The Long-Run Effects of America's First Paid Maternity Leave Policy

Brenden Timpe *

October 24, 2019

CLICK HERE FOR LATEST VERSION

Abstract

This paper provides the first evidence of the effect of a U.S. paid maternity leave policy on the long-run outcomes of children. I exploit variation in access to paid leave that was created by long-standing state differences in short-term disability insurance coverage and the staggered enactment of laws that banned discrimination against pregnant workers in the 1960s and 1970s. While the availability of these benefits sparked a substantial expansion of leave-taking by new mothers, it also came with a cost. I find the enactment of paid leave led to shifts in labor supply and demand that decreased wages and family income among women of child-bearing age. In addition, the first generation of children born to mothers with access to maternity leave benefits were 1.9 percent less likely to attend college and 3.1 percent less likely to earn a four-year college degree.

*University of Nebraska-Lincoln Department of Economics. btimpe@unl.edu. 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, and seminar participants at the University of Michigan, the U.S. Census Bureau, the National Tax Association, the Society of Economics of the Household, and the Population Association of America for helpful comments and discussions. Thanks to J. Clint Carter and Lori Reeder for help with confidential data. This research has been supported in part by an NIA training grant to the Population Studies Center at the University of Michigan (T32 AG000221). Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.

1 Introduction

As the role of women in the labor force has grown over the last 50 years, so too has interest in parental leave. Billed as a means to promote child health and help women pursue more continuous, higher-paying careers, nearly every developed nation has adopted policies providing income and job protection to new mothers who take leave as long as one year or more (OECD, 2018). Even in the United States, where maternity leave benefits are allotted far less generously, policymakers and analysts from a wide range of backgrounds have coalesced around the idea that an expansion of paid leave would benefit families and the economy overall (The White House Council of Economic Advisers, 2014; Sholar, 2016).

Despite this growing consensus, little evidence exists on the potential long-run effects of parental leave policies. While a robust body of literature has documented the positive effect that parental leave policies have on the use and length of maternity leave among working women (Han, Ruhm and Waldfogel, 2009; Waldfogel, 1999; Baum, 2003; Berger and Waldfogel, 2004; Baum and Ruhm, 2016; Byker, 2016; Rossin-Slater, Ruhm and Waldfogel, 2013), less is known about the consequences for women's labor-market outcomes or the health and human capital of children. Economists have long understood that mandated parental leave benefits could in theory affect the labor-market prospects of women (Summers, 1989; Gruber, 1994; Klerman et al., 1997), but recent reviews have 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). In addition, proponents of parental leave argue that it promotes child health and human capital in the long run by giving mothers and infants more time to bond at a critical period of development. However, these theoretical effects have proven much more challenging to test empirically, largely because most relevant public policy changes are so recent that the first generations of children exposed to these mandates have not fully reached adulthood (Rossin-Slater, 2018).

This paper provides new evidence on parental leave's long-run effects by exploiting a little-studied interaction between U.S. disability policy and anti-discrimination statutes enacted in the 1960s and 1970s. My research design draws on long-standing, cross-state variation in the availability of short-term disability insurance (STDI). These insurance policies, which were originally designed to provide income insurance for temporarily disabled manual laborers, became a source of paid maternity leave benefits when a series of state and federal anti-discrimination laws required them to cover childbirth as a disability. 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."

I use the staggered enactment of these anti-discrimination laws and the pre-existing, cross-state variation in access to STDI to produce new estimates of the impact of paid leave on women's labor-market outcomes and the long-run human capital development of children. My difference-in-difference framework compares outcomes before and after the enactment of STDI maternity benefits, and in states with different pre-existing levels of STDI coverage. I show that these long-standing differences in STDI coverage are predictive of take-up of maternity benefits after the reform. However, I find no evidence that the roll-out of STDI-funded paid leave was correlated with potential confounding factors such as the receipt of benefits from public-assistance programs or the demographic characteristics of the population Pei, Pischke and Schwandt (2018).

I then consider the effect of this expansion of access to paid leave on women's labor-market outcomes. I find that the adoption of STDI maternity benefits led to a decrease of 4 to 5 percent in women's hourly wages, with no statistically significant changes in women's employment. I argue that while this effect may seem surprising at first blush, it is consistent with literature examining the labor-market implications of mandated benefits (Summers, 1989; Gruber, 1994). The effects on wages are highly robust and persistent, with no evidence of a comparable effect on men's labor-market outcomes, and they are concentrated among occupations where we would expect workers' absences to be most costly from a firm's perspective. Moreover, these negative effects on wages translated into a decrease in family income that was concentrated among women in the middle of the income distribution.

I also present evidence that this deterioration in women's labor-market conditions imposed costs on the next generation. I find that the children of mothers exposed to STDI maternity benefits achieved worse human capital outcomes in the long run, a result driven by a 1.9 percent decrease in college attendance and a 3.1 percent decrease in the likelihood of earning a 4-year college degree. My estimates of negative effects on women's family income suggests that these results may be driven by a decrease in family resources during children's formative years. The magnitudes of these long-run impacts are consistent with previous estimates of the effect of measures of family resources on child outcomes (Aizer et al., 2016; Stuart, 2018), and they are economically meaningful. For instance, the effect of exposure to STDI maternity leave benefits at birth is large enough to offset roughly one-sixth of the long-run educational benefit enjoyed by Head Start attendees and one-quarter of the benefit accrued to prenatal Medicaid beneficiaries (Brown, Kowalski and Lurie, 2015; Bailey, Sun and Timpe, 2018).

While this paper is the first to report the long-run effects of a maternity leave policy on American children, my results contrast starkly with the benefits enjoyed by Norwegian children born just after an expansion of paid leave in 1977 (Carneiro, Løken and Salvanes, 2015). While the Norwegian and U.S. labor markets have important institutional differences that may contribute to these opposite-signed results, the disparity also highlights a tradeoff inherent in regression discontinuity designs, which identify a local average treatment effect with a high degree of internal validity but may net out policy-relevant general equilibrium effects. Overall, these estimates suggest that while paid leave policies confer important benefits on working mothers, they may also carry potentially significant costs that should be incorporated in any comprehensive analysis of such policies.

2 The creation of America's first paid maternity leave policy

The United States is widely known to be 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 Family and Medical Leave Act (Klerman, Daley and Pozniak, 2012). In addition, a handful of states have enacted paid family leave programs in the last 15 years. However, no national policy guarantees paid leave for parents who wish to take time away from work before or after the birth of a new child. In fact, while most new parents in Europe and Canada enjoy generous allotments of leave, in 2017 only 15 percent of private-industry workers in the United States report having access to paid family leave (U.S. Bureau of Labor Statistics, 2017).

Less well-known is the fact that many American mothers have access to paid maternity leave through STDI. These policies are required to pay benefits to new mothers by anti-discrimination laws that were enacted nationally in 1979 and even earlier in some U.S. states. The passage of these laws, coupled with pre-existing differences in access to STDI across U.S. states, led to the state-by-state implementation of a paid maternity leave mandate that offers an opportunity to evaluate the long-run effects of such a policy in a U.S. context.

2.1 The role of state disability laws and anti-discrimination policy

The U.S. short-term disability insurance industry got its start in the mid-19th century and grew substantially over the next century, driven by the demand for a source of income replacement for temporarily disabled workers (Faulkner, 1940). While coverage varied widely across states and industries, by 1954 the industry covered about 48 percent of workers in most states, with coverage more widespread among unionized workers and large firms (Price, 1986; Levy, 2004). However, coverage is much wider in five states and Puerto Rico, where state law makes access to STDI virtually universal. Rhode Island became the first state to expand access to disability insurance in 1942 when lawmakers 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 in the next few years, while Hawaii and Puerto Rico adopted their own programs in the 1960s (Kamerman, Kahn and Kingston, 1983; Wisensale, 2001). This progression resulted in wide variation across states in access to STDI, with the state-level share covered dependent on the industrial mix in most states but nearly universal for the large fraction of workers in states with STDI guarantees.

This pre-existing variation in STDI coverage became particularly consequential for working women when a series of state and federal laws effectively required them to cover childbirth as a disability. The change came as women's rights groups spoke out against policies around the country that disadvantaged working women, such as insurance policies – including STDI – that excluded coverage of pregnancy. During the 1970s, more than a dozen states enacted policies forbidding discrimination against pregnant workers. 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, the same policy was imposed on the rest of the nation, effectively creating America's first paid maternity leave policy.[1]

[1] To assemble evidence on the enactment of state anti-discrimination laws, I rely on several primary and secondary sources, including Congressional testimony, correspondence with state officials, newspaper articles, and published histories of anti-discrimination laws (Gladstone, Williams and Belous, 1985; Kamerman, Kahn and Kingston, 1983; U.S. Senate, 1979; U.S. House of Representatives, 1977). The history of these laws is described further in the appendix.

2.2 Characteristics of STDI maternity coverage

The STDI maternity benefits provided to women were relatively modest by the standards of most OECD countries. They generally covered between one-half and two-thirds of usual weekly wages and lasted between 6 and 10 weeks. 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 did not preclude employers from uniformly revoking the right to disability leave from all workers.

In practice, the reform amounted to a large expansion of paid leave at a time when American women received few maternity benefits. Figure 1a 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.[2]

Figure 1b shows 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 is much higher in universal-STDI states (solid line) than among women in all other states (dashed line).[3]

Additional context is provided in Table 1, which shows that the share of new mothers reporting receipt of STDI benefits around childbirth was 18 percent in universal-STDI states but only 2 percent in other states. This difference is highly statistically significant. The table also shows that claiming was much more common among married women, white women, and

[2]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.

[3]Note that these figures imply lower takeup than that implied by the administrative data in Figure 1a. This difference is consistent with evidence that receipt of transfer income is significantly underreported in survey data (Meyer, Mok and Sullivan, 2015).

women in the middle of the education distribution. These figures suggest the policy was most impactful for middle-class women. The enactment of leave may have been more likely to replicate existing, privately provisioned benefits for highly educated women, while the most disadvantaged groups would have been less likely to work for employers who would agree to an extended absence. In addition, recent survey evidence suggests that women of lower socioeconomic status are less likely to be aware of the availability of paid leave (Applebaum and Milkman, 2011).

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. The staggered implementation of anti-pregnancy discrimination laws at the state and federal levels, combined with long-standing variation in access to STDI, meant that paid maternity leave was expanded differentially across states and time. These policies carried the potential for important effects not only on working mothers, but on children and the entire U.S. workforce.

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 rather than transitioning to life as a stay-at-home parent. Yet these effects on leave-taking and employment in the short run are only one way that parental leave policies can impact economic and demographic outcomes. Such policies may also have important effects on the employment prospects of the female workforce as a whole by altering women's incentives to work and the hiring and promotion decisions of firms. They may also affect children by changing the mix of time and resources that parents invest in them. Below I discuss the expected effects of STDI-funded maternity leave on each of these groups.

3.1 Short-run effects on leave-taking and labor supply

The most immediate effect of the enactment of a maternity leave policy is to alter women's labor-supply decisions in the weeks and months surrounding the birth of a child. New parents face a tradeoff between allocating their time to the firm and home-production tasks related to a child (Klerman and Leibowitz, 1997). In this context, the short-run implications of parental leave policies depend on the presence of two features: wage replacement and job protection.

Paid leave benefits reduce the cost of absence from work, leading to greater leave-taking, and may be particularly important in the presence of liquidity constraints (Rossin-Slater, Ruhm and Waldfogel, 2013; Byker, 2016; Bana, Bedard and Rossin-Slater, 2018). Mandated job protection, on the other hand, allows new mothers to take a longer leave than their employers might otherwise be willing to bear. These policies, alone or in concert, should lead unambiguously to an increase in take-up and the length of maternity leave, while the impact on women's attachment to the labor force is less certain. While job protection should increase the share of women returning to the same job after childbirth, paid leave benefits also create an offsetting income effect.[4]

The principal definining feature of STDI-funded maternity leave was its offer of wage replacement in the weeks around childbirth. However, as discussed in Section 2.2, these policies also provided "soft" job protection by forbidding employers from treating pregnant women differently than other workers on disability leave. The result is an ambiguous theoretical prediction regarding job retention, but a clear prediction the we should see an increase in maternity leave-taking among working mothers.

3.2 Effects on labor demand and supply

In addition to the potential effects on working mothers, the enactment of paid leave mandates may change incentives for firms and the broader set of 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, ν_i. This disutility of work, which is distributed in the population according to cumulative distribution function $F(\nu)$, 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

[4] A robust body of empirical research on maternity leave has produced evidence largely consistent with these theoretical predictions. New mothers, especially those in Europe, Canada, and other OECD countries, tend to respond to maternity leave mandates by taking more time away from work, while effects on job retention are more difficult to estimate precisely but often positive (Han, Ruhm and Waldfogel, 2009; Waldfogel, 1999; Baum, 2003; Berger and Waldfogel, 2004; Mukhopadhyay, 2012; Baum and Ruhm, 2016; Byker, 2016; Rossin-Slater, Ruhm and Waldfogel, 2013; Bana, Bedard and Rossin-Slater, 2018). Because women who take leave from a job, rather than quitting, retain firm-specific human capital that can translate into higher earnings later on, these results have led a number of commentators to suggest that parental leave policies promote gender equality in the labor market (Waldfogel, 1998).

greater flexibility. A convenient functional form would be:

$$U(L_i;\ \nu_i) = wL_i - \nu_i L_i Z \tag{1}$$

In this simple framework, workers choose to enter the labor force if $w \geq \nu_i Z$; 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:

$$\pi(L_j) = G(L_j) - wL_j - \delta_j(1 - Z)L_j \tag{2}$$

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:

$$\textit{Aggregate labor supply} : L^S = \int 1\left\{\nu_i < \frac{w}{Z}\right\} dF(\nu) \tag{3}$$

$$\textit{Aggregate labor demand} : L^D = \int L_j^D\left(w + \delta(1 - Z)\right) dH(\delta) \tag{4}$$

Equilibrium wages and employment are then determined at equilibrium, where $L^S = L^D$. This simple model replicates the basic insights of Summers (1989) and Gruber (1994). Figure 2 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 2a 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 2b. 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 2c, 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 2d. 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.

The historical record provides important context when considering the importance of δ_j, the cost of accommodating female workers after the enactment of STDI maternity benefits. After the passage of an anti-discrimination bill in the Maryland legislature 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). In particular, industry representatives objected not only to direct costs of the policy, but also to the cost of replacing workers who would be taking maternity leave rather than returning quickly to their job.[5] Ardie Epranian, a representative of the AVX Corporation, warned members of Congress in a hearing on the 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 figures from a recent Supreme Court decision, *General Electric Co. v. Geduldig*, 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).

