Baby Steps to Success?

The impact of paid maternity leave on children's long-term outcomes in the United States

Krishna Regmi* Le Wang†

February 3, 2025

Abstract

Paid leave policy continues to be heatedly debated in the United States, with its potential impact on child development often cited as a reason for support, but with limited empirical evidence. This paper fills this gap by examining the effects of paid maternity leave on children's long-term educational and employment outcomes. Using the variation in availability of paid leave created by the Pregnancy Discrimination Act of 1978, we find that individuals born after the policy was implemented are more likely to have a college degree at age 26, particularly among black children, a more economically disadvantaged group. However, this has not yet been translated into a significant improvement in employment opportunities in their early career. More importantly, our event study estimates indicate that the beneficial effects decreased rapidly over time and the impacts were more pronounced among the initial cohorts exposed to these effects. The US evidence highlights the potential for such policies to benefit children's education through increased intra-household specialization and financial resources, and increased marital stability.

JEL classification: J13, J18, J24
Keywords: Pregnancy Discrimination Act, Maternity Leave, Children's Education

*The authors would like to thank Klaus F. Zimmermann (the Editor) for his guidance and support throughout the process, and three anonymous referees for their helpful comments. Department of Economics, Florida Gulf Coast University. Contact information: Lutgert Hall 3342, 10501 FGCU Blvd. South Fort Myers, FL 33965; Email: krishna.regmi.econ@gmail.com

†Department of Ag and Applied Economics, Virginia Tech. Contact Information: 321-B Hutcheson Hall, 250 Drillfield Drive, Blacksburg, VA 24061; Email: le.wang.econ@gmail.com; authors are listed in alphabetical order.

1 Introduction

In US, children today are nearly twice as likely as those in 1967 to live in a household where both parents work (Fox et al., 2013). Women's labor force participation (LFP) has risen to high levels.[1] One important implication of women's growing participation in the labor market is that more children are born and live with working parents (Waldfogel, Doran, and Pac, 2019). Notwithstanding such drastic changes in the labor market and family structure, there remains a lack of universal childcare and paid family leave programs in the US. By comparison, every other country in the OECD offers national paid maternity leave programs, many of which run into several months after child birth (see Figure 1).[2] The lack of support for working women in the US continues to be heatedly debated. The advocates present the child development case with its support, as early months and years of life play an important role in determining a child's health and shaping his/her cognitive development (Cunha and Heckman, 2007, Todd and Wolpin, 2007, Regmi and Henderson, 2019). The paid leave program can also affect many things that can have a long-lasting impact on one's life, such as health outcomes and family structure. There is, however, limited empirical evidence on the effectiveness of paid family leave programs in improving child outcomes in the long term (see, e.g., Currie and Rossin-Slater (2015) and Regmi and Wang (2022) for more comprehensive reviews of the literature).

Against this backdrop, we examine the effect of the first introduction of paid maternity leave entitlement on children's long-term educational and employment outcomes in the US. Specifically, we exploit exogenous variations in the state provision of paid maternity leave resulted from the Pregnancy Discrimination Act (PDA) of 1978.[3] The Act mandated that employers treat pregnancy and childbirth-related illnesses similarly to short-term disabilities resulting from injuries or accidents. The PDA requires that if employers offer paid leave for short-term disabilities, such as those arising from injuries or accidents, they must extend the same leave benefits to women for pregnancy or other issues related to childbirth. Consequently, five states, California, Hawaii, New Jersey, New York, and Rhode Island that had enacted the Temporary Disability Insurance (TDI) program by state mandate long before the 1978 PDA, were then required to provide around six to eight weeks of paid maternity leave to pregnant women or new mothers. Although their TDI program was initially a policy choice,

[1] According to the Organization for Economic Co-operation and Development (OECD) estimates, the women's LFP rate in the U.S. was 56.68 percent in 2016, compared to the average rate of 51.6 percent among the OECD countries in the same year and about 35 percent in the 1950s.

[2] Source: http://www.oecd.org/gender/data/length-of-maternity-leave-parental-leave-and-paid-father-specific-leave.htm. The available maternity leave policy (including unpaid leave and paid leave at the state level) in the US has been very limited compared to other OECD countries. For example, according to the Department of Labor, as of 2015, only 12 percent employees in the private sector have the opportunity to have paid leave (see https://www.dol.gov/wb/paidleave/PDF/PaidLeave.pdf).

[3] This policy preceded new initiatives at both federal and state levels. First, the Family and Medical Leave Act (FMLA), introduced in 1993, is the only nation-wide policy in the US, with the provision of unpaid leave and being limited to employers with a certain size. The main provision of this policy is to provide employees with unpaid leave of up to 12 weeks over a year for their illness, including pregnancy and childbirth, and for taking care of their child and spouse. In addition, four states–California (since 2004), New Jersey (since 2009), Rhode Island (since 2014), and New York (since 2018)–have introduced new paid family leave programs.

becoming a paid leave program is not, an important feature that will become relevant for our research design below.[4]

Our paper contributes to the literature by being the first study, to the best of our knowledge, on the *long-term* effects of paid maternity leave on children's educational and employment outcomes in the US Prior work has not yet studied these issues presumably because the children exposed to the earliest paid leave program are only now old enough to be completing their education and entering the adult labor market. We use the standard difference-in-differences (DID) approach, exploiting the important exogenous source of identification provided by the PDA for three reasons. First, in the US context, the coverage and benefits of paid leave resulting from the PDA are significant and meaningful, even though they may appear to be small compared to other European countries. The paid leave policy covers around 60 percent of new mothers in the affected states and replaces approximately 60 percent of their pre-childbirth earnings. Second, the paid leave policy we consider here has been in place long enough to shed light on the long-term success of children's educational and labor market. Finally, and most importantly, as mentioned above, the policy variations are exogenous, a crucial assumption needed for the DID approach. A state's treatment status (providing leave benefits to women for pregnancy) arises from a U.S. Supreme Court decision rather than being a response to unobserved economic shocks or other influences correlated with the outcomes of interest, as is the case with many other state-level policies. We provide evidence supporting this feature.

Using data from the American Community Survey (ACS) from 2001 to 2007, we focus on the sample of 26-year-olds from each ACS, who are likely to have completed their education and started their careers. Focusing on the same age-group enables us to further eliminate the possibility of life-cycle effects (that is, the results may be driven by the fact that individuals are at the different stages of their lives). The individuals studied are those born from 1975 to 1981, that is, the cohorts born immediately before and after the Act (similar in spirit to the regression discontinuity DID approach (Dustmann and Schönberg, 2012)). Our analysis also focuses on the prerecession period to avoid the possible heterogeneous responses to the economic downturn associated with the Great Recession of 2008-2009 across cohorts and states that could bias the DID estimates (but our results are also robust to the inclusion of the post-Recession periods).

We reach three main conclusions. First, we find statistically significant effects of maternity leave coverage on long-term educational outcomes, particularly on the completion of college degrees. The introduction of paid leave leads to approximately a 1.1 percentage points increase in the probability that an individual holds a college degree at age 26. The effects on high school graduation and employment status are imprecisely estimated and statistically insignificant.[5] Second, our sub-

[4]For instance, Rhode Island enacted its first state law in 1942, with Hawaii being the last of these five states to do so in 1969.

[5]We also examine the effect on wages as well. However, that result may be a less reliable measure of labor market outcome for a relatively younger age group in our analysis. Further, this specification does not survive some of the tests of the underlying common trend assumption. Therefore, we are cautious with the reporting and interpreting the

group analysis shows that the beneficial effect of the paid maternity leave program on educational attainment can accrue more to black children, a more economically disadvantaged minority, consistent with the fact that paid leave programs, unlike unpaid leave under the FMLA, generally benefits low-income families more than their high-income counterparts.

Third, the literature that typically relies on the regression discontinuity design primarily identifies the immediate one-time impact of paid leave. In contrast, our event study analysis goes beyond examining average effects and such immediate impacts. Our results reveal an important time-varying pattern that has been overlooked in existing research. Consistent with Carneiro, Løken, and Salvanes (2015) in the European context, we observe beneficial effects on college attainment in the period immediately following the introduction of the policy. However, these benefits diminish rapidly with time, indicating their short-lived nature. Amid inconclusive literature, with studies finding null, positive, and sometimes even negative effects of paid leave on children's outcomes, our paper introduces a new dimension to the debate.

