirth: I find a statistically insignificant, 1.9 percentage-point *decrease* in employment in the calendar year prior to birth. I find no effect on employment in the full sample, but a large and statistically robust increase in employment among women who worked the previous year – and who were therefore likely to be eligible for STDI benefits. However, these employment effects did not persist: Column 5 of panel F shows no employment effect among women with slightly older children (i.e., 3-6 months of age), suggesting STDI maternity benefits led to only a short-lived increase in employment possibly driven by mothers who exit after taking leave.

Overall, these results suggest little reason to believe STDI maternity benefits induced large compositional changes in the labor force. Contrary to the concerns of the industry representatives who testified before Congress in opposition (see section 2), these benefits did not appear to draw women into the labor force prior to childbirth. Nor did employment rise among mothers of young children; if anything, employment fell, an effect that we might expect to *increase* wages given the reduction in earnings that mothers experience in the United States and other developed countries (Angelov, Johansson and Lindahl, 2016; Kleven, Landais and Søgaard, 2019).

While I find little decisive evidence of a shift in labor supply, an alternative explanation of my findings is that firms responded to the enactment of STDI maternity benefits by reducing demand for women's labor. A final analysis provides suggestive evidence that these effects were, in fact, driven by firms' response to the perceived cost of providing paid leave. To the extent that firms viewed flexible maternity-leave policies as a costly amenity for female workers, we would expect these costs to be passed on in the form of lower wages. Furthermore, we would expect the effect to be largest in firms or occupations where absence and turnover is especially costly. I explore this possibility by constructing an occupation-specific measure of the cost of flexibility, and then defining variable $Cost_o$ as a binary indicator for occupations with higher-than-median costs. I then interact this indicator with the key parameters in specification 3 to separately estimate the effect τ_k^F on "flexible" occupations and the effect τ_k^I on "inflexible" occupations:

$$\begin{aligned} y_{iost} = & Cost_o \times \left[\tau_{SR}^I STDI_s \mathbb{1}\{0 \leq k \leq 4\} + \tau_{LR}^I STDI_s \mathbb{1}\{5 \leq k \leq 9\}\right] \\ & + (1 - Cost_o) \times \left[\tau_{SR}^F STDI_s \mathbb{1}\{0 \leq k \leq 4\} + \tau_{LR}^F STDI_s \mathbb{1}\{5 \leq k \leq 9\}\right] \\ & + \psi Cost_o + X_{iost}'\beta + \delta_s + \theta_{r(s)t} + \varepsilon_{iost} \end{aligned} \quad (4)$$

I construct three measures of $Cost_o$ using different sources of data. First, I follow Hudomiet

(2015) to measure occupation-specific "adjustment costs" using data from the Multi-City Study of Urban Inequality (MCSUI), which surveyed employers in four U.S. cities between 1992 and 1994. The MCSUI asked employers how long a new employee would take to become fully productive if hired into a given occupation. While the MCSUI is drawn from a different universe than my own, this question is valuable because it speaks directly to the productivity losses that a firm would expect if an employee took an extended leave or quit.

My second proxy for the cost of absence and turnover is motivated by literature on the wage premium earned by workers who are willing to supply long, inflexible hours (Goldin, 2014; Cortés and Pan, 2018). Using a sample of prime-age men from the March CPS (Ruggles et al., 2017), I regress the log of wage earnings on weeks worked per year and the log of hours worked interacted with occupation. I interpret the coefficient on the occupation-specific hours worked variable as the elasticity of annual income with respect to weekly hours, a measure of the return to the willingness to work long hours.

My third proxy draws on Census Bureau occupation codes. I code occupations as high-cost if they fall under "managerial and professional occupations" as defined by the standardized 1990 occupation code structure from IPUMS (Ruggles et al., 2017).

One potential concern about interpreting these variables as proxies for job inflexibility is that they may in fact be correlated with characteristics of the worker, such as unobserved skills. It is thus noteworthy that while each of these three proxies is designed to the capture features related to occupation-specific cost of turnover and absence from work, they are only modestly correlated with one another. My measure of professional occupation is modestly correlated with high adjustment-cost occupations (correlation coefficient 0.39) and high-returns-to-hours occupations (0.33). However, the correlation between the high-returns and high-adjustment-cost measures is only 0.07. These measures also cut across the educational distribution: The correlation between an indicator for some college experience and each measure is 0.29 for adjustment costs, 0.11 for returns to hours worked, and 0.33 for professional occupations. The limited overlap provides reassurance that the three variables are not simply capturing the same unobserved worker characteristics, but rather alternative measures of the flexibility of the job.[23]

My estimated effects for the first five years after enactment of the pregnancy discrimination law are shown graphically in Figure 7. Panel A examines whether women were more likely to move into a less flexible occupation using estimates from equation 3. While the point estimates are negative, I cannot rule out null effects, suggesting that shifts across occupation were small if they occurred at all. This finding is consistent with the results in Table 3, which showed that decreases

[23] I also explore heterogeneity in wage effects by three other characteristics: Educational attainment, predicted labor-force attachment, and the share male in an occupation. Wage effects are larger in the short run for women with college experience and with high predicted labor-force participation, consistent with the effects loading onto populations that were more likely to take advantage of STDI maternity benefits. Results are available in Appendix Tables A7 and A8.

in women's wages were a within-occupation phenomenon.

Panel B suggests that rather than shifting women out of inflexible jobs, the expansion of STDI maternity benefits led to a decrease in pay for women in these occupations. In all three categories – professional jobs, jobs with high returns to weekly hours, and jobs with high adjustment costs – I see large negative effects on hourly wages. The effects on occupations in the "more flexible" category are also negative but statistically indistinguishable from 0. Viewed in concert with the results in Table 3 and Table 4, these findings suggest that the benefits provided to women through the expansion of paid leave were offset to some extent by a decrease in employers' willingness to pay female workers.

In fact, while sizeable, the decreases in female wages documented above are broadly consistent with pass-through of reasonable estimates of firms' cost of providing paid leave. Data from the state of New York suggests that the average STDI maternity benefit paid between 1978 and 1985 was \$3,484 in 2019 dollars.[24] This suggests that a conservative estimate of the expected *direct* cost of providing STDI maternity benefits is at least one-half of 1 percent of a female worker's expected annual earnings.[25] However, this figure can be viewed as a lower bound on the total cost of providing benefits. A full accounting would include not only the direct cost of the benefits paid to the mother, but also any effects on productivity and turnover. Previous literature has found that employee turnover imposes substantial costs on firms as diverse as clothing retailers (Kuhn and Yu, 2021) and hospitals (Bartel et al., 2014). Manning (2011) surveys the literature on hiring costs and concludes that and estimate of 5 percent of total labor costs is "in the right ballpark." Boushey and Glynn (2012) survey case studies that capture a more comprehensive set of turnover costs – including recruitment and lost productivity – and find a median of around one-fifth of annual salary, and one-sixth for occupations where the typical worker earns less than \$30,000. These estimates suggest the firm's expectation of the cost of absence and turnover due to paid leave could easily reach the magnitude observed in my analysis, even without considering other sources of wage losses, such as discrimination.

[24] Data on STDI pregnancy benefits paid nationally is generally not available. I use New York's figures because the benefit amounts were relatively modest (50 percent of weekly earnings up to a cap) and the state Workers Compensation Board provided reports that include claims, average length, and total payments by year.

[25] The direct cost of STDI maternity benefits is calculated as follows. According to data from the Current Population Survey, about 1 in 20 working women age 18-45 gave birth in 1976. The expected STDI maternity payment to female employees over the course of a year was therefore $0.05 \times 3,484 = \$174$. Among this same CPS sample, average weekly earnings were \$626 in 2019 dollars. Assuming women who do not give birth would work 52 weeks, the expected STDI maternity benefit is thus 0.53% of expected annual earnings. Note that this is a lower bound on the direct cost of providing benefits because New York's replacement rate was relatively low, women who actually gave birth in 1976 tended to have lower weekly earnings, the assumption of 52 weeks worked likely overstates annual earnings, and the calculation ignores any loading factor charged by the insurer.

5 Conclusion

Even as the public and policymakers grow increasingly interested in policies that provide paid time off for the parents of newborn children, the academic literature has struggled to generate empirical evidence on the labor-market implications of parental leave. These policies are often justified in part on the theory that, by giving mothers more flexibility around the time of a child's birth, they will promote labor-force attachment and gender equity in the long run. However, parental leave policies also have the potential to create unintended consequences.

This paper contributes to this literature by studying a series of state and federal laws that expanded paid leave benefits to millions of American women in the form of STDI. While most empirical evidence on the labor-market impacts of paid leave comes from relatively generous expansions in countries with relatively rich support for working parents, this setting provides an opportunity to study the impact of a relatively modest expansion of benefits – the payments amounted to between one-half and two-thirds of usual wages for 6-12 weeks, depending on the state and employer – in a setting with very few policies designed to support working parents.

I find that the expansion of benefits dramatically increased the amount of time women spent at home after childbirth. Mothers who were eligible for paid leave benefits spent nearly 4 weeks extra at home relative to mothers who gave birth too early to collect them. As expected, this extra time away from work is concentrated in the first few months of the child's life. More surprisingly, the benefits also seem to delay the return to work for some mothers even as long as a year after the child's birth.

I also show that the expansion of STDI maternity benefits had broader implications for the labor market. Hourly wages among women of child-bearing age fell by about 5-6 log points after paid leave became more widely available. I also find a reduction in employment for women in this age range, although these results are more sensitive. In addition, I find evidence that these wage and employment decreases resulted in a decrease in family income for women that was concentrated in the middle of the income distribution.

The policy implications of these results depend on the mechanism by which STDI maternity benefits led to a decrease in women's labor-market outcomes. While I cannot rule out any mechanisms with certainty, I providence evidence that these effects cannot be explained by changes in occupation or differential selection into the labor force, e.g., among women who saw paid leave as a valuable amenity and responded by entering or remaining in the labor force. However, the reduction in wages is concentrated in occupations where the cost of employee absence and turnover is likely to be highest. All in all, these results are most consistent with the conclusion that the expansion of paid leave led to shifts in labor demand, which in turn passed the perceived cost of maternity leave on to women of child-bearing age in the form of lower wages.

It is important to acknowledge that these estimates do not necessarily shed light on the welfare effects of an expansion of paid leave benefits, which depend on women's valuation of maternity benefits as well as effects not examined here, such as impacts on mothers' health (Bullinger, 2019; Persson and Rossin-Slater, 2019; Bütikofer, Riise and Skira, 2021). However, they provide new evidence that policies designed to provide additional flexibility to parents can – at least in certain contexts – exacerbate gender gaps in the labor market.

The extent to which such unintended consequences would accompany an expansion of paid parental leave in the United States today is an open question. The policy studied in this paper expanded a relatively modest level of maternity benefits to a large and diverse group of American women, many of whom previously had no access to maternity leave at all. In other words, the "treated" population looks in some ways very much like the population that would benefit from a national family leave policy today, when fewer than one in four workers have access to a formal paid leave policy (U.S. Bureau of Labor Statistics, 2019) and the most prominent proposals for national paid-leave policies offer wage replacement rates and durations that are strikingly similar to the generosity of STDI in the 1970s (Sholar, 2016; Konish, 2018; Nova, 2021; Peck, 2022). Furthermore, the consequences of expanding paid leave through STDI may be of interest given that policymakers often turn to social insurance programs to fund parental leave, as Canada did when adopting maternity benefits under the umbrella of unemployment insurance in 1971 (Trzcinski and Alpert, 1994).

At the same time, 40 years of evolving cultural attitudes and labor-market institutions may result in an environment that more easily accommodates an expansion of paid leave. Women in the United States comprise a larger share of the workforce, as labor force participation for 25- to 35-year-old women has surged at least 20 percentage points since the 1970s (Goldin and Mitchell, 2017). Women are also more likely to occupy corporate positions where they hold sway over firms' approach to managing a workforce on matters such as hiring, wage-setting, and accommodating family leave. This may be an important consideration given the evidence in this paper that firm responses drove the deterioration in women's wages and employment. Finally, policymakers today have a wider menu of options to support parents on leave, including policies that encourage fathers and other caregivers to contribute (Bartel et al., 2018; Patnaik, 2019). In any case, an examination of the expansion of STDI maternity benefits suggests that policymakers will need to consider the potential for unintended consequences when designing the next generation of parental leave policies.

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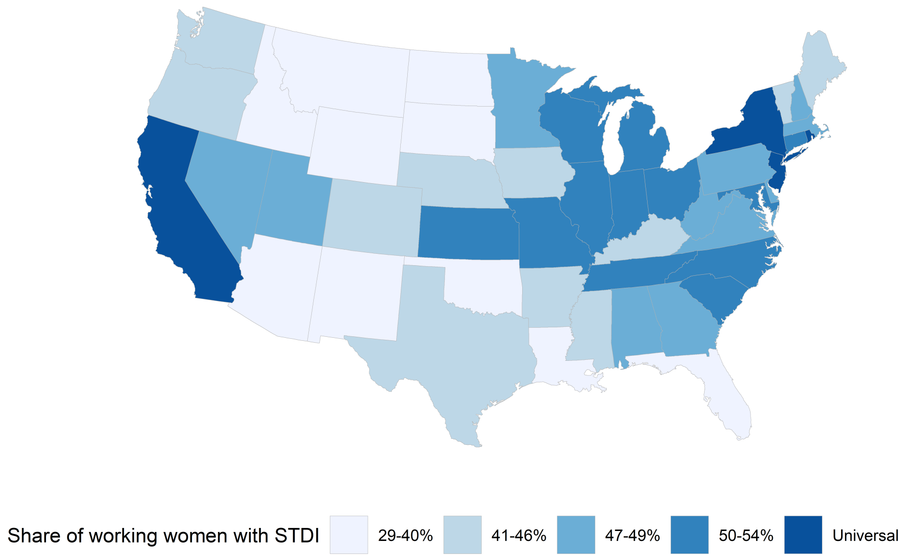
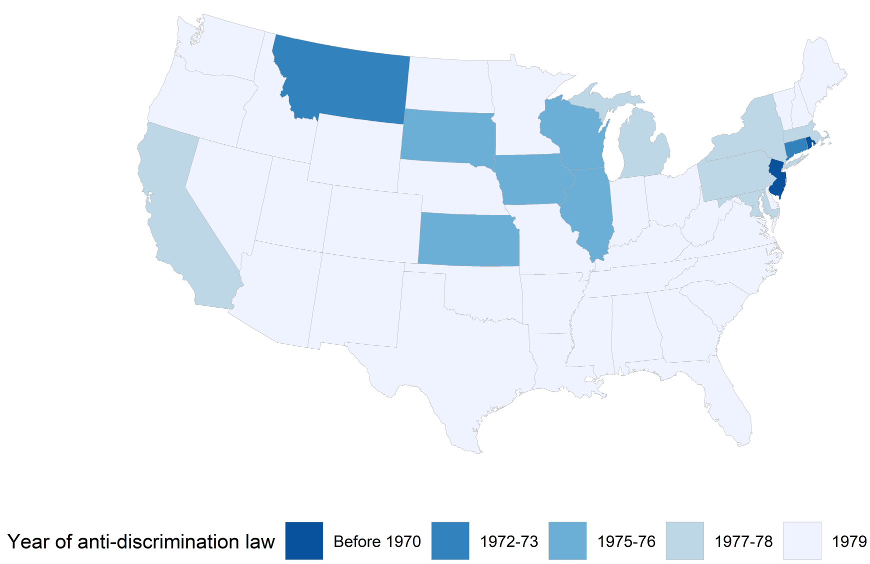
Figures and Tables

Figure 1: Variation in access to paid maternity leave through short-term disability insurance

(a) Estimated share of working women with STDI coverage, 1976



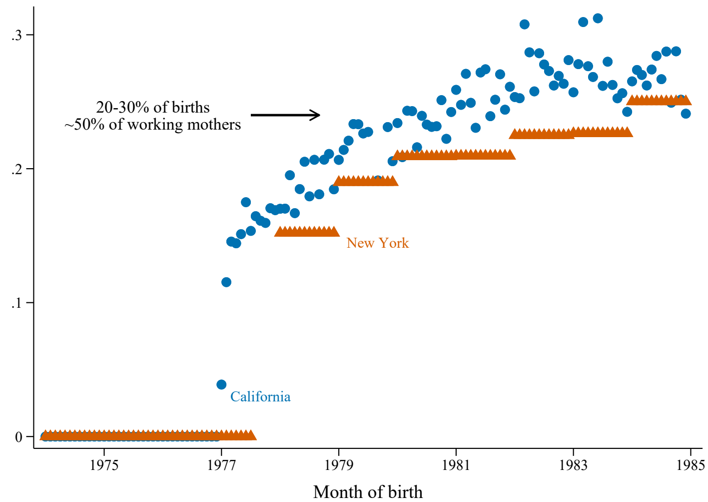
(b) Timing of state pregnancy discrimination laws



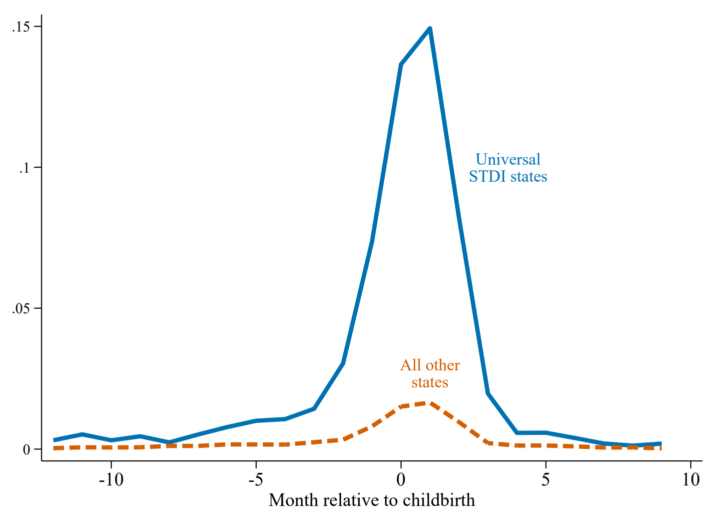
Notes: Share of women with STDI coverage is estimated using sample of working women age 18-45 in the 1976 Survey of Income and Education, using employer- or union-sponsored health insurance coverage as a proxy for employer- or union-sponsored STDI. See Appendix section A.1 for details. See Appendix section A for details regarding the state-by-state enactment of pregnancy discrimination laws. States where federal Pregnancy Discrimination Act was binding are shown as enacting the law in 1979. States not shown are Alaska (estimated 40% coverage, 1975 adoption of pregnancy discrimination law) and Hawaii (universal coverage, 1973 adoption).