In short, the enactment of paid maternity leave should lead to lower wages for working women but ambiguous effects on employment. This could reflect a reduced willingness to hire women but also a reluctance to promote women within the firm (Thomas, 2018). In

[5]Several groups prepared estimates of the cost of expanding STDI maternity benefits while Congress debated the Pregnancy Discrimination Act of 1978, but they varied widely – from a figure of only $130 million from the AFL-CIO to $571 million from the Health Insurance Industry Association. Using data on the annual earnings and family structure of women in the 1976 March Current Population Survey, I estimate that the expected benefits would have amounted to roughly one-half of 1 percent of the annual earnings of the average woman age 18-45.

addition, to the extent that these changes are driven by labor demand, the negative wage and employment effects may be driven by occupations where leaves of absence are especially disruptive.

Thus far, the literature has produced only limited evidence on the empirical importance of these well-known theoretical implications for the labor market (Olivetti and Petrongolo, 2017; Rossin-Slater, 2018). One limitation, especially in the U.S. context, is related to the fact that the bulk of policy changes have featured complicated eligibility requirements or affected parents in only a handful of states, making inference difficult. Even so, Das and Polachek (2015) and Sarin (2017) use the 2004 expansion of paid family leave in California and find evidence of negative effects on female employment. However, despite the clear theoretical predictions, little evidence has been generated on the effects on women's hourly wages or family income.

A growing body of research has examined the closely related question of whether firms see parental leave policies as costly and respond accordingly. Thomas (2018) finds evidence in the Panel Study of Income Dynamics (PSID) that the job-protected, unpaid leave offered by the Family and Medical Leave Act of 1993 discouraged firms from promoting women to higher-profile positions. However, a different picture emerges in research from Europe, where access to relatively detailed administrative data allows more precise measurements of the effects of generous paid leave policies. Two recent papers using data from Denmark find little or no effect of maternity leave policies or leave-taking on the success of firms and co-workers (Brenøe et al., 2018; Gallen, 2018). It is not yet clear whether the disparity between findings in the United States and Europe can be attributed to differences in data quality or differences in the setting; given the generous, long-standing social safety net, greater occupational segregation and other features of the labor may make the cost of paid leave less salient to European firms (Blau and Kahn, 2013). Overall, the lack of consensus suggests the debate over the labor-market consequences of parental leave is far from settled.

3.3 Effects on children

A final group that may be affected by the enactment of paid maternity leave is the population of children exposed to the policy. Consider the following human capital production function:

$$H = h\left(1 - L_m, w_m, w_f\right) \tag{5}$$

where $h(\cdot)$ is a function of the following variables: $1 - L_m$, the mother's time investment mothers make in the child; w_m, the mother's wage; and w_f, the father's wage. The literature on child development suggests H is weakly increasing in each argument (Dahl and Lochner, 2012; Heckman and Mosso, 2014; Agostinelli and Sorrenti, 2018).

Proponents of paid maternity leave often argue that the effects on children will be positive because the policies increase time investments early in life and, by encouraging greater attachment to the labor force for mothers, increase women's effective wage. The United States' professional association of pediatric physicians has gone so far as to endorse a national paid-leave policy, arguing that "when parents have paid family leave following the birth of a child, mothers breastfeed longer and parents are more likely to take children for immunizations and well-child care... paid family leave can have effects that last throughout life" (American Academy of Pediatrics and Pediatric Policy Council, 2015).

However, the analysis of Section 3.2 suggests the paid-leave policy could also lead to a decrease in w_m that could in turn reduce child human capital accumulation. In addition, while the availability of paid leave increases time investments in the child's first months of life, parents may invest *less* time in the long run if the policy encourages greater attachment to the workforce. The ultimate effects on time and resource investments are therefore theoretically ambiguous.

The empirical evidence on parental leave's long-run effects on children offers few hints of the relative importance of these potentially conflicting theoretical forces.[6] The most compelling findings come from Carneiro, Løken and Salvanes (2015), who use a regression discontinuity approach to estimate the effects of an expansion of Norwegian policy from 12 weeks of unpaid leave to 4 months of fully paid leave plus 1 year of unpaid leave. They find substantial effects on children in the long run: a 2 percentage-point decrease in high school dropout rates and a 5 percent increase in wages at age 30. After exploring potential

[6] Studies of short- and medium-run effects on child health or test scores have produced estimates that are generally, but not exclusively, positive. While Ruhm (2000) finds a link between more generous leave policies and lower infant and child death rates in a cross-country analysis, several other papers find no effect on child health and schooling outcomes (Dahl et al., 2016; Baker and Milligan, 2010; Dustmann and Schönberg, 2012; Ahammer, Halla and Schneeweis, 2018). However, Baker and Milligan (2014) find evidence of lower test scores for Canadian children born after an expansion, while Danzer and Lavy (2018) conclude that an Austrian expansion of paid leave led to lower test scores at age 15 among boys with low-educated mothers but benefited boys with mothers who attended post-secondary school. In the United States, Stoddard, Stock and Hogenson (2016) conclude that leave mandates decrease the likelihood of Cesarean delivery, but this effect is reversed if the leave comes with health insurance that would otherwise have been foregone. In addition, two papers associate U.S. expansions of maternity leave with improvements in infant health (Rossin, 2011; Stearns, 2015)

channels, they conclude this effect is the result of increased time spent under the care of the mother, rather than a child-care worker or more distant relative.

While these positive results are striking, several factors limit their generalizability. First, the use of a regression discontinuity design implicitly differences out a number of policy-relevant margins of response for women, such as changes in labor-market conditions, that would be better captured by a difference-in-difference design. In addition, the generous social safety net long present in Norway suggests the labor market may have been better adapted to absorb an expansion of paid leave without a measurable deterioration in wages or employment (Blau and Kahn, 2013). Finally, the Norwegian expansion amounted to an expansion of parental leave allotments for mothers who had already enjoyed more generous benefits than many American workers, even today. Altogether, these considerations suggest reason for caution when using the findings of Carneiro, Løken and Salvanes (2015) to think about long-run, general-equilibrium effects of an expansion of paid leave in the United States.

Estimating such effects in the very long run has been difficult in the U.S. context, largely because most expansions of parental leave were enacted relatively recently – the early 1990s for the unpaid leave granted by the FMLA, and 2004 and later for state paid-leave programs. Another challenge is the availability of data that can link individuals' outcomes as adults to their exposure to the policy as infants, and with sample sizes sufficient to estimate effects with precision. Given the era in which it occurred and the scale at which benefits were expanded, the enactment of STDI-funded maternity leave in the 1960s and 1970s offers a unique opportunity to evaluate these hypotheses in the U.S. context.

4 Data and research design

A thorough evaluation of the impact of STDI paid maternity benefits requires data on a wide range of outcomes – including fertility, labor supply, hourly wages, and long-run child outcomes – that are not captured by any single source. I rely on instead on three separate sources of data for my main results.

To document the differential receipt of STDI-funded maternity benefits and the impact on leave-taking and employment in the short run, I construct a sample of women from the 1984-1989 panels of the Survey of Income and Program Participation (SIPP). The SIPP's longitudinal data provides detailed information on labor-market activity and receipt of income from a variety of sources, including STDI. In addition, the 1984 and 1985 panels include retrospective reports on fertility, which I use to construct a month-by-month panel of labor supply for each mother, from 9 months before childbirth to 12 months after.[7] I use these data to examine changes in women's employment and leave-taking around childbirth, as well as their receipt of STDI maternity benefits.

To examine impacts on the broader labor market, I use two sources of data available through the National Bureau of Economic Research (NBER): the Current Population Survey's (CPS) May installment, which provides a continuous measure of hourly wage rates beginning in 1973, and the CPS Multiple Outgoing Rotation Group files, which provide responses to the same hourly wage questions in every month beginning in 1979. Following Lemieux (2006), I use the wage reports of both hourly and salaried workers, dropping imputed values and observations with an hourly wage less than $1 or greater than $100 in 1979 dollars. In addition to hourly wages, I examine effects on employment using the indicator constructed by the Bureau of Labor Statistics, which infers labor-force status from a series of questions about activity in the previous week and other factors. In order to focus on women of child-bearing age and their closest male counterparts, I limit the sample to individuals age 18 to 45. Because earlier years of the CPS do not identify all U.S. states, I consolidate states into 21 groups that can be consistently identified over the course of the sample.

Finally, the estimation of long-run effects requires a source of data that can connect individuals' exposure as children to their economic and demographic outcomes many years later, as well as sample sizes large enough to generate precise estimates of potentially small effects. For this exercise I rely on restricted-use versions of the complete long-form 2000 decennial Census and the 2001-2016 American Community Survey (ACS). These data have been linked to the Social Security Administration's Numident file, which provides a measure of the exact place of birth that has been matched to individuals' county of birth (Stuart, Taylor and Bailey, 2016).[8] To measure outcomes for several years before and after the enactment of paid leave in all states, I restrict the sample to individuals born between 1954

[7]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?

[8]The restricted-use versions of the 2000 Census and 2001-2016 ACS include exact date of birth and state of birth, which is sufficient to infer exposure to the policy. However, the link to the SSA Numident file provides additional flexibility in several ways. First, my preferred specification includes county-of-birth fixed effects, which may improve the precision of my estimates. In addition, observation of county of birth allows me to include specifications that follow previous literature on long-run outcomes by controlling for county-of-birth characteristics and dropping individuals born in large cities such as New York, San Francisco, and Los Angeles (Bailey and Goodman-Bacon, 2015; Hoynes, Schanzenbach and Almond, 2016).

and 1985.[9] I use measures of educational attainment in the Census and ACS to construct four variables of interest: years of schooling and indicators for high school completion, college attendance, and attainment of a four-year college degree. In addition, to increase statistical power, I construct an index of human capital outcomes that consists of the unweighted mean of standarized versions of my measures of educational attainment (Kling, Liebman and Katz, 2007).

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.

4.1 Research design

The history of STDI maternity benefits suggests a research design that makes use of both the variation in timing of state-level anti-discrimination laws and the differential "bite" of these laws in states with more and less widespread access to STDI. Building on Card (1992), I therefore estimate the following event-study specification:

$$y_{ist} = STDI_{s,1970} \sum_{k \neq -1} \tau_k 1\left\{k = t - T_s^*\right\} + \boldsymbol{\delta_s} + \boldsymbol{\theta_{r(s)t}} + \boldsymbol{X'_{ist}\beta} + \epsilon_{ist} \tag{6}$$

where y_{ist} is a measure of women's labor-market outcomes, fertility, or a child's long-run educational attainment and is defined for individual i in state s at time t. This specification includes state fixed effects, δ_s, that control for time-invariant determinants of outcome y_{ist} that may vary across states, as well as a vector of covariates X_{ist} that includes other exogenous determinants of y_{ist}. In my preferred specification, I include fixed effects at the Census-division-by-year level, $\theta_{r(s)t}$, to control nonparametrically for differential trends by region of the country.

The key variable $STDI_{s,1970}$ is designed to capture the variation across states in the share of female workers with access to STDI benefits. Because I do not observe eligibility or receipt of STDI benefits directly, I instead construct a measure of exposure that is not contaminated by firm responses to the anti-discrimination laws[10] My preferred parameterization

[9]Rhode Island was the first state to pay pregnancy disability benefits, beginning in 1942. Given the advanced age of this cohort in my Census and ACS sample and the difficulty of drawing conclusions from a reform enacted in the middle of World War II, I do not make use of the policy variation in Rhode Island. All other states adopted STDI disability benefits between 1961 (New Jersey) and 1979 (the national Pregnancy Discrimination Act).

[10]For example, because anti-discrimination laws required firms only to treat women equally and not necessarily to offer STDI, some may have responded by dropping STDI coverage altogether. Aggregate data

of $STDI_{s,1970}$ therefore matches data on female employment by state and industry from the 1970 decennial Census to a tabulation of STDI coverage by three-digit NAICS industry that was prepared by the BLS National Compensation Survey for Autor et al. (2013). This allows me to estimate the share of working women age 18-45 in each state who would have been exposed to STDI maternity benefits:

$$STDI_{s,1970} = \frac{\sum_a \gamma_a \, FemEmp_{as,1970}}{\sum_a FemEmp_{as,1970}} \tag{7}$$

where $FemEmp_{as,1970}$ is the number of women age 18-45 employed in industry a in state s in 1970 and γ_a is the national industry-level share of workers with STDI from Autor et al. (2013).[11] In states where STDI is universal, $STDI_{s,1970}$ is assumed to be 1. This measure of the "bite" of the paid-leave policy thus relies only on the national share of covered workers and the pre-reform industrial mix and disability policy of each state.

The parameters of interest from equation (6), 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, a trend in y_{ist} over the pre-reform event-time periods would suggest other determinants of the outcome are changing in a way that is correlated with the enactment of paid leave, complicating my estimates of the effect of STDI. Second, a break in unobserved determinants of outcome y_{ist}, if correlated with the enactment of paid leave, would lead me to erroneously attribute the changes in the outcome to STDI maternity benefits.

My flexible event-study specification provides a built-in test of the former assumption. To the extent that confounding pre-trends exist in the data, they would be likely to appear in the form of estimates of τ_k for pre-reform periods that are significantly different from 0. The latter potential confounder is fundamentally untestable. However, I will discuss this assumption further and provide some suggestive evidence of its validity in section 4.3.

from the Social Security Administration suggests this was not a common response. Even so, such responses would most likely attenuate my estimates on women's wages.

[11] An alternative approach would define $STDI_{s,1970}$ as a binary indicator for universal-STDI states. Results using this definition are qualitatively similar and available upon request.

4.2 Take-up of STDI maternity benefits

While the descriptive evidence provided in Figure 1 suggests that the enactment of STDI maternity benefits led to an increase in leave-taking among new mothers, this section tests the short-run effects more formally using the regression framework of equation (6). To do so, I use the sample of women from the SIPP who respond to the retrospective questions about fertility, limiting the sample to women who gave birth between 1970 and 1984 while between the ages of 18 and 45. Given the relatively small size of the sample, I then restrict the event-time variables of equation (6) to a binary indicator for giving birth before or after enactment of STDI maternity benefits. This allows me to estimate a difference-in-difference specification, separately for each month relative to childbirth, to estimate the effect of the policy on the propensity to be with a job and at work.