We also explore potential mechanisms that could explain our findings. We find that the policy led to an increase in both intra-household specialization and family income and reduced divorce. Furthermore, dynamic mechanism analysis similarly portrays the short-lived nature of the policy, corroborating the findings on college attainment. Some explanations emerge as to why the effects may fade over time. One reason is that over time other unintended consequences of paid leave–such as employer discrimination against expectant mothers or new mothers (Gruber 1994), changes in family dynamics (Avdic and Karimi 2018), and shifts in women's career trajectory (Bailey et al. Forthcoming)–could emerge, thus dampening the initial effects. Second, unintended consequences can lead women not to take full advantage of benefits (Bana, Bedard, and Rossin-Slater 2020). Third, the introduction of the paid leave policy in certain states could raise public awareness and pressure over time, thus leading employers to (in)formally provide and protect some benefits for pregnant and new mothers. This could narrow any policy-induced observed gap in outcomes between the treated and control states.

The rest of the paper proceeds as follows. Section 2 provides a brief literature review, and Section 3 describes our empirical method and data. Section 4 presents the baseline results and validity tests, and Section 5 offers robustness checks. Section 6 presents some preliminary investigations of potential mechanisms. Finally, Section 7 provides concluding remarks.

2 Prior research

Despite an extensive literature on various types of paid leave policies, our understanding of their long-term impact remains limited (see Regmi and Wang 2022 for a more detailed survey of the literature).

results.

By examining the long-term effects, we not only provide the first evidence in the US context but also help clarify and reconcile previous inconclusive findings from outside the US.

The literature is mixed in the effects of paid leave on long-term outcomes of a child. Using an early change in the coverage of maternity leave in Norway, Carneiro, Løken, and Salvanes (2015) show statistically significant effects of the policy on long-term outcomes measured by high school dropout and earnings at age 30, but limited effect on college attendance. The policy they considered came into effect on 1 July, 1977, providing new mothers with 4 months of paid leave and 12 weeks of unpaid leave, up from 12 weeks of unpaid leave. In contrast, leveraging subsequent expansions in Norway coverage that occurred between 1987 and 1992, Dahl et al. (2016) find no evidence of expansions in children's educational outcomes at age 20 (measured by scores in the writing exam at the end of high school and the status of high school dropout). Using three major extensions in a maternity leave program in Germany, Dustmann and Schönberg (2012) also fail to find any statistically significant effect on long-run educational outcomes. Rasmussen (2010), who studies the expansion of parental birth-related leave from 14 to 20 weeks in Denmark, also finds no evidence of the expansion of coverage impacting children's long-term educational outcomes. Likewise, investigating an Austrian parental leave policy that extended leave to second years, Danzer et al. (2022) show that while the reform improved health outcomes for exposed children in adulthood, it did not significantly affect labor market and educational outcomes. Our findings reflect a pattern seen in these studies: an introduction or an initial reform can be more effective than subsequent expansions in improving labor market and educational outcomes in adulthood.

Our paper contributes to the literature on the impacts of paid leave programs on the short-term outcomes of children. Our paper is closely related to Stearns (2015), who examined the effect of the PDA of 1978 on short-term health outcomes and provided a possible channel for the long-term effectiveness of the policy. Our findings indicate that the short-term positive health effects found in Stearns (2015) can persist into adulthood. The effects of paid leave on children's cognitive and non-cognitive achievements are also inconclusive, depending on outcomes and samples analyzed. Using a change in maternity leave in Chile in July 2011 (which allowed 24 weeks of paid leave for mothers, up from 12 weeks), Albagli and Rau (2019) find statistically significant and positive effects on children's cognitive and non-cognitive development. In contrast, Danzer and Lavy (2018), who leverage an unanticipated reform in Austria that increased paid parental leave by 12 months to study the impact on children's cognitive development, find no evidence of any statistically significant effect on child outcomes, except for boys from educated mothers. When examining the expansion of parental leave entitlement from 12 to 15 months in Sweden, Liu and Skans (2010) find statistically insignificant effects on child test scores and grades, measured at 16. Baker and Milligan (2010) show a very limited impact of expansions of maternal leave in Canada on early child development. Using the same policy change, Baker and Milligan (2015) also cannot find any link between the policy and the outcomes of children at ages 4 and 5. For the PPVT outcome scores, they even find a small negative effect.

By carrying out mechanism analyses, our paper connects to the literature on the role of family leave programs in influencing postchildbirth parental employment and fertility. Similar to our findings, Han, Ruhm, and Waldfogel (2009) fail to find any effect on the employment rate among mothers after childbirth during the period from 1987 to 2004. Evidence coming from policies outside the US is similarly inconclusive (e.g., Rege and Solli 2013, Ekberg, Eriksson, and Friebel 2013, Kluve and Tamm 2013, and Ginja, Jans, and Karimi 2018). Regarding fertility, as with other outcomes, the results are mixed. Lalive and Zweimüller (2009) find extension in parental leave policy improved fertility over time, while Farré and González, 2019 like ours show negative effects. Changes in fertility associated with paid leave could be one potential channel that affects children's long-term outcomes by altering resources available to newborns. This subsequent effect on fertility has implications for interpreting the impact of paid leave policies on child development.

In general, previous studies on the effects on long-term educational and employment outcomes of children use countries other than the US, varying institutional settings, data, and methods, thus not surprisingly reaching mixed conclusions. Furthermore, the situation of new mothers in the US is very different from those in other developed countries because of the differences in the generosity of social safety nets and the structure of the labor market. When new mothers have fewer safeguards against the uncertainty of the labor market and fewer options for public assistance programs, paid leave can have greater impacts on their families. As a result, our understanding of the full impact of paid leave remains incomplete and even lacking in the US context. Our paper fills this gap.

3 Empirical method and data

3.1 Empirical strategy

To examine the link between maternity leave and children's educational outcomes and employment at age 26, we leverage the variations on maternity leave across states, induced by the 1978 Pregnancy Discrimination Act. This enables us to employ a standard difference-in-differences (DID) model comparing the outcomes of children who were born in the five states introducing the leave policy to those of the children who were born in the rest of the country before and after the policy. Specifically, we estimate the following model:

$$y_{ist} = \beta_0 + \beta_3 Post78 * Treat + \varsigma_s + \lambda_t + \delta X_i + \epsilon_{ist}, \tag{1}$$

where y represents outcome variables of interest (in our case, high school completion, college degree, and employment status) of an individual i born in state s and in year t. $Post78$ is an indicator variable for the post-treatment period (birth years 1979 to 1981).

Our pre-treatment period covers the years 1975 to 1977. We exclude the year of policy enactment (1978) from our analysis because the policy was initiated earlier in the year and came into effect on October 31, 1978. Similar to Stearns (2015), who uses monthly data and defines the period post-November 1978 as the treatment period, we define 1979 and later as the treatment period, as our data are annual. Most of the children born during 1979 were exposed to the treatment. In the following, we also experiment with the choice of different timings and conduct event study analysis to further differentiate the effects between birth cohorts; our results are robust to these changes and indeed indicate an interesting temporal pattern.

In our analysis, $Treat$ is an indicator variable for the treated states (California, Hawaii, New Jersey, New York and Rhode Island). The control group includes the remaining states of the United States, except Colorado. We drop Colorado as maternity leave was available for public employees. $Post78 * Treat$ is an interaction between indicators for the post-treatment period and for the treated states, and β_3 is the parameter of interest. The inclusion of $Post78$ and $Treat$ is unnecessary since they will be subsumed by the fixed effects of state and time.