Figure 2: Varying take-up of STDI pregnancy benefits over time and across states

(a) Share of new mothers receiving STDI benefits in two high-coverage states

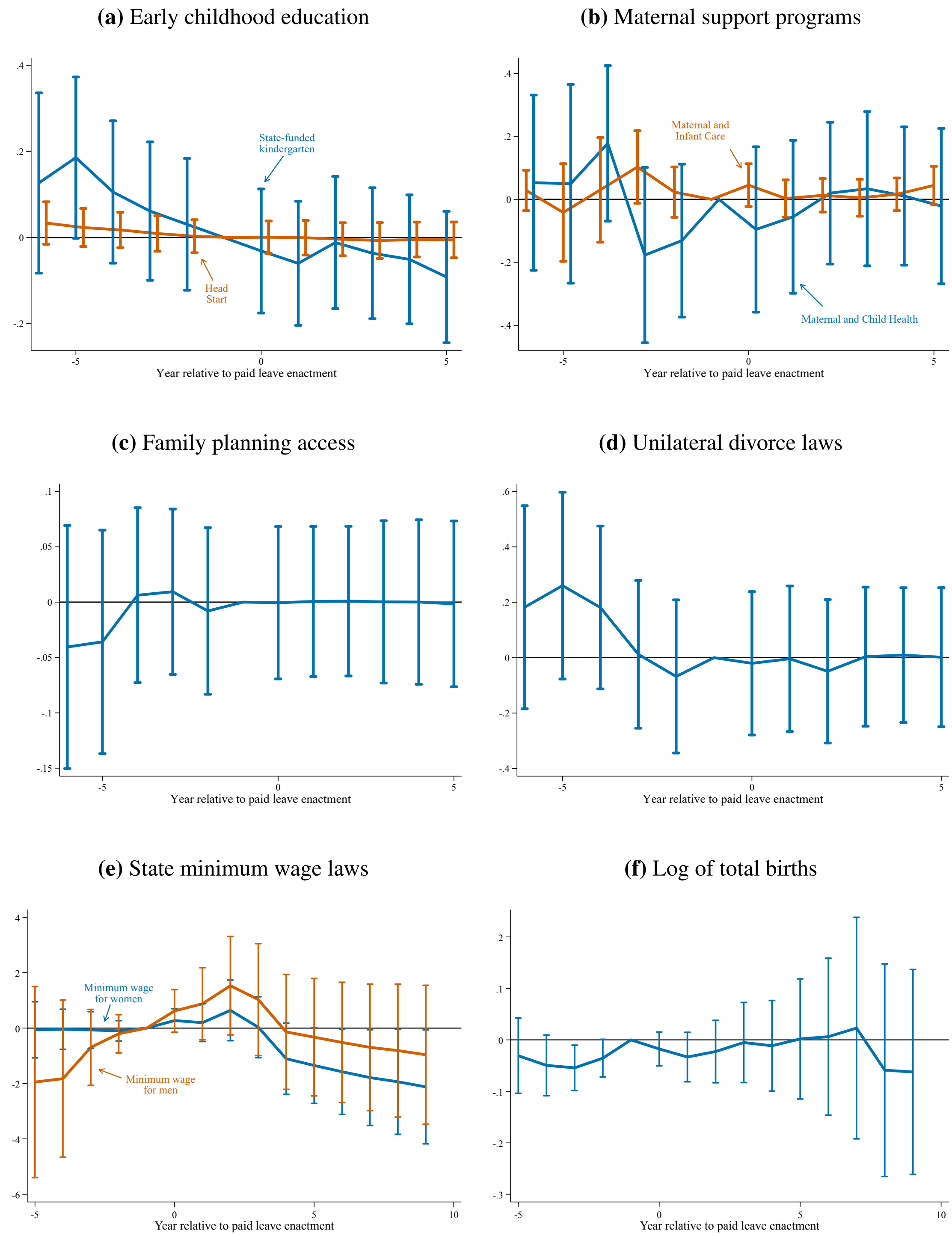


(b) Take-up of STDI benefits was higher in high-coverage states



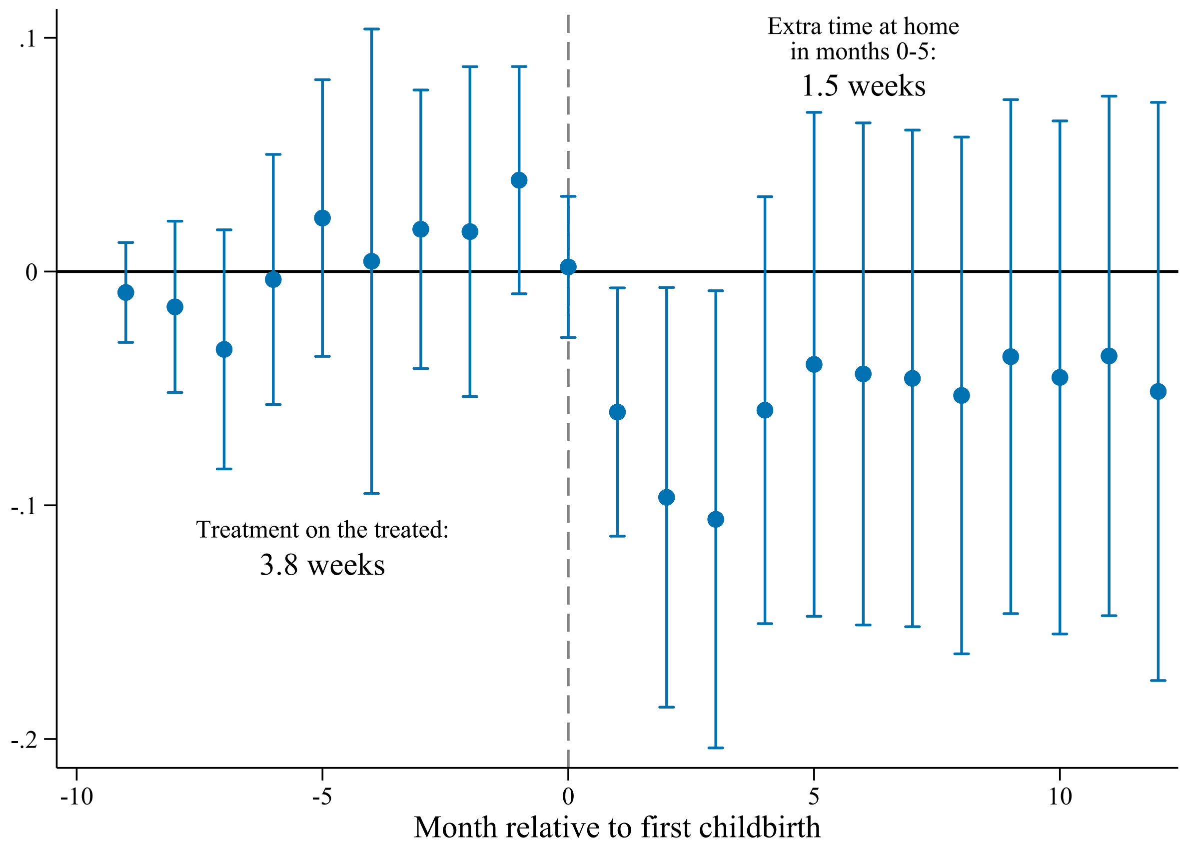
Notes: STDI maternity benefits were distributed beginning in January 1977 in California and August 1977 in New York. Data in Figure 2a is constructed by dividing the number of STDI pregnancy claims by month or year in California and New York by the number of births to residents of those states. STDI pregnancy claims provided by California Employment Development Department and New York Workers Compensation Board. Birth records come from Natality Detail Files accessed via ICPSR. Data in Figure 2b comes from sample of women age 18-45 who gave birth during the 1984-1989 panels of the Survey of Income and Program Participation. Solid line shows share of women receiving STDI maternity benefits, by month relative to childbirth, in the universal-STDI states of California, New York, New Jersey, Hawaii, and Rhode Island. Dashed line shows share receiving benefits by month in all other states.

Figure 3: Evaluating the internal validity of the expansion of STDI maternity benefits



Notes: Panels show estimates of τ_k from equation (1) where dependent variable is a measure of exposure to policies that may affect women's labor-market outcomes and fertility and family formation decisions. Outcomes drawn from publicly available data on state-funded kindergarten (Cascio, 2009), Head Start (Bailey, Sun and Timpe, 2021), federally funded Maternal and Child Health or Maternal and Infant Care programs (Bailey and Danziger, 2013), family planning programs (Bailey, 2012), state unilateral divorce laws (Gruber, 2004; Wolfers, 2006), state minimum wages (Vaghul and Zipperer, 2016; Derenoncourt and Montialoux, 2021), and Natality Detail File from ICPSR (National Center for Health Statistics, 2015). See Appendix A.2 for more details on data sources and construction of outcome variables. Regressions run at the state-year level, weighted by state population. Error bars show pointwise 95% confidence intervals constructed using standard errors clustered at the state level.

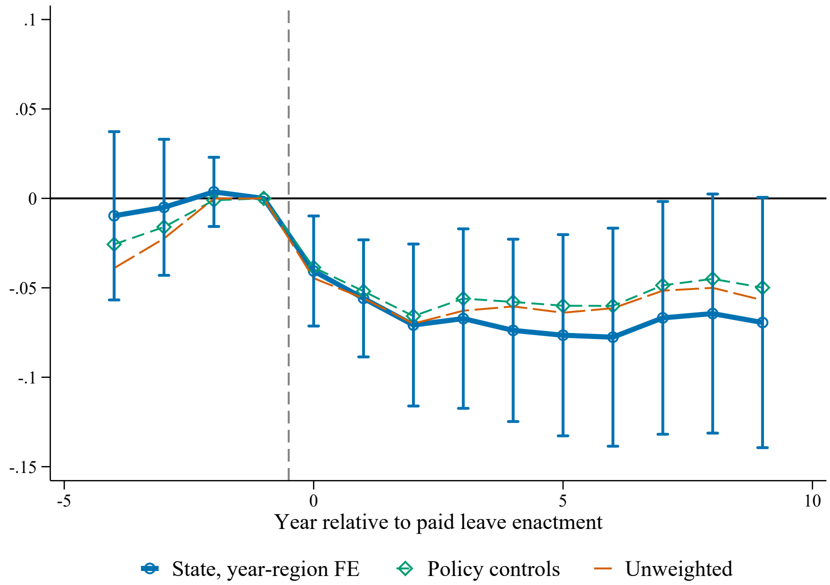
Figure 4: Effect of paid leave on mothers' labor supply in months around childbirth



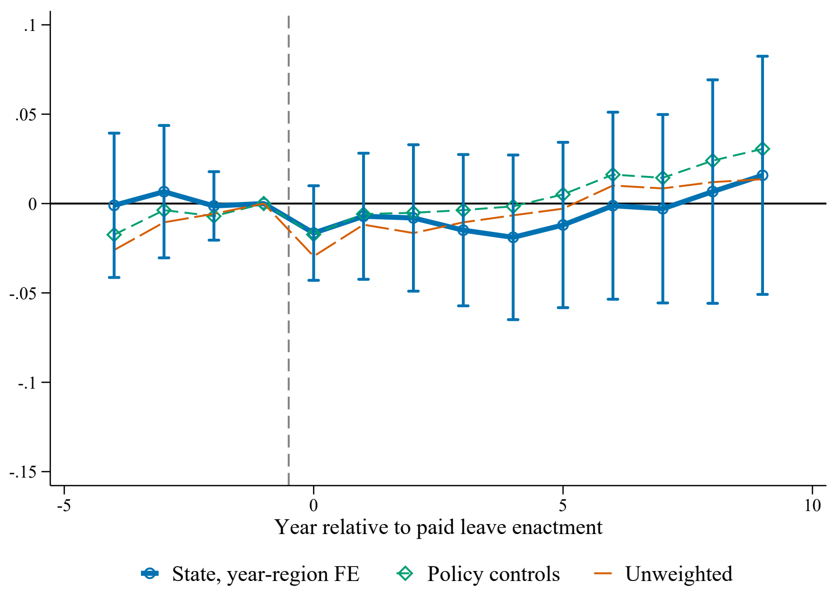
Notes: Data includes women from the retrospective fertility module in the 1984 and 1985 SIPP. Sample is limited to women whose first child was born between 1970 and 1985 while between the ages of 18 and 45. Women are asked about labor supply by month only if they worked during their first pregnancy. Figure shows intent-to-treat estimates of STDI exposure on time spent at work by month relative to childbirth, using a version of equation (1) that restricts event time to dummies indicating birth before or after the reform. Standard errors in are clustered at the state level.

Figure 5: Effects of paid leave on hourly wages

(a) Women age 18-45

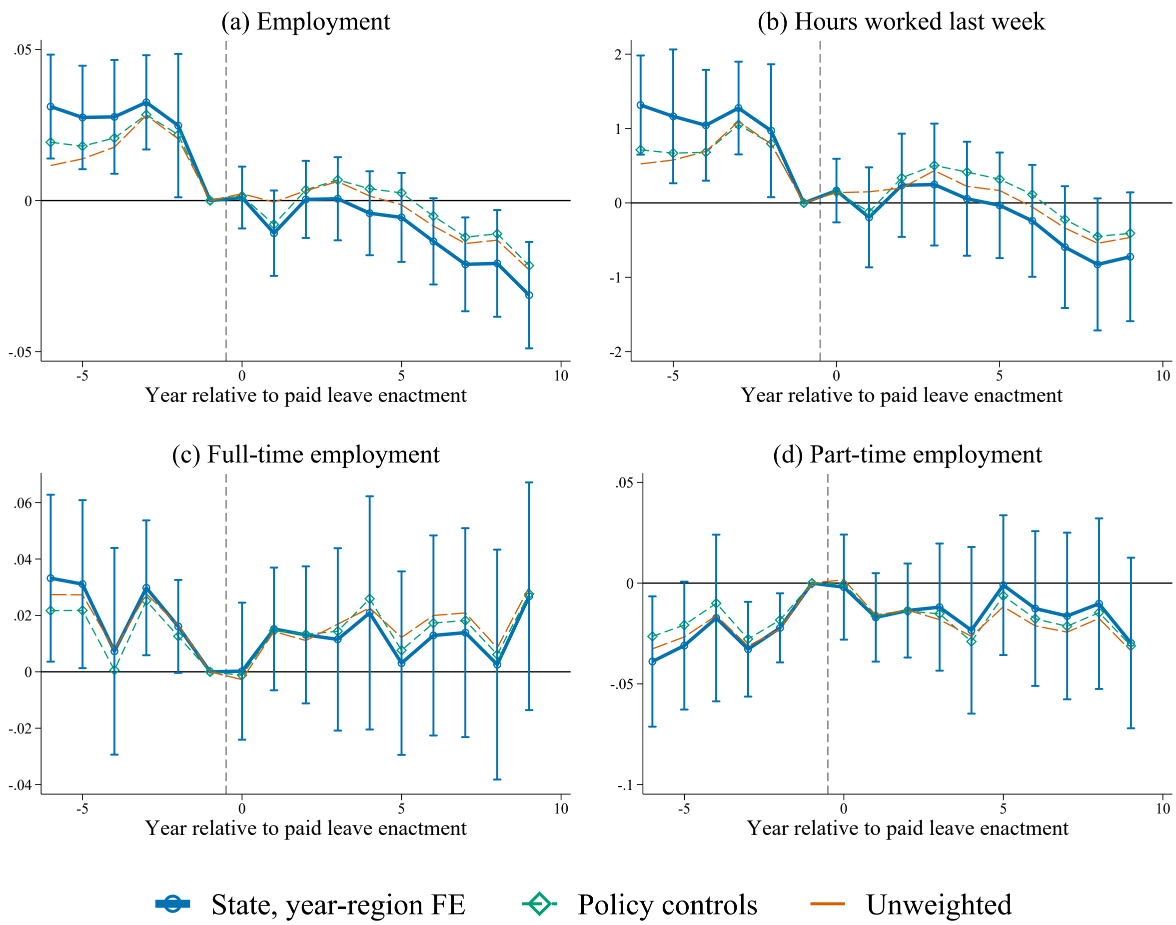


(b) Men age 18-45



Notes: Graph shows event-study estimates from equation (1). Figure 5a shows effect on log of hourly wages in a sample of women age 18-45, and Figure 5b shows effect on log of hourly wages in a sample of men age 18-45. Data drawn from the 1973-1987 May CPS and 1979-1987 Merged Outgoing Rotation Group files. Following Lemieux (2006), sample excludes self-employed and farm workers, as well as wages greater than \$100 or less than \$1 in 1979 dollars. All regressions use CPS sampling weights unless otherwise specified. Dependent variable is the natural log of the reported hourly wage, converted to 2019 dollars using the CPI. Basic specification includes fixed effects for year, month, state or state group, and a quadratic in age interacted with indicator of nonwhite race and Hispanic ethnicity. Policy controls include measures of income transfers per capita, EITC payments per capita, real state GDP per capita, and an indicator for state-level unilateral divorce laws. Standard errors are clustered at the state-group level.

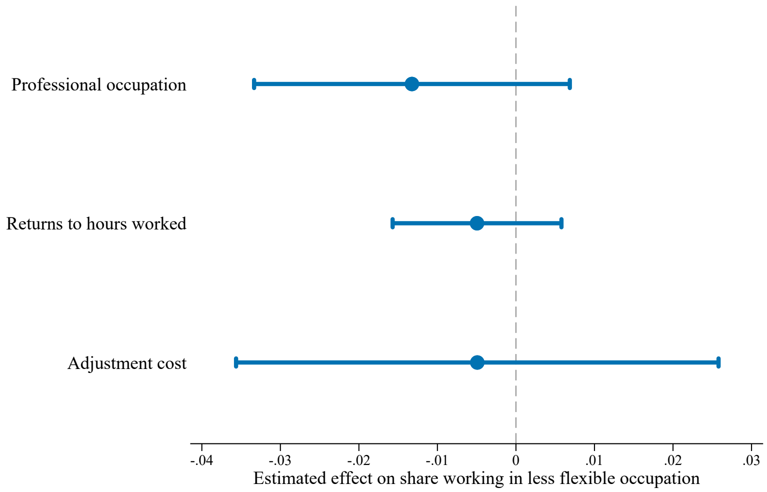
Figure 6: Effect of STDI maternity benefits on women's employment and hours worked



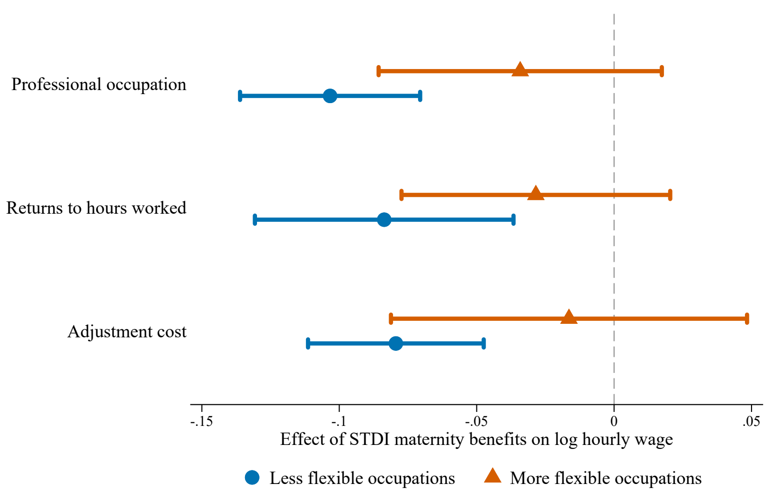
Notes: Figure shows event-study estimates from equation (1) using sample of women and men age 18-45 from the 1969-1987 CPS May extracts and MORG files. Employment is a CPS-generated binary indicator for having a job in the reference week. Hours worked are measured as of the previous week. Full-time and part-time employment are measured conditional on employment, and respondents with 35 or more hours worked in the previous week are considered full-time workers. Standard errors are clustered at the state-group level.

Figure 7: Heterogeneity in the effect of STDI maternity benefits on women's labor-market outcomes

(a) Effect on probability of working in an inflexible occupation



(b) Effect on women's hourly wages by occupational flexibility



Notes: Graph shows estimate of τ_k from equation 1 for event-time k pooled into the first five years after enactment of STDI maternity benefits. Error bars show 95% confidence interval constructed using standard error that is clustered at the state-group level. In Figure 7a, dependent variable is a binary indicator for working in a less flexible occupation, defined as an occupation that is above the median in one of three measures of the cost of employee absence: Professional jobs, the return to working more hours per week, and the length of adjustment time necessary for a new employee to become fully productive. See section 4 for more details on construction of these measures of flexibility. Data drawn from the 1973-1987 May CPS and 1979-1987 Merged Outgoing Rotation Group files. Following Lemieux (2006), sample excludes self-employed and farm workers, as well as wages greater than $100 or less than $1 in 1979 dollars. All regressions use CPS sampling weights. Specification includes fixed effects for year interacted with Census region, month, state or state group, and a quadratic in age interacted with indicator of nonwhite race and Hispanic ethnicity.

Table 1: Summary statistics

	(1) SIPP mothers	(2) CPS women	(3) CPS men
Age	23.8	30.3	30.4
	(4.08)	(7.82)	(7.85)
Nonwhite	0.12	0.15	0.12
	(0.32)	(0.35)	(0.33)
HS graduate	0.89	0.82	0.81
	(0.31)	(0.38)	(0.39)
Some college	0.41	0.25	0.25
	(0.49)	(0.05)	(0.05)
College graduate	0.19	0.16	0.21
	(0.39)	(0.37)	(0.41)
Married	0.83	0.63	0.59
	(0.38)	(0.48)	(0.49)
Employed	0.67	0.61	0.84
	(0.47)	(0.49)	(0.36)
Hours worked last week	.	20.33	34.68
	(.)	(19.73)	(20.73)
Hourly wage (2019$)	.	16.03	22.91
	(.)	(9.12)	(13.45)
Observations	4,401	1,373,712	1,254,487

Notes: Tables shows sample means (standard deviations) from the main analysis samples from the 1984-1985 Survey of Income and Program Participation (SIPP) fertility and migration modules and the Current Population Survey (CPS) May and Multiple Outgoing Rotation Groups, 1969-1987. *SIPP mothers*: Sample includes women who report giving birth to their first child between 1970 and 1985. Age and marital status are measured as of first birth, and employment is an indicator for being employed at any point during the first pregnancy. *CPS women and men*: Sample includes women (column 2) and men (column 3) between ages 18-45. Employment is an indicator for holding a job in the reference week. Hourly wage is collected starting in 1973 and is not available from labor force nonparticipants. In addition, following Lemieux (2006), I use reports from both hourly and salaried workers, and I drop imputed hourly wages and values less than $1 or greater than $100 in 1979 dollars. Resulting sample sizes are 570,902 (column 2) and 658,639 (column 3).