The results of this exercise are shown in Figure 3. For the first two trimesters of pregnancy, the labor supply of first-time mothers changed little as a result of the enactment of STDI benefits, although there is suggestive evidence that the policy led some women to remain in the workforce during the second trimester. Consistent with the structure of most STDI policies, which often covered several weeks before and several weeks after birth, the largest effects come just before and after the month of birth. Women who would have returned to work in the first and second months after giving birth were roughly 10 percentage points more likely to stay home instead. The effect disappears completely by 7 months after childbirth. In short, the policy appears to have achieved paid leave's goal of increasing the time women spend at home with a new child. While I see positive point estimates on labor supply in months 9 through 12, suggesting the potential for increased job retention among new mothers, I cannot rule out effects of meaningful size in either direction.[12]

To get a sense of the impact on time spent at home in the aggregate, we can simply add up coefficients from months -3 through 6, the primary period during which women take maternity leave. This sum amounts to an intent-to-treat effect of -0.56 months, or about 2.4 extra weeks spent at home relative to the counterfactual. However, we can get an estimate of the treatment effect on mothers who received STDI by scaling these figures by 0.4, my best estimate of the effect of the expansion of STDI maternity benefits on maternity benefit receipt.[13] This exercise suggests that women who received STDI benefits took nearly 6 weeks

[12] In a complementary analysis in the appendix using decennial Census data that affords larger samples, I find evidence that women with access to STDI maternity benefits were more likely to be employed after childbirth.

[13] This figure is calculated as follows: Data from the 1984-1989 panels of the SIPP suggests roughly 18

extra away from work on average. Given that STDI generally provided only between 6 and 10 weeks of wage replacement, this amounts to nearly full take-up of the time allotted by the benefits.

Perhaps unsurprisingly, given the broad nature of the policy and the scarcity of maternity leave allotments at the time, the enactment of STDI maternity benefits compares favorably 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. The relatively large magnitude of the effect of STDI maternity benefits suggests there may be scope for downstream effects of the policy, as employers may have been more likely to alter their demand for female labor and children may have been more likely to experience a change in their early environment that could have effects in the long run.

4.3 Internal validity of the research design

My estimates of the causal effect of paid leave on the outcomes of women and children rely on the assumption that no unobserved determinant of the dependent variable is correlated with the cross-sectional and time variation in access to paid maternity leave. One way to evaluate the validity of this assumption is to estimate equation (6) using other indicators that are drivers of women's labor-market conditions or child well-being (Pei, Pischke and Schwandt, 2018). A pre-trend or sharp break in other important determinants of labor-market or child outcomes may be signs that confounding factors are at work.

I focus on two public programs, the Earned Income Tax Credit (EITC) and Food Stamps, which were rolled out during a similar time frame and have been shown to have significant positive effects on women's labor-force participation, children's long-run outcomes, or both (Bastian, 2018; Bastian and Michelmore, 2018; Hoynes, Schanzenbach and Almond, 2016). I construct these variables using state-by-year expenditures from the Bureau of Economic Analysis Regional Income Division and convert them to per-capita terms using the annual population counts from the Surveillance, Epidemiology, and End Results (SEER)

percent of new mothers receive STDI benefits in universal-STDI states, but only 2 percent in other states (see Table 1). While these estimates are known to be downward biased (Meyer, Mok and Sullivan, 2015), 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, $\frac{0.02\times 0.3}{0.18} = 0.033$. 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 $\frac{0.3-0.033}{0.65} \approx 0.4$.

program of the National Cancer Institute. In addition, for a measure of public benefit receipt that focuses more directly on the population of interest, I also use the March Current Population Survey (Ruggles et al., 2017) to construct the share of women age 18-45 receiving income from welfare programs and from other government programs, by state group and year, from 1968 to 1984.

The results of this exercise are shown in Figure 4. While the EITC's 1975 launch was national, rather than on a state-by-state basis as in the case of paid maternity leave, the program could nevertheless confound my estimates if eligibility or take-up were correlated with the enactment of anti-pregnancy discrimination laws and the availability of STDI. However, Figure 4a suggests little reason for this concern; the trend in per-capita EITC receipt is quite flat and statistically insignificant once I include controls that account for demographic differences across states.

Estimates of the correlation of paid maternity leave and Food Stamps also lead to a null result in Figure 4b. All specifications show a relatively flat pre-trend. There is a slight increase in Food Stamp benefit per capita after the reform, but the estimates are statistically insignificant.[14] However, an increase following the reform could in fact be partially attributed to maternity leave, if negative effects on female wages led more women to become eligible for the program. If so, this increase in food assistance would be expected to improve children's well-being or at least attenuate any negative effects, given the findings of previous literature on the link between Food Stamps and long-run outcomes (Hoynes, Schanzenbach and Almond, 2016).

The results for March CPS measures of the share of women receiving welfare and other goverment income are also consistent with my identifying assumptions. Figure 4c shows no sign of changes in welfare receipt around the reform. Similarly, the trend in Figure 4d is flat before the reform and there is no statistically significant evidence of a change afterward.[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 and child

[14] A joint test of significance of τ_k for the post-reform event-years delivers a p-value of 0.64.

[15] The statistically insignificant jump in the share of women receiving government benefits after the reform may in fact be driven by STDI. In several univeral-STDI states, most notably California, STDI is a state-run program, so beneficiaries may report receiving it under this March CPS category. In fact, the post-reform jump in government income receipt is larger if I restrict the sample to women with a child age 0. This suggests the flat pre-trend and the small increase post-reform are consistent with my identifying assumptions.

human capital accumulation.[16]

5 Effects on women's employment and wages

The predictions of the stylized model in section 3.2 are explored in Table 2, which reports estimates from equation (6) with τ_k grouped into three-year bins. Column 1 reports the estimated effect on the outcome for which there is a clear prediction, women's log wages. In event years -4 to -2, before STDI maternity benefits were available, I see no effect on wages, consistent with a flat pre-trend. However, wages drop sharply in the first few years after the reform, falling more than 4 percent and remaining at this level even in event years 3 through 5. By contrast, column 2 shows little robust evidence of systematic changes in women's employment.

While the estimated effects on women's wages are strongly statistically significant, there is reason to suspect conventional robust standard errors could be underestimated in settings such as this one, particularly when treatment assignment is clustered (Moulton, 1990; Bertrand, Duflo and Mullainathan, 2004; Kezdi, 2004; Cameron and Miller, 2015; Abadie et al., 2017). One conservative approach to inference in this case is to use a randomization procedure that reassigns treatment assignment at the state level and re-estimates the specification as a test of the null hypothesis that the reform had no effect on wages or employment. In brackets I report p-values from such a procedure using 1,000 replications. Even under this conservative approach, the effect on women's wages remains marginally statistically significant.

Additional detail on the evolution of the effects on wages can be seen in Figure 5a, which plots τ_k by event time. My main specification is shown by the navy line with circle markers and confidence intervals. Women's wages were flat in the years leading up to the reform, but this trend broke sharply after the passage of paid leave. The effects remain individually statistically significant even five years after the reform. Figure 5a also displays

[16] In the appendix I also report additional estimates from an exercise that follows Bailey (2006) in testing for systematic relationships between state characteristics and the timing of the roll-out 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. The exception is a statistically significant positive correlation between the average education among adult women and the year in which the relevant anti-pregnancy discrimination law was enacted. While this single statistically significant relationship may well be by chance, given that I perform 21 tests in this exercise, it nevertheless provides a counterpoint to the possibility that early-adopting states were systematically driven by a more educated, empowered female electorate.

results from several alternative specifications, but the estimated effects change very little, underscoring the robustness of this result.

Do these effects show up in men's labor-market outcomes? The theoretical implications are ambiguous; while we would not expect the enactment of paid maternity leave to have a direct effect on men's labor supply decisions, it could affect intra-household decision-making. In addition, it is possible that labor demand shifted in ways that impact men's wages or employment, with the direction of the effect depending on whether men's labor services are complements or substitutes for those of women. However, the empirical evidence suggests that men saw little or no effect of the policy. The event-study results of Figure 5b show no significant effects on the wages of men age 18-45. In line with this visual impression, the results from several specifications in Table 2 suggest that the effect on men's wages is small and statistically insignificant.

Given that I observe a significant decrease in wages but little change in female employment or men's labor-market outcomes, a natural question is whether these effects translated to changes in family income. While my sample of May CPS and MORG files do not include measures of family income for my full sample period, the May CPS from 1974-1981 includes a categorical variable corresponding to 13 ranges of family income. I use this variable to construct a series of indicators for family income falling above a given threshold. I then estimate equation (6) for each of these thresholds and show the effect at several points in the income distribution.[17]

Figures 6a and 6b show event-study estimates for two thresholds: The share of families earning more than $1,000 and the share earning more than $7,500, respectively, in nominal terms. I see little effect on family income at the lower threshold. However, there is a clear drop of 2-3 percentage points in the share of families at the higher threshold.

Figure 6c shows difference-in-difference estimates at each threshold identified by the May CPS. Consistent with the event-study results, I see little change in family circumstances at the bottom of the income distribution. Families at the top also appear to see little effect. However, families in the middle of the distribution saw statistically significant decreases in the probability of earning above each threshold.[18] This suggests that family income

[17] An alternative approach is to use questions from the March CPS. I use the May CPS to ensure my estimates rely on a sample as similar as possible to my earlier wage and employment estimates. However, in practice, I obtain similar results with the March CPS.

[18] Median family income during the middle and late 1970s time frame was between $11,000 and $16,000 in nominal terms (U.S. Census Bureau, 1981).

was affected most in exactly the families where women were more likely to take up STDI maternity benefits – those in the middle of the skill distribution, as shown in Table 1.

5.1 Heterogeneity and robustness of labor-market effects

Economic theory suggests the deterioration in women's labor-market outcomes described above comes as a result of an increase in labor supply on the part of women and a decrease in labor demand on the part of firms that worry about the costs of absent workers. In addition, the stylized model of section 3 suggests an additional test: To the extent that the cost of accommodating maternity leave varies across firms or occupations, we would expect to see demand shifts of different magnitudes. To test this hypothesis, I follow Hudomiet (2015) and adopt a concept of "adjustment costs" that serves as a proxy for the severity of the disruption a firm would bear due to women taking maternity leave.

To operationalize this concept, I use data from the Multi-City Study of Urban Inequality, which surveyed employers in four U.S. cities between 1992 and 1994 about a range of issues related to hiring and vacancies (Bobo et al., 2008). The survey asked employers how long a new employee would take to become fully productive if hired into a given occupation. I use these data to construct occupation-specific estimates of the adjustment cost and link it to my data from the CPS. I then assign individuals' occupation to above- or below-median adjustment-cost groups, and conduct analyses designed to ask two questions: First, did wages fall more among women in high-adjustment-cost occupations? Second, did working women become more likely to hold a job in a low-adjustment-cost occupation?

The results in Table 3 suggest that firm demand did in fact respond more strongly for occupations where absences would be relatively costly. Column 1 tests for a sorting effect by regressing an indicator for working in a high-cost occupation on the specification in equation (6). While I find a negative point estimate, it is too imprecise to distinguish from a null effect. However, columns 2 and 3 show that wages did indeed fall further for women in occupations associated with high adjustment costs: I find a 3-percent drop for low-cost occupations but a much larger 8-percent drop in occupations where adjustment costs are above the median. A joint test rejects the null hypothesis that these two estimates are equal, with a p-value of 0.001, suggesting that the enactment of paid leave resulted in disproportionately large wage declines for women whose absence would likely be most costly to the firm. This result is consistent with the suggestion that firms expected women to take more frequent and longer leaves after STDI benefits became available and factored the costs of these disruptions into their hiring and promotion decisions.

A second analysis investigates the mechanisms by which paid leave was enacted in different states. As described in Section 2, the state-level roll-out of anti-discrimination laws can be divided into two categories: Those in which the law was enacted by a politically representative body such as the legislature, and those in which it was imposed by force that is less responsive to local political pressure, such as the courts or Congress. If the effects of the paid leave were larger in one group of states than the other, it could raise concerns that the results are driven by a selected group of states with fundamentally different political and economic trends.

Columns 4 and 5 of Table 3 shows separate wage estimates by category of state anti-discrimination law. In fact, the estimated wage effect in states where the anti-discrimination law was enacted by the legislature or an administrative body is quite similar to the effect where it was imposed from outside, with an estimate of -0.03 in the former and -0.05 in the latter. A test of the equality of these two coefficients delivers a p-value of 0.332, suggesting we cannot reject the null hypothesis that they are equal. These estimates are consistent with the historical narrative, which suggested that the roll-out of anti-pregnancy discrimination laws was driven more by quirks of the legislative process than systematic differences across states. This bolsters the case that these estimates are picking up the effects of the paid-leave policy rather than other legislation or confounding factors.

Finally, columns 6 and 7 of Table 3 provide a check of my research design by splitting the sample by state disability policy. Column 6 provides estimates for universal-STDI states; given the lack of unique identifiers for some small states early in the CPS sample, this group is made up solely of New York and California. Column 7 provides estimates for all other states. The point estimate for universal-STDI states is large and statistically significant, underscoring the binding nature of the policy in those states. However, the result in column 7 makes clear that women in states with lower STDI coverage also saw a decrease in wages; it is smaller, at 3.7 percent, but an F-test cannot reject the null hypothesis that the effect is equal across the two groups of states.

5.2 Interpretation of labor-market effects

What drove the deterioration of women's labor-market prospects described in the results of this section? The evidence suggests that firms responded to the enactment of STDI maternity leave by reducing demand for female labor. While positive supply shifts could also lead to lower wages, null or negative changes in female employment suggest that demand was at least as important of a driver. This response by firms is also evident in the larger wage reductions in occupations with high adjustment costs, where we would expect a larger response in labor demand but not supply.

These results are closely related to the effects estimated by Gruber (1994), who evaluated the effect of the Pregnancy Discrimination Act, as well as the corresponding statutes in a handful of states, on employment and wages. The analysis of Gruber (1994) focuses on another consequence of the anti-discrimination laws: The requirement that employer-sponsored health insurance must cover maternity care. This paper exploits similar variation in anti-discrimination policies but also the variation in state STDI coverage.

In the appendix, I provide evidence that suggests there is reason to reinterpret the findings of Gruber (1994). I exploit the fact that in some states the timing of adoption of STDI maternity benefits was different than the timing of adoption of health insurance benefits. Appendix Table 4 replicates a key result of Gruber (1994) that suggested the health-insurance mandate led to a 4.3 percent decrease in women's wages. This estimate draws on variation in anti-discrimination laws enacted in three states – New York, New Jersey, and Illinois. However, when I allow the triple-difference estimate to vary by state, I find that the negative effect is driven by the two states that adopted STDI benefits at the same time they required health insurance policies to cover maternity benefits. In contrast, I find no detectable effect in New Jersey, where the state-run STDI system had been paying benefits for more than a decade before the reform examined in Gruber (1994). This exercise suggests that, while I cannot rule out the possibility that health insurance mandates play some role in my findings, maternity leave was probably the primary driver of the deterioration I observe in women's labor-market outcomes.