Our model further controls for many determinants of the adult outcomes above, be it observable or unobservable. X_i is a vector of individual-specific characteristics such as race, sex, and the age of the mother. ς_s is a vector of state-of-birth fixed effects, which accounts for all unobserved state-specific factors that do not vary over time. For example, some birth states may have other different educational, health, and labor market policies that could affect a child's development. Likewise, λ_t represents birth-year fixed effects, which can also capture both unobserved cohort differences and unobserved factors that affect all states equally but vary over time, examples of which include the federal health insurance policy or other federal fiscal and monetary policies. After controlling for these variables, we assume that β_3 is driven by the variation only related to the introduction of the leave policy.

We use cross-sectional data on individuals, with an individual being the unit of analysis. We use the survey weight throughout our analysis to ensure the representativeness of our sample. Since our specification relies on variation in outcomes within states, the standard errors are clustered at the state-of-birth level to account for any possible correlations among unobserved components within a state.

We also repeat our analysis using the probit models and report the marginal effects of them in Table A1 of the Appendix. As we report later, the estimated marginal effects using the probit models are rather similar with respect to sign, significance, and magnitudes, to our baseline results based on the linear DID model below. As noted in Kitchens, Makofske, and Wang (2019), the linear DID model is generally preferred over nonlinear models containing interaction terms, since the latter is actually based not on the common trend assumption, but on a less transparent nonlinear restriction that is difficult to verify and less plausible in practice. We therefore focus on our DID estimates in

Equation (1) below.

3.2 Data

Our data are from the 2001-2007 American Community Survey (ACS) (see Ruggles et al., 2019 for a description of the data). The main advantage of the ACS data is that they provide information on individuals' birth year and birth state, which are critical for the purpose of our study. We focus on children's outcomes at age 26. Similar to Dustmann and Schönberg (2012), we focus on children who were born immediately before and after the policy change. In particular, we use those born from 1975 to 1981, a period around the introduction of the 1978 Pregnancy Discrimination Act (PDA). These sample restrictions allow us to focus on the prerecession period to avoid the potential confounding effects of the Great Recession. We also exclude individuals born outside the US and in Colorado (as maternity leave was available for public employees). In our robustness checks, we show that our results are robust to inclusion of Colorado and inclusion of the 1978 cohort.

In our analysis, the outcome variables of interest are indicators of high school graduation, college degree attainment, and current employment. We also include racial dummies for Hispanic/Spanish/Latino origin, non-Hispanic white, non-Hispanic black, and non-Hispanic other races, and a gender dummy for being female as controls.

We further merge the data with state-level information on (seasonally adjusted) unemployment rates from the Bureau of Labor Statistics, which are measured in the survey year, that is, when individuals are 26 years old.[6] In our robustness checks, we also include flexible state-specific trends to control for other possible unobservable variables that vary over time.

We provide the summary statistics in Table 1 by treatment status. To assess whether individual characteristics differ systematically by treatment status, we follow Imbens and Wooldridge (2009) and report the normalized difference for each covariate. The normalized difference is calculated as the difference in the means of a covariate by treatment status, divided by the square root of the sum of the means' variances. Following Imbens (2015), a difference below 0.30 generally indicates sufficient balance in the covariate distributions between the treatment and control groups. We calculate these differences separately for the post-treatment and pretreatment periods. As shown in Table 1, the normalized differences for all covariates are well below 0.30, except for Hispanic, which is marginally above this threshold. In general, these differences indicate the covariate balance between the treatment and control groups. Furthermore, the common trend assumption for DID can accommodate any systematic differences in observable characteristics between the treatment and control groups. We will provide strong evidence supporting the assumption of the underlying common trend in the following.

[6] The link is https://www.bls.gov/data/.

3.2.1 Inferring the effects on the uptake of benefits

A direct assessment of the effects of the PDI on the uptake of benefits is not possible for our analysis since, at that time, public surveys, including the Current Population Survey, did not include direct information on whether individuals took maternity and paternity leave until 1994. However, we could obtain information on the possible effects by relying on surveys and historical documents. Using the CPS data (1979-1981), we find that around 60 percent of women with a child less than a year old were working. Citing the legislative history of the Pregnancy Discrimination Act (US Senate 1980), Trzcinski and Alpert (1994) note that around the time of the introduction, approximately 60 percent of employed women were eligible for paid leave. Combining these two pieces of information, we estimate that the take-up rate was around 36 percent (the employment rate of women $\times$ the eligibility rate of employed women).[7]

4 Main results

The underlying identification assumption of our model is that the introduction of the maternity leave policy in the five states is not correlated with the trend in potential outcomes; absent the policy, those born in the treatment states and control states would have similar trajectories for their educational and labor market outcomes.[8] As argued earlier, this assumption is likely to hold because the 1978 Pregnancy Discrimination Act here was enacted through a Federal Court decision. Moreover, to the best of our knowledge, the policy change also did not coincide with any other policies that occurred at approximately the same time and precisely in the five states that we examine here. Our focus on cohorts immediately before and after the PDA also ensures a relatively stable policy and economic environment. In the following, we carry out several analyses to address these concerns, lending strong support to the validity of our method and the credibility of our findings.

4.1 Validity tests of the DID

Prior to discussing our main results, we first assess the validity of our identification strategy employed in Equation (1) by examining pre-existing trends or the common trend assumption in the data. We provide three sets of evidence here, both graphical and statistical, that children's long-term outcomes

[7]Hence, our estimated rate falls within the range of the TDI claim rates for pregnancy in the three states reported in Brusentsev and Vroman (2007). Using historical documents collected from different sources,Brusentsev and Vroman (2007) calculate that the claim rate for TDI due to pregnancy (total claims divided by total live births) was approximately 38 percent in Rhode Island, 27 percent in California, and 21 percent in New Jersey for the period 1985 to 1989, the years closest to our analysis sample period that we could obtain. There was no such calculation available for Hawaii and New York. Likewise, the replacement rate (average weekly benefits/average weekly wages) was 37.5 percent in California, 40.5 percent in New Jersey, 41.7 percent in Rhode Island, 50.2 percent in Hawaii, and 36.4 percent in New York, during that period.

[8]One may be concerned with migration. However, since we are able to observe the birth state and our goal is to examine the effect of policy exposure in the first six to eight weeks of life, migration should not be a concern.

between the treated and control states indeed followed similar paths before the treatment. Although such pretrend similarities are neither necessary nor sufficient to guarantee the validity of the common trend assumptions, they do strengthen the credibility of our estimates (Kahn-Lang and Lang, 2020).

Graphical evidence: First, we provide visual evidence of whether individuals born in the five treatment states and the rest of the county except Colorado (control states) exhibited similar trends in children's long-term outcomes at the age of 26 (high school graduation, college degree obtainment, and employment status). Specifically, we compare the average outcomes between the treated and control states for each birth year. Figure 2 presents the results for educational outcomes, and Figure 3 for employment. These visual analyses provide little reason to believe that treatment and control states are systematically different with the confounding trends that preceded the policy change. We also notice an increase in college degree obtainment among those children exposed to the treatment.

Pre-trend analysis: We now turn to two additional formal pre-trend analyses to assess whether or not there exist any small differences in the educational and employment outcomes during the pretreatment period that cannot be visually detected but are statistically significant. Note that as our objective is to analyze trends in outcomes during the pretreatment period, we limit our analysis to the period from 1975 to 1977. First, we check the prevalence of the continuous trend, using the following model:

$$y_{ist} = \beta_0 + \beta_1 Trend * Treat + \beta_2 Trend + \beta_3 Treat + \lambda_t + \delta X_{ist} + \epsilon_{ist} \tag{2}$$

where y, again, is one of the outcome variables (which could be high school graduation, college completion, and employment status), $Treat$ is whether or not an individual was born in one of the five treated states, and $Trend$ is a linear time trend. The remaining variables are defined as before. If the treated and control states are indeed comparable and the common trend assumption holds, we would expect the coefficient on the interaction between treatment and trend (β_1) to be statistically indistinguishable from zero. Panel A of Table 2 presents these results for differential trends[9]. Consistent with the graphical evidence, we fail to reject the common trend assumption across all three outcomes at any conventional significance levels, regardless of model specifications.