Table 2: Effect of access to STDI benefits on the share of women at work, by time relative to first childbirth

	(1) All	(2) White	(3) Married	(4) HS only	(5) College
Panel A: All first-time mothers					
Anytime during pregnancy	-0.028	-0.057	-0.053	-0.035	-0.039
	(0.040)	(0.045)	(0.045)	(0.045)	(0.061)
	[0.68]	[0.71]	[0.71]	[0.60]	[0.79]
Observations	4,368	3,867	3,614	2,565	1,802
Panel B: Conditional on ever working during 1st pregnancy					
Second trimester	0.008	-0.000	-0.020	-0.005	0.003
	(0.032)	(0.033)	(0.032)	(0.042)	(0.044)
	[0.86]	[0.86]	[0.87]	[0.83]	[0.89]
Third trimester	0.025	0.009	0.006	-0.019	0.050
	(0.022)	(0.025)	(0.028)	(0.056)	(0.036)
	[0.52]	[0.52]	[0.53]	[0.49]	[0.55]
Child age 1-3 months	-0.087**	-0.106***	-0.107***	-0.038	-0.145***
	(0.036)	(0.034)	(0.031)	(0.057)	(0.033)
	[0.27]	[0.27]	[0.26]	[0.26]	[0.29]
Child age 4-6 months	-0.048	-0.068	-0.081*	0.012	-0.116**
	(0.051)	(0.049)	(0.048)	(0.075)	(0.050)
	[0.43]	[0.42]	[0.42]	[0.40]	[0.46]
Child age 7-12 months	-0.045	-0.073	-0.086	0.003	-0.109**
	(0.055)	(0.053)	(0.057)	(0.066)	(0.056)
	[0.49]	[0.49]	[0.49]	[0.46]	[0.53]
Observations	2,924	2,674	2,535	1,523	1,401

Notes: Table displays estimates of the effect of gaining access to STDI maternity benefits on the probability of working during the time frame specified in the left-hand column. Panel B is conditional on working anytime during 1st pregnancy because work history is required for STDI eligibility, and because the SIPP measured post-childbirth employment only among those who worked during pregnancy. Each cell is a separate estimate of τ_{SR} from equation 3 with event-time pooled to capture the first 5 years post-reform. The dependent variable is an indicator for being at work during the time frame specified in the left-hand column. Data is drawn from 1984 and 1985 Survey of Income and Program Participation topical module on fertility and migration. Sample includes women who gave birth to their first children in the United States between 1970 and 1985. Panel B additionally restricts the sample to women who reported working at some point during the first pregnancy. The specification includes fixed effects for state of birth, year of childbirth, and month of childbirth. Standard error reported in parenthesis is clustered at the state of birth level. Figure in brackets is the counterfactual mean of the dependent variable.

Table 3: Effects of paid maternity leave on hourly wages

	Dependent variable: Log wages					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Women age 18-45						
First 5 years	-0.056**	-0.046**	-0.049**	-0.059***	-0.049**	-0.044**
	(0.024)	(0.022)	(0.020)	(0.021)	(0.022)	(0.019)
Second 5 years	-0.059**	-0.045*	-0.048**	-0.063**	-0.051*	-0.042*
	(0.029)	(0.025)	(0.023)	(0.027)	(0.027)	(0.023)
Age, race	X	X	X	X	X	X
Policy controls		X	X			
Unweighted			X			
Linear pre-trend				X		
Education					X	
Occupation						X
Mean (2019)	14.93	14.93	14.93	14.93	14.93	14.93
R-squared	0.15	0.15	0.15	0.15	0.27	0.43
Observations	570,902	570,902	570,902	570,902	570,902	570,720
Panel B: Men age 18-45						
First 5 years	-0.011	-0.002	-0.011	-0.011	-0.011	-0.013
	(0.019)	(0.020)	(0.022)	(0.016)	(0.019)	(0.016)
Second 5 years	0.000	0.011	0.001	0.000	-0.001	-0.009
	(0.021)	(0.021)	(0.023)	(0.019)	(0.021)	(0.019)
Age, race	X	X	X	X	X	X
Policy controls		X	X			
Unweighted			X			
Linear pre-trend				X		
Education					X	
Occupation						X
Mean (2019)	21.23	21.23	21.23	21.23	21.23	21.23
R-squared	0.28	0.28	0.28	0.28	0.35	0.43
Observations	658,639	658,639	658,639	658,639	658,639	658,512

Notes: Coefficients displayed are estimates of τ_{SR} and τ_{LR} from equation (3). Standard errors in parentheses are clustered by state group. Sample includes women (panel A) and men (panel B) age 18-45 in the 1973-1987 CPS May/MORG. All specifications include fixed effects for state group, month, and the interaction of year and Census region. Age and race controls include a quadratic in age interacted with an indicator for nonwhite race. Policy controls include measures of income transfers per capita, EITC payments per capita, and real state GDP per capita from the Bureau of Economic Analysis, plus an indicator for state-level unilateral divorce laws (Gruber, 2004; Wolfers, 2006). Specifications with a linear pre-trend include a linear term in event-time interacted with $STDI_s$ for observations in the four years preceding the reform that are balanced in event time. Occupation fixed effects are implemented using time-consistent version of 1990 Census occupational coding scheme developed by Ruggles et al. (2017). Mean is calculated in the year prior to the reform. Following Lemieux (2006), individuals with imputed wages or hourly wages below $1 or above $100 in 1979 dollars ($3.52 and $352 in 2019 dollars, respectively) have been dropped. Wages are converted to 2019 dollars using the CPI. Regressions are weighted using CPS sampling weights unless otherwise specified.

Table 4: Effects of paid maternity leave on labor supply of all women age 18-45

	Outcome: Employed			Outcome: Hours worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All women age 18-45						
STDI x Years 0-4	-0.026***	-0.014**	-0.013*	-0.838***	-0.310	-0.341
	(0.006)	(0.007)	(0.007)	(0.292)	(0.351)	(0.353)
STDI x Years 5-9	-0.039***	-0.024***	-0.027***	-1.326***	-0.682**	-0.783**
	(0.004)	(0.006)	(0.005)	(0.259)	(0.282)	(0.328)
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Mean	0.58	0.58	0.58	19.1	19.1	19.1
R-squared	0.02	0.02	0.02	0.02	0.02	0.02
Observations	1,373,712	1,373,712	1,373,712	1,373,712	1,373,712	1,373,712
Panel B: Women with some college experience						
STDI x Years 0-4	-0.027*	-0.010	-0.026*	-0.628	0.108	-0.708
	(0.015)	(0.016)	(0.015)	(0.684)	(0.739)	(0.720)
STDI x Years 5-9	-0.045***	-0.022*	-0.045***	-1.178**	-0.221	-1.278**
	(0.013)	(0.012)	(0.013)	(0.536)	(0.537)	(0.611)
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Mean	0.66	0.66	0.66	22.0	22.0	22.0
R-squared	0.02	0.02	0.02	0.03	0.03	0.02
Observations	509,068	509,068	509,068	509,068	509,068	509,068
Panel C: Women with no education beyond high school						
STDI x Years 0-4	-0.032***	-0.021***	-0.010*	-1.133***	-0.643**	-0.241
	(0.004)	(0.006)	(0.006)	(0.207)	(0.279)	(0.229)
STDI x Years 5-9	-0.047***	-0.034***	-0.026***	-1.774***	-1.205***	-0.797***
	(0.006)	(0.008)	(0.005)	(0.245)	(0.364)	(0.250)
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Mean	0.53	0.53	0.53	17.5	17.5	17.5
R-squared	0.02	0.02	0.02	0.02	0.02	0.02
Observations	864,644	864,644	864,644	864,644	864,644	864,644

Notes: Coefficients displayed are estimates of τ_{SR} and τ_{LR} from equation (3). Standard errors in parentheses are clustered by state group. Sample includes women age 18-45 in the 1969-1987 CPS May/MORG. All specifications include fixed effects for state group, month, and the interaction of year and Census region. Age and race controls include a quadratic in age interacted with an indicator for nonwhite race. Policy controls include measures of income transfers per capita, EITC payments per capita, and real state GDP per capita from the Bureau of Economic Analysis, plus an indicator for state-level unilateral divorce laws (Gruber, 2004; Wolfers, 2006). Specifications with a linear pre-trend include a linear term in event-time interacted with $STDI_s$ for observations in the six years preceding the reform that are balanced in event time. Mean is calculated in the year prior to the reform.

Online Appendix

A STDI coverage and the implementation of anti-pregnancy discrimination laws

My identification strategy relies on two sources of variation that interacted to create a staggered, state-level expansion of paid maternity leave in the United States. First, a series of states, and eventually the federal government, enacted anti-discrimination laws that required short-term disability insurance (STDI) to cover childbirth as a disability. These laws effectively created a source of paid maternity leave benefits for women covered by STDI, and the differential timing of their enactment allows me to compare outcomes of women and children within states and over time in an event-study specification. The second source of variation comes from long-standing differences in *access* to short-term disability insurance, driven largely by state disability policies and industrial mix. This second source of variation meant that the enactment of anti-discrimination laws had more "bite" in some states than in others.

To assemble evidence on the enactment of state anti-discrimination laws and the receipt of STDI benefits, I rely on several primary and secondary sources, including Congressional testimony, correspondence with state officials, newspaper articles, and published histories of anti-discrimination laws (Koontz, 1971; U.S. House of Representatives, 1977; U.S. Senate, 1979; Gladstone, Williams and Belous, 1985; Kamerman, Kahn and Kingston, 1983).

The anti-discrimination laws were not universally popular. In many cases, industry groups organized to oppose the expansion of STDI benefits to childbirth, arguing that they would increase costs not only in the form of additional benefits paid, but also because employee absences and turnover could reduce productivity. Figure A6 provides one such example from the state of Maryland.

These laws also varied in their specifics and in the way they were enacted. While anti-discrimination policies were enacted in some states by legislative action, others were created by a ruling through the state Supreme Court or an action of the executive branch of government. For example, two rulings by the Wisconsin Superior Court in 1975 faulted employers for withholding or providing less generous disability benefits to women on maternity leave than men in similar circumstances. The judiciary was less sympathetic to efforts to expand California's state-run STDI system to "normal" pregnancy, with two cases reaching the U.S. Supreme Court. However, the state's lawmakers responded in August 1976 when they enacted legislation providing maternity leave benefits (Los Angeles Times Staff, 1976). Such efforts were not always successful; Nebraska's Supreme Court sided with Omaha public schools over a teacher who had suffered discrimination on the basis of pregnancy, and legislators declined to update state employment law (Willborn, 1983). Nevertheless, for Nebraska and other states that did not enact their own anti-discrimination laws before 1979, the policy was imposed on them by the U.S. Congress through the Pregnancy Discrimination Act of 1978. The resulting timeline is laid out in Table A1.

These anti-discrimination laws also varied in their scope. Many affected a very broad range of workers. In the case of the Pregnancy Discrimination Act, the ban on pregnancy discrimination affected firms with 15 or more employees (Kamerman, Kahn and Kingston, 1983). However, some state-level expansions of early anti-discrimination laws affected a broader group of workers. In states where STDI was nearly universal, the share of affected workers would have been even larger. In addition, many of the laws, including the federal Pregnancy Discrimination Act, required only that "women affected by pregnancy, childbirth, or related medical conditions shall be treated the same for all employment-related purposes" as men or women who were not pregnant but had conditions that affected their ability to work in similar ways. In these cases, STDI maternity benefits were not the only effect of the laws. For example, Gruber (1994) explores the wage and employment effects of Pregnancy Discrimination Act-driven changes in health insurance benefits for childbirth. As a result, I cannot completely rule out the possibility that the labor-market effects I estimate aren't driven in part by other subtle changes that resulted from the anti-discrimination laws. However, a reading of the legislative history and newspaper accounts suggests that maternity leave was the dominant concern among proponents and opponents of the legislation. I specifically discuss my results in the context of Gruber (1994) in section D below.

The second source of variation that I exploit in my research design – differences in *access* to STDI – dates to the origins of the industry in the 19th century. The goal of early STDI policies was to provide financial stability to workers, typically males, who wanted insurance against the risk of an injury or illness that would prevent them from earning income (Faulkner, 1940). The STDI industry grew significantly over the early 20th century, and throughout the second half of the 20th century, about 60 percent of workers were covered (Price, 1986). However, the stability of aggregate STDI coverage rates belies substantial variation across states. Coverage was much more prevalent among workers in certain industries, such as manufacturing (Levy, 2004). As a result, state-level STDI coverage rates varied with the mix of industries in existence. In addition, five states – Rhode Island, New Jersey, New York, California, and Hawaii – enacted laws in the 1940s (and in the 1960s, in the case of Hawaii) that made access to STDI virtually universal. This variation in access to STDI existed decades before the anti-pregnancy-discrimination laws of the 1970s and was driven primarily by the desire for wage insurance among workers, rather than concerns about allowing women to take leave after the birth of a child.

Historically, the Social Security Administration assembled data tracking *national* STDI coverage levels. Figure A2 shows the time series from the middle 20th century through the 1980s. The national share of workers covered by STDI was fairly stable throughout this period. Survey data from the U.S. Bureau of Labor Statistics suggests coverage fell below 40 percent in the early 21st century. The National Compensation Survey estimates that 42 percent of private-industry workers were covered in 2019.

A.1 Measuring access to STDI

To take advantage of the cross-sectional variation in access to STDI, my research design requires a measure of STDI coverage by state before the enactment of the anti-discrimination laws that required these policies to provide maternity benefits. Unfortunately, data on coverage rates at the sub-national level are scarce. I instead draw on several sources of data to estimate STDI coverage.

My main estimates take advantage of the observation that employer offers of private disability coverage are highly correlated with employer offers of health insurance. This relationship can be seen in Figure A7, which plots industry-level shares of STDI and employer- or union-sponsored health insurance coverage in 2012. Health insurance coverage is considerably more widespread, but it is highly correlated with STDI coverage. It is useful to note that the relationship between these two measures is complicated by the fact that coverage is universal in five states that represent about 22% of workers. This presumably breaks the link between health insurance coverage and disability coverage for a large proportion of workers. It is no surprise, then, that if I consider only the industries in which the share of workers who live in universal STDI states is below the median, the relationship is even stronger: A simple regression of the share with STDI on the share with health insurance delivers a slope of 1.06 (s.e. 0.18), relative to 0.80 (s.e. 0.20) in the full sample.

Given this robust relationship, I use health insurance coverage as a high-quality proxy that allows me to estimate the share of female workers age 18-45 with STDI coverage. Health insurance coverage is not well-measured during this time frame, but an exception is the 1976 Survey of Income and Education, a large-scale supplement to the CPS that samples more than 150,000 households in an effort to construct state-level estimates of poverty rates and other characteristics. I first limit the sample to individuals who were employed and between ages 18-64. I then calculate the share with health insurance through an employer or union. I rescale this figure to match industry estimates of the share of workers in non-universal states with STDI coverage in 1976 (Price, 1986). I using the resulting value to calculate the estimated share of employed women age 18-45 with STDI coverage in each state.

Given the lack of data on STDI coverage by geographic or demographic group, I test the validity of this measure by comparing it to the share of new mothers who received STDI maternity benefits in the 1980s, after the passage of all anti-discrimination laws at the state and federal level. Data on take-up comes from the Survey of Income and Program Participation's 1984-1989 panels, which provide information on the exact month of birth and the receipt of various sources of income (including STDI benefits) by month. To focus on women who were likely eligible for the benefits, I restrict the SIPP sample to women who were employed 12 months before the birth of their child. Receipt of benefits such as STDI are known to suffer from underreporting in survey data such as the SIPP (Meyer, Mok and Sullivan, 2015). The relatively small sample sizes inject additional noise into this exercise. Nevertheless, Figure A8 shows a strong relationship between my estimate of the share of working women with STDI coverage and the share of new mothers who received STDI benefits. The correlation coefficient (weighted by SIPP sample size) is 0.45; if I include the universal states, where presumably all working women were eligible, the correlation coefficient rises to 0.84. This provides reassurance that my estimates of STDI coverage are picking up meaningful cross-state variation in the share of women who were made eligible for maternity benefits as a result of the Pregnancy Discrimination Act and its state-level counterparts.[26]

A.2 Additional data sources

Estimates in this paper rely on several sources of publicly available data. The main samples of Current Population Survey and Survey of Income and Program Participation data are described in section 3.

In addition, I draw on several sources of data to explore the possibility that other determinants of women's labor-market outcomes changed in a way that was correlated with the expansion of STDI maternity benefits. For example, the 1960s and 1970s were a time of active expansion of many federal social programs. If these expansions happened at the same time as the passage of pregnancy discrimination laws, and if they had a disproportionate impact on states with more widespread STDI coverage, my estimates may mistakenly attribute the effect of these policies to

[26]I also experimented with other methods of measuring STDI coverage in the 1970s. These alternative measures tend to be positively correlated with the measured described here, but were ultimately less attractive for theoretical or empirical reasons. The industry-level data from Autor et al. (2013) can be linked to the 1970 Census to construct state-level estimates, but this approach was discarded because it relies on 2012 estimates of STDI coverage. I also constructed estimates using the 1965-1969 Source Book of Health Insurance, but these data do not allow for estimates specific to women of child-bearing age. The National Health Interview Survey was a third potential source, but its limited geographic and industry information provided little traction in measuring meaningful state-level differences.

the availability of paid maternity leave. However, as shown in Figure 3, I find no evidence of any confounding policies.

The first set of policies analyzed are those related to early childhood education. I construct two measures of access to such policies. The first draws on the expansion of state-funded kindergarten programs in the 1960s and 1970s. Specifically, drawing on Table 1 of Cascio (2009), I construct an indicator that takes the value of 1 if a state has established a kindergarten program and 0 otherwise. The second policy draws on access to Head Start, which was funded by the federal government beginning in 1965 and provided preschool to millions of low-income children ages 3-5. Following Bailey, Sun and Timpe (2021), I construct an indicator that takes the value 1 if a county has ever received a federal Head Start grant. I then aggregate this county-year panel by taking a population-weighted average at the state-year level. I use this state-level average as my measure of Head Start access.

Another possible concern is that STDI maternity benefits' availability was correlated with access to programs that provided support to mothers. Drawing on data from the replication package of Bailey, Sun and Timpe (2021), which itself draws on data from Bailey and Goodman-Bacon (2015) and Bailey (2012), I construct indicators for receipt of federal grants for Maternal and Infant Care or Maternal and Child Health programs. In addition, I construct an indicator for access to family planning programs, which altered women's fertility decisions and by extension likely played a role in women's labor market outcomes (Bailey, 2012). Like the data on Head Start, these county-level measures are aggregated by taking population-weighted means by state and year.

In addition, the 1960s, 1970s, and 1980s also saw large changes in laws related to family formation. In particular, many states enacted unilateral divorce laws that may have altered women's bargaining position in the household and incentives to invest in their careers (Friedberg, 1998; Gruber, 2004; Wolfers, 2006). I use data from Wolfers (2006) to construct an indicator for existence of a state-level unilateral divorce law and regress this measure on my treatment data to test whether changes in divorce laws coincided with access to paid maternity leave.

Another important feature of the U.S. labor market is the minimum wage. Minimum wages peaked in the 1960s and have generally fallen in real terms ever since Bailey, DiNardo and Stuart (2021). In addition, states have adopted their own minimum wages that exceed federal standards. These policies clearly impact wages and arguably impact women disproportionately given gender differences in the distribution of wages. To explore the possibility that STDI maternity benefit expansions were correlated with state-level minimum wages, I construct a state-year panel using data from Vaghul and Zipperer (2016) and Derenoncourt and Montialoux (2021).

Finally, to explore the impact of fertility, I assemble a state-year-month panel of the log of total births using birth-record data from National Center for Health Statistics (2015).

A.3 An additional test of the research design

One possible concern about the use of variation in STDI coverage and the timing of pregnancy discrimination laws to estimate the effects of paid maternity leave is that the staggered implementation of anti-discrimination laws may be systematically related to other state-level characteristics that are related to working mothers' leave-taking behavior, women's labor-market outcomes, or children's long-run educational attainment. Such cross-state differences could confound my estimates of the effect of STDI maternity benefits.

The main text of this paper discusses the key assumptions behind my identification strategy. Several tests of these assumptions provide little evidence for concern. First, the use of state fixed effects will eliminate any time-invariant confounding factors, while region-by-year fixed effects go further by netting out any differential trends common to certain regions of the United States. Differential *trends* could also present problems; however, my event-study provides a built-in placebo test for this type of confounding factor. I also find no evidence of coincident changes in other programs, such as access to Head Start and kindergarten, that could explain the sharp break in female hourly wages that I report.

Figure A9 provides an additional test for systematic relationships between the implementation of STDI maternity benefits and state-level characteristics that could drive the changes in labor-market activity and educational attainment that I report. The figure shows the t-statistics from a regression of the year of enactment of the state anti-discrimination law on a set of 21 measures of economic and demographic characteristics from the 1960 Census. I restrict the set of characteristics to those used in a similar exercise by Bailey (2006) in an analysis of the the effect of state laws that affected access to the birth control pill. In addition, I drop New Jersey and Rhode Island from the sample because these states began paying STDI maternity benefits at least a decade before any other state.

Figure A9 displays results that are consistent with a legislative history, as reflected in Table A1, that suggests the implementation of STDI benefits were driven more by idiosyncratic factors than systematic differences across states. Only one of the 21 covariates delivers a t-statistic that exceeds the traditional 5% level, an outcome we would expect in an exercise that features 21 statistical tests. In addition, the lone significant result suggests that average education among women in early-adopting states was lower, providing a counterpoint to the possibility that early-adopting states were systematically driven by a more educated, empowered female electorate.