6 Effects on children

The results outlined in Section 5 suggest that women faced significant deterioration in the labor market in the years immediately following enactment of STDI-funded paid maternity leave. There are two channels through which these changes could have affected children. The first would amount to a composition effect if changing labor-market conditions affected women's fertility decisions. The second channel would affect children by altering the investments of time and resources that parents make in their offspring. In the following section I provide evidence that children were impacted primarily by changes in parental investment rather than fertility.

6.1 Did paid leave affect fertility patterns?

Given that labor supply decisions are generally thought to be determined jointly with fertility, the changes in women's wages and family income documented in the previous section raise the question of whether women altered their patterns of child-bearing. It is important to understand the effects on fertility because they are of interest in themselves. However, they also play a key role in the interpretation of the long-run effects on children described in the next section. If paid leave altered the size of cohorts born in the wake of the reform or changed the composition of the group of women bearing children, this could lead to a selection effect that drives changes in average outcomes for the group years later.

To test for changes in fertility, I assemble a state-by-month panel using birth records from the 1974-1984 Natality Detail Files, available through ICPSR, and population counts from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program. I examine effects on the fertility rate, birthweight, and mother's race, since changes in any of these characteristics could be signs that women altered their child-bearing patterns. In addition, because the quality of education data in the Natality Detail Files is low during this time frame, I use a sample of children from the 1980 decennial Census to test for changes in the composition of women giving birth, as proxied by years of education.

Figure 7a shows estimates of the effect on the fertility rate from (6). Despite the visual impression of a dip in fertility rates after the enactment of paid leave, formal tests of the significance of these effects suggest we cannot distinguish them from 0.[19] Nor do I see compelling evidence of a change in birthweight, a common marker of child health, which is displayed in Figure 7b. Finally, my estimates for a change in the composition of women giving birth is also null, with no apparent effect on the mother's race or years of education in figures 7c and 7d.

These estimates suggest there is a little scope for fertility changes to drive long-run effects on children's long-run outcomes. If anything, the slight but statistically insignificant rise in average weight at birth and drop in share nonwhite suggests positive selection of the cohorts born immediately after the reform. As I will show in the next section, the ultimate effect on the long-run outcomes of these first exposed cohorts suggests any positive selection

[19] A test of the joint significance of the coefficients on event time 0 through 2 in Figure 7a results in a p-value of 0.71.

that may have existed was offset by other factors.

6.2 Effects on children in the long run

My estimates of the intent-to-treat effect of STDI maternity benefits on the index of children's long-run education outcomes, constructed as the unweighted men of standardized versions of my primary outcomes, is shown graphically in Figure 8. The red line with triangle markers shows results from a specification that includes only county of birth fixed effects, fixed effects by birth and survey years to control nonparametrically for age and cohort effects, and a month of birth fixed effect to control for the seasonality in socioeconomic status of new births (Buckles and Hungerman, 2013). The green line with circle markers shows results from a specification that adds fixed effects at the year of birth by Census division level, which accounts nonparametrically for trends that vary across regions of the United States. Finally, my preferred specification, the blue line with no markers, follows the literature on long-run effects by adding predetermined characteristics of the county of birth interacted with a linear trend in year of birth (Almond, Hoynes and Schanzenbach, 2011; Bailey and Goodman-Bacon, 2015).[20]

The stability of my estimates across these three specifications underscore the robustness of the result: A drop in the human capital accumulation of the first generation born to mothers who were eligible for STDI maternity benefits. If these effects are driven by reductions in female wages and family income, we would expect spillover effects on older children who were born before the reform but are nevertheless exposed to some extent to the decrease in family resources. The slight negative slope of the pre-reform coefficients is consistent with this explanation; however, a joint test of the pre-reform coefficients in Figure 8, with a p-value of 0.43, fails to reject the null hypothesis that there is no pre-trend in educational outcomes. The flexible event-study specification also allows us to distinguish the dynamic effects of the policy after the reform. After a sharp break downward at event-time 0, the negative effects continue to grow in magnitude as more women take up STDI benefits (see Figure 1a) and the labor-market conditions experienced over the lifetime of the average mother continue to deteriorate.

These results are summarized in row 1 of Table 4. Column 2 shows the result of a

[20] These characteristics include county-level measures from the 1960 Census: Share of population living in an urban area, share in a rural area, share under 5 years of age, share over 65 years of age, share nonwhite, share with 12 years of education or more, share with less than $3,000 in annual income, and share with greater than $10,000 in annual income. Each of these characteristics is interacted with year of birth.

simple difference-in-difference estimate estimate of the intent-to-treat effect on the index of educational outcomes; children who were exposed to the policy saw a decrease of 1.9 percent of a standard deviation. Column 3 shows the F-statistic and p-value from a test of the null hypothesis that the pre-reform event-time coefficients are jointly equal to 0. The p-value of 0.43 provides little cause to reject the null hypothesis that there is no confounding pre-trend in the outcome variable.

While these negative effects on the index of education outcomes suggest children exposed to maternity leave saw a deterioration in their long-run outcomes, these results tell us little about the magnitude of the impact. For additional context, Table 4 also includes estimates for each of the four components of the index: years of education, high school completion, college attendance, and 4-year college completion. Column 2 suggests that children exposed to the paid-leave policy achieved 0.05 fewer years of education, or a decrease of two-fifths of 1 percent. The remaining results suggest that this decrease in educational attainment was concentrated at the upper end of the distribution: while the effect on high school completion is statistically indistinguishable from 0, college attendance fell by 1.9 percent among exposed children, and college completion fell by 3.1 percent. Figure 9 shows the results for college attendance and completion graphically, and they are qualitatively similar to the effects on the educational index, with downward break in the trend that levels off only after four to five years.

What could explain these sizable decreases in educational attainment for children exposed to STDI maternity benefits? One possible mechanism is a decrease in investment of resources in the first generation of children born after enactment of paid leave. Using data from the March CPS accessed via IPUMS (Ruggles et al., 2017), I calculate that the effects on family income reported in Section 5 amounted to a decrease of about 2 percent in family resources. If we assume this effect was persistent, and that the negative effects are driven solely by this change in family income, it suggests that a maternity-leave driven decrease in family income of 10 percent leads to a reduction in years of schooling of roughly one quarter of a year, or nearly 2 percent, and a nearly 5 percentage-point decrease in four-year college degree attainment.

These large effects are comparable to estimates of long-run impacts from other settings. For example, Stuart (2018) finds a 3 percentage-point decrease in college degree attainment for every 10 percent decrease in earnings per capita driven by the double-dip recession of the early 1980s. Similarly, in a study of an early welfare program, Aizer et al. (2016) conclude that an early welfare program raised family income during childhood by 20-30 percent and schooling by 4.3 percent of the control-group mean; an extrapolation of my results would suggest that a drop of income of similar magnitude would reduce years of schooling by a comparable 4 to 6 percent. While the settings examined by these papers are quite different from that of the expansion of STDI maternity benefits, the similar magnitudes of the effects provide assurance that the deterioration in child educational outcomes could reasonably be driven by the unintended decrease in family income.

Another way to place my estimates in context is to compare them to estimates of the long-run effects of other policies designed to improve children's long-run outcomes. For example, Bailey, Sun and Timpe (2018) examine the roll-out of Head Start and find an intent-to-treat effect of 0.29 extra years of schooling for children who attended fully implemented programs, or 0.043 years for all children exposed to the launch of a local Head Start center. An expansion of Medicaid coverage for pregnant women increased their children's high-school completion rates by nearly 4 percentage points, with suggestive evidence of effects of a similar magnitude on college attendance (Miller and Wherry, 2017). Similarly, Brown, Kowalski and Lurie (2015) find that a year of Medicaid enrollment in childhood raises the probability of enrolling in college by age 20 by 0.55 percentage points.[21] My estimates suggest that the magnitude of the effect of the enactment of paid leave was roughly one-sixth the size of the long-run educational-attainment benefit received by Head Start attendees, one-quarter of the college-attendance benefit enjoyed by beneficiaries of Medicaid while in utero, and the equivalent of about two years of Medicaid coverage in childhood. Overall, these results suggest the enactment of paid maternity benefits sparked a series of changes in the labor market and ultimately children's outcomes, with a magnitude similar to that of some the United States' most highly touted public programs, but in the opposite direction.

6.3 Reconciling long-run estimates with previous literature

My findings may be surprising in light of theoretical literature that emphasizes the importance of mother-child bonding time during critical periods of life, as well as empirical evidence that has suggested maternity leave policies improve infant health. While I do not find positive impacts on infant health, my results do not necessarily contradict the hypothesis that infants benefit from the increased bonding time and reduced stress conferred by

[21] Brown, Kowalski and Lurie (2015) find an effect on college attendance of 0.24 percentage points for men and 0.4 percentage points for women. The simple average of these two figures, divided by an estimate of 0.58 years of enrollment per year of eligibility, delivers an estimate of 0.55 percentage points.

paid maternity benefits. Rather, they suggest that to the extent such benefits exist, they are at risk of being attenuated or even reversed by unintended consequences of maternity leave policies, such as a deterioration of labor-market conditions that leaves families with fewer resources to invest in children during their formative years.

My findings are also at odds with the results of Carneiro, Løken and Salvanes (2015), who study a 1977 maternity leave expansion in Norway and find large positive effects on children in the long run. Two key differences may help reconcile these disparate findings.

The first key difference is related to research design and its implications for the interpretation of the estimates. Given the sharp policy change and rich data available, Carneiro, Løken and Salvanes (2015) use a regression discontinuity design that effective compares children born under a more generous policy regime to those born just a few days earlier. Such a design approximates an experiment in which paid maternity benefits are randomly assigned to expectant mothers, ruling out the possibility of general-equilibrium effects such as changes in the labor market that could differentially affect the treatment and control groups. The policy experiment in this paper, on the other hand, approximates a more general – and, arguably, more policy-relevant – experiment in which women and firms are allowed to respond across all possible margins to the introduction of paid leave benefits. In short, while Carneiro, Løken and Salvanes (2015) demonstrate compellingly that increased mother-child bonding time can lead to valuable improvements in long-run human capital accumulation, my results demonstrate that such effects can also be reversed by the unintended consequences of mandated maternity benefits.

The second key difference is related to the context of the two studies. The natural experiment examined by Carneiro, Løken and Salvanes (2015) took place in a country with a long-standing, relatively generous social safety net, including subsidies for relatively high-quality child care. Labor demand responses may be muted in countries where a higher degree of occupational segregation makes maternity leave less costly from the perspective of the firm (Blau and Kahn, 2013). In fact, findings from the nascent literature examining firm responses to maternity leave mandates suggests just such a dynamic, with firms in the United States displaying elastic demand for female labor while European firms respond less dramatically to parental leave policy (Thomas, 2018; Brenøe et al., 2018; Gallen, 2018).

7 Conclusion

The robust body of literature on the effects of family leave policies has demonstrated clearly that parents, and especially mothers, greatly value the opportunity to take an extended absence after the birth of a child without surrendering a job match or the stream of income that comes with it. However, the recent nature of U.S. parental-leave policies has made it difficult to evaluate effects that may take years or even decades to materialize.

This paper provides the first estimates of these long-run effects from the United States by constructing a history of the country's first expansion of paid maternity leave. While the policy greatly expanded the availability of maternity benefits and increased the amount of time new mothers spent on leave with a newborn child, it did not come without costs: I find evidence that women's wages fell by about 5 percent, leading to a decrease in the incomes of middle-class families. Furthermore, these effects persisted into the next generation, reducing children's educational attainment by 0.05 years and decreasing their probability of attending or graduating from college by 1.9 and 3.1 percent, respectively.

The finding of negative wage and family income effects, paired with a long-run decrease in children's educational attainment, suggests maternity leave policies may not necessarily achieve the goal of promoting gender equity and improving the welfare of the next generation. On the contrary, my results suggest that parental leave may bestow benefits on parents and their children in the short run while accruing significant costs in the long run. Furthermore, to the extent that long-run negative effects are passed through the channel of a reduction in family income, the families that bear the costs of parental leave policies may not be the same families that enjoy their benefits, suggesting the policy has distributional consequences.

How do we weigh these long-run costs against the short-term benefits of an expansion of paid maternity leave? In the literature on long-run effects of childhood interventions, one common way to quantify these costs is to generate an internal rate of return on the resources invested in the child. This exercise provides a way to scale the benefits – or, in this case, the costs – by relating their discounted future value to the initial amount invested.

To calculate the internal rate of return on STDI maternity benefits, I follow previous literature and first convert my estimates of the long-run effects on children's education to effects on potential earnings (Neal and Johnson, 1996; Deming, 2009; Bailey, Sun and Timpe, 2018). While realized earnings may be affected in subtle ways by changes in selection into the workforce, the impact on potential earnings can provide a sense of the opportunities gained or lost as a result of the treatment. Using a sample from the National Longitudinal Survey of Youth (NLSY) 1979 cohort, I regress log earnings on educational attainment and demographic covariates.[22] The use of the NLSY allows me to include AFQT scores, a proxy for ability, in the specification to alleviate concern about omitted variables bias. I then convert these estimates to the present value of lost potential earnings between age 25 and 54. If we scale this figure by the STDI maternity benefit take-up rate and compare it to the average benefit, we get a sense of the internal rate of return of the initial investment to the average child. Note that this calculation is inherently conservative because it abstracts from the cost of raising the funds and the immediate costs of the decrease in wages and family income that resulted from the reform.

My estimates from the log-earnings equation are shown in Panel A of Table 5. To facilitate comparisons to my long-run results, my preferred specification includes a linear term in years of schooling plus dummy variables to capture the effects of completing high school, attending college, and graduating from a four-year college. Column 1 includes only controls for education, age, race, and survey year. The effect of accounting for a proxy for underlying ability can be seen clearly in column 2, where I add a quadratic in AFQT to the specification and the coefficients on education fall considerably. For robustness, column 3 shows that I obtain similar estimates from a more standard specification where an indicator for college attendance is omitted. Finally, columns 4 and 5 break down the sample by gender, showing that returns to high school are higher for women but that college attendance and graduation are particularly profitable for men.