Second, we check the prevalence of discrete (nonlinear) trend, extending the above-mentioned exercise that is better suited if the trends evolve linearly over time. It is possible that states may have nonlinear trends across birth cohorts. Therefore, we next use a fully saturated model in which we interact the treatment variable with every birthcohort dummy variable. Specifically, we estimate the following specification for the period from 1975 to 1977:

[9] We also estimate the model using controls in panel A of Table B1.

$$y_{ist} = \beta_0 + \sum_{j=1975}^{1977} \beta_j Treat * D_j + \varsigma_s + \lambda_t + \delta X_{ist} + \epsilon_{ist}, \tag{3}$$

where D_j is a dummy variable that takes the value of one if an individual is born in year $j \in [1975, 1977]$. The birth year 1977 is used as the base year and we do not include an interaction between treatment and the year 1977. Again, if the assumption of common trend holds, we should expect all the coefficients of the interactions, β_j, to be statistically insignificant. In fact, this is what we find in Table 2 (panel B).[10] With individual and joint tests of the significance of coefficients, we do not find statistically significant effects on any of the outcome variables of interest, thus supporting the assumption of common trend.

Placebo test: We provide a placebo test to examine whether our DID results are an artifact of any preexisting changes prior to the Act. We re-estimate our model (1) using data from the years prior to the Act and including an artificial treatment indicator. Specifically, we assign 1976 as the artificial treatment period, while the artificial pretreatment period includes the year 1975. If the treatment and control states had confounding preexisting trends, we should find statistically significant effects on the coefficient of the interaction. The results are presented in panel C of Table (2).[11] We again fail to find any statistically significant effects in these experiments.

4.2 Baseline results

In this section, we examine how children's exposure to the paid maternity leave program, induced by the 1978 Pregnancy Discrimination Act, impacted children's long-term educational and employment outcomes. Having provided strong evidence supporting the validity of our research design, we report our DID estimates for each of our outcome variables (which include high school graduation, college degree attainment, and employment at age 26) in Table 3.

Panel A reports the results without controls (using only the interaction term, fixed effects of the birth state, and fixed effects of the birth year). As we can see, the maternity leave coverage significantly improves an individual's long-term educational outcome, measured by his/her college degree attainment at age 26. Specifically, our estimates suggest that individuals who were exposed to the policy have approximately 1.6 higher percentage points in the probability of completing a college degree compared to those who were not exposed to the policy. In contrast, we find small, statistically insignificant impacts of the paid leave policy on both children's high school graduation and employment status at age 26.

Our results are robust to the inclusion of controls in models. Panel B presents the results. As we can see, we continue to find a significant impact of the maternity leave coverage on individuals'

[10] We report the results estimated using controls in panel B of B1.

[11] We also report the results estimated using controls in panel C of B1.

likelihood of holding a bachelor's degree at age 26. Our estimates, after controlling for covariates, decrease only slightly. Individuals who were exposed to the policy change have approximately an increase in 1.1 percentage points in the probability of completing a college degree. In contrast, again we cannot find statistically significant effects on the probability of having a high school degree and being currently employed.[12]

It is important to put the results in perspective. Since we do not observe whether mothers actually took paid leave, our estimates should be interpreted as intent-to-treat (ITT) effects. To further translate this ITT effect into the treatment-on-treated (TOT) effect, we apply the 36 percent uptake rate that we estimated above. Hence, our estimates of the impacts on the completion of college degree yield the actual treatment effect of 3.05 percentage points ($\frac{\text{ITT}}{\text{uptake}} = \frac{1.1}{0.36}$). Considering the economic returns to the college degree and its increasing importance, this effect can be particularly substantial.

Are these estimates reasonable? To answer this question, we can consider Carneiro, Løken, and Salvanes (2015) who also find that the introduction of a paid leave program, the 1997 Norwegian reform, had a significant impact on a child's long-term educational outcomes. The authors find that the effect on high school dropout rates is 2.2 percentage points, while the effects on college attendance is 2.7 percentage points (although imprecisely estimated). Our estimates of similar outcomes are close to their estimates in terms of magnitudes. It is important to know that the paid leave policy does more than simply providing 6-8 weeks of maternity leave to take care of a newborn, and replacement incomes during the leave. In fact, it goes beyond the leave period by affecting many things that can have a long-lasting impact on one's life, such as better health outcomes at birth, mother's stable employment and increased labor market attachment (which is associated with steady income stream), marital stability, and smaller family size (less within-household competition for resources, be it financial or parental time, among siblings throughout one's early years).

It is interesting to note that while broadly consistent with those in Carneiro, Løken, and Salvanes (2015), our results differ from theirs in that we find a statistically significant impact on college education and an insignificant impact on high school graduation, whereas they find the opposite. The subtle but important differences between our results likely arise from: (1) differences in institutional and educational settings; and (2) differences in the mechanisms driving the main effects. First, since the high school equivalent graduation rate in the US is already near universal, with on average 90.95 percent in our analytical sample, any policy may have minimal influence on it.[13] This is in stark contrast to Norway. Norway has a highly ranked education system in terms of performance, graduation rates, and funding, but student dropout rates remain an issue, with a completion rate of 59% for

[12]Since we examine only two educational outcomes, the multiple testing issue is not a concern. Adjusting the p-value even using the more conservative Bonferroni corrected approach for two outcomes is inconsequential, and the effect is still significant at the conventional levels.

[13]The US has a higher rate of high school graduation and provides multiple options for obtaining the equivalent certification, such as the General Educational Development (GED) tests.

students enrolled in upper secondary school starting in 2010. Conversely, lower college attendance and completion rates in the US compared to Norway imply a potentially larger scope for a paid leave policy to impact this outcome. Furthermore, since our cohorts are in the early stages of their careers or could still be in school, we might have been unable to uncover any effect on employment.

In the following, we also provide evidence that the mechanisms underlying the long-term effects may differ between Norway and the US due to differences in social safety net generosity and labor market structure. Consistent with Carneiro, Løken, and Salvanes (2015) and Dahl et al. (2016), we find that the policy had little impact on the employment of mothers. However, unlike their results, we find that the policy increased intra-household specialization and family income, and reduced the risk of divorce. The divorce channel may play a key role in explaining these differences, as the negative effects of divorce on the educational attainment of a child are driven primarily by noncognitive skills rather than cognitive skills (Brand et al., 2019). Combined with evidence that non-cognitive skills are more useful for college completion in the US context (Heckman, Humphries, and Kautz (2014); Heckman, Pinto, and Savelyev (2013)), our results suggest that the potential positive impact of paid leave on marital stability and non-cognitive skills lead to a larger impact on college education than on high school completion.

4.3 Heterogeneity and further robustness checks

4.3.1 Heterogeneity by race

We also examine the heterogeneous effects between races. The literature has found that the effects of the paid leave program vary with respect to family characteristics. For example, using the same policy changes as ours, Stearns (2015) finds statistically significant effects on birth weight, but the effects are stronger for children of unmarried mothers and black. Rossin-Slater, Ruhm, and Waldfogel (2013) find that the effects on maternal outcomes are particularly large for the less-advantaged groups. Carneiro, Løken, and Salvanes (2015) find the beneficial effects on long-term educational attainment are primarily concentrated in children born to mothers with low education. As such, we reestimate the effects for white, black, and Hispanic children separately. The results are reported in Table 5 (panel A for white, panel B for black, and panel C for Hispanic). We find that the effect on college degree completion is concentrated among black children, with paid leave increasing a black individual's likelihood of attaining a college degree by around 3.8 percentage points, consistent with the stronger health effects among blacks found in Stearns (2015).

4.3.2 Directly controlling for time-varying state-specific variables and trends

One possible empirical concern is whether or not time-varying unobservable variables across states drive our results. Business cycles differ considerably across states, which, in turn, could affect a younger cohort's employment outcomes disproportionately, and, subsequently, their educational decision. A contraction in the business cycle reduces the opportunity cost of education, which can "push" younger cohorts back to school or keep them in school longer to complete their degrees, while an expansion in the business cycle can lead to the opposite effects. For example, Sakellaris and Spilimbergo (2000) show that adverse business cycle conditions increase college enrollment. Hence, business cycles could be a source of omitted variables that may confound our analysis. To address this concern, we further add state-level unemployment rates as a control in our models. As reported in columns 1-3 of Table 4, the addition of state-level unemployment rates has little effect on our estimates.