B A simple theoretical model of paid maternity leave

Most research on maternity leave policies has focused on the decision-making of mothers, who must balance time devoted to market work and time devoted to caring for a newborn child. However, such policies also have the potential to affect other workers.

To illustrate, consider a simple model of a static labor market in a compensating differentials framework. The market includes a unit measure of female workers who make an extensive-margin labor supply decision, $L \in \{0,1\}$, to maximize a utility function that is increasing in wage income but decreasing in an individual-specific distaste for work, v_i. This disutility of work, which is distributed in the population according to cumulative distribution function $F(v)$, can be interpreted as the cost of maintaining an inflexible work schedule that, for example, limits the amount of time a worker can spend with a newborn child. In that case, we may think of paid leave as a parameter $Z \in [0,1]$ that moderates the disutility of work by providing greater flexibility. A convenient functional form would be:

$$U(L_i;\ v_i) = wL_i - v_i L_i Z \tag{5}$$

In this simple framework, workers choose to enter the labor force if $w \geq v_i Z$; that is, if the market wage is sufficiently high to make up for the inflexibility and other sources of disutility of work. This disutility can be offset if employers take steps to provide workers with more flexibility or reduce other disamenities.

However, efforts to reduce the disamenity of work come at a cost to firms, which must take steps to accommodate extended absences from female workers. Furthermore, the cost of providing flexibility may vary across firms if the absence of a worker is more disruptive in some settings than others. To capture this feature, I model the cost to firm j as a parameter $\delta_j \sim H(\delta)$ that monetizes Z:

$$\pi(L_j) = G(L_j) - wL_j - \delta_j(1-Z)L_j \tag{6}$$

where $G(L_j)$ is an twice-differentiable, concave production function, w is the market wage, and L_j is labor demanded by firm j. Integration of these supply and demand functions leads to the following system of aggregate labor supply and demand that determines equilibrium wages and employment:

$$Aggregate\ labor\ supply: L^S = \int 1\left\{\nu_i < \frac{w}{Z}\right\} dF(\nu) \tag{7}$$

$$Aggregate\ labor\ demand: L^D = \int L_j^D\left(w + \delta(1-Z)\right) dH(\delta) \tag{8}$$

Equilibrium wages and employment are then determined at equilibrium, where $L^S = L^D$. This simple model captures the basic insights of Summers (1989) and Gruber (1994). Figure A10 provides a graphical representation of the theoretical implications of the introduction of paid leave, which we can think of as an exogenous decrease in Z. The initial equilibrium represented in Figure A10a is disrupted by the enactment of paid leave, which makes work relatively attractive to women and shifts the labor-supply curve rightward as shown in Figure A10b. In the absence of changes in labor demand, the result would be an expansion of female employment but a drop in wages. However, when we take the response of firms into account, as shown in Figure A10c, we see that labor demand will *reinforce* the tendency of wages to fall but *offset* the tendency of employment to rise. Absent any intra-household responses or changes in male labor-market outcomes, which are omitted here for simplicitiy, these changes could lead to a decrease in income for women even if employment remains unchanged, as shown in Figure A10d. An additional prediction is that there will be a sorting effect as the policy elicits a larger demand response among firms where the cost of accommodating maternity leave is higher.

C Supplemental estimates of labor-market effects of STDI maternity benefits

C.1 Effects on women's leave-taking: Evidence from the decennial Census

While the results from SIPP retrospective data analyzed in section 4.1 provide the most detailed look available at mothers' short-term labor-market responses to STDI benefits, the drawback of these data include their relatively small sample size and the fact that detailed employment histories can be reconstructed only for the months around the first birth to mothers who worked during pregnancy.

An alternative source of data is the 1970 and 1980 decennial Census, accessed via IPUMS (Ruggles et al., 2017). These data report the year, quarter, and state of birth for each respondent, and allow them to be connected to parents if they reside in the same household. In addition, they include questions about employment status in the previous week and the previous year.

I construct a sample of women ages 18-45 who gave birth to a child in the calendar quarter preceding the Census reference date, which was April 1 in each Census year. For an additional comparison group, I also use a sample of women age 18-45 who report that they have never given birth. I then estimate my main difference-in-difference specification using three binary outcomes: being employed but absent from a job in the previous week, employed in the previous week, and working for pay at any time during the previous year. The first outcome provides an estimate of the effect on leave-taking. The second provides some additional context about the effect of the policy; a rise in leave-taking accompanied by a rise in employment would suggest the paid-leave benefits increase mothers' attachment to the workforce, while a rise in leave-taking without a change in employment suggests STDI-funded leave results in a short-run substitution from time at work to time at home with no longer-run implications for mothers' labor-market attachment (Baker and Milligan, 2008*a*). Finally, the measurement of whether mothers worked for pay in the previous calendar year provides a look at whether the availability of paid-leave benefits affected women's labor-supply before childbirth. Since STDI benefits were generally available only to mothers with a labor-market history, I also examine effects on leave-taking, employment, and hours worked for the subset of new mothers who report working in the previous year. To explore heterogeneity, I estimate the effects not only on the full sample of mothers, but also on subsamples that split by birth parity and level of education. In addition, as a further test of the extent to which STDI benefits increased attachment to the labor force, I examine impacts on a set of mothers of slightly older children who were born in the fourth quarter of the year prior to the Census. These children would have been at least 3 months old by this point, after the age when STDI benefits would have expired.

Table A4 displays the results. Column A shows that the availability of STDI benefits led to a 2.3 percentage-point increase in the share of women who were on leave from a job at the beginning of the quarter after childbirth. In line with the results from the SIPP retrospective data, I find no evidence of an increase in the share of women who worked before childbirth (column 2), nor do I find significant evidence of a change in the share of women with a job after childbirth (column 3).

If we assume there is truly no effect on pre-childbirth selection into the labor force, column 4 provides an estimate of the treatment effect of paid leave benefits on the short-run responses of workers: Women made eligible for paid leave benefits were nearly 5 percentage points more likely to be on leave. Benefit-eligible women were also more likely to remain employed: They were 6 percentage points more likely to have a job after childbirth relative to the counterfactual world where STDI plans were not required to cover maternity. It is important to emphasize, however, that these should be interpreted as very short-run effects: It measures employment and leave-taking effects between 1 day and 3 months after childbirth. I return to this point a few paragraphs below, where I look at employment among mothers after more time has passed since birth.

Panels B and C suggests that there is indeed a difference in the effects by parity of birth. The point estimate on leave-taking for first-time mothers is 3 times that of higher-parity mothers. My estimates on the share of mothers who worked in the previous year or are employed just after birth are too noisy to be informative. However, when the sample is restricted to women who worked in the previous year (and are therefore likely to be eligible to collect STDI maternity benefits), I find an increase in leave-taking for first-time mothers of 4.75 percentage points and a nearly identical effect on employment of 4.77 percentage points. Estimated effects on mothers of higher-parity children are still positive but noisier – a statistically insignificant 4-percentage-point increase in leave-taking and a more statistically robust estimate of a 6-percentage-point increase in employment. All in all, these estimates suggest that the availability of maternity benefits mattered for all mothers – but because first-time mothers were more likely to be in the labor force prior to childbirth (as shown in Table A4, 75 percent of 1970 first-time mothers worked the previous year, relative to only 38 percent of mothers giving birth to a younger sibling), they felt the effects most significantly.

Consistent with the results of Table 2, panels D and E of Table A4 also provide additional evidence of substantial heterogeneity by education. Among mothers with some college education, STDI maternity benefits drive very large increases in the share of women taking leave – a 6.7 percentage-point increase for all mothers and a 9.2 percentage-point increase for mothers who worked the previous year. There is no measurable impact on eligibility or employment after birth. In contrast, I cannot rule out 0 effect for any outcomes for mothers with no more than a high school education.

The positive and statistically significant estimated effects on employment in column 5 for most groups may at first glance appear at odds with the evidence from the retrospective SIPP that STDI-benefit-eligible mothers were *less* likely to return to work after childbirth. However, it is noteworthy that the estimates in Table A4 are specific to the share of women employed at the end of the calendar quarter in which they gave birth. If, as is likely in many cases, women must remain employed to continue collecting their disability benefits, then the estimated for employment outcomes may in fact be picking up increases in the share on disability leave. The retrospective SIPP data does not allow me to distinguish between women who are employed but on leave and women who are not employed.

In fact, the results in panel F of Table A4 suggest that the positive employment effects on STDI benefits are short-lived. These results focus on women who gave birth in the fourth quarter of 1969 or 1979. These mothers' children are slightly older, and specifically have aged past the 6- to 10-week time period covered by most maternity benefits at the time. This is consistent with the null effect on leave-taking in column 1. In addition, note that the employment effects in columns 3 and 5 are both smaller and statistically insignificant, suggesting that paid-leave-eligible mothers responded by remaining employed for only a short time.

I find similar results when using the decennial Census in a separate exercise to measure "medium-run" impacts on mothers' employment. To do so, I restrict the sample to mothers in the 1980 Census. I use all births that occur between 1970 and the first quarter of 1980. I "stack" mother-child pairs and assign treatment status using the date and state of the child's birth. I then estimate equation 1 using an indicator for employment as the dependent variable. Note that I can only observe a single balanced year of event-time after treatment, since the federal Pregnancy Discrimination Act went into effect in the second quarter of 1979. Since I observe employment only at a single point in time in 1980, we can interpret these estimates as a weighted average of effects of STDI maternity benefits on mothers' outcomes in the short and medium run.

The results of this exercise are shown in Figure A4. The estimates suggest that in the years after childbirth, mothers who had access to STDI maternity benefits were about 1.5 percentage points less likely to be employed. In this case, the estimated effects are not substantially different for first births relative to higher-order births. One drawback of these estimates is that they pool effects from a number of years relative to the child's birth, which complicates interpretation since we are pooling effects at different times post-childbirth, and that I am not able to observe pre-childbirth labor-market activity. Nevertheless, these estimates are consistent with the finding in section 4.1 that STDI maternity benefits actually *decreased* labor-force attachment for many mothers.

C.2 Robustness to measurement of STDI exposure

My main results rely on estimates of equation 1, where the key variable $STDI_s$ captures cross-sectional variation in the share of working women with access to employer- or union-provided disability insurance. For states without compulsory laws, the share $STDI_s$ is estimated using data from the Survey of Income and Education of 1976. In this section, I present estimates using alternative methods of measuring the cross-sectional differences in the "bite" of the policy.

One alternative approach would compare universal STDI states to non-universal states. This amounts to coding $STDI_s = 1$ for universal states and 0 for all other states. While this approach neglects potentially important cross-state variation in non-universal states, it provides a useful robustness check to my main results. Figure A11 displays the results for hourly wages and employment. The pattern delivered by this approach are broadly similar, with women seeing a sharp decrease in hourly wages after the reform. Men's wages also dip in the years after the reform, but this appears to be driven be pre-existing trends, and the decrease in much smaller, shorter-lasting, and never statistically significant.

One feature of note is that the decrease in women's hourly wages is not as persistent in this simpler specification. The reason is that the comparison group is not truly untreated. This attenuates the estimated effects, especially in the long run. This can be seen clearly by contrasting Figure A11 with Figure A12, which shows estimates from a modified version of specification 1 that retains the more detailed measure of $STDI_s$ but allows for heterogeneous effects in states with and without universal STDI. The figure makes clear that women in both groups of states saw substantial deterioration in their hourly wages, although it was larger in the short run for women in states where STDI was universal. The larger effect in universal states can likely be attributed to the fact that employers in states with STDI mandates had fewer options to respond to the perceived costs of maternity benefits – for example, they did not have the ability to drop STDI coverage – forcing the adjustment to come disproportionately in the form of lower wages.

In addition, two alternative approaches are shown in Table A10. Column 1 repeats my main estimates from column 1 of Table 3. Column 2 repeats the exercise but sets $STDI_s = 1$ for all states, effectively ignoring cross-sectional differences and assuming all states receive an equal "dose" of treatment. Unsurprisingly, the estimates are much smaller, since this specification implicitly assumes that states with universal STDI access have effects the same size as states where a much smaller share have access. However, the differential timing does provide enough variation to see the negative impacts on women's wages, particularly in the long run.

Finally, columns 3-5 of Table A10 report estimates in which the effect of the policy is estimated separately for each of three groups of states: universal states, states without compulsory STDI laws but with estimated STDI shares above the median of the remaining states, and states with relatively low STDI shares. As we would expect, the effects are largest in the states where STDI is universal. However, they are also statistically detectable in the "medium" states, although substantially smaller. In the lowest-coverage states, there is no detectable effect on wages. These results underscore the importance of the dosage of the treatment on the impact of the anti-discrimination laws on women's labor market outcomes.

C.3 Additional robustness checks of the effect on women's labor hourly wages

My main estimates rely on estimates of the cross-state variation in STDI that is imputed based on *health* insurance coverage in the 1976 Survey of Income and Education. There are many advantages to the use of these data. First, the survey measured coverage at a time before most states adopted anti-discrimination laws, alleviating endogeneity concerns.[27] Furthermore, to the extent that employers dropped STDI coverage by 1976 in response to the laws, the use of health insurance coverage may skirt endogeneity concerns as long as firms retained some of their benefits. However, the drawback is that the use of health insurance as a proxy for STDI may introduce noise.

Although I am not aware of any source of data on STDI coverage at the sub-national level in the 1970s, the stability of STDI coverage over time in Figure A2 suggests that industry-level STDI coverage from a later date may be a reasonable proxy for industry-level coverage in the 1970s. In this section, I present estimates of equation 1 using an alternative source of data on STDI coverage: A tabulation of coverage rates by industry prepared by the Bureau of Labor Statistics National Compensation Survey and published in Autor et al. (2013). I merge this tabulation to the 1970 long-form decennial Census, accessed via IPUMS (Ruggles et al., 2017), which contains information on the industry of employment. I then estimate the share of working women age 18-45 that had access to STDI by state in 1970. The resulting estimates range of STDI coverage range from 26 percent to universal coverage.

The resulting estimates are shown in Figure A13. The estimates are very similar to the main results. The slightly larger magnitude suggests that the measurement error caused by using health insurance as a proxy for STDI might be slightly larger than the measurement error that results from any industry-level changes in coverage over time. However, I adopt the measure of STDI from the 1976 SIE for my main results as a conservative approach that is less likely to suffer from endogenous evolution of benefits in response to the policy.

I also explore the sensitivity of my results to alternative estimators. A series of papers have highlighted shortcomings in two-way fixed effect models similar to equation 1 (Goodman-Bacon, 2021; Borusyak, Jaravel and Spiess, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). In a setting with staggered implementation of a permanent policy, event-study and difference-in-difference estimators deliver a weighted average of treatment effects from various time periods relative to treatment and various "treatment cohorts" (i.e., groups that receive the

[27] For example, firms in non-universal states could have responded to the anti-discrimination laws by dropping STDI coverage for all workers, thus averting the need to pay maternity benefits. However, it should be noted that there is no evidence of such responses in the aggregate data (see Figure A2), and the savings from dropping coverage would have been attenuated by the fact that this would amount to a real reduction in compensation, including for men.

treatment at different times). Estimates can therefore be strikingly misleading when treatment effects are heterogeneous across groups, heterogeneous across relative time, or both. These issues are further complicated in settings without an untreated group, or even when the untreated group is relatively small (Borusyak and Jaravel, 2018). These concerns are relative to my setting because adoption of STDI was staggered and there is only one "control" state in the sample: New Jersey, which obtained STDI maternity benefits in the early 1960s and therefore experiences no reform in my panel from 1973-1987.

I explore the sensitivity of my estimates to these issues by implementing the "interaction-weighted" estimator of Sun and Abraham (2021) describe the procedure intuitively here and refer readers to their paper for full details. The sensitivity of event-study estimators stems from their tendency to combine treatment effects from multiple cohorts and multiple periods relative to the reform. For example, an estimate of τ_0 from equation 1 is not simply a weighted average of state-specific average treatment effects on the treated at $k = 0$, but also of state-specific treatment effects from *other* periods relative to the reform. To avoid confounding these effects, I first limit the sample to a single treated state (i.e., women age 18-45 in a state that adopted STDI maternity benefits between 1975 and 1979) and a control group that was not treated in that time. I then regress log wages on specification 1. I repeat for each treated state to obtain $\hat{\tau}_{ks}$. I then construct my estimates by averaging $\hat{\tau}_{ks}$, weighting each state-specific treatment effect by the state s's estimated share of the sample in event-year k.

The interaction-weighted estimator of Sun and Abraham (2021) requires a control group. I provide estimates using four different choices of control group. The first option is based on the observation that while all states eventually adopted STDI maternity benefits, New Jersey adopted them in 1961, long before my sample begins in 1973.[28] I therefore use New Jersey as my first control group. Second, I use late-adopting states where STDI maternity benefits were enacted as a result of the Pregnancy Discrimination Act of 1978. This choice of control group has the merit of employing a wider sample of states in the control group, but limits the number of post-reform periods in which I can estimate a treatment effect. Third, to avoid the limitations of relying on New Jersey as a control group or limiting the post-period estimates, I use women age 46-64 as a control group and estimate τ_{ks} using solely within-state variation. The drawback to this approach is that it will underestimate the effect of STDI maternity benefits to the extent that older women were affected by the policy, which is likely to have occurred given that some women in the 18-45 age group will have aged into the 46-64 age group over the course of the sample. Finally, as a fourth option, I employ a triple-difference specification, using men age 18-64 *and* women age

[28] Rhode Island adopted maternity benefits even earlier, but it is pooled with other small New England states in my CPS sample.

18-45 from New Jersey as comparison groups. The triple-difference approach allows me to use men as a comparison while avoiding the likely confounding influence of the secular increase in women's relative wages over this time period.

The results of this exercise are shown in Figure A15. Estimates from this routine tend to be noisier than my standard event-study specification, so I pool event-time into two-year groups except when using the "not-yet-treated" control group where I am limited to only a few periods. Each comparison group results in an estimate that is qualitatively similar to my main results. When using not-yet-treated states and women age 46-64 as comparison groups, I see a flat pre-trend followed by a sharp decrease of roughly 0.05 log points – strikingly similar to my main estimates. New Jersey and men age 18-64 provide less compelling comparison groups, as each show evidence of a upward pre-trend. However, this pre-trend works *against* finding a negative effect on women's log wages, and in each case the trend breaks sharply downward after enactment of STDI maternity benefits. Overall, these results are closely in line with my estimates using the standard event-study framework and suggest that any heterogeneity across states or event-cohorts is not substantially altering my estimates of the effect of maternity benefits on women's labor-market outcomes.

Finally, another concern may stem from performing statistical inference in a setting where treatment is set at the state level, but there are relatively few states (Bertrand, Duflo and Mullainathan, 2004). In my main estimates, I conduct statistical inference based on conventional estimators of variance that are robust to heteroskedasticity both off and on the diagonal within a state. As an alternative, I also use randomization to conduct inference on my estimates of the effects on women's log wages, as reported in column 1 of Table 3 (Fisher, 1935; Buchmueller, DiNardo and Valletta, 2011). Specifically, to estimate the sampling distribution of my estimates, I draw a value of $STDI_S$ and a date of enactment of the state-level anti-discrimination law without replacement for each state. I then assign these randomly drawn policies to the states and re-estimated equation 3. I repeat this procedure 500 times. Under the null hypothesis that the expansion of STDI maternity benefits have 0 effect on log wages, this procedure should routinely give me an estimate of short- and long-run effects that are as large as my true estimate. Importantly, this method does not rely on traditional assumptions related to the number of clusters approaching infinity, and is thus a reasonable alternative in settings with a fixed number of clusters (Athey and Imbens, 2017).

The results for women's log wages are shown in Appendix Figure A17. The histograms show the distribution of estimates of short- and long-run effects, and the vertical lines mark the estimates reported in Table 3 (column 1). The two-sided p-value for the short-run effect is 0.026. Not surprisingly, the long-run estimates is noisier, with a p-value of 0.076. I interpret these results as reassuring in two ways. First, the similarity of the p-values to those implied by the results in

Table 3 provides reassurance that the more traditional approach to variance estimation performs well in this setting. Second, these results provide reassurance that my empirical approach is picking up real impacts on women's outcomes in the labor market.

C.4 Descriptive evidence on effects on women's labor market outcomes

The main estimates reported in section 4 draw on data from the Current Population Survey's May extracts from 1973-1978 and the Outgoing Rotation Group files from 1979 to 1987. Figure A14 provides descriptive evidence of the effect on women's log wages. The enactment of STDI maternity benefits came at a time of rapid growth in female labor-force participation, and just before an historic, sustained increase in women's relative wages. I use the data to construct the gender wage gap (women age 18-45 relative to men age 18-64) for each year from 1973 to 1990. I then fit a quadratic trend and plot the residuals for two groups of states: States that adopted STDI maternity benefits in 1976-1977, and states that adopted STDI maternity benefits in 1979 as a result of the federal Pregnancy Discrimination Act. While the residuals from the trend in the raw data are somewhat noisy, it is clear that in both groups of states, the upward trend in women's relative wages saw a setback for several years after maternity benefits became available.