Panel B summarizes the implied effects on potential earnings between ages 25 and 54. Data from the state of New York suggests that the average mother who benefited from STDI between 1978 and 1985 received $3,129 in 2012 dollars.[23] In my preferred specification, the education effects suggest a decrease in potential earnings of about one-half of 1 percent per year. At birth, assuming a 5 percent discount rate, this equates to a cost of roughly $532 in 2012 dollars. However, if we scale it using a conservative estimate of a 25 percent STDI take-up rate, it becomes clear that the cost per treated child is much higher: more than $2,000 in discounted earnings, or an internal rate of return of -68 percent. Although the

[22]To match the individuals in my sample from the 2000 Census and 2001-2016 ACS, I use individuals only over the age of 25. I also drop individuals older than 54 to avoid concerns about retirement. Earnings has been converted to 2012 dollars using the CPI-U.

[23]Data on STDI pregnacy 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.

effects on education are comparable by gender, the differences in the return to college and potential earnings make the implied effect on men much larger: An 80 percent IRR for men relative to 58 percent for women.

This simple calculation does not account for the potential social costs of paid leave, including the cost of raising funds and the decreases in family income that occurred immediately in the wake of the policy. Even so, it suggests that maternity benefits come with significant long-run costs. These very large and negative internal rates of return are driven largely by the fact that the cost of paid leave is not limited to, and perhaps not even driven by, the direct cost of the benefit. Rather, the provision of these relatively modest benefits triggered large responses in the labor market because of the cost of disruptions for firms, whether real or perceived. An additional consideration is that, while my data does not allow me to observe variables about the family characteristics of the children who were affected, data from the SIPP suggests women who made use of STDI maternity benefits were relatively advantaged. This observation raises questions about the distributional consequences of the policy. Overall, the enactment of STDI maternity benefits suggests that future paid-leave policies must take into account the potential that they could alter labor-market opportunities for women and the well-being of future generations.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge. 2017. "When should you adjust standard errors for clustering?" NBER Working Paper 24003.

Agostinelli, Francesco, and Giuseppe Sorrenti. 2018. "Money vs. time: Family income, maternal labor supply, and child development." HCEO Working Paper 2018-017.

Ahammer, Alexander, Martin Halla, and Nicole Schneeweis. 2018. "The Effect of Prenatal Maternity Leave on Short and Long-Term Child Outcomes." IZA DP No. 11394.

Aizer, Anna, Shari Eli, Joseph Ferrie, and Adriana Lleras-Muney. 2016. "The long-run impact of cash transfers to poor families." *The American Economic Review*, 106(4): 935–971.

Almond, Douglas, Hilary W. Hoynes, and Diane Whitmore Schanzenbach. 2011. "Inside the War on Poverty: The impact of Food Stamps on birth outcomes." *The Review of Economics and Statistics*, 93(2): 387–403.

American Academy of Pediatrics, and Pediatric Policy Council. 2015. "Major pediatric associations call for congressional action on paid leave."

Applebaum, Eileen, and Ruth Milkman. 2011. "Leaves that pay: Employer and work experiences with paid family leave in California." Center for Economic and Policy Research.

Autor, David, Mark Duggan, Jonathan Gruber, and Catherine Maclean. 2013. "How does Access to Short Term Disability Insurance Impact SSDI Claiming?" National Bureau of Economic Research.

Bailey, Martha J. 2006. "More power to the pill: The impact of contraceptive freedom on women's life cycle labor supply." *Quarterly Journal of Economics*, 121(1): 289–320.

Bailey, Martha J., and Andrew Goodman-Bacon. 2015. "The War on Poverty's Experiment in Public Medicine: Community Health Centers and the Mortality of Older Americans." *American Economic Review*, 105(3): 1067–1104.

Bailey, Martha J., Shuqiao Sun, and Brenden Timpe. 2018. "Prep school for poor kids: The long-run impacts of Head Start on human capital and economic self-sufficiency."

Baker, Michael, and Kevin Milligan. 2010. "Evidence from Maternity Leave Expansions of the Impact of Maternal Care on Early Child Development." *Journal of Human Resources*, 45(1): 1–32.

Baker, Michael, and Kevin Milligan. 2014. "Maternity leave and children's cognitive and behavioral development." *Journal of Population Economics*, 28(2): 373–391.

Bana, Sarah, Kelly Bedard, and Maya Rossin-Slater. 2018. "The impacts of paid family leave benefits: regression kink evidence from California administrative data." IZA DP No. 11381.

Bastian, Jacob. 2018. "The rise of working mothers and the 1975 Earned Income Tax Credit."

Bastian, Jacob, and Kathy Michelmore. 2018. "The long-term impact of the Earned Income Tax Credit on children's education and employment outcomes." *Journal of Labor Economics*, 36(4): 1127–1163.

Baum, Charles L. 2003. "The effect of state maternity leave legislation and the 1993 Family and Medical Leave Act on employment and wages." *Labour Economics*, 10(5): 573–596.

Baum, Charles L., and Christopher J. Ruhm. 2016. "The effects of paid family leave in California on labor market outcomes." *Journal of Policy Analysis and Management*, 35(2): 333–356.

Berger, Lawrence M., and Jane Waldfogel. 2004. "Maternity leave and the employment of new mothers in the United States." *Journal of Population Economics*, 17(2): 331–349.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *The Quarterly Journal of Economics*, 119(1): 249–275.

Blau, Francine D., and Lawrence M. Kahn. 2013. "Female Labor Supply: Why is the US Falling Behind?" NBER Working Paper 18702.

Bobo, Lawrence, James Johnson, Barry Bluestone, Irene Browne, Sheldon Danziger, Philip Moss, Gary P. Green, Harry Holzer, Joleen Kirschenman, Maria Krysan, Camille Zubrinsky Charles, Michael Massagli, Melvin Oliver, Reynolds Farley, and Chris Tilly. 2008. "Multi-City Study of Urban Inequality, 1992-1994: [Atlanta, Boston, Detroit, and Los Angeles]."

Brenøe, Anne A., Serena Canaan, Nikolaj A. Harmon, and Heather Royer. 2018. "Is parental leave costly for firms and coworkers?"

Brown, David W., Amanda E. Kowalski, and Ithai Z. Lurie. 2015. "Medicaid as an investment in children: What is the long-term impact on tax receipts?" NBER Working Paper No. 20835.

Buckles, Kasey S., and Daniel M. Hungerman. 2013. "Season of birth and later outcomes: Old questions, new answers." *The Review of Economics and Statistics*, 95(3): 711–724.

Byker, Tanya S. 2016. "Paid Parental Leave Laws in the United States: Does Short-Duration Leave Affect Women's Labor-Force Attachment?" *American Economic Review*, 106(5): 242–46.

Cameron, A. Colin, and Douglas L. Miller. 2015. "A practitioner's guide to cluster-robust inference." *Journal of Human Resources*, 50(2): 317–372.

Card, David. 1992. "Using Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage." *Industrial and Labor Relations Review*, 46(1): 22–37.

Carneiro, Pedro, Katrine V. Løken, and Kjell G. Salvanes. 2015. "A flying start? Maternity leave benefits and long-run outcomes of children." *Journal of Political Economy*, 123(2): 365–412.

Dahl, Gordon B., and Lance Lochner. 2012. "The impact of family income on child achievement: Evidence from the Earned Income Tax Credit." *American Economic Review*, 102(5): 1927–56.

Dahl, Gordon B., Katrine V. Løken, Magne Mogstad, and Kari Vea Salvanes. 2016. "What is the case for paid maternity leave?" *Review of Economics and Statistics*, 98(4): 655–670.

Danzer, Natalia, and Victor Lavy. 2018. "Paid parental leave and children's schooling outcomes." *The Economic Journal*, 128(608): 81–117.

Das, Tirthatanmoy, and Solomon W. Polachek. 2015. "Unanticipated effects of California's paid family leave program." *Contemporary Economic Policy*, 33(4): 619–635.

Deming, David. 2009. "Early childhood intervention and life-cycle skill development: Evidence from Head Start." *American Economic Journal: Applied Economics*, 1(3): 111–134.

Dustmann, Christian, and Uta Schönberg. 2012. "Expansions in Maternity Leave Coverage and Children's Long-Term Outcomes." *American Economic Journal: Applied Economics*, 4(3): 190–224.

Faulkner, Edwin J. 1940. *Accident-and-Health Insurance.* New York and London:McGraw-Hill Book Company Inc.

Gallen, Yana. 2018. "The effect of maternity leave extensions on firms and coworkers." Working paper.

Gladstone, Leslie W., Jennifer D. Williams, and Richard S. Belous. 1985. "Maternity and parental leave policies: A comparative analysis." Congressional Research Service 85-184 GOV.

Gruber, Jonathan. 1994. "The incidence of mandated maternity benefits." *The American Economic Review*, 622–641.

Han, Wen-Jui, Christopher Ruhm, and Jane Waldfogel. 2009. "Parental leave policies and parents' employment and leave-taking." *Journal of Policy Analysis and Management*, 28(1): 29–54.

Heckman, James J., and Stefano Mosso. 2014. "The economics of human development and social mobility." *Annu. Rev. Econ.*, 6(1): 689–733.

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond. 2016. "Long-run impacts of childhood access to the safety net." *American Economic Review*, 106(4): 903–934.

Hudomiet, Peter. 2015. "The role of ocupation specific adaptation costs in explaining the educational gap in unemployment." Working paper.

Kamerman, Sheila B., Alfred J. Kahn, and Paul Kingston. 1983. *Maternity policies and working women.* New York:Columbia University Press.

Kezdi, Gabor. 2004. "Robust standard error estimation in fixed-effects panel models." *Hungarian Statistical Review*, Special English Volume 9: 95–116.

Klerman, Jacob Alex, and Arleen Leibowitz. 1997. "Labor supply effects of state maternity leave legislation." *Gender and Family Issues in the Workplace. New York: Russell Sage*, 65–85.

Klerman, Jacob Alex, Arleen Leibowitz, F. Blau, and R. Ehrenberg. 1997. "Gender and Family Issues in the Workplace."

Klerman, Jacob Alex, Kelly Daley, and Alyssa Pozniak. 2012. "Family and medical leave in 2012: Technical report." Abt Associates, Cambridge, MA.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. "Experimental analysis of neighborhood effects." *Econometrica*, 75(1): 83–119.

Lemieux, Thomas. 2006. "Increasing residual wage inequality: Composition effects, noisy data, or rising demand for skill?" *American Economic Review*, 96(3): 461–498.

Levy, Helen. 2004. "Employer-sponsored disability insurance: where are the gaps in coverage?" NBER Working Paper 10382.

Meyer, Bruce D., Wallace K.C. Mok, and James X. Sullivan. 2015. "Household surveys in crisis." *Journal of Economic Perspectives*, 29(4): 199–226.

Miller, Sarah, and Laura R. Wherry. 2017. "The long-term effects of early life Medicaid coverage." *Journal of Human Resources*. Forthcoming.

Moulton, Brent R. 1990. "An illustration of a pitfall in estimating the effects of aggregate variables on micro units." *The Review of Economics and Statistics*, 334–338.

Mukhopadhyay, Sankar. 2012. "The Effects of the 1978 Pregnancy Discrimination Act on Female Labor Supply." *International Economic Review*, 53(4): 1133–1153.

National Center for Health Statistics. 2015. "Natality Detail File, 1970-1984: [United States]." U.S. Department of Health and Human Services [producer]. Inter-university Consortium for Political and Social Research [distributor].

Neal, Derek A., and William R. Johnson. 1996. "The role of premarket factors in black-white wage differences." *Journal of Political Economy*, 104(5): 869–895.

OECD. 2018. "OECD Family Database."

Olivetti, Claudia, and Barbara Petrongolo. 2017. "The economic consequences of family policies: Lessons from a century of legislation in high-income countries." *Journal of Economic Perspectives*, 31(1): 205–230.

Pei, Zhuan, Jorn-Steffen Pischke, and Hannes Schwandt. 2018. "Poorly measured confounders are more useful on the left than on the right." *Journal of Business and Economic Statistics.*

Price, Daniel N. 1986. “Cash benefits for short-term sickness: Thirty-five years of data, 1948-83.” *Social Security Bulletin*, 49: 5.

Rossin, Maya. 2011. “The effects of maternity leave on children’s birth and infant health outcomes in the United States.” *Journal of Health Economics*, 30(2): 221–239.

Rossin-Slater, Maya. 2018. “Maternity and family leave policy.” In *The Oxford Handbook of Women and the Economy*. Oxford University Press.

Rossin-Slater, Maya, Christopher J. Ruhm, and Jane Waldfogel. 2013. “The Effects of California’s Paid Family Leave Program on Mothers’ Leave-Taking and Subsequent Labor Market Outcomes.” *Journal of Policy Analysis and Management*, 32(2): 224–245.

Rousmaniere, Jr., James. 1977. “Chamber to press for veto of pregnancy benefit bill.” *The Baltimore Sun*, A11.

Ruggles, Steven, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. 2017. “Integrated Public Use Microdata Series: Version 7.0 [dataset].” University of Minnesota.

Ruhm, Christopher J. 2000. “Parental leave and child health.” *Journal of Health Economics*, 19(6): 931–960.

Sarin, Natasha. 2017. “The impact of paid leave programs on female employment outcomes.” Working paper.

Sholar, Megan A. 2016. “Donald Trump and Hillary Clinton both support paid family leave. That’s a breakthrough.” *The Washington Post*.

Stearns, Jenna. 2015. “The effects of paid maternity leave: Evidence from Temporary Disability Insurance.” *Journal of Health Economics*, 43: 85–102.

Stoddard, Christiana, Wendy A. Stock, and Elise Hogenson. 2016. “The impact of maternity leave laws on Cesarean delivery.” *BE Journal of Economic Analysis and Policy*, 16(1): 321–364.

Stuart, Bryan. 2018. “The long-run effects of recessions on education and income.” Working paper.

Stuart, Bryan, Evan Taylor, and Martha Bailey. 2016. “Summary of procedure to match Numident place of birth county to GNIS places.” U.S. Census Bureau 1284 Technical Memo 2.

Summers, Lawrence H. 1989. “Some simple economics of mandated benefits.” *The American Economic Review*, 79(2): 177–183.

The White House Council of Economic Advisers. 2014. “The economics of paid and unpaid leave.” Technical report.

Thomas, Mallika. 2018. “The Impact of Mandated Maternity Benefits on the Gender Differential in Promotions: Examining the Role of Adverse Selection.” Working paper.