One may be concerned about other time-varying state-level policies/variables than just unemployment rates. To further control for other possible time-varying unobservable variables at the state level, we also repeat our analysis by including birth-state-specific trends. The results using linear birth-state specific trends are reported in columns 4-6 of Table 4, and the results using quadratic trends in columns 7-9. Overall, our results are robust to the inclusion of these flexible time trends, and, more importantly, our finding on college degree completion is further strengthened. The robustness of our results is probably unsurprising given that we do not observe a clear trend in Figure (2) that we expect to be extrapolated into the post-treatment period; nor do we find a differing time path between the treated and control states during the pretreatment period.

4.3.3 Alternative control group: robustness to dropping southern states

The southern states were trying to improve their educational policies in the 1970s and 1980s to catch up with the rest of the country, as a result of which there may have been substantial changes in the quality of education in the south (Loeb and Page, 2000). Southern states were also earlier adopters of school accountability policies, which began in the 1980s with the aim of improving student achievement (Figlio and Loeb, 2011). Moreover, the Civil Rights Act in 1964 coupled with desegregation in school districts occurring in the 1970s and 1980s could have disproportionately affected southern states (Guryan, 2004; Jennings, 2012). Therefore, we may be concerned that these states may have different trends than those of the treated states. However, as argued and shown above, this is not necessarily the case in our context because our previous tests of the common trend assumption fail to find evidence to support such a concern, and the inclusion of state-specific trends does not impact our estimates. However, we further assess the robustness of our results by excluding the southern states. Specifically, we reestimate our model using an alternative control group that excludes 16 southern states from our comparison group.[14] The results, which are reported in Table A3, are similar to the baseline results. More importantly, the estimated effect for individuals' exposure to paid family leave on college degree completion is nearly identical to that of the baseline estimate.

[14]We use the classification of the Census Bureau to identify southern states, which are Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

4.3.4 Inclusion of the post-recession period

We have thus far focused on the cohorts immediately born before and after the Act, and that is, the outcomes before the prerecession period. As mentioned above, this eliminates two possible sources of threats to our analysis. First, there could be subsequent reforms or policies in later periods. Such a comparison of later treated vs. early treated in the DID context would also bias the estimates. Second, the Great Recession may have a differential impact on individuals' educational decisions and employment outcomes across cohorts and states; for example, younger cohorts in certain states can be hit harder in the labor market during the Great Recession, which impacts not only the labor market outcome directly, but also incentives to stay in school. It is unclear a priori how these two events may bias our analysis. Nevertheless, we also perform our analysis by extending the sample to later cohorts until the 1993 cohort (using the latest 2019 ACS data available as of writing). The results are reported in Table (A3). We continue to find that the effects on college education are similar to our benchmark results and decline slightly in terms of magnitude, about 1.12 percentage points (without controls) and 0.97 percentage points (with controls); and both estimates are statistically significant at the 1 percent level.

4.3.5 Including Colorado and the 1978 cohort

We reestimate our model, incorporating Colorado into the control group, because the state already had existing maternity leave benefits for public employees before the introduction of the PDA. Therefore, there was no significant expansion of paid leave coverage after 1978. The results remain qualitatively unchanged. As expected, this produces the estimates that are marginally smaller in magnitude compared to baseline (panel A of Table A2).

Furthermore, we include the 1978 cohort, which was previously excluded from the baseline estimation due to their partial treatment. The results, reported in panel B of Table A2, are again qualitatively and quantitatively similar to our baseline estimates. Consistent with our main findings, which indicate that the effects are more pronounced in the period immediately after the introduction of the paid-leave program, the inclusion of the 1978 cohort does not impact our estimates.

5 Dynamic treatment effects

In this section, we estimate the dynamic treatment effect of the paid leave program using event studies and synthetic difference-in-differences (SDID). Our baseline line confirms the finding of Carneiro, Løken, and Salvanes (2015). But our current analysis adds a new perspective to the existing literature that identifies one-off effects by using regression discontinuity designs that compare cohorts born immediately before and after policy implementation. In addition to being useful for testing the common trend assumption, identifying dynamic effects is important to uncover whether the patterns of findings vary over time. This is especially relevant considering that the paid leave program can generate varying unintended consequences for employers and mothers over time, which could change the effectiveness of the program. Our results below indeed are consistent with such a time-varying pattern.

5.1 Event-study effects

We carry out an event study analysis to test the assumption of common trend and investigate the dynamic treatment effects or heterogeneous effects over time. The common trend assumption implies the following result for the pretreatment periods:

$$\begin{aligned}
&E\left(Y_i|Treat_i=1,T_i=t\right)-E\left(Y_i|Treat_i=1,T_i=1977\right)\\
&-(E\left(Y_i|Treat_i=0,T_i=t\right)-E\left(Y_i|Treat_i=0,T_i=1977\right))\\
&=0 \quad \text{for all } t<1978.
\end{aligned}$$

For the post-treatment periods, $t>1978$, it implies the following result:

$$\begin{aligned}
&E\left(Y_i|Treat_i=1,T_i=t\right)-E\left(Y_i|Treat_i=1,T_i=1977\right)\\
&-\left(E\left(Y_i|Treat_i=0,T_i=t\right)-E\left(Y_i|Treat_i=0,T_i=1977\right)\right)\\
&=E\left(Y_i(1)-Y_i(0)|Treat_i=1,T_i=t\right),
\end{aligned}$$

where $Y(d), d=\{0,1\}$ is the potential outcome when the individual receives $Treat=d$. The common trend assumption implies that the DID estimate for the pretreatment data should be statistically insignificant, while the treatment effect on the treated for each posttreatment period is also identified. Together, it suggests the following event-study model.

$$y_{ist}=\beta_0+\sum_{j=1975}^{1981}\beta_j Treat*D_j+\varsigma_s+\lambda_t+\delta X_{ist}+\epsilon_{ist}, \tag{4}$$

In the model, we use interactions between an indicator for the treatment group and dummy variables for the birth years. We omit the birth year 1977, thereby using it as the base period. Figures 4 and 5 plot the estimates with the 95 percent confidence interval. To provide the precise magnitude of the estimates, we also report them in Table A4. None of the effects during the pretreatment period are statistically significant, thereby providing further support for the validity of our empirical strategy. We continue to find strong effects on college degree completion. Specifically, we find the strongest effect on college degree completion for cohorts born in 1979. The estimated beneficial effect is 3.0 percentage points. However, the effect started to fade in later years. The effect fluctuated and decreased to 1.5 percentage point for the cohort born in 1981.[15]

5.2 Synthetic difference in differences

As another robustness check, we employ the synthetic difference-in-differences (SDID) model proposed in Arkhangelsky et al. (2021), which combines features of the traditional DID method and the synthetic control approach (SC) and improves both methods. The traditional synthetic control approach employed in Stearns (2015) relies on having a sufficiently long pretreatment period to construct an accurate synthetic control group. Stearns (2015) benefits from monthly-level data that provide several pretreatment periods. In contrast, our dataset is limited to annual data with only three pre-treatment periods. The asymptotic properties and reliability of the method depend on a larger number of pretreatment observations. SDID addresses this issue. Similar to the SC approach, SDID matches the characteristics of the treated states based on pre-treatment outcomes, thereby relaxing the need for parallel trends. To operationalize SDID in our setting, we collapse the individual-level data to the year-state level. We use the state-level marriage rate, the employment rate, the high school graduation rate, the college graduation rate, and income as covariates, all calculated using the Current Population Survey (CPS) data corresponding to birth years from 1975 to 1985. Since the CPS lacks state-level information for 1975 and 1976, we use data from 1977 for these years.

The SDID procedure first removes the effect of these covariates on outcomes and then calculates optimal weights to find more suitable control groups. We report the estimates in Figures B1 and B2, which include the ATT. The patterns of the results confirm our baseline findings, with a continued statistically significant effect on college graduation and a larger impact among the cohort born in 1979.