D Maternity leave and the results of Gruber (1994)

My identification strategy is based on an expansion of paid maternity leave via STDI, which was required to cover childbirth as a disability as a result of the Pregnancy Discrimination Act of 1978 and a number of state-level precursors. A closely related paper is Gruber (1994), which examined the effect of some of these same policies on the wages and employment of married men and women. Unlike this paper, Gruber (1994) focuses on another consequences of these anti-discrimination laws: Health insurance policies were required to cover the hospital charges of women who give birth.

The Pregnancy Discrimination Act did not explicitly specify conditions that STDI or health insurance plans were required to cover. Rather, it stated only that firms must treat women who cannot work before, after, or during the birth of a child the same way they would treat any other employee who is temporarily unable to work. One consequence of this broadly worded policy is that it is ultimately not possible to separate labor-market effects that are driven by maternity leave from those driven by health insurance or other factors.

However, several pieces of evidence suggest that maternity leave benefits were indeed one of the most significant consequences of these anti-discrimination laws, and that the results of Gruber

(1994) may be worth reinterpreting accordingly.

The first set of evidence worth noting is the qualitative evidence from debates in Congress and statehouses over the Pregnancy Discrimination Act and its state-level counterparts. Maternity benefits through STDI were a primary objection from business groups opposing the legislation, who argued that it would not only raise STDI premiums but would also lead to longer and more frequent leaves that would force firms to hire less productive, temporary workers and increase turnover.

Additional evidence can be gleaned from a useful feature of the state-level variation used to estimate the main wage effects in Gruber (1994). In particular, some of the main results use a triple-difference strategy that, in part, compares married women from three early-adopting states – Illinois, New York, and New Jersey – to women from a set of control states that enacted anti-discrimination laws later. In two of these three states, STDI maternity benefits and health insurance maternity benefits were enacted at the same time. However, in New Jersey, STDI maternity benefits were enacted much earlier, in 1961. If the observed effects on wages were driven by factors other than STDI maternity benefits, we would expect to see strong wage responses in all three states if we estimated the effects separately. However, if STDI maternity benefits are the most salient consequence of the anti-discrimination laws, then New Jersey should react quite differently than the other states.

In fact, the evidence from a replication of the findings of Gruber (1994) suggests that STDI maternity benefits were indeed the major driver of labor-market responses to the anti-discrimination laws of thoe 1970s. These results are shown in Table A6. Columns 1 and 2 show the main result from Table 4 of Gruber (1994) and my replication, respectively. In column 3, I alter the specification by replace the indicator for treated states (referred to as "experimental" states in the paper) with a binary indicator for each treatment state, allowing me to estimate the wage effect separately for each state. The results show that while New York and Illinois saw large negative wage effects in the wake of the passage of their respective anti-discrimination laws, New Jersey saw virtually no impact. This suggests that the anti-discrimination laws led to substantial negative wage effects in states where they expanded access to paid maternity leave, but had little or no impact in the state where paid leave had already been available for more than a decade.

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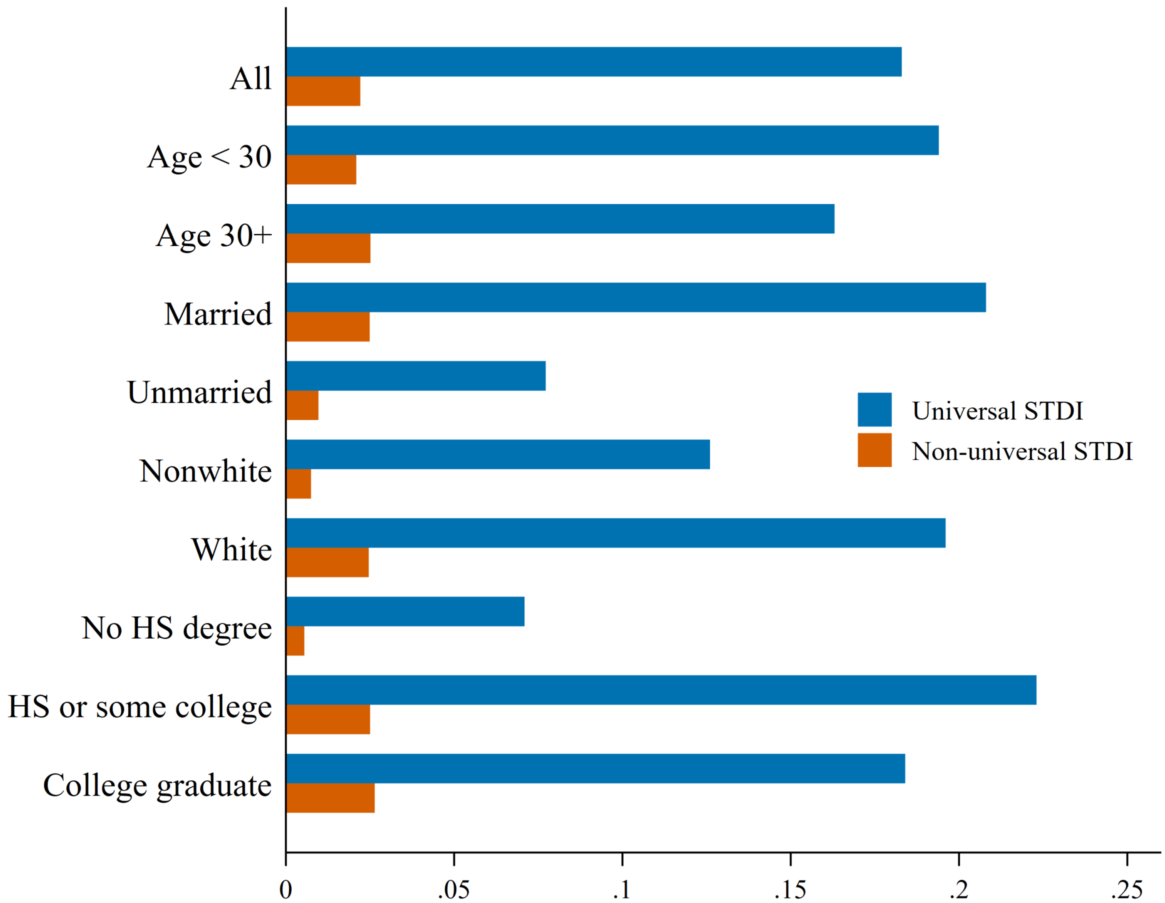
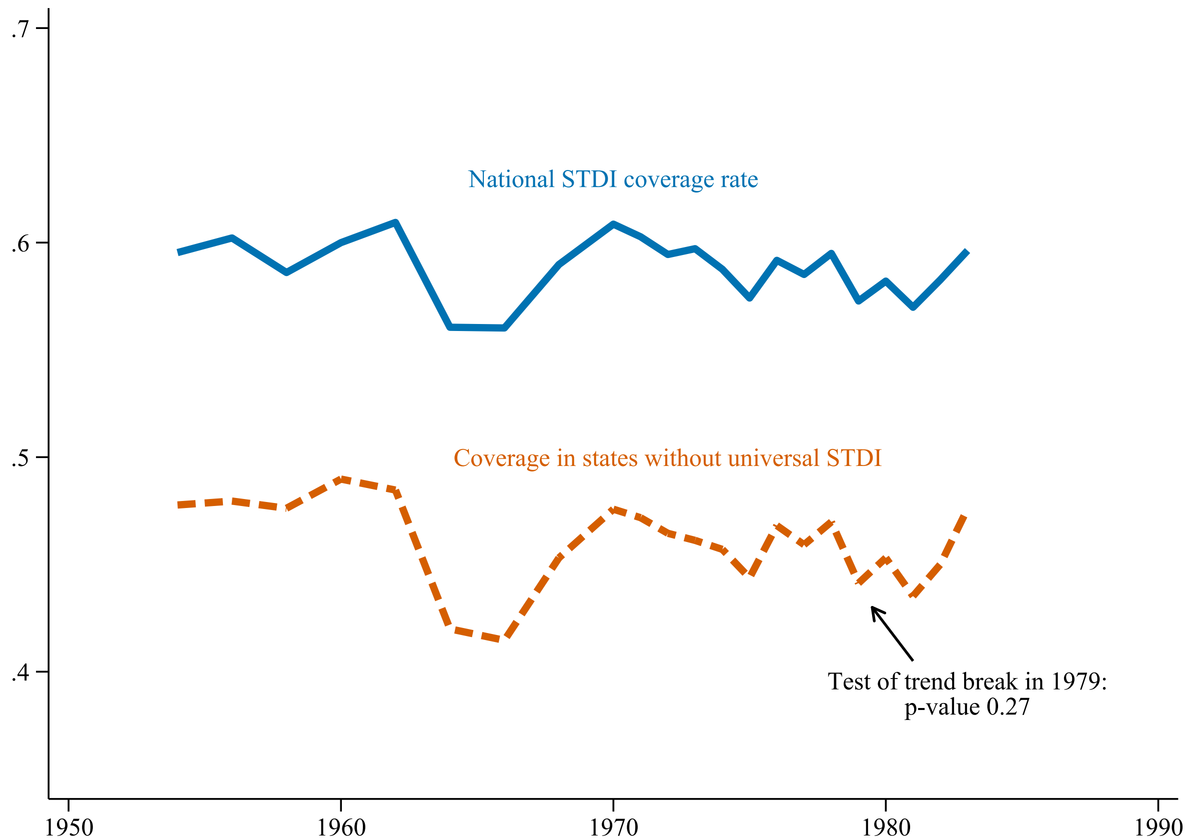
Appendix Figures and Tables

Figure A1: Share of new mothers claiming STDI maternity benefits by subgroup, 1984-1989



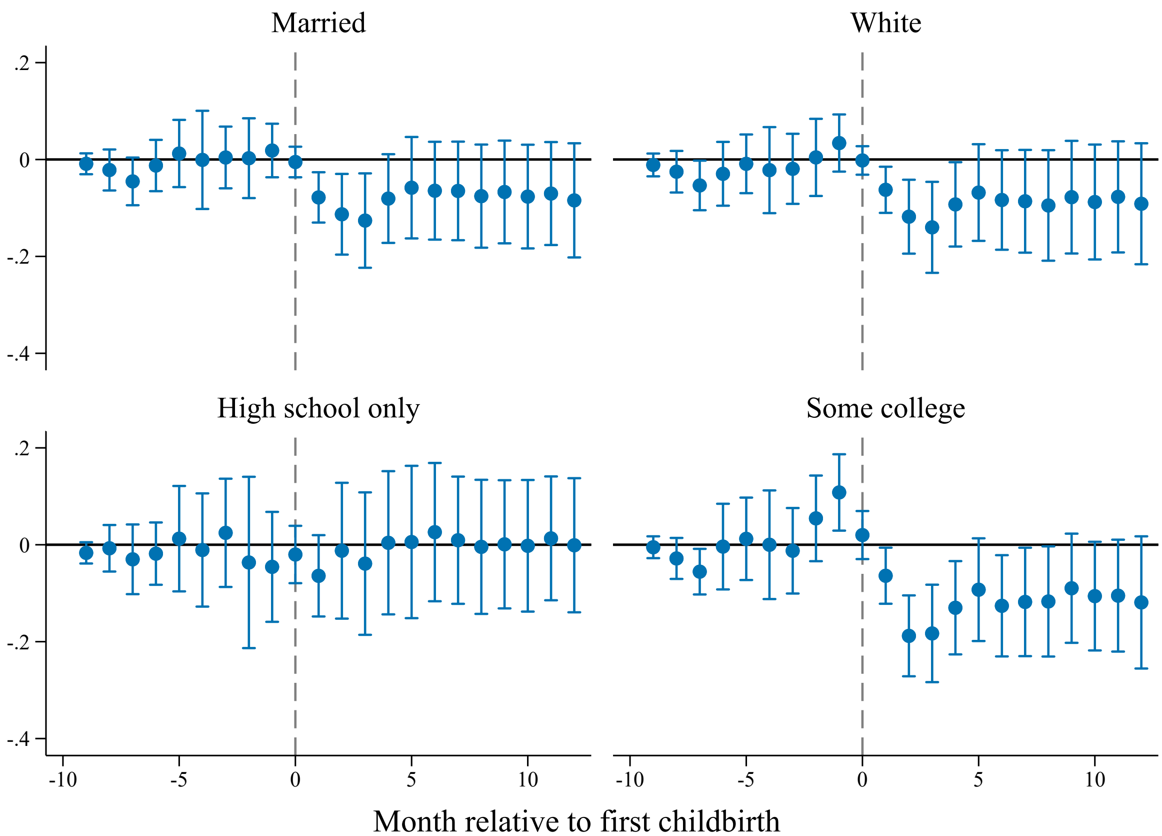
Notes: Data comes from sample of women age 18-45 who give birth during the 1984-1989 panels of the Survey of Income and Program Participation. Blue bars show share receiving STDI maternity benefits during the third trimester, the month of birth, or the three months after birth in universal-STDI states of California, New York, New Jersey, Hawaii, and Rhode Island. Orange bars show share receiving benefits in all other states. All differences are statistically significant at the 1 percent level.

Figure A2: Share of U.S. workers with STDI coverage



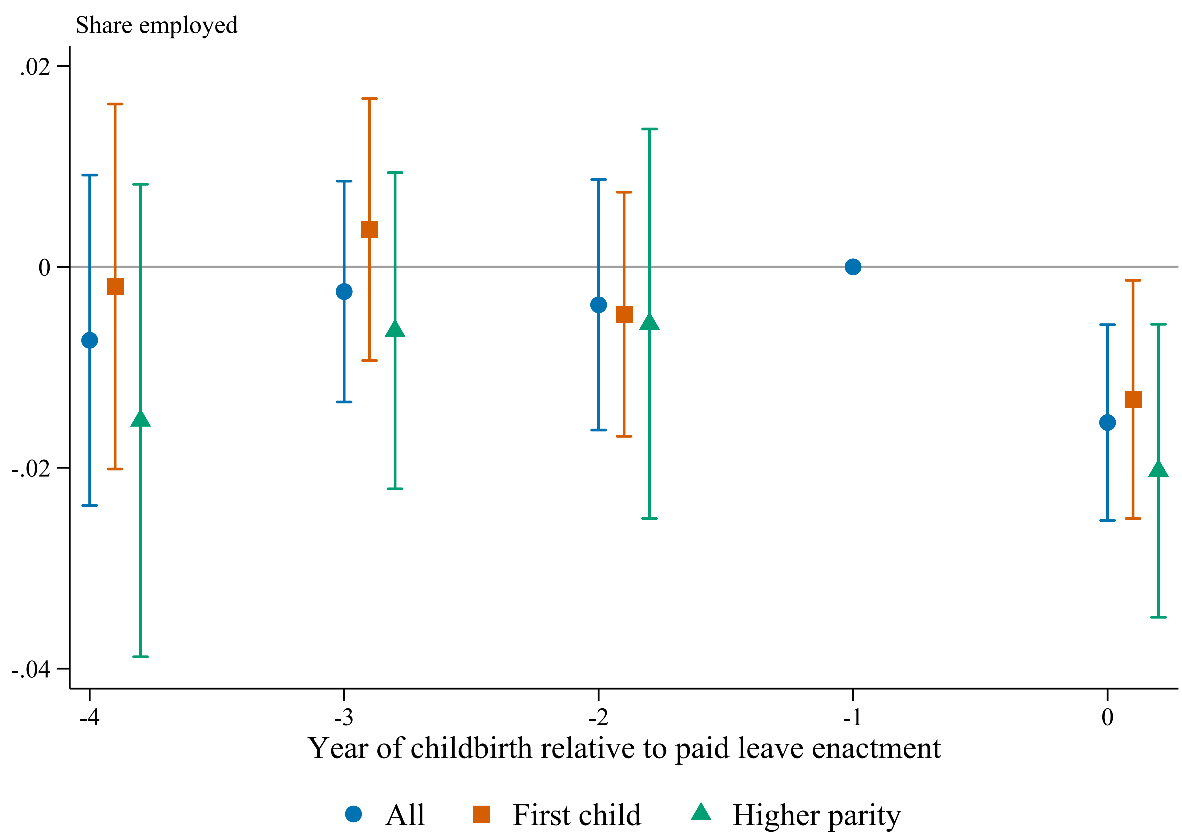
Notes: Figures come from Social Security Administration estimates of the share of private-industry workers with short-term sickness protection (Price, 1986), Table 2. States with universal STDI include California, New Jersey, New York, and Rhode Island. Hawaii adopted universal STDI in 1969. Prior to 1968, data includes children age 14-15 and excludes certain groups of self-employed workers.

Figure A3: Short-run effect on mothers' labor supply in months around childbirth, by subgroup



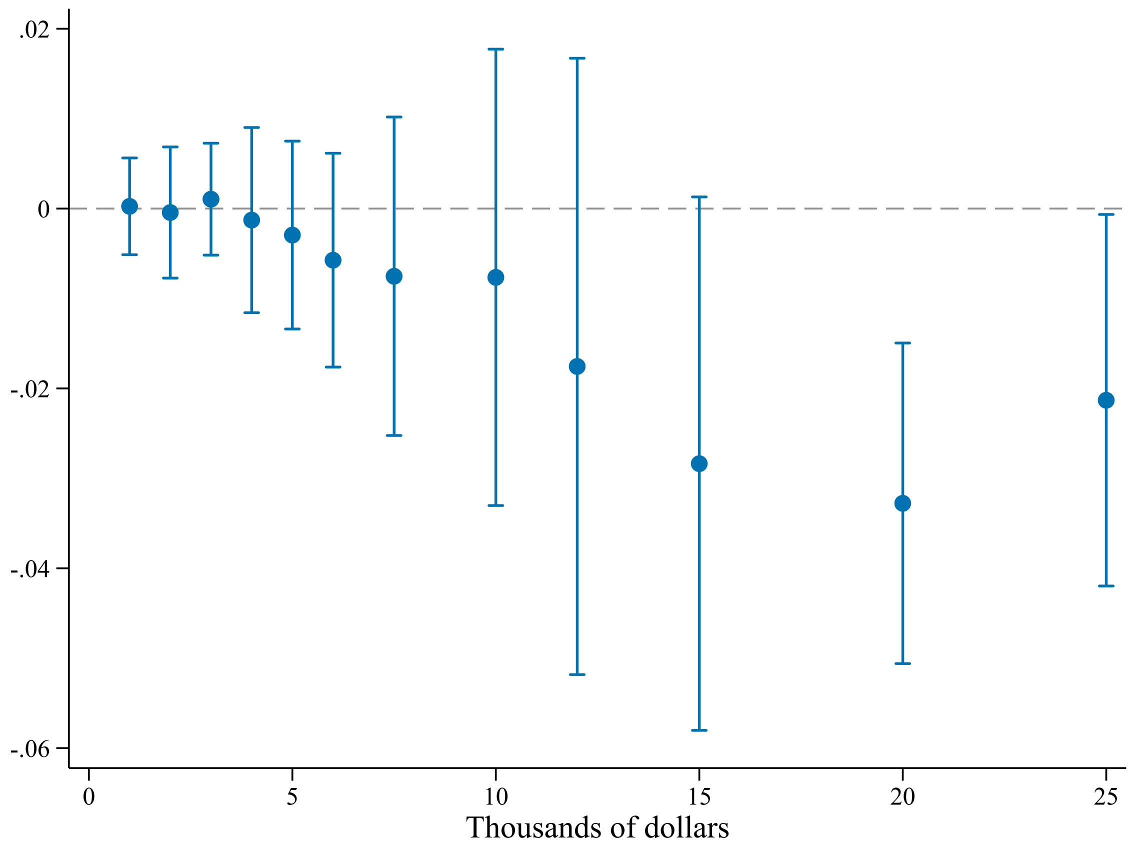
Notes: Figure shows estimated effect of access to STDI maternity benefits on the share of mothers at work, separately by month relative to first childbirth. Data includes women from the retrospective fertility module in the 1984 and 1985 SIPP. Sample is limited to women whose first child was born between 1970 and 1985 while between the ages of 18 and 45, and who worked at some point during their first pregnancy. Each panel is additionally limited to the specified subgroup. Point estimates for nonwhite and unmarried subgroups (not shown) are smaller in magnitude but too noisy to rule out effects equal to those for white and married subgroups. Women are asked about labor supply by month only if they worked during their first pregnancy. Figure shows intent-to-treat estimates of STDI exposure on time spent at work by month relative to childbirth, using a version of equation (1) that pools event time such that each estimate corresponds to τ_k where $k \in [0,7]$. Specification includes fixed effect for state of childbirth, year of childbirth, and month of childbirth. Standard errors in are clustered at the state of birth level.