U.S. Bureau of Labor Statistics. 2017. "National Compensation Survey: Employee Benefits in the United States, March 2017." U.S. Department of Labor.

U.S. Census Bureau. 1981. "Money income of households in the United States: 1979." U.S. Commerce Department Current Population Reports 126, Washington, DC.

U.S. House of Representatives. 1977. *Legislation to prohibit sex discrimination on the basis of pregnancy: hearing before the Subcommittee on Employment Opportunities of the Committee on Education and Labor, House of Representatives, Ninety-fifth Congress, first session, on H.R. 5055 and H.R. 6075 ... held in Washington, D.C., April 6-June 29, 1977.* Washington:U.S. Govt. Print. Off. : [For sale by the Supt. of Docs., U.S. G.P.O., Congressional Sales Office].

U.S. Senate. 1979. *Legislative history of the Pregnancy Discrimination Act of 1978, public law 95-555: prepared for the Committee on Labor and Human Resources, United States Senate.* Washington:U.S. Govt. Print. Off.

Waldfogel, Jane. 1998. "Understanding the "Family Gap" in Pay for Women with Children." *The Journal of Economic Perspectives*, 12(1): 137–156.

Waldfogel, Jane. 1999. "The impact of the Family and Medical Leave Act." *Journal of Policy Analysis and Management*, 281–302.

Wisensale, Steven K. 2001. *Family Leave Policy: The Political Economy of Work and Family in America.* M.E. Sharpe, Inc.

Table 1: Share of new mothers claiming STDI maternity benefits, 1984-1989

	(1) Universal STDI states	(2) All other states	(3) P-value: Test of difference
All mothers	0.18	0.02	0.000
	(0.39)	(0.15)	
Age 18-29	0.19	0.02	0.000
	(0.40)	(0.14)	
Age 30-45	0.16	0.03	0.001
	(0.37)	(0.16)	
Married	0.21	0.02	0.000
	(0.41)	(0.16)	
Unmarried	0.08	0.01	0.004
	(0.27)	(0.10)	
Nonwhite	0.13	0.01	0.001
	(0.33)	(0.09)	
White	0.20	0.02	0.000
	(0.40)	(0.16)	
HS dropout	0.07	0.01	0.002
	(0.26)	(0.07)	
HS grad & some college	0.22	0.02	0.000
	(0.42)	(0.16)	
Four-year college graduate	0.18	0.03	0.009
	(0.39)	(0.16)	
Observations	1,265	4,486	

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. Column 1 shows 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. Column 2 shows share receiving benefits in all other states. Standard deviations are in parentheses. Column 3 shows p-value from test of null hypothesis of no difference in share receiving benefits across the two groups of states.

Table 2: Effects of paid maternity leave on hourly wages and employment

	Women		Men	
	(1)	(2)	(3)	(4)
	Log wages	Employed	Log wages	Employed
Event years -4 to -2	0.000717	0.0158	0.0126	-0.0101*
	(0.0100)	(0.0110)	(0.0117)	(0.00560)
	[1.00]	[0.27]	[0.64]	[0.62]
Event years 0 to 2	-0.0436***	-0.00110	-0.00623	-0.0114
	(0.0128)	(0.00473)	(0.0137)	(0.00665)
	[0.09]	[0.89]	[0.81]	[0.39]
Event years 3 to 5	-0.0432**	0.0134*	-0.00247	-0.0120
	(0.0193)	(0.00766)	(0.0153)	(0.00915)
	[0.15]	[0.28]	[0.95]	[0.43]
Observations	584,761	1,063,681	673,816	973,623
R-squared	0.271	0.063	0.357	0.114
Control mean	4.22	0.630	5.93	0.847

Notes: Coefficients displayed are estimates of τ_k from equation (6) with event time pooled into three-year bins. Standard errors in parentheses are clustered by state group. Figure in brackets is p-value from a randomization inference procedure based on 1,000 draws of state-level STDI coverage and anti-discrimination law enactment date. All specifications includes a quadratic in age interacted with indicators for Hispanic ethnicity and nonwhite race, years of education, indicators for completing high school and four-year college, and fixed effects for year-by-month, state-group, and Census-division-by-year. Specification also includes linear trend in survey year interacted with the following state-level characteristics from the 1970 decennial Census via IPUMS (Ruggles et al., 2017): share black, average years of education among women, share with high school degree, share with college degree, number of children born to women, and share in poverty.Sample includes men and women age 18-45 from the 1973-1987 May and Merged Outgoing Rotation Group CPS files. Individuals with imputed values have been dropped, as have wage observations below $1 or above $100 in 1979 dollars. Wages are converted to 1979 dollars using the CPI. Regressions are weighted using CPS earnings weights.

Table 3: Heterogeneity of the effect of paid leave on wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	High-cost job	Log wages		Log wages		Log wages	
	All employed women	Low-cost occupations	High-cost occupations	Legislative reform	Congress or courts	Universal STDI	Non-universal
Event years -4 to -2	-0.0132*	0.00180	0.00638	-0.0136**	0.00745	-0.00597	0.000799
	(0.00734)	(0.0106)	(0.0141)	(0.00622)	(0.0153)	(0.00621)	(0.0168)
Event years 0-2	-0.0172	-0.0309***	-0.0836***	-0.0301**	-0.0529***	-0.0641***	-0.0370*
	(0.0121)	(0.0107)	(0.0116)	(0.0119)	(0.0168)	(0.0193)	(0.0204)
Observations	791,386	582,514		584,761		584,761	
R-squared	0.182	0.302		0.271		0.271	
F-statistic: Equal effects		14.70		0.988		0.804	
P-value: Equal effects		0.00104		0.332		0.381	

Notes: Coefficients in column 1 are estimates of τ_k from equation (6) with event time pooled into three-year bins, and $STDI_{s,1970}$ calculated using equation (7). In columns 2-7, a separate τ_k is estimated for each group specified. Dependent variable in column 1 is a dependent variable indicating employment in an occupation in which the time required to become fully productive is above the median, as measured in the Multi-City Study of Urban Inequality (Bobo et al., 2008). In columns 2 and 3, effect on log wages is estimated separately for for low- and high-cost occupations. In columns 4 and 5, effect on log wages is estimated separately for states where paid leave was enacted due to an act of the Legislature or executive branch (column 4) or due to a state supreme court decision or act of U.S. Congress (column 5). Sample includes men and women age 18-45 from the 1973-1978 May CPS and 1979-1987 Merged Outgoing Rotation Group CPS files. Standard errors in parentheses clustered at state group level. Wages are converted to 1979 dollars using the CPI.

Table 4: Long-run effects on child educational outcomes

	(1) Sample mean	(2) Intent-to-treat effect	(3) Test for pre-trend	(4) Percent change
HC index		-0.0186***	0.97	
		(0.004)	[0.43]	
Years of schooling	13.7	-0.0538***	1.71	-0.4%
		(0.009)	[0.16]	
High school graduate	0.93	-0.00118	1.05	-0.1%
		(0.001)	[0.39]	
Some college	0.66	-0.0125***	0.4	-1.9%
		(0.003)	[0.81]	
College graduate	0.32	-0.00998***	1.44	-3.1%
		(0.002)	[0.23]	

Notes: Coefficients displayed in column 2 are estimated intent-to-treat effects of exposure to paid maternity benefits on children in the long run. Sample includes individuals in the 2000 long-form decennial Census and 2001-2016 American Community Survey linked to the Social Security Administration's Numident file, born in the United States between 1954 and 1985 and age 25 or older when surveyed. Column 3 shows F-statistic and p-value from a test of the null hypothesis that the pre-reform coefficients are jointly equal to 0. Column 4 shows estimate as a percent change relative to sample mean. Standard errors in parentheses clustered at state of birth level. Estimated using equation (6) with D_s equal to the estimated share of working women of child-bearing age with access to STDI through an employer in 1970.

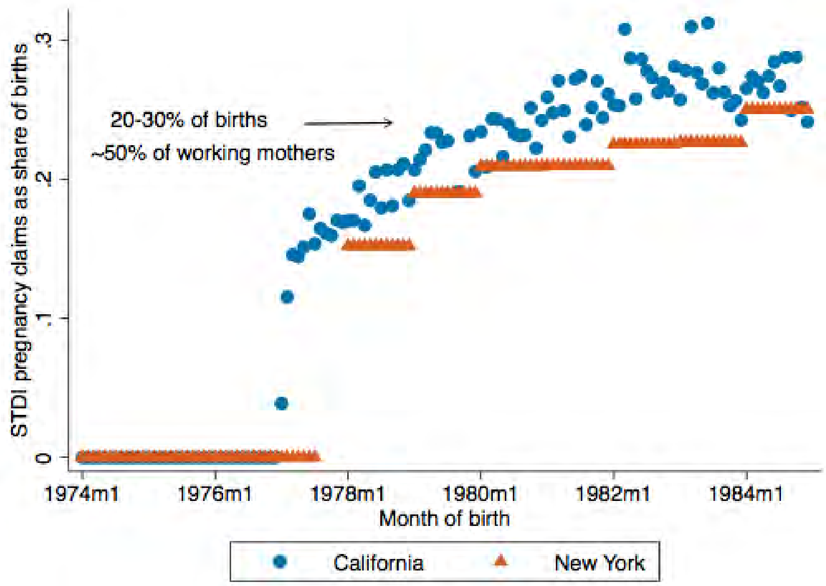
Table 5: Estimated effects of educational attainment on potential income

	(1) Log earnings	(2) Log earning	(3) Log earnings	(4) Log earnings	(5) Log earnings
Panel A: Log earnings estimates					
Years of education	0.0175*	0.00328	0.00781	0.0252*	-0.0185
	(0.00944)	(0.00950)	(0.00759)	(0.0138)	(0.0126)
High school degree	0.355***	0.227***	0.224***	0.340***	0.231***
	(0.0395)	(0.0394)	(0.0417)	(0.0675)	(0.0505)
Some college	0.177***	0.106***		0.0960**	0.126***
	(0.0274)	(0.0273)		(0.0396)	(0.0376)
College degree	0.314***	0.288***	0.289***	0.243***	0.317***
	(0.0367)	(0.0364)	(0.0373)	(0.0536)	(0.0489)
AFQT	No	Yes	Yes	Yes	Yes
Sample	All	All	All	Women	Men
Observations	129,536	129,536	129,536	62,838	66,698
R-squared	0.204	0.222	0.183	0.138	0.199
Mean	26,370	26,370	26,370	20,848	33,190
Panel B: Discounted value of change in potential earnings					
Annual change	-176	-122	-94	-104	-143
Total discounted value	-768	-532	-409	-452	-625
Total per treated child	-3072	-2129	-1637	-1807	-2501
Internal rate of return	-98%	-68%	-52%	-58%	-80%

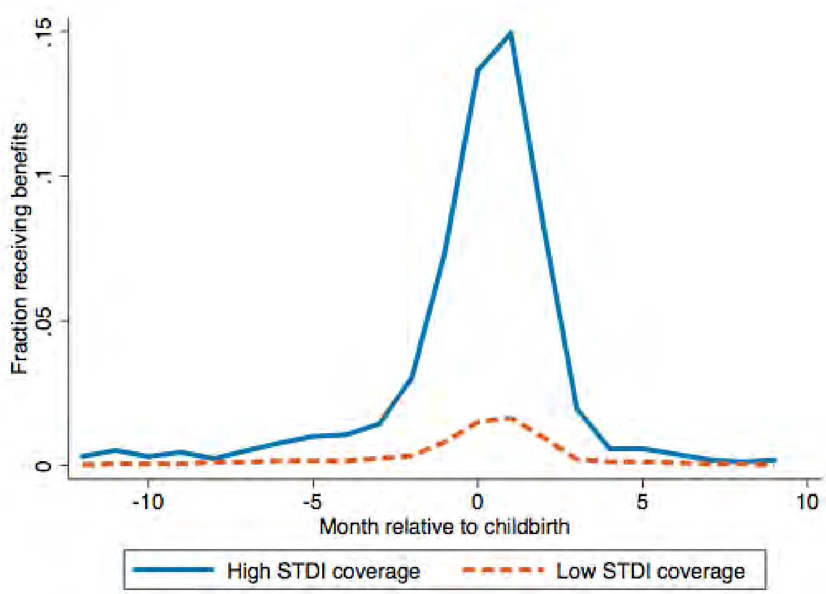
Notes: Data includes individuals from National Longitudinal Survey of Youth 1979 cohort, age 25 and older. In addition to education variables show, specifications include survey year, quadratic in age interacted with race and gender, and a quartic in AFQT score. AFQT score has been standardized within the sample by year of birth. Standard errors are clustered by individual to adjust for within-person correlation in error term over time. Discounted value of lost potential earnings assumes fixed discount rate of 5 percent. Total per treated child assumes take-up rate of STDI benefits of 25 percent. Internal rate of return is constructed using an estimated average STDI maternity benefit of $3,129 in 2012 dollars. All figures are expressed in 2012 dollars, adjusted using the CPI.