5.3 Further discussions of the dynamic effects

Our baseline findings align with those of Carneiro, Løken, and Salvanes (2015), but our analysis of dynamic treatment effects extends their results by demonstrating that the positive impact is more pronounced for the 1979 birth cohorts - immediately following the policy's implementation - than for later years. We emphasize that discovering a temporal pattern in which the effect fades over time is not a limitation but a valuable finding that improves our understanding of policy impacts. This temporal pattern can potentially shed light on why subsequent reforms generally have limited effects and how analyses based on regression discontinuity that identify significant effects may mask important dynamics. It suggests that the initial rollout induces significant behavioral and institutional changes, while later adjustments may yield diminishing returns. Recognizing that the impact fades over time

[15]When we extend our event-study analysis to include the 1978 cohort, a similar pattern emerges (Table A5). Although the effect on college degree attainment appears to be concentrated around the period when the paid leave program was introduced, it persists, albeit at a lower magnitude. The coefficient for 1981 is 0.013 (p-value = 0.13).

highlights the importance of supporting measures to maintain initial benefits and considering how to enhance the longevity of policy effectiveness.

This section builds on the literature to provide a preliminary discussion of factors that could counteract the initially observed beneficial effect. First, unintended workplace consequences can emerge over time, negating the beneficial effects, as employers can discriminate against expectant mothers or new mothers in hiring and wages. Gruber (1994) shows that employers in the US pass on the increased costs of maternity leave policies to women by reducing their wages. This may negatively affect women's long-term employment prospects, as found by Bailey et al. (Forthcoming). Consistent with these findings, our analysis of the mechanism below reveals a lower level of employment for women in the second year following the implementation of the paid leave program. Furthermore, we find that the program significantly increased family income in the first year (1979), which may have contributed to the beneficial effects on college degree attainment.

Second, over time, new or expectant mothers can perceive potential unintended impacts and risks to their long-term career prospects. As a result, they may return to work earlier than they would prefer after childbirth, thus not fully achieving the potential benefits. Bana, Bedard, and Rossin-Slater (2020) find evidence that women eligible for leave may not take full advantage of the maximum amount allowed when leave policies such as the PDA lack job protection and provide only partial wage replacements. This action may lead to a reduced beneficial effect of the paid leave program for mothers in subsequent years.

Third, there is evidence suggesting that employers' reactions appear to be driven by increased costs to employers over time, particularly after the second year. For example, Brenøe et al. (2020) and Ginja, Karim, and Xiao (forthcoming) find that adjustments to employee absences due to maternity leave are costly, as new hires and additional working hours increase total wage bills. It is not surprising that such costs are passed down to employees, impacting their behavior from the second year onward. For example, Bartel et al. (2021) collect a unique data set to study the impact of New York's paid leave policy on how employers manage employee absences. The authors find an increase in employers' rating of their ease of handling workers' absences in the first year of the policy; however, such an ease did not sustain and quickly disappeared by the second year. Bartel et al. (2021) also find evidence supporting a small increase in the share of firms opposing the paid leave law over time.

Lastly, the introduction of a leave program in these five states may have cascading effects in other states over time, increasing public awareness. This increased awareness could pressure employers in nontreated states to formally or informally provide and protect benefits for pregnant and new mothers, thereby narrowing any policy-induced gaps in outcomes observed between treated and control states.

In general, our dynamic effects suggest that the impact of the paid leave program diminishes with time. Our findings open avenues for further investigation into the mechanisms behind fading benefits and how policies can be designed to maintain positive outcomes over longer periods. This underscores the importance of temporal analysis in policy evaluation. To better offer explanations behind our findings, we also conduct event study designs for the mechanisms below.

6 Investigation of possible mechanisms

In this section, we investigate possible mechanisms that can explain our finding of the beneficial impact of the paid leave program on a child's long-term educational outcome. There are many potential candidates, and it is simply impossible to examine all of them. All the analyses here should be considered as preliminary guidance for what can be ruled out or would be more promising avenues going forward when we try to open the black box behind the results found above. We also do not examine the impacts of the PDA on some of the outcomes that have been thoroughly examined in the literature. For example, Stearns (2015) provided strong evidence supporting the positive effects of the same paid leave program on health at birth. Our positive findings align with Stearns (2015), suggesting the possibility of positive health effects persisting into adulthood and health at birth as a potential channel.

Here, we focus on the impacts of the PDA on two sets of alternative channels: three family labor market outcomes after childbirth (maternal employment, spousal employment, and family income), and two demographic variables such as fertility and divorce decisions in the short run. The recent literature has shown the importance of early months of life in shaping children's health, cognitive, and noncognitive development (Cunha and Heckman 2007, Todd and Wolpin 2007, 2003, Regmi and Henderson 2019). The variables considered here have implications for both the time and the resources available to newborns in these early periods. More importantly, they could also have long-lasting impacts on women's increased attachment to the labor market, family income, resources available, and family structure that could shape one's environment even later stages of her life and hence her long-term outcomes.

For this purpose, we turn to the March Current Population Survey (CPS) data from 1977 to 1982, trying to best match the CPS data's timeframe to that of the birth years of the cohorts in our main analysis above.[16] Prior to 1977, 30 states cannot be separately identified in the CPS data. Income is measured from the past calendar year. Being up to one year of age in March increases the likelihood of being born in the previous year. Therefore, the March interview cohort better reflects and represents the parents of the previous year's birth cohort, promoting us to treat it as such in our notation. For instance, the 1977 March CPS data are referred to as the 1976 cohort (specifically, the representative parents of that cohort), and so on. Similar to our main analysis, we exclude the 1978 cohort.[17] Our analytical sample consists of the 1976 and 1977 cohorts for the pretreatment period,

[16]We extract data from the IPUMS Flood et al. (2018).

[17]We also exclude individuals in the armed forces, limiting our sample to the civilian population.

and the 1979, 1980, and 1981 as the post-treatment cohorts.

We restrict the sample to parents aged 18 to 45 years, the type who are more likely to be new mothers. In analyzing the labor market outcomes, we further limit the analysis to parents who have a child under one year of age. We estimate the following model, parallel to our baseline specification:

$$y_{ist} = \beta_0 + \beta_1 Post78 * Treat + \varsigma_s + \lambda_t + \delta X_i + \epsilon_{ist}, \tag{5}$$

where y_{ist} is an outcome variable for individual i in state s and year t. X_i includes age, age-squared, education, marital status, and race. Other variables are defined as before.

6.1 Labor market outcomes: maternal employment, husband employment, and family income

First, we examine the effect on maternal employment. With the theoretical framework proposed in Klerman and Leibowitz (1995) (also Han, Ruhm, and Waldfogel, 2009; Baum and Ruhm, 2016), we note that the short-term effects on maternal work and employment are, in theory, ambiguous. We present the results in column 1 of Table (6). Our results show that the effect on a mother's employment is not statistically different from zero. To analyze the dynamic effects, we operationalize an event study analysis parallel to Equation 4. Panel B of Figure 6 presents the estimates.[18]

Second, we examine the effect on father employment. Paid leave can also affect the supply of spousal labor. As noted in Ginjia et al. (2022), the reduced opportunity cost of home time may impact intra-household division of labor by leading to a higher extent of specialization within the household and a higher likelihood of husband employment. There can also be insurance effects: husbands may increase their labor supply to offset leave-taking by wives and compensate for loss of family income. We present the results in column 2 of Table (6). Our results show that the paid leave policy increased the likelihood that husbands were employed. As presented in panel B of Figure 6, our event study analysis shows a spike in fathers' employment for the 1979 birth cohorts. However, the effect fades away. This finding aligns with our main effect on college degree attainment, which is more pronounced for the 1979 birth cohorts.

Finally, we examine the impacts on family income. Availability of the paid leave policy is indeed to provide wage replacement and could increase family income for those who took up the leave. As alluded above, increased husband employment can also lead to a higher level of family income. Also, having some time off for a wife during and after a child's birth may facilitate her husband who is already employed to specialize in their work (including their business), which in turn lead to a higher level of productivity and family income. The results using the log of family income are presented in column 3 of Table (6). We find that the paid leave program indeed has a positive

[18]Table A6 reports the same results for clearer illustration of the magnitudes.

effect on family income. Again, the event study analysis effects mirror the pattern found for college degree attainment.