Figure A4: Effect of STDI maternity benefits on mothers' labor supply in 1980



Notes: Figure shows estimates of τ_k from equation 1 using an indicator for employment as the dependent variable. Sample constructed from the 1980 decennial Census (Ruggles et al., 2017). Sample includes all child-mother pairs in which child was born between 1970 and 1980 and in which child can be linked to the mother using household relationship variables. Standard errors are clustered by child's state of birth and mother.

Figure A5: Effect of maternity benefits on women's family income



Notes: Figure plots estimates of the effect of STDI maternity benefit enactment on the share of women age 18-45 with family income above the specified threshold. Sample includes women age 18-45 from the 1972-1981 CPS May extracts. Family income is measured in nominal dollars. For reference, $20,000 in 1979 dollars is about $70,000 in 2019 dollars when adjusted using the CPI. Regressions are weighted using CPS sampling weights. Standard errors are clustered at the state-group level.

Figure A6: Baltimore Sun newspaper account of industry lobbying against STDI maternity benefits

Chamber to press for veto of pregnancy benefit bill

By JAMES A. ROUSMANIERE, JR.

The Maryland Chamber of Commerce has put out an urgent call to its member companies to pressure for a veto of newly-passed pregnancy benefit legislation.

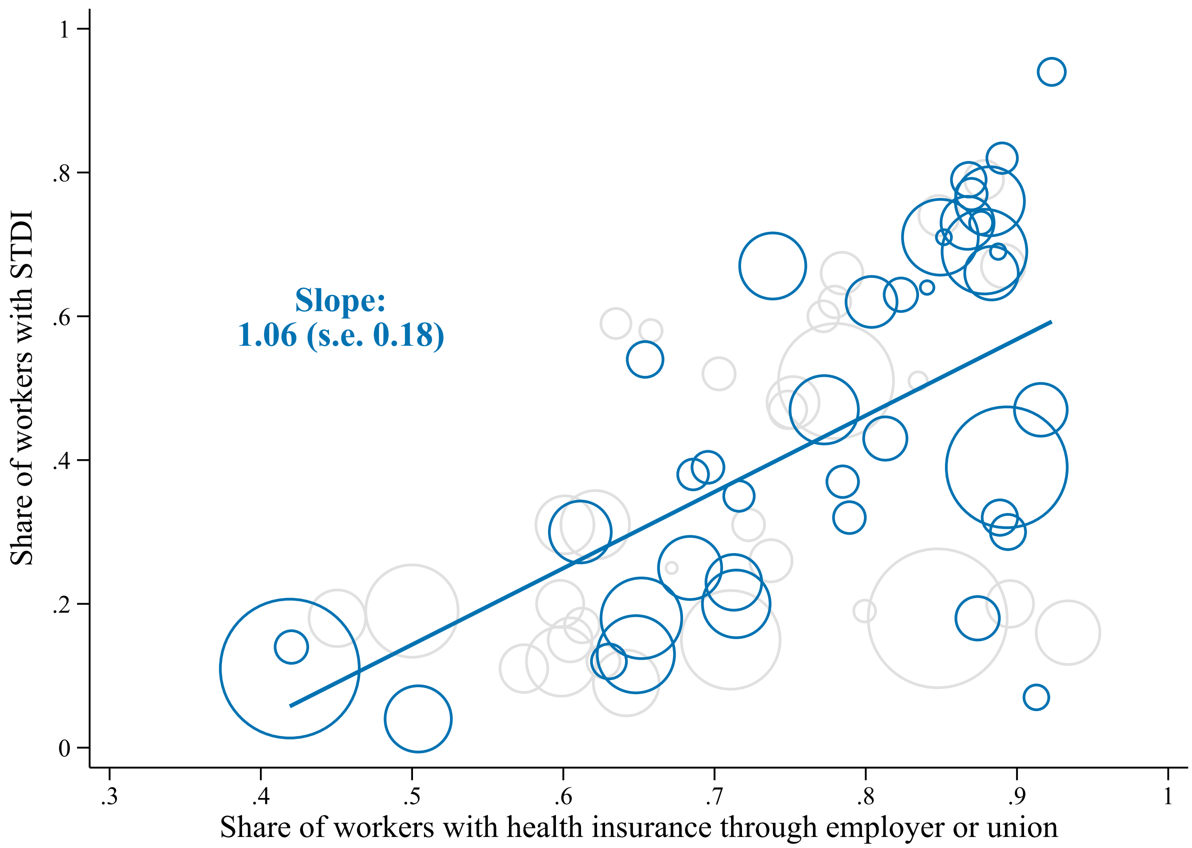
The business group said costs to employers would rise substantially if the bill is signed into law by the governor. It added, in a letter to Governor Mandel, that the added costs would impede efforts to draw more industry to the state.

The bill, allowing up to six weeks of disability payments for pregnancy, was to," said Eugene B. Moore, president of the chamber. He said enactment of the bill will "greatly" raise employers costs. Illustrations were provided in the chamber's letter to the governor, signed by Chairman W. Gordon Yates:

"A large Maryland utility having few female employees had 24 pregnancy cases [in 1976]. Multiplying this by an average wage of $210 weekly times six weeks of disability would result in an annual additional costs of over $30,000."

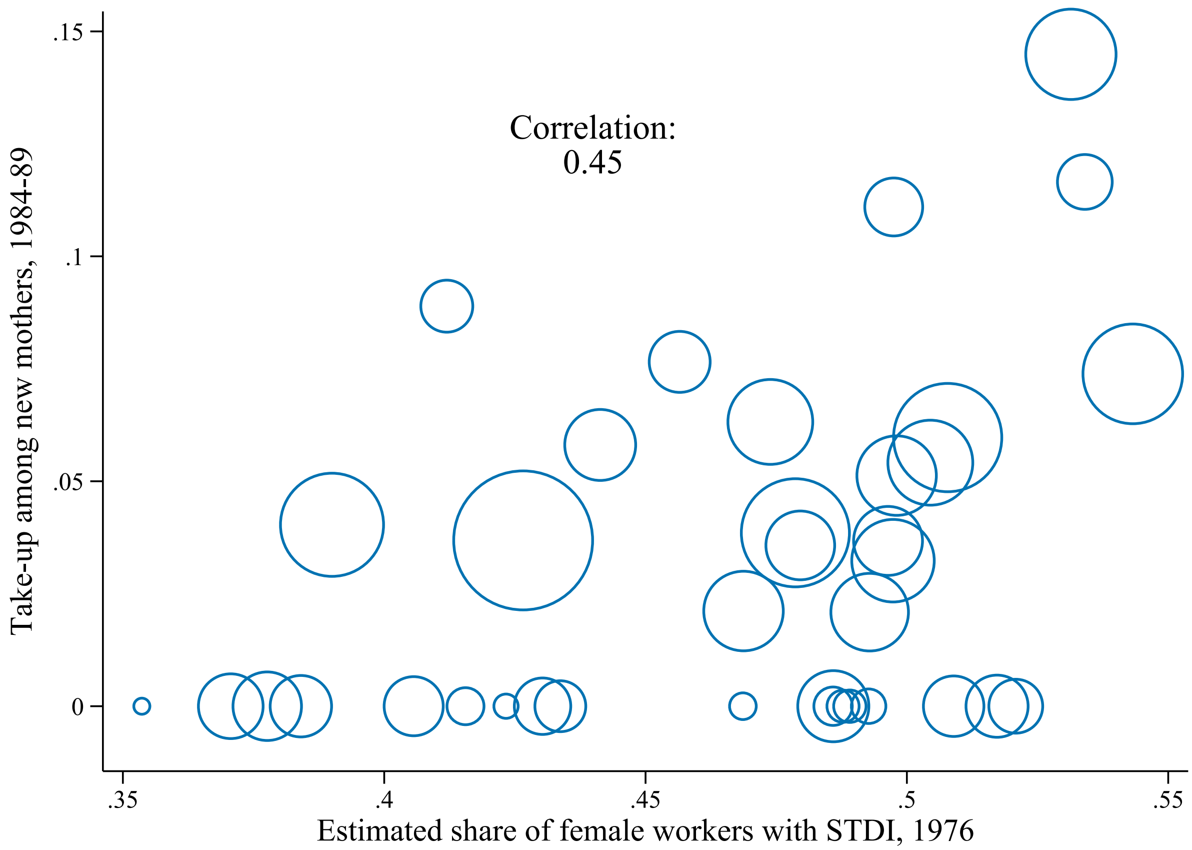
Notes: Baltimore Sun article from May 13, 1977, on Maryland business groups' unsuccessful push against STDI maternity benefits.

Figure A7: STDI coverage and health insurance coverage by industry, 2012



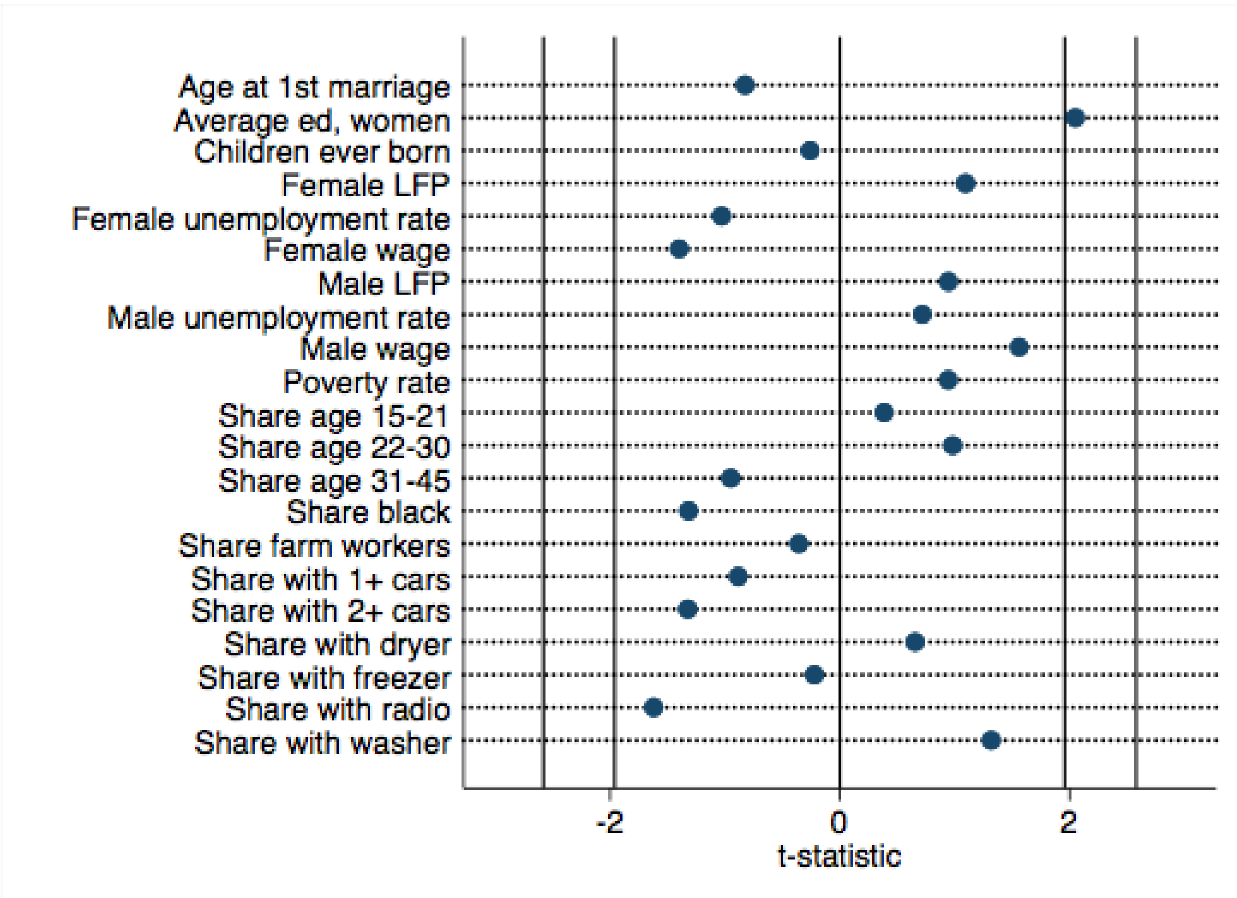
Notes: Data on health insurance coverage comes from the American Community Survey 2012 accessed via IPUMS (Ruggles et al., 2017). Each circle shows the industry share with coverage under an employer or union plan. Data on STDI coverage is drawn from tabulation in Autor et al. (2013). Circle size is proportional to the number of working women age 18-45 employed in the industry. Blue circles represent industries with below-median share of workers located in universal STDI states of California, Hawaii, New Jersey, New York, and Rhode Island. Fitted line comes from simple regression of industry-level STDI coverage on industry-level health insurance coverage, weighted by worker population using sample of industries with below-median share of workers located in universal STDI states. Regression using full sample results in estimated slope of 0.80 (s.e. 0.20).

Figure A8: STDI coverage and take-up among new mothers



Notes: The take-up rate on the vertical axis is the share of mothers who gave birth during the 1984-1989 panels of the Survey of Income and Program Participation and received STDI benefits in the two months before, month of, or three months after childbirth. Share of working women with STDI coverage is estimated using the 1976 Survey of Income and Education as described in section A.1. Circle size is proportional to the SIPP sample size in each state or state group. All five universal STDI states are excluded for ease of comparison among states where STDI coverage is estimated. Including the five universal STDI states in the analysis, and assuming universal coverage, results in a correlation coefficient of 0.84.

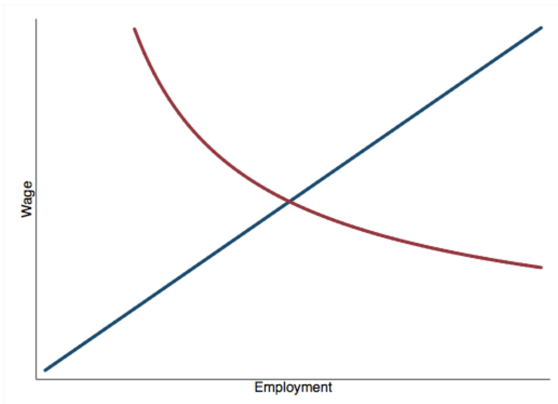
Figure A9: Correlation of anti-discrimination law passage and state characteristics



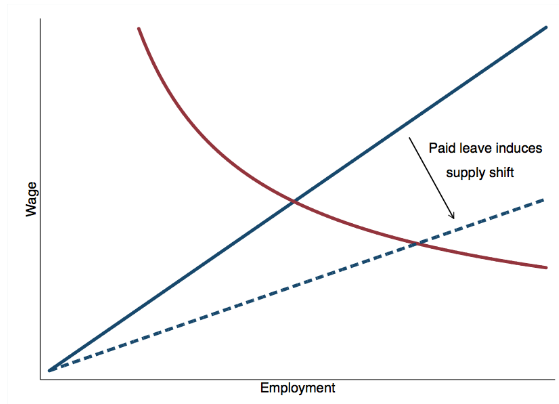
Notes: Plot shows t-statistics from multivariate regression with dependent variable of year STDI-funded maternity leave benefits were enacted at the state level. Regressions are weighted by the 1960 state population. Data on state characteristics comes from the 1960 long-form decennial Census accessed via IPUMS (Ruggles et al., 2017).

Figure A10: Expected labor-market effects of paid maternity leave

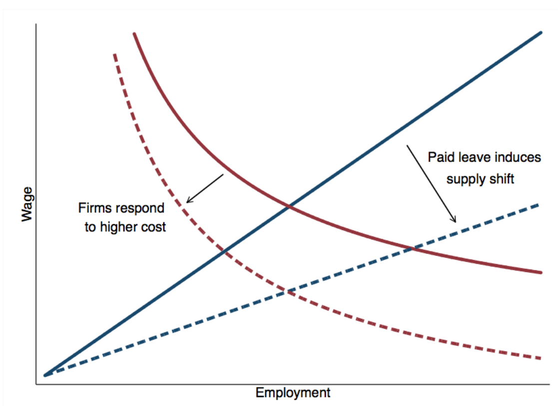
(a) Labor supply and demand



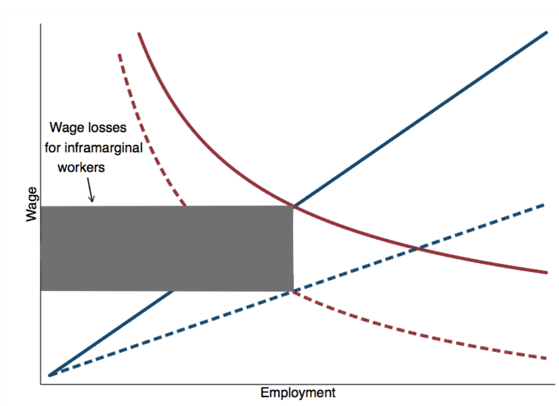
(b) Labor supply shifts



(c) Labor demand response



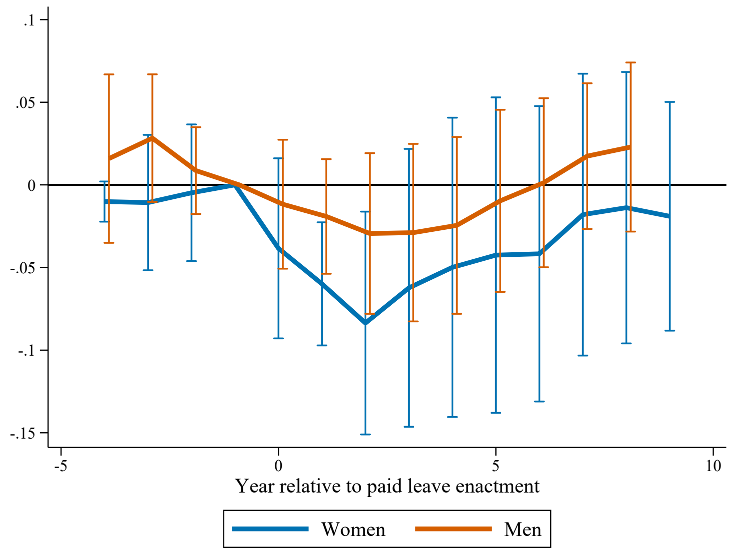
(d) Income loss for inframarginal women



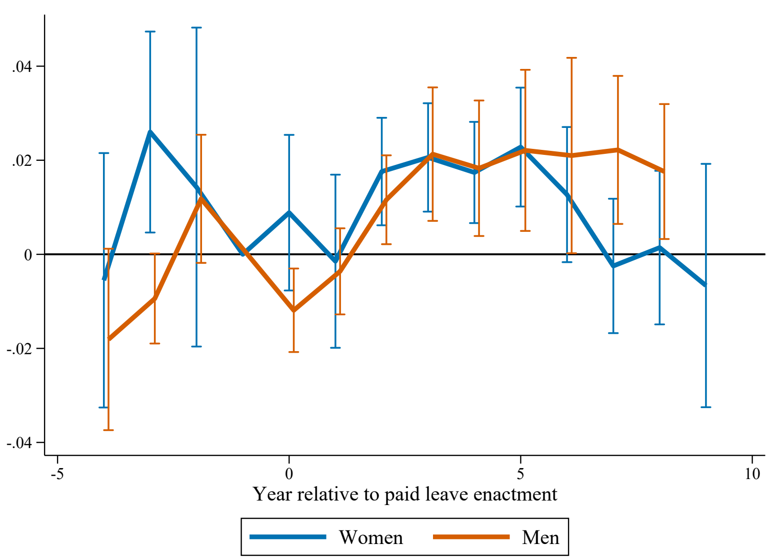
Notes: Figure shows graphical representation of stylized labor-market model outlined in Section 3. Panel A10a shows initial labor-market equilibrium. Panel A10b shows response of women to enactment of benefit that reduces disutility of work. In Panel A10c, firms respond to the cost of providing the benefit. Panel A10d shows the impact of wages lost among inframarginal workers who are impacted by the change in the equilibrium wage but would have remained in the labor force in the absence of paid leave.