Figure 1: Roll-out of STDI pregnancy benefits creates variation over time and across states

(a) Launch of STDI benefits in two high-coverage states



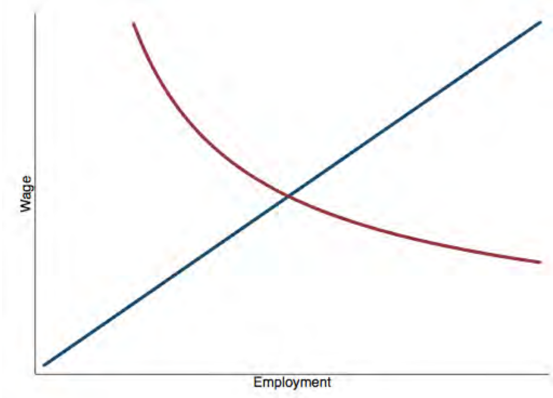
(b) Take-up of STDI benefits wider in high-coverage states



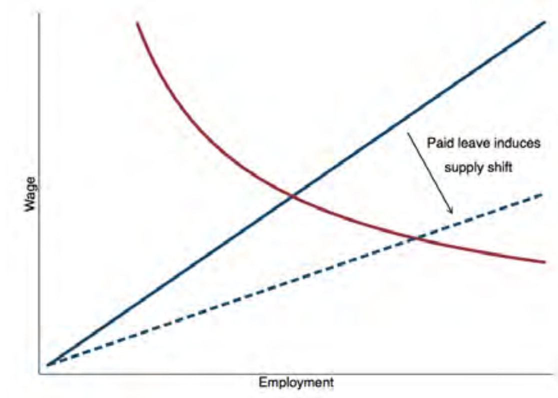
Notes: STDI maternity benefits were enacted in January 1977 in California and August 1977 in New York. Data in Figure 1a is constructed by dividing the number of STDI pregnancy claims by month or year in California and New York by the number of births to residents of those states. STDI pregnancy claims provided by California Employment Development Department and New York Workers Compensation Board. Birth records come from Natality Detail Files (National Center for Health Statistics, 2015). Data in Figure 1b 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 2: Expected labor-market effects of paid maternity leave

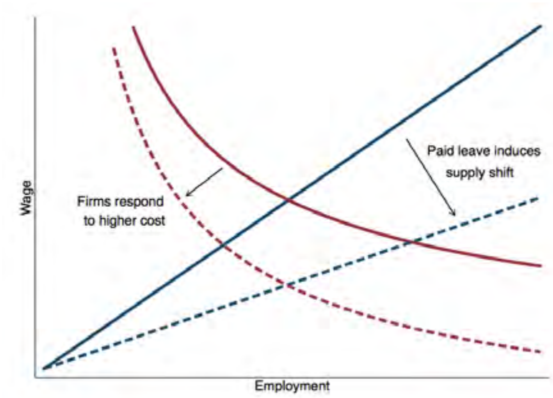
(a) Labor supply and demand



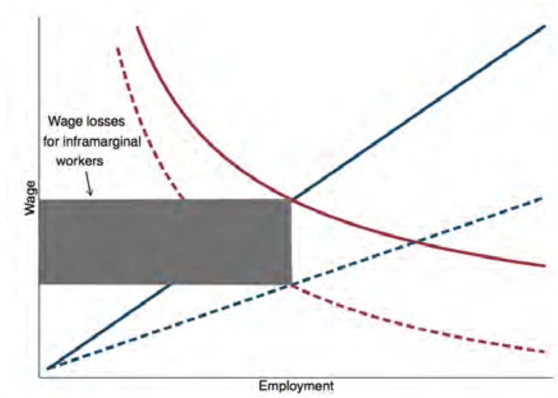
(b) Labor supply shifts



(c) Labor demand response

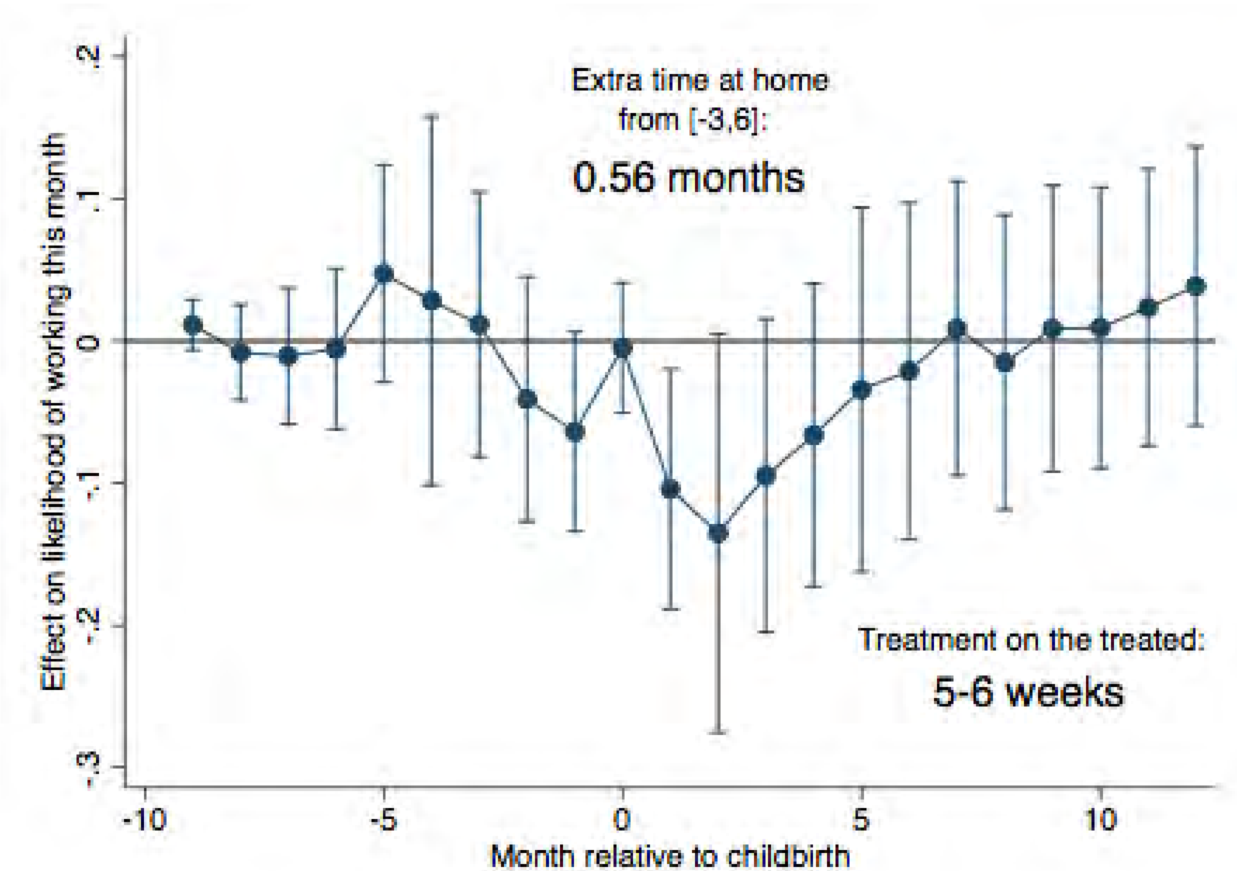


(d) Income loss for inframarginal women



Notes: Figure shows graphical representation of stylized labor-market model outlined in Section 3. Panel 2a shows initial labor-market equilibrium. Panel 2b shows response of women to enactment of benefit that reduces disutility of work. In Panel 2c, firms respond to the cost of providing the benefit. Panel 2d 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.

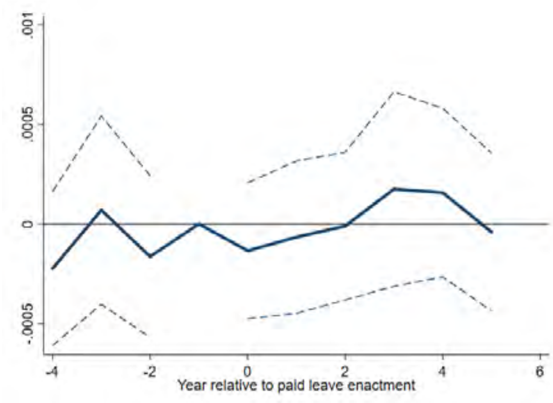
Figure 3: Short-run effect on time spent at work in months around childbirth



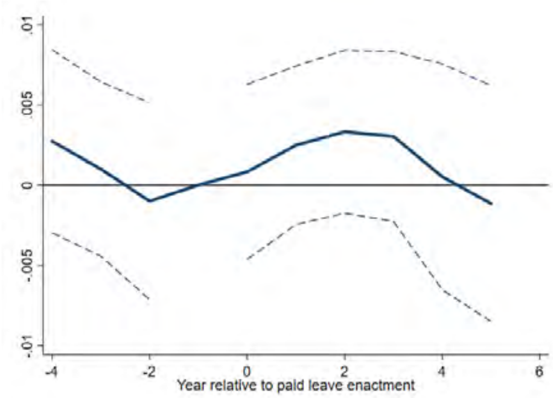
Notes: Data includes women from the retrospective fertility module in the 1984 and 1985 SIPP. Sample is limited to women whose first child was born between 1970 and 1984 while between the ages of 18 and 45. Women are asked about labor supply by month only if they worked during their first pregnancy. Figure shows intent-to-treat estimates of STDI exposure on time spent at work by month relative to childbirth, using a version of equation (6) that restricts event time to dummies indicating birth before or after the reform. Standard errors in Panel B are clustered at the state-group level.

Figure 4: Evaluating the internal validity of the roll-out of STDI maternity benefits

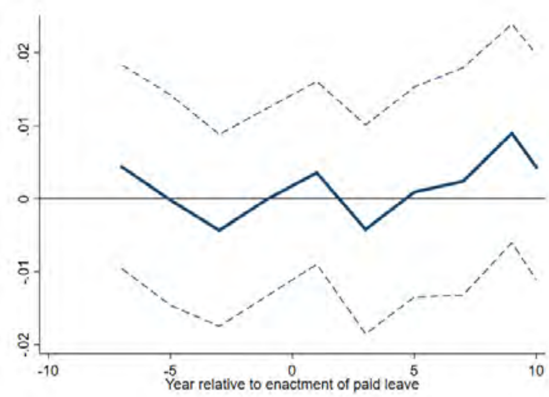
(a) Earned Income Tax Credit per capita



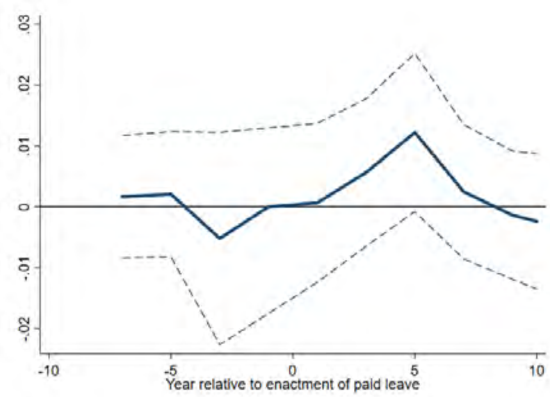
(b) Food Stamps per capita



(c) Share of women receiving welfare



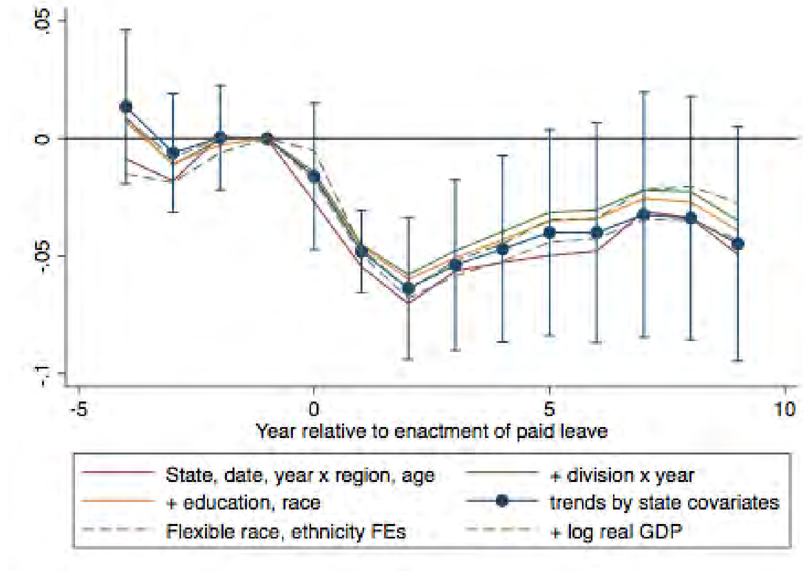
(d) Share of women with any govt income



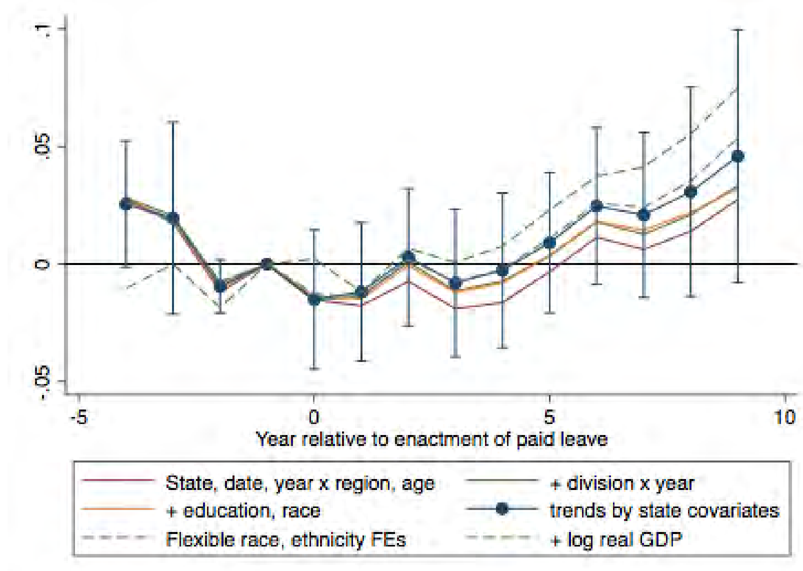
Notes: Panels show estimates of τ from equation (6) using measures of transfer income per capita constructed using data from the BEA Regional Income Division and population counts from the National Cancer Institute or data from the March CPS, 1968-1984, accessed via IPUMS (Ruggles et al., 2017).

Figure 5: Effects of paid leave on hourly wages

(a) Women age 18-45



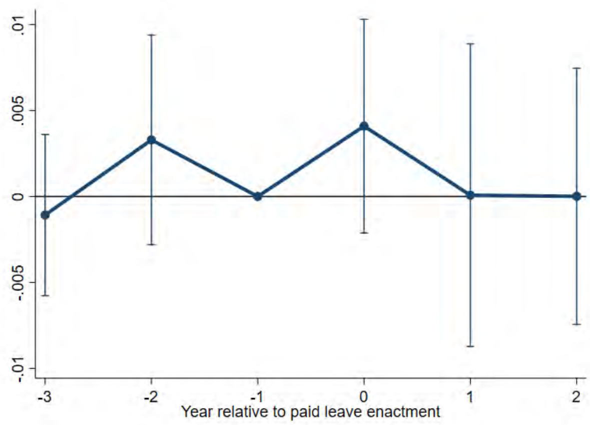
(b) Men age 18-45



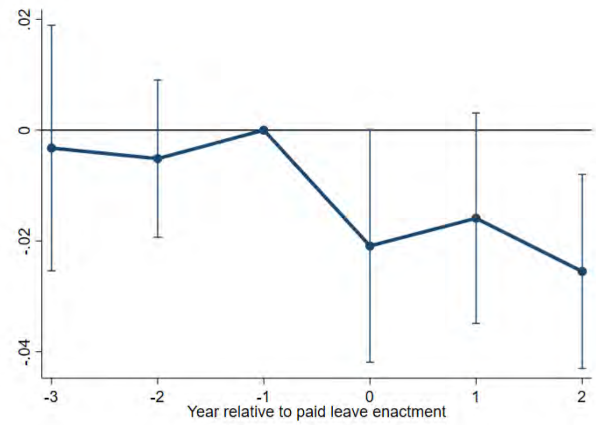
Notes: Graph shows event-study estimates from equation (6) using samples of women and men age 18-45 from the 1973-1987 May CPS and 1979-1987 Merged Outgoing Rotation Group files. Sample excludes self-employed and farm workers, as well as wages greater than \$100 or less than \$1 in 1979 dollars. Weighted regressions use CPS earnings weights where available, and standard CPS sampling weights from 1973-1978. Basic controls include fixed effects for month and year of the survey, state, and a quadratic in age interacted with indicators of nonwhite race and Hispanic ethnicity. Education controls include a linear term in years of schooling plus indicators for completing high school and college. Standard errors are clustered at the state-group level.