6.2 Demographic uutcomes (I): divorce and marital instability

We next examine the effect of paid leave on divorce risk. Divorce has been linked to a range of negative outcomes for children, including lower academic achievement (McLanahan, Tach, and Schneider, 2013) and reduced noncognitive and psychosocial skills such as emotional and behavioral skills (Brand et al., 2019).

The paid leave policy can also have a complex impact on marital stability and the risk of divorce. On the one hand, paid leave may have a positive impact on marital stability by providing economic security and stability to families. As discussed above, paid leave can increase family income by both allowing a mother to maintain employment stability and (partial) income, and her spouse to increase labor supply after childbirth. On the other hand, paid leave may also have a negative impact on marital stability by increasing role conflict and stress within the family. The role conflict model in psychology and sociology suggests that the arrival of a new child can lead to a reorganization of social roles along more traditional lines, with the mother often taking on a greater share of childcare responsibilities (Twenge, Campbell, and Foster (2003); Olafsson and Steingrimsdottir (2020)). This reorganization of roles may lead to increased stress and conflict within the family, particularly for mothers who do not prefer traditional roles or who wish to maintain their careers. This increased stress and conflict may in turn increase the risk of divorce, especially among couples where the mother has higher or equal educational attainment to the father and prefers less traditional roles (Regmi and Wang, 2022). Furthermore, if employers respond to the increased costs of the paid leave program by reducing women's wages and hiring—as shown by Gruber (1994)—this could decrease women's bargaining power at home and the extent of related role conflicts, potentially reducing divorce rates.

In Table (6), column (4) presents the results on the effect of paid leave on the risk of divorce. The results indicate that paid leave significantly reduces the risk of divorce by approximately 1 percentage points. As reported in panel A of Figure 8, the analysis of the event study shows that the effect on divorce is more substantial in magnitude for the 1979 cohort, aligning with our findings on college degree attainment.[19]

6.3 Demographic outcomes (II): fertility

The paid leave policy can affect child outcomes through fertility; for example, family size could affect the availability and allocation of family resources to children, subsequently affecting a child's development (the well-known quantity-quality trade-off proposed by Gary Becker). To the extent

[19]Table A6 reports the same results for clearer illustration of the magnitudes.

that a paid leave policy affects women's labor market positions and the wage penalty associated with child birth, it can also motivate families to reconsider their fertility decisions.

However, the effects of the paid leave policy on fertility are theoretically unclear and empirical evidence is relatively limited and inconclusive. On the one hand, availability of paid leave and higher benefits lowers the opportunity cost of time after childbirth, which can lead to a higher level of fertility. On the other hand, the paid leave program can also keep women staying employed after childbirth and increase their long-term labor market attachment, which in turn raises the opportunity cost of an additional child and reduces women's desire for a large family (Farré and González, 2019).

To shed light on this issue, we create a proxy variable for giving birth by identifying women with a child less than one year old at home. The results are reported in column (4) of Table (6). We find a small but statistically significant effects on fertility. The event-study analysis shows that the decline is more evident in 1979 than in the second year (panel B of Figure 8).[20]

6.4 Analysis by race

Considering that we observe more pronounced effects on black individuals, we further explore potential mechanisms by race. We use the same set of outcomes analyzed earlier. Splitting sample by race drastically reduces the sample size, which precludes an event study analysis, we thus focus on the main effect for each race, and Table B2 reports the results. We find that the effect of the paid leave on family income is much larger on black individuals, potentially suggesting that family income explains the main effect. Since black families tend to face greater income constraints, the marginal value of additional income on children's education is likely higher, which amplifies the effect on college graduation that we observed. The suggestive evidence that we uncover that higher income has a greater impact on black people is consistent with Bastian and Michelmore (2018), who show that the impact of the Earned Income Tax Credit is greater for black children.

7 Conclusion

In examining the relationship between the maternity leave policy and long-term outcomes, to the best of our knowledge, we provide the first US based evidence of newborns' educational and employment outcomes in adulthood. This is particularly important since previous studies examining parental leave policies on children's educational attainment and labor market outcomes in other developed countries have produced mixed results (see Danzer and Lavy, 2018; Dustmann and Schönberg, 2012; Dahl et al., 2016). Further, it is hard to extrapolate these studies' findings to understand the US paid leave polices because of differences in their generosity, labor market structures, and other socio-economic and cultural factors.

[20] Table A6 reports the same results for a clearer illustration of the magnitudes.

In our analysis, we document and isolate the causal effects of maternity leave coverage, induced by the Pregnancy Discrimination Act of 1978 in the five states (California, Hawaii, New Jersey, New York, and Rhode Island), on children's long-term educational attainment and employment status at age 26. We find a significant and positive effect of the maternity leave entitlement on the long-term educational outcome of children measured by the completion of the college degree at age 26. We also find positive effects on high school graduation and being employed at age 26, but they are imprecisely estimated. Our results are robust to different specifications and to further robustness checks.

Our findings shed a new light on the literature that is fairly inconclusive. Our results are similar to those of Carneiro, Løken, and Salvanes (2015), but differ from those of Dahl et al. (2016). Both Carneiro, Løken, and Salvanes (2015) and our paper find a significant effect of paid leave on children's long-term outcomes, but Dahl et al. (2016) fail to find any effects. Carneiro, Løken, and Salvanes (2015) and Dahl et al. (2016) both use the data from Norway but exploit the reforms at different time periods. Investigating the potential reasons for such a difference is challenging even for these two studies using the same datasets, but together with our results presented here, it seems to suggest that policies, which are initial or substantially expand already existing policies whose benefit levels are low, play a more significant role in determining children's future outcomes than those subsequent reforms. Our findings may be stemmed from the fact that we evaluate a policy that was the first of its kind in the US.

Our results also have important policy implications as countries attempt to strengthen a case for paid leave policies. This is especially important in the US where a growing interest among state and federal policymakers in enacting a national paid leave program has emerged. Our findings, taken together with the existing literature, raise the need for future research to advance our understanding of what the optimal level of paid leave should be, thus providing better guidance for policymakers in enacting an effective paid leave program.

It is important to note that our findings on college attainment are most evident in the 1979 cohort. Although we provide several explanations as to why such patterns of dynamic effects may exist, further careful research of what actually counteract potentially beneficial effects can be particularly useful.

To better understand mechanisms through which paid leave affected a child's college graduation, we investigate effects on parents' employment, family income, divorce, and fertility. We find some evidence that the policy positively affects family income and decreases divorce. However, this is by no means a comprehensive analysis of possible channels. A thorough future investigation of various mediating factors (including the quality and quantity of time parents spend with their newborns) is warranted.

Declarations

Competing Interests The authors have no relevant financial or non-financial interests to disclose.

Data Availability The data that support the findings of this study are available from the corresponding author on request.

References

Albagli, P., and T. Rau. 2019. "The Effects of a Maternity Leave Reform on Children's Abilities and Maternal Outcomes in Chile." The Economic Journal 619:1015?1047.

Arkhangelsky, D., S. Athey, D.A. Hirshberg, G.W. Imbens, and S. Wager. 2021. "Synthetic difference-in-differences." American Economic Review 111:4088–4118.

Avdic, D., and A. Karimi. 2018. "Modern family? Paternity leave and marital stability." American Economic Journal: Applied Economics 10:283–307.

Bailey, M., T. Byker, E. Patel, and S. Ramnath. Forthcoming. "The Long-Run Effects of California's Paid Family Leave Act on Women's Careers and Childbearing: New Evidence from a Regression Discontinuity Design and U.S. Tax Data." American Economic Journal: Economic Policy, pp. .

Baker, M., and K. Milligan. 2010. "Evidence from Maternity Leave Expansions of the Impact of Maternal Care on Early Child Development." The Journal of Human Resources 45:1–32.

—. 2015. "Maternity Leave and Children's Cognitive and Behavioral Development." Journal of Population Economics 28:373–391.