Figure A11: Effect of STDI maternity benefits on labor-market outcomes, universal STDI states vs. non-universal states

(a) Log wages, universal STDI vs. non-universal states

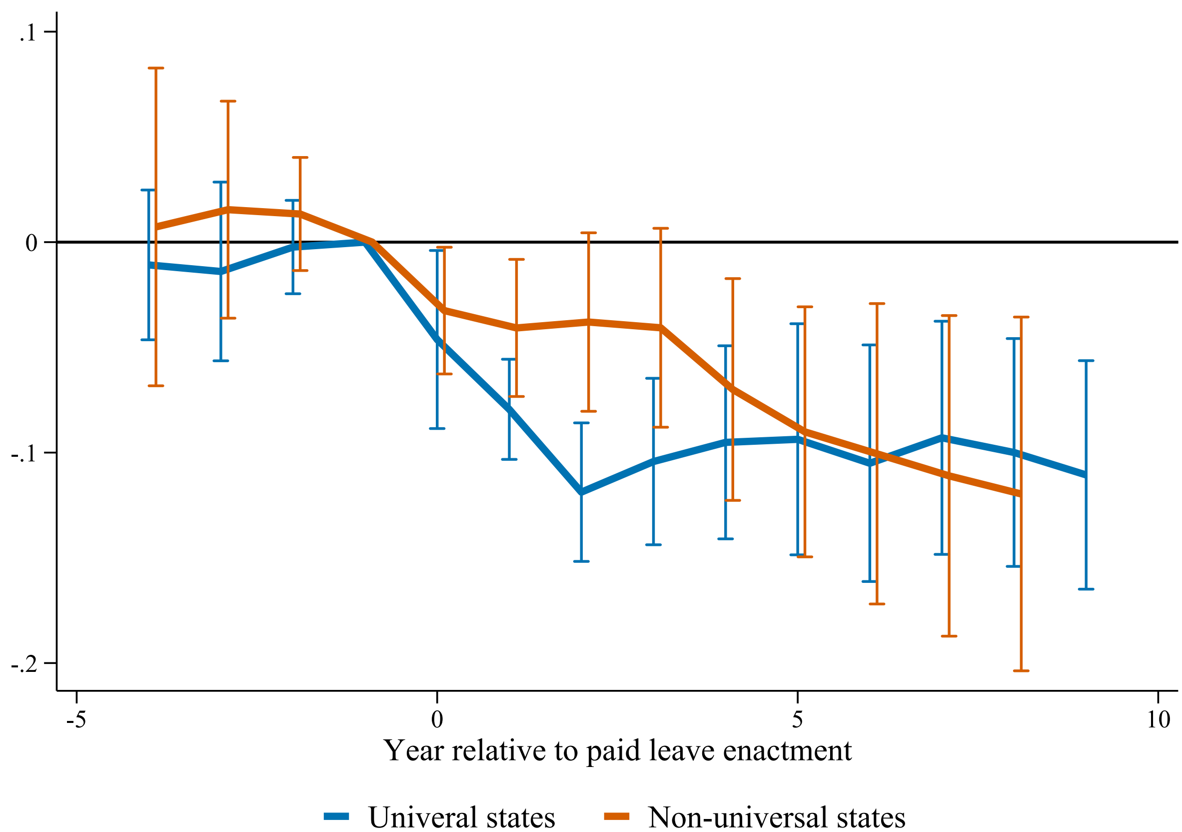


(b) Employment



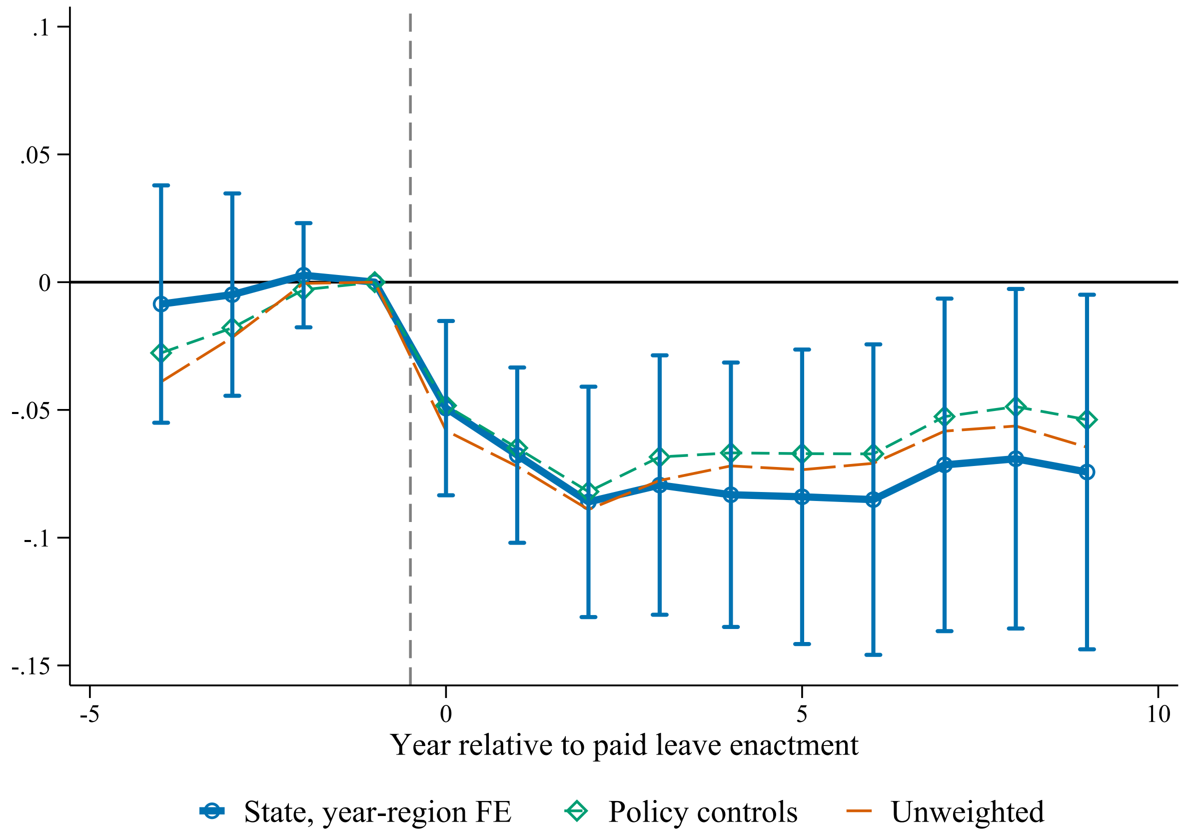
Notes: Plot shows estimated effects of STDI maternity benefits on the log wages and employment of women age 18-45 from equation 1, with $STDI_s$ defined as a binary indicator of a universal STDI state and all other states used as a comparison group. Data comes from the CPS May and MORG files, 1973-1987. Confidence intervals constructed using standard errors clustered at the state or state-group level.

Figure A12: Heterogeneity in the effect of STDI maternity benefits on women's log wages, by state disability policy



Notes: Plot shows estimated effects of STDI maternity benefits on the log wages of women age 18-45 from a modified version of equation 1 that allows for separate estimates of τ_k for states with and without universal STDI. Data comes from the CPS May and MORG files, 1973-1987. Confidence intervals constructed using standard errors clustered at the state or state-group level.

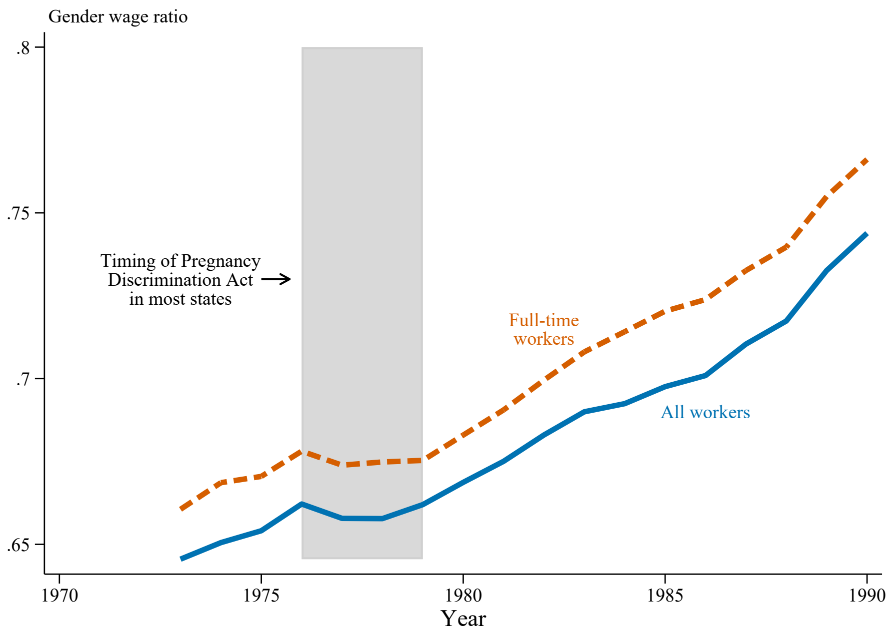
Figure A13: Effect of STDI maternity benefits on women's log wages, using alternative measure of STDI coverage



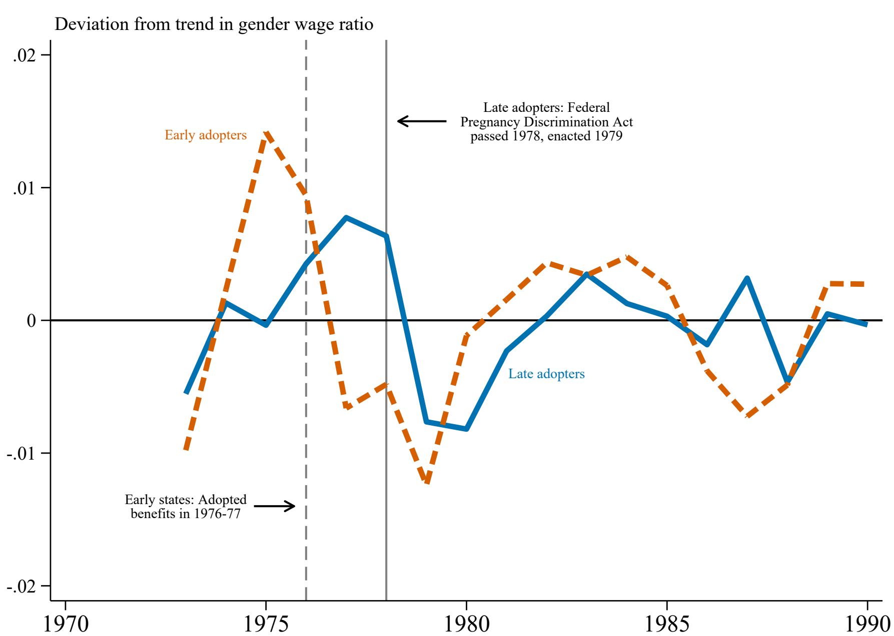
Notes: Plot shows estimated effects of STDI maternity benefits on the log wages of women age 18-45 from equation 1, with $STDI_s$ estimated using industry-level disability insurance coverage from Autor et al. (2013), as described in Appendix C.3. Data comes from the CPS May and MORG files, 1973-1987. Confidence intervals constructed using standard errors clustered at the state or state-group level.

Figure A14: Wage convergence slows after maternity benefits adopted

(a) Trend in the gender wage gap

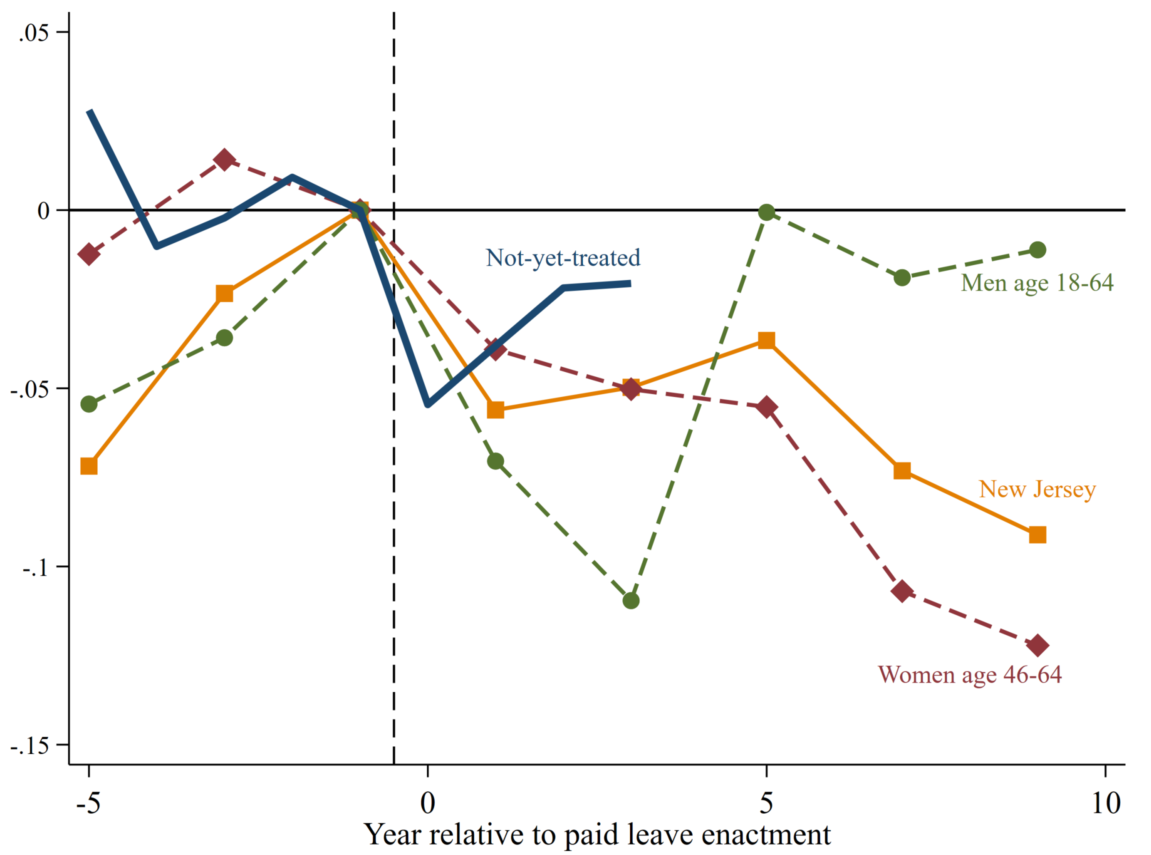


(b) Deviation from trend, by date of anti-discrimination law



Notes: Data from 1973-1993 CPS May and MORG files. Figure plots the deviation from trend of the gender wage ratio for full-time workers, separately for two groups of states: One that adopted STDI maternity benefits in late 1976 and early 1977, and one that adopted benefits after passage of the Pregnancy Discrimination Act of 1978. The gender wage ratio is calculated as the exponential of the difference in the average log hourly wage for women age 18-45 and the average log hourly wage for men age 18-64 in each year. Deviation from trend is calculated as the residual from a regression of the gender wage gap for full-time employees on a quadratic time trend.

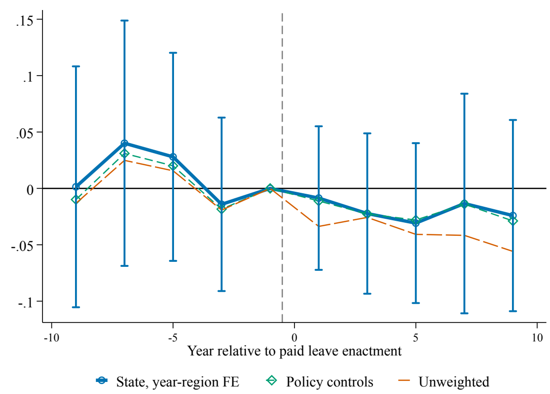
Figure A15: Effect on women's log wages using alternative estimator



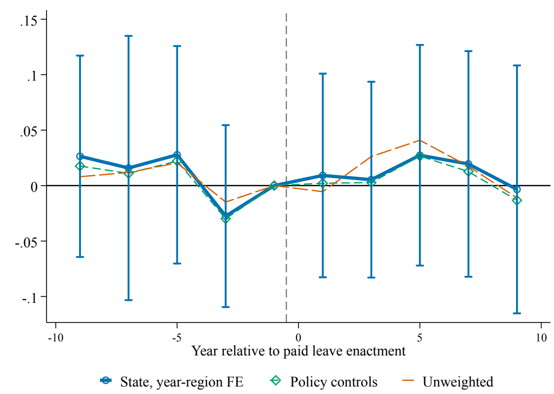
Notes: Plot shows estimated effects on the log wages of women age 18-45 using an alternative estimator proposed by Sun and Abraham (2021). Text annotations specify the comparison group used for each set of estimates. Data comes from the CPS May and MORG files, 1973-1987, accessed via the NBER.

Figure A16: Effect of STDI maternity benefits on mothers' employment before and after childbirth

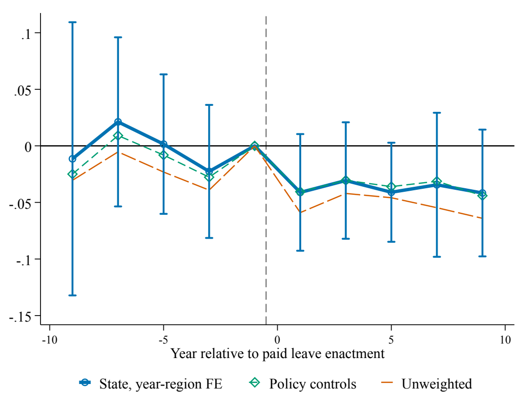
(a) Share employed before childbirth



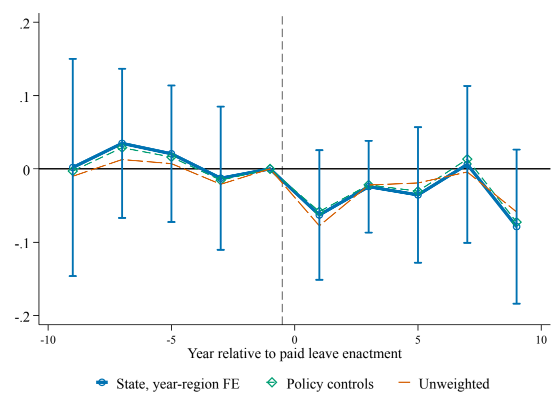
(b) Share employed before first childbirth



(c) Share employed after childbirth



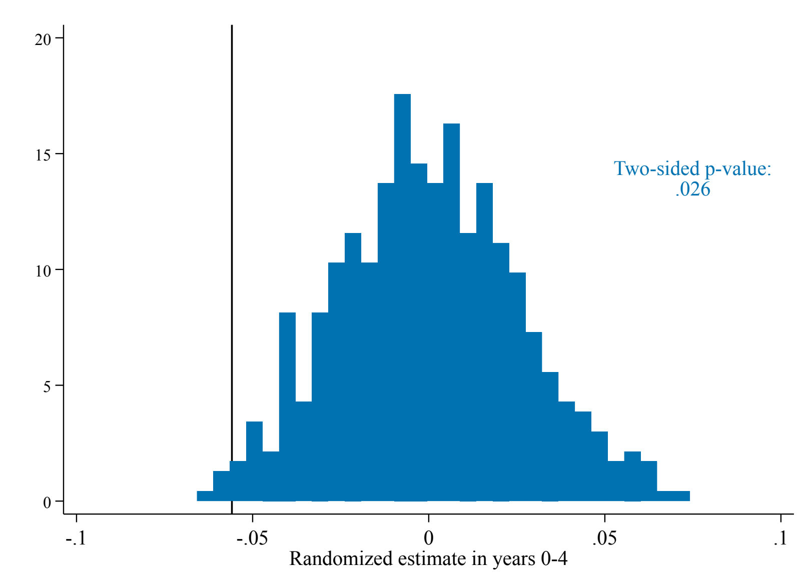
(d) Share employed after first childbirth



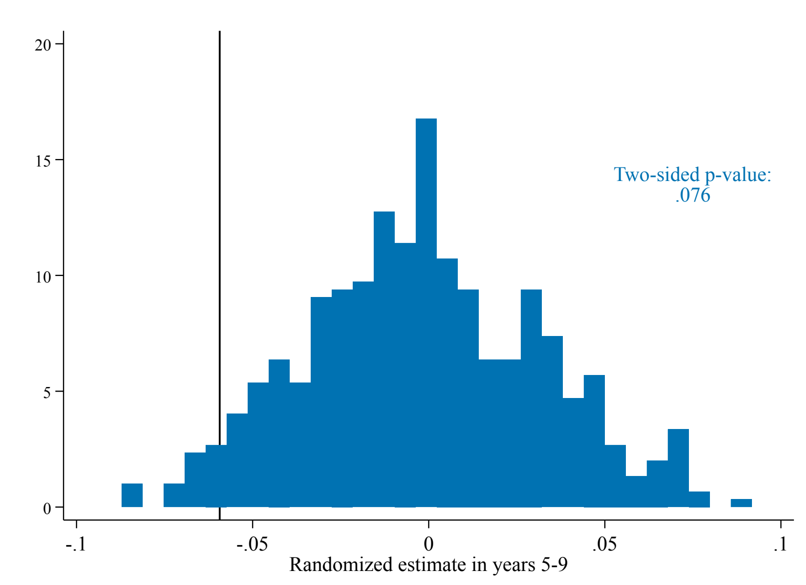
Notes: Figure shows event-study estimates from equation (1) using sample of women and men age 18-45 from the 1964-1988 CPS ASEC (Ruggles et al., 2017). To improve precision, event time indicators are binned into two-year groups. Sample includes women who have a child age 0 in the household. First-time mothers are the subsample in which the newborn child is the only child in the household. Employment prior to childbirth (Figures A16a and A16b) is a binary indicator for working any weeks during the previous calendar year. Employment after childbirth is measured as of the prior week. Standard errors are clustered at the state-group level.