Figure 6: Effects of paid leave on family income

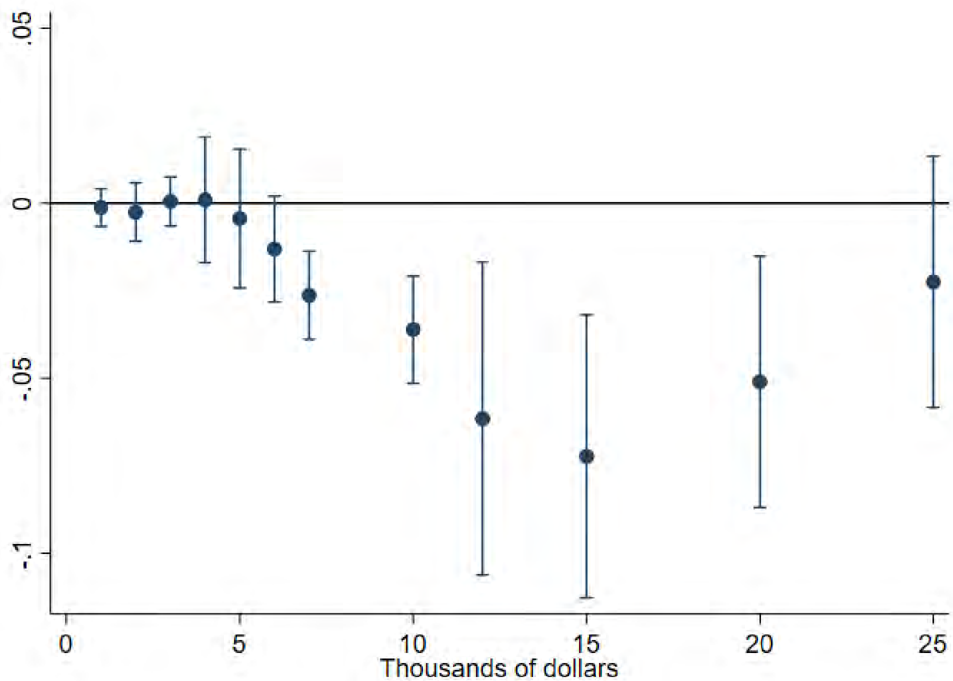
(a) Share with income $\geq$ \$1,000



(b) Share with income $\geq$ \$7,500



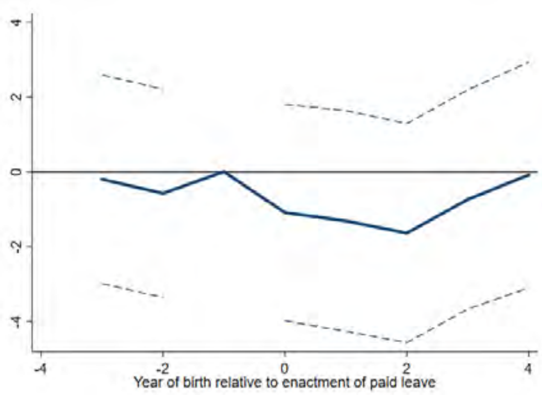
(c) Effect on the distribution of family income



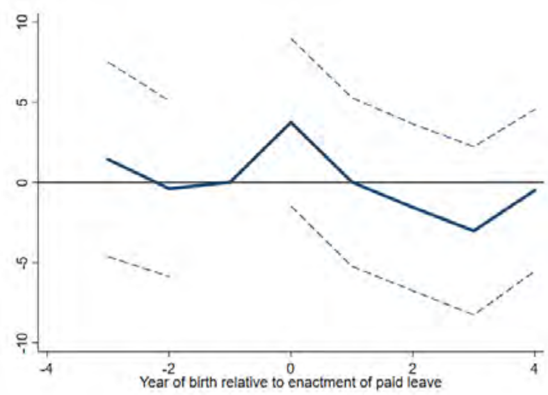
Notes: Figures 6a and 6b show event-study estimates of the effect of the enactment of paid leave on the share of women age 18-45 in families with income greater than \$1,000 and \$7,500, respectively. Figure 6c shows difference-in-difference estimates of the same effect at various thresholds of family income. Sample includes women age 18-45 from the 1974-1981 May CPS who are the head or wife of the household head. Weighted regressions use CPS earnings weights. Standard errors are clustered at the state-group level.

Figure 7: Effect of STDI maternity benefits on fertility

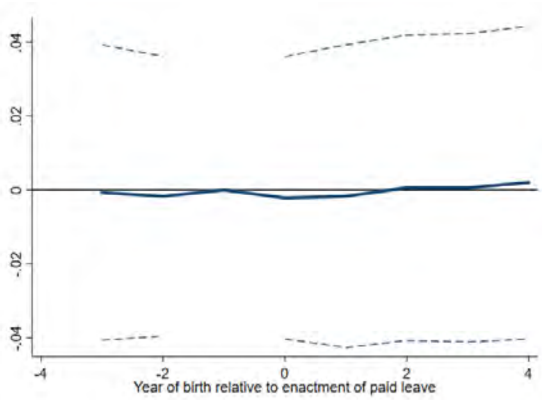
(a) Fertility rates



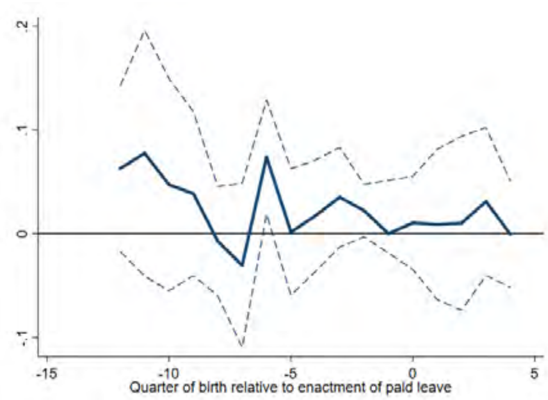
(b) Average birthweight



(c) Share born to nonwhite mothers

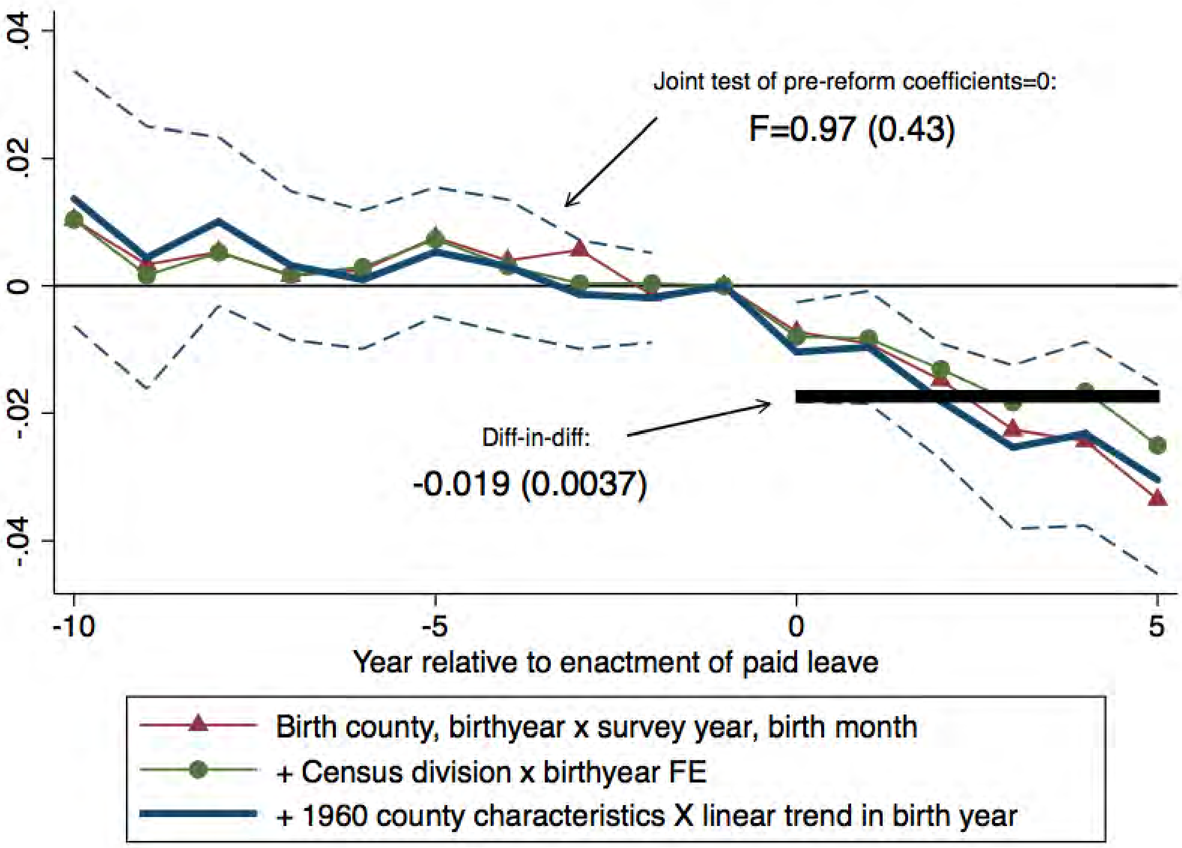


(d) Mothers' education



Notes: Estimates in Figures 7a, 7b, and 7c use birth record data from the Natality Detail File, 1974-1984, accessed via ICPSR, and population counts by age, sex, and race from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program. In Figure 7d, data on mother's education comes from 1980 long-form decennial Census accessed via IPUMS (Ruggles et al., 2017). Standard errors are adjusted for heteroskedasticity. In Figure 7d, standard errors are also adjusted for intracluster correlation within states and individual mothers.

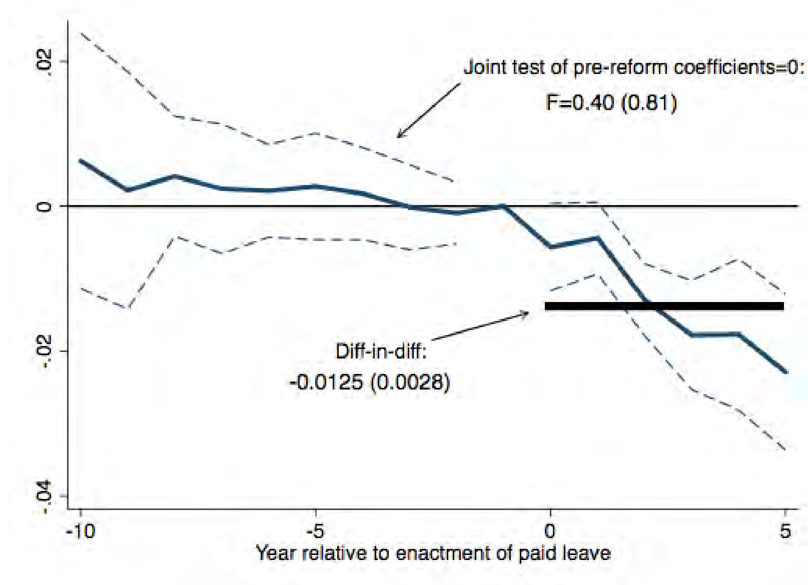
Figure 8: ITT effect of paid leave enactment on index of educational outcomes



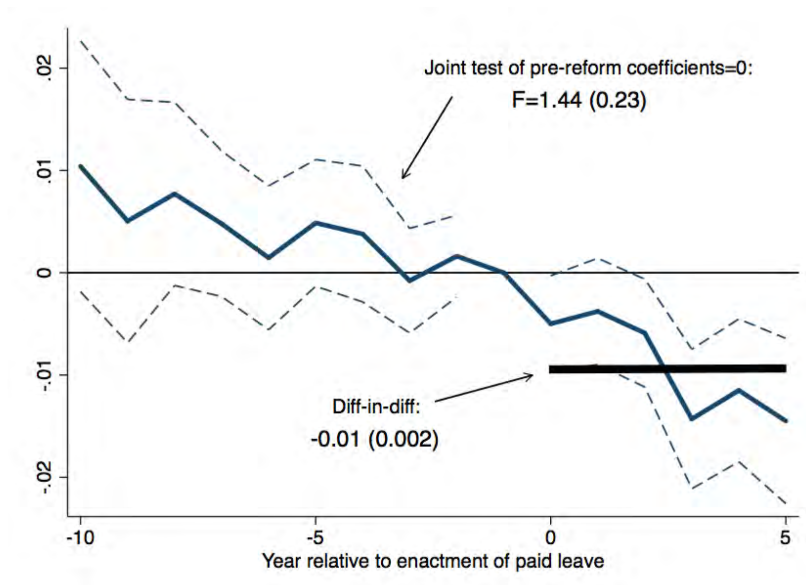
Notes: Coefficients displayed are estimated intent-to-treat effects of exposure to paid maternity benefits on children in the long run. Standard errors clustered at state level. Sample includes individuals in the restricted 2000 long-form decennial Census and 2001-2016 American Community Survey, using cohorts born in the United States between 1954-1985, and individuals age 25 or older when surveyed. Estimated using equation (6) with $STDI_{s,1970}$ calculated using industry-level STDI coverage shares from Autor et al. (2013) and 1970 decennial Census microdata.

Figure 9: The long-run effects of STDI maternity benefits on children's education

(a) College attendance



(b) College completion



Notes: Coefficients displayed are estimated intent-to-treat effects of exposure to paid maternity benefits on children in the long run. Standard errors clustered at state level. Sample includes individuals in the restricted 2000 long-form decennial Census and 2001-2016 American Community Survey, using cohorts born in the United States between 1954-1985, and individuals age 25 or older when surveyed. Estimated using equation (6) with $STDI_{s,1970}$ calculated using industry-level STDI coverage shares from Autor et al. (2013) and 1970 decennial Census microdata.