Figure A17: Randomization test of no effect on women's log wages

(a) First five years after reform



(b) Second five years after reform



Notes: Figure shows the distribution of estimates of τ_k from equation 3 with $STDI_{s,1970}$ and date of anti-discrimination laws assigned randomly across states without replacement. Two-sided p-value is the share of 500 replications in which the absolute value of $\hat{\tau}_k$ is larger than the absolute value of the estimates shown in column 1 of Table 3 (shown as vertical line).

Table A1: Variation in timing and intensity of the expansion of STDI pregnancy benefits

	Universal STDI adopted	Pregnancy benefits adopted	Mode of passage
Rhode Island	1942	1942	Legislature
New Jersey	1948	1961	Legislature
Montana	–	1972	Legislature
Connecticut	–	1973	Legislature
Hawaii	1969	1973	Legislature
Alaska	–	1975	Legislature
Iowa	–	1975	State court
Kansas	–	1975	Administrative
South Dakota	–	1975	Administrative
Wisconsin	–	1975	State court
Illinois	–	1976	Administrative
California	1946	1977	Legislature
Maryland	–	1977	Legislature
Michigan	–	1977	Legislature
Minnesota	–	1977	Legislature
New York	1949	1977	State court
Pennsylvania	–	1977	State court
Washington, DC	–	1977	Legislature
Massachusetts	–	1978	State court
All other states	–	1979	Congress

Notes: Column 1 lists date that state adopted universal STDI law, where applicable. Column 2 shows the year each state's anti-pregnancy-discrimination law was enacted. Note that this may differ from the date the relevant law or policy was approved. In the analysis of labor-market effects, I use the date of approval rather than enactment where possible (e.g., August 1976 for California, December 1976 for New York). Column 3 lists the political entity that spurred enactment of the anti-pregnancy-discrimination law.

Table A2: Details of state and private STDI maternity benefits

State	Coverage threshold	Funding mechanism	Eligibility criteria	Benefit level	Benefit duration
California	1 or more employees	Worker contributes 0.8% of earnings (capped)	$300 in wages in 12 months prior to disability	Schedule amounts to 55% of earnings	6 weeks
Hawaii	1 or more employees	Worker contributes up to $1.61, employers the remainder	14 weeks with 20+ hours worked in previous year	55% of average weekly wage	Physician's discretion
New Jersey	1 or more employees, $\geq$ $1,000 payroll	Worker contributes 0.5% of earnings (capped)	17 "base" weeks with $15+ in earnings or $2,200 previous year for 4 weeks	2/3 of weekly wage	8 weeks
New York	1 or more employees	Employer mandate	Employed by covered firm for 4 weeks	50% of weekly wage	8-12 weeks
Rhode Island	1 or more employees	Workers pay 1.5% of first earnings up to a cap	20 weeks with $62 earnings or $3,720 total in last year	55% of weekly wage + $5/month per child (capped)	14 weeks
Private plans	N/A	Varies; mix of employer and worker contributions	Varies	50-67% of recent wages (usually capped)	7.5 weeks on average

Notes: Adapted from Kamerman, Kahn and Kingston (1983) and supplemented with information from Skolnik (1968), Skolnik (1976) and Gladstone, Williams and Belous (1985). Rhode Island's benefits were capped at $250 from 1969-1980, then at $500 until 1982, then raised to 60 percent of average weekly wage of covered employees.

Table A3: State groups in CPS samples

State	May CPS 1973-87	May CPS 1969-87	March CPS 1964-88	State	May CPS 1973-87	May CPS 1969-87	March CPS 1964-88
Alabama	AL/MS/TN	AL/MS/TN	AL/MS	Montana	Mountain*	Mountain*	Mountain*
Alaska	AK/OR/WA	AK/HI/OR/WA	AK/HI/OR/WA	Nebraska	Midwest***	IA/Midwest***	IA/MO/Midwest***
Arizona	Mountain*	Mountain*	Mountain*	Nevada	Mountain*	Mountain*	Mountain*
Arkansas	AR/OK	AR/OK	AR/LA/OK	New Hampshire	Northeast**	MA/Northeast**	CT/MA/Northeast**
California	CA	CA	CA	New Jersey	NJ	NJ	NJ
Colorado	Mountain*	Mountain*	Mountain*	New Mexico	Mountain*	Mountain*	Mountain*
Connecticut	CT	CT	CT/MA/Northeast**	New York	NY	NY	NY
Delaware	DE/MD/SC/WV	DE/MD/NC/SC/VA/WV	DE/MD/VA/WV	North Carolina	NC	DE/MD/NC/SC/VA/WV	GA/NC/SC
Florida	FL	FL	FL	North Dakota	Midwest***	IA/Midwest***	IA/MO/Midwest***
Georgia	GA	GA	GA/NC/SC	Ohio	OH	OH	OH
Hawaii	HI	AK/HI/OR/WA	AK/HI/OR/WA	Oklahoma	AR/OK	AR/OK	AR/LA/OK
Idaho	Mountain*	Mountain*	Mountain*	Oregon	AK/OR/WA	AK/HI/OR/WA	AK/HI/OR/WA
Illinois	IL	IL	IL	Pennsylania	PA	PA	PA
Indiana	IN	IN	IN	Rhode Island	Northeast**	CT/Northeast**	CT/MA/Northeast**
Iowa	IA	IA/Midwest***	IA/MO/Midwest***	South Carolina	DE/MD/SC/WV	DE/MD/NC/SC/VA/WV	GA/NC/SC
Kansas	Midwest***	IA/Midwest***	IA/MO/Midwest***	South Dakota	Midwest***	IA/Midwest***	IA/MO/Midwest***
Kentucky	KY	KY	KY/TN	Tennessee	AL/MS/TN	AL/MS/TN	KY/TN
Louisiana	LA	LA	AR/LA/OK	Texas	TX	TX	TX
Maine	Northeast**	CT/Northeast**	CT/MA/Northeast**	Utah	Mountain*	Mountain*	Mountain*
Maryland	DE/MD/SC/WV	DE/MD/NC/SC/VA/WV	DE/MD/VA/WV	Vermont	Northeast**	MA/Northeast**	CT/MA/Northeast**
Massachusetts	MA	MA/Northeast**	CT/MA/Northeast**	Virginia	VA	DE/MD/NC/SC/VA/WV	DE/MD/VA/WV
Michigan	MI	MI/WI	MI/WI	Washington	AK/OR/WA	AK/HI/OR/WA	AK/HI/OR/WA
Minnesota	Midwest***	IA/Midwest***	IA/MO/Midwest***	West Virginia	DE/MD/SC/WV	DE/MD/NC/SC/VA/WV	DE/MD/VA/WV
Mississippi	AL/MS/TN	AL/MS/TN	AL/MS	Wisconsin	WI	MI/WI	MI/WI
Missouri	MO	MO	IA/MO/Midwest***	Wyoming	Mountain*	Mountain*	Mountain*

Notes: Table shows state groups used to maintain consistent geographical units in each CPS sample. There are 29 state groups in the May CPS 1973-87 (27 after dropping Connecticut and Hawaii, which adopted STDI maternity benefits too early to see a pre-period), 23 state groups in the May CPS 1969-87, and 20 state groups in the March CPS. CPS May and MORG extracts obtained from NBER. March CPS samples from IPUMS (Ruggles et al., 2017).
*Mountain states are Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming.
**Northeast states are Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont. Connecticut is separately identified in IPUMS data but still combined with the northastern states in the March CPS because analysis of demographic trends suggest the observations identified as from Connecticut are misidentified in the early 1970s.
***Midwest states are Minnesota, North Dakota, South Dakota, Nebraska, and Kansas.

Table A4: Short-run effects on mothers' leave-taking and employment

	All mothers			Worked last year	
	(1) On leave	(2) Worked last year	(3) Employed	(4) On leave	(5) Employed
Panel A: Full sample					
STDI x Post	0.0229**	-0.0192	0.0160	0.0489**	0.0593***
	(0.0097)	(0.0162)	(0.0221)	(0.0227)	(0.0171)
1970 mean	0.04	0.51	0.14	0.08	0.25
Observations	54,579	54,579	54,579	32,401	32,401
Panel B: First-time mothers					
STDI x Post	0.0304**	-0.0312	0.0205	0.0475**	0.0477***
	(0.0122)	(0.0325)	(0.0174)	(0.0186)	(0.0165)
1970 mean	0.05	0.75	0.16	0.07	0.20
Observations	20,806	20,806	20,806	16,493	16,493
Panel C: Higher-parity mothers					
STDI x Post	0.0099	-0.0362	0.0007	0.0395	0.0598*
	(0.0087)	(0.0227)	(0.0296)	(0.0254)	(0.0306)
1970 mean	0.03	0.38	0.12	0.09	0.29
Observations	33,773	33,773	33,773	15,907	15,907
Panel D: Some college or more					
STDI x Post	0.0663***	0.0003	0.0257	0.0917***	0.0523**
	(0.0231)	(0.0534)	(0.0223)	(0.0324)	(0.0256)
1970 mean	0.05	0.60	0.14	0.08	0.23
Observations	16,891	16,891	16,891	11,702	11,702
Panel E: High school or less					
STDI x Post	-0.0074	-0.0309	-0.0014	0.0046	0.0424
	(0.0091)	(0.0332)	(0.0315)	(0.0188)	(0.0281)
1970 mean	0.04	0.49	0.13	0.08	0.25
Observations	37,687	37,687	37,687	20,696	20,696
Panel F: Children born 4th quarter of 1969 or 1979					
STDI x Post	0.0063	-0.0242	0.0112	0.0142	0.0277
	(0.0050)	(0.0272)	(0.0185)	(0.0097)	(0.0189)
1970 mean	0.01	0.47	0.19	0.02	0.35
Observations	56,614	56,614	56,614	31,076	31,076

Notes: Estimates come from simple difference-in-difference estimates (equation 1 with k restricted to pre- and post-reform) of the effect of access to STDI maternity benefits on the labor-market outcomes of mothers of newborn children. Covariates include indicators for non-Hispanic and nonwhite interacted with survey year. Samples in panels A-E include women age 18-45 in the 1970 and 1980 long-form Census who gave birth in the 1st quarter of the year of the Census (Ruggles et al., 2017). Sample in panel F includes women who gave birth in 4th quarter of year prior to the Census. Women are coded as on leave if they had a job but were not at work in the previous week. Women are coded as working in the previous year if they reported working for pay at any time during the previous year. First-time mothers include women who gave birth in the previous quarter but could not be linked to any other children in the household. Higher-parity mothers are women who gave birth in the previous quarter but are also linked as mothers to other, older children in the household. Standard errors are robust to heteroskedasticity and arbitrary correlation within state of child's birth.

Table A5: Tests for demographic changes in CPS sample

	(1)	(2)	(3)	(4)	(5)
	Nonwhite	Hispanic	Age	Years of education	Married
Panel A: All women age 18-45					
STDI x Years 0-4	0.002	0.002	0.067	-0.081*	-0.005
	(0.004)	(0.006)	(0.088)	(0.045)	(0.008)
STDI x Years 5-9	0.004	0.007	-0.125	-0.124**	-0.020*
	(0.006)	(0.008)	(0.091)	(0.054)	(0.011)
Mean	0.15	0.07	30.18	12.57	0.61
R-squared	0.05	0.08	0.00	0.01	0.01
Observations	1,252,517	1,252,517	1,252,517	1,252,517	1,252,517
Panel B: Working women age 18-45					
STDI x Years 0-4	0.001	-0.003	0.111	-0.061	0.012
	(0.004)	(0.005)	(0.109)	(0.056)	(0.010)
STDI x Years 5-9	0.003	-0.002	-0.170	-0.084	-0.002
	(0.006)	(0.005)	(0.147)	(0.067)	(0.014)
Mean	0.14	0.05	30.26	12.96	0.56
R-squared	0.05	0.07	0.01	0.01	0.01
Observations	777,396	777,396	777,396	777,396	777,396

Notes: Table displays estimates of τ_k from equation 3, using demographic characteristics as the outcome. Sample in panel A includes all women age 18-45. Sample in panel B is restricted to women with a valid hourly wage. Mean outcomes are averaged across women in my sample in the year prior to the enactment of STDI maternity benefits in their state of residence. Standard errors are robust to heteroskedasticity and arbitrary correlation within state of child's birth.

Table A6: Replication of Gruber (1994): Effect on log wages

	(1) Gruber 1994	(2) Replication	(3) Replication, by state
Treatment x Post x Experimental State	-0.043*	-0.0467**	
	(0.023)	(0.0224)	
Treatment x Post x Illinois			-0.0627**
			(0.0311)
Treatment x Post x New York			-0.0594**
			(0.0288)
Treatment x Post x New Jersey			-0.00692
			(0.0382)
Observations	27,033	26,971	26,971
R-squared		0.414	0.415

Notes: Table shows replication of results of Gruber (1994) Table 4. Sample includes married women ages 20-40 ("treatment" group) and single men age 20-40 from the 1974, 1975, 1977, and 1978 May CPS. Specification is based on equation 1 in Gruber (1994) and includes controls for years of education, quadratic in potential experience, full interaction of gender and marital status, indicators for nonwhite race and union membership, and year fixed effects. In column 3, equation 1 has been modified to report β_8 separately for each "experimental" state.

Table A7: Heterogeneity of effects on log wages, women age 18-45

	(1) Adjustment costs	(2) Returns to hours worked	(3) Professional occupation	(4) College attendance	(5) Predicted LFP	(6) Occupation share male
Main effect: First 5 years	-0.016	-0.028	-0.034	-0.029	-0.056	-0.059**
	(0.033)	(0.025)	(0.026)	(0.031)	(0.037)	(0.025)
Interaction: First 5 years	-0.063***	-0.055***	-0.069**	-0.057**	-0.006	0.026
	(0.022)	(0.010)	(0.029)	(0.024)	(0.035)	(0.024)
Main effect: Second 5 years	-0.101***	-0.053	-0.066**	-0.072**	-0.075*	-0.065**
	(0.034)	(0.033)	(0.030)	(0.034)	(0.045)	(0.031)
Main effect: Second 5 years	0.065***	-0.012	0.059**	0.030	0.025	0.056***
	(0.021)	(0.015)	(0.025)	(0.022)	(0.041)	(0.021)
Mean	14.92	14.93	14.93	14.93	14.93	14.93
R-squared	0.05	0.03	0.05	0.05	0.23	0.01
Observations	568,687	570,723	570,723	570,902	570,902	570,902

Notes: Table shows estimated effects from short run (years 0-4 relative to enactment of STDI maternity benefits) and long run (years 5-9 relative to enactment of STDI maternity benefits) from equation 4. Sample includes women age 18-45 from 1973-1987 CPS May and MORG extracts. Each column displays an estimated main effect on women's log wages, plus the coefficient from an interaction between an indicator for time relative to the reform and the binary measure of heterogeneity indicated at the top of the column. Adjustment costs are measured at the occupation level using data from the Multi-City Study of Urban Inequality on the length of time a new employee needs to become fully productive (Bobo et al., 2008). Returns to hours worked is an occupation-level measure of the wage premium for working long hours (Cortés and Pan, 2018). Professional occupation is based on Census occupation codes. College attendance is an individual-level measure that equals 1 for individuals with greater than 12 years of education. Predicted labor force participation is constructed by estimating a logit specification with age, race, and education interacted with state of residence in a sample of all women age 18-45 observed 2 years prior to their state's enactment of STDI maternity benefits, then predicting labor force participation for the full sample. Occupation share male is calculated using all years in the sample and all individuals age 18-64. Continuous measures of heterogeneity are split at the median for women age 18-45. Standard errors are clustered at the state-group level.

Table A8: Effect on occupational choice and educational attainment, women age 18-45

	(1) Adjustment costs	(2) Returns to hours worked	(3) Professional occupation	(4) College attendance	(5) Predicted LFP	(6) Occupation share male
Panel A: Unconditional						
First 5 years	-0.017*	-0.012***	-0.011*	-0.010	0.017	-0.003
	(0.010)	(0.004)	(0.006)	(0.008)	(0.013)	(0.002)
Second 5 years	-0.026*	-0.017***	-0.012	-0.009	0.029	-0.003
	(0.014)	(0.006)	(0.008)	(0.010)	(0.018)	(0.002)
Mean	0.45	0.35	0.16	0.37	0.78	0.07
R-squared	0.04	0.02	0.04	0.05	0.23	0.01
Observations	1,239,810	1,245,109	1,245,186	1,252,517	1,252,517	1,252,517
Panel B: Conditional on employment						
First 5 years	0.002	-0.004	-0.020	-0.017	0.008	-0.001
	(0.018)	(0.008)	(0.014)	(0.014)	(0.014)	(0.004)
Second 5 years	-0.010	-0.005	-0.025	-0.020	0.022	0.000
	(0.024)	(0.010)	(0.016)	(0.016)	(0.019)	(0.005)
Mean	0.66	0.49	0.23	0.42	0.82	0.09
R-squared	0.05	0.03	0.05	0.05	0.23	0.01
Observations	568,687	570,723	570,723	570,902	570,902	570,902

Notes: Table shows estimated effects from short run (years 0-4 relative to enactment of STDI maternity benefits) and long run (years 5-9 relative to enactment of STDI maternity benefits) from equation 4. Dependent variable is a binary indicator for being above-median in the measure of occupational characteristics, educational attainment, or predicted labor-force attachment listed at the top of each column. Sample includes women age 18-45 from 1973-1987 CPS May and MORG extracts. See notes to Table A7 or section 4.6 for details on construction of each dependent variable. Standard errors are clustered at the state-group level.

Table A9: Robustness checks: Effect on employment and hourly wages of older women and men

	Outcome: Log wages			Outcome: Employed		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Women age 46-64						
STDI x Years 0-4	-0.026	-0.026	-0.011	-0.011	-0.009	-0.003
	(0.016)	(0.017)	(0.015)	(0.011)	(0.011)	(0.010)
STDI x Years 5-9	-0.024	-0.021	-0.009	-0.022	-0.020	-0.014
	(0.016)	(0.017)	(0.016)	(0.015)	(0.016)	(0.013)
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Mean	15.45	15.45	15.45	0.50	0.50	0.50
R-squared	0.06	0.06	0.06	0.05	0.05	0.05
Observations	191,832	191,832	191,832	630,297	630,297	630,297
Panel B: Men age 46-64						
STDI x Years 0-4	-0.031	-0.027	-0.024	-0.010	-0.007	-0.004
	(0.024)	(0.025)	(0.021)	(0.014)	(0.014)	(0.016)
STDI x Years 5-9	-0.019	-0.013	-0.012	-0.007	-0.002	-0.001
	(0.021)	(0.022)	(0.019)	(0.017)	(0.014)	(0.018)
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Mean	25.77	25.77	25.77	0.82	0.82	0.82
R-squared	0.07	0.07	0.07	0.11	0.11	0.11
Observations	237,326	237,326	237,326	565,504	565,504	565,504

Notes: Table shows estimated effects of STDI maternity benefits on log hourly wages in the short run (years 0-4 relative to enactment of STDI maternity benefits) and long run (years 5-9 relative to enactment of STDI maternity benefits) from equation 1. Sample includes women (panel A) and men (panel B) age 46-64 from 1973-1987 CPS May and MORG extracts. Standard errors are clustered at the state-group level.

Table A10: Effect of using different identifying variation to study impact on women's log wages

	(1) Main specification	(2) Drop STDI_s	(3) Universal states	(4) Medium STDI_s	(5) Low STDI_s
STDI x Years 0-4	-0.056**	-0.018	-0.074***	-0.020*	0.007
	(0.024)	(0.011)	(0.025)	(0.011)	(0.010)
STDI x Years 5-9	-0.059**	-0.030**	-0.066**	-0.036**	-0.008
	(0.029)	(0.015)	(0.033)	(0.017)	(0.016)
Mean	15.05	15.05	15.69	13.85	13.76
R-squared	0.15	0.15	0.15	0.15	0.15
Observations	570,902	570,902	570,902	570,902	570,902

Notes: Table shows estimates of τ_{SR} and τ_{LR} from equation 3 using different methods of assigning values to $STDI_s$. Column 1 repeats the main estimates from column 1 of Table 3. Column 2 assigns $STDI_s = 1$ to all states, ignoring cross-sectional variation in the "bite" of the policy. Columns 3-5 show estimates from a specification that estimates effects separately for universal, "medium," and "low" STDI exposure states. Effects in column 3-5 are estimated jointly. Sample includes women age 18-45 from 1973-1987 CPS May and MORG extracts. Standard errors are clustered at the state-group level.

Table A11: P-values from tests for differences in short- and long-run effects on employment, hours worked

	Outcome: Employed			Outcome: Hours worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All women age 18-45						
P-value	0.02	0.06	0.00	0.05	0.13	0.03
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Panel B: Women with some college experience						
P-value	0.00	0.08	0.00	0.08	0.36	0.05
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X
Panel C: Women with no education beyond high school						
P-value	0.02	0.06	0.01	0.02	0.05	0.02
Age, race	X	X	X	X	X	X
Policy		X			X	
Linear pre-trend			X			X

Notes: Table shows p-values from a test of equal short- and long-run effects on employment and hours worked. Main estimates are shown in Table 4.