Bana, S.H., K. Bedard, and M. Rossin-Slater. 2020. "The impacts of paid family leave benefits: regression kink evidence from California administrative data." Journal of Policy Analysis and Management 39:888–929.

Bartel, A.P., M. Rossin-Slater, C.J. Ruhm, M. Slopen, and J. Waldfogel. 2021. "The Impact of Paid Family Leave on Employers: Evidence from New York." Working paper, National Bureau of Economic Research.

Bastian, J., and K. Michelmore. 2018. "The Long-Term Impact of the Earned Income Tax Credit on Children's Education and Employment Outcomes." Journal of Labor Economics 36:1127–1163.

Baum, C.L., and C.J. Ruhm. 2016. "The Effects of Paid Family Leave in California on Labor Market Outcomes." Journal of Policy Analysis and Management 35:333–356.

Brand, J.E., R. Moore, X. Song, and Y. Xie. 2019. "Why does parental divorce lower children's educational attainment? A causal mediation analysis." Sociological science 6:264–292.

Brenøe, A.A., S.P. Canaan, N.A. Harmon, and H.N. Royer. 2020. "Is Parental Leave Costly for Firms and Coworkers?" Working Paper No. 26622, National Bureau of Economic Research.

Brusentsev, V., and W. Vroman. 2007. "Compensating for Birth and Adoption." Working paper, The Urban Institute.

Carneiro, P., K.V. Løken, and K.G. Salvanes. 2015. "A Flying Start? Maternity Leave Benefits and Long-Run Outcomes of Children." Journal of Political Economy 123:365–412.

Cunha, F., and J. Heckman. 2007. "The Technology of Skill Formation." American Economic Review 97.

Currie, J., and M. Rossin-Slater. 2015. "Early-life origins of life-cycle well-being: Research and policy implications." Journal of policy Analysis and management 34:208–242.

Dahl, G.B., K.V. Løken, M. Mogstad, and K.V. Salvanes. 2016. "What Is the Case for Paid Maternity Leave?" The Review of Economics and Statistics 98:655–670.

Danzer, N., M. Halla, N. Schneeweis, and M. Zweimüller. 2022. "Parental Leave, (In)formal Childcare, and Long-Term Child Outcomes." Journal of Human Resources 57:1826–1884.

Danzer, N., and V. Lavy. 2018. "Paid Parental Leave and Children's Schooling Outcomes." The Economic Journal 128:81–117.

Dustmann, C., and U. Schönberg. 2012. "Expansions in Maternity Leave Coverage and Children's Long-Term Outcomes." American Economic Journal: Applied Economics 4:190–224.

Ekberg, J., R. Eriksson, and G. Friebel. 2013. "Parental leave — A Policy Evaluation of the Swedish "Daddy-Month" reform." Journal of Public Economics 97:131 – 143.

Farré, L., and L. González. 2019. "Does Paternity Leave Reduce Fertility?" Journal of Public Economics 172:52–66.

Figlio, D., and S. Loeb. 2011. "Chapter 8 - School Accountability." In E. A. Hanushek, S. Machin, and L. Woessmann, eds. Handbook of the Economics of Education. Elsevier, vol. 3, pp. 383 – 421.

Flood, S., M. King, R. Rodgers, S. Ruggles, and J.R. Warren. 2018. "Integrated Public Use Microdata Series, Current Population Survey: Version 6.0 [dataset]." Working paper, Minneapolis, MN: IPUMS. https://doi.org/10.18128/D030.V6.0.

Fox, L., W.J. Han, C. Ruhm, and J. Waldfogel. 2013. "Time for children: Trends in the Employment Patterns of Parents, 1967–2009." Demography 50:25–49.

Ginja, R., J. Jans, and A. Karimi. 2018. "Parental Leave Benefits, Household Labor Supply and Children's Long-run Outcomes." Journal of Labor Economics Forthcoming.

Ginja, R., A. Karim, and P. Xiao. forthcoming. "Employer Responses to Family Leave Programs." American Economic Journal: Applied Economics, pp. .

Gruber, J. 1994. "The Incidence of Mandated Maternity Benefits." The American Economic Review 84:622–641.

Guryan, J. 2004. "Desegregation and Black Dropout Rates." American Economic Review 94:919–943.

Han, W.J., C. Ruhm, and J. Waldfogel. 2009. "Parental Leave Policies and Parents' Employment and Leave-Taking." Journal of Policy Analysis and Management 28:29–54.

Heckman, J., R. Pinto, and P. Savelyev. 2013. "Understanding the mechanisms through which an influential early childhood program boosted adult outcomes." American Economic Review 103:2052–86.

Heckman, J.J., J.E. Humphries, and T. Kautz. 2014. The myth of achievement tests: The GED and the role of character in American life. University of Chicago Press.

Imbens, G.W. 2015. "Matching methods in practice: Three examples." Journal of Human Resources 50:373–419.

Imbens, G.W., and J.M. Wooldridge. 2009. "Recent Developments in the Econometrics of Program Evaluation." Journal of Economic Literature 47:5–86.

Jennings, J. 2012. "Reflections on a Half-Century of School Reform: Why Have We Fallen Short and Where Do We Go from Here?" Working paper, Washington, DC: Center on Education Policy.

Kahn-Lang, A., and K. Lang. 2020. "The promise and pitfalls of differences-in-differences: Reflections on 16 and pregnant and other applications." Journal of Business & Economic Statistics 38:613–620.

Kitchens, C., M.P. Makofske, and L. Wang. 2019. ""Crime" on the Field." Southern Economic Journal 85:821–864.

Klerman, J., and A. Leibowitz. 1995. "Labor Supply Effects of State Maternity Leave Legislation." Working paper, RAND - Labor and Population Program.

Kluve, J., and M. Tamm. 2013. "Parental Leave Regulations, Mothers' Labor Force Attachment and Fathers' Childcare Involvement: Evidence from a Natural Experiment." Journal of Population Economics 26:983–1005.

Lalive, R., and J. Zweimüller. 2009. "How Does Parental Leave Affect Fertility and Return to Work? Evidence from Two Natural Experiments." The Quarterly Journal of Economics 124:1363–1402.

Liu, Q., and O. Skans. 2010. "The Duration of Paid Parental Leave and Children's Scholastic Performance." The B.E. Journal of Economic Analysis & Policy 10:1–35.

Loeb, S., and M.E. Page. 2000. "Examining the Link between Teacher Wages and Student Outcomes: The Importance of Alternative Labor Market Opportunities and Non-pecuniary Variation." Review of Economics and Statistics 82:393–408.

McLanahan, S., L. Tach, and D. Schneider. 2013. "The causal effects of father absence." Annual review of sociology 39:399.

Olafsson, A., and H. Steingrimsdottir. 2020. "How does daddy at home affect marital stability?" The Economic Journal 130:1471–1500.

Rasmussen, A.W. 2010. "Increasing the Length of Parents' Birth-Related Leave: The Effect on Children's Long-Term Educational Outcomes." Labour Economics 17:91–100.

Rege, M., and I.F. Solli. 2013. "The Impact of Paternity Leave on Fathers' Future Earnings." Demography 50:2255–2277.

Regmi, K., and D.J. Henderson. 2019. "Labor Demand Shocks at Birth and Cognitive Achievement during Childhood." Economics of Education Review 73:101917.

Regmi, K., and L. Wang. 2022. "Maternity Leave." In K. F. Zimmermann, ed. Handbook of Labor, Human Resources and Population Economics. Cham: Springer International Publishing.

Rossin-Slater, M., C.J. Ruhm, and J. 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 32:224–245.

Ruggles, S., S. Flood, R. Goeken, J. Grover, E. Meyer, J. Pacas, and M. Sobek. 2019. "IPUMS USA: Version 9.0 [dataset]." Working paper, Minneapolis, MN: IPUMS. https://doi.org/10.18128/D010.V9.0.

Sakellaris, P., and A. Spilimbergo. 2000. "Business Cycles and Investment in Human Capital: International Evidence on Higher Education." In Carnegie-Rochester Conference Series on Public Policy. Elsevier, vol. 52, pp. 221–256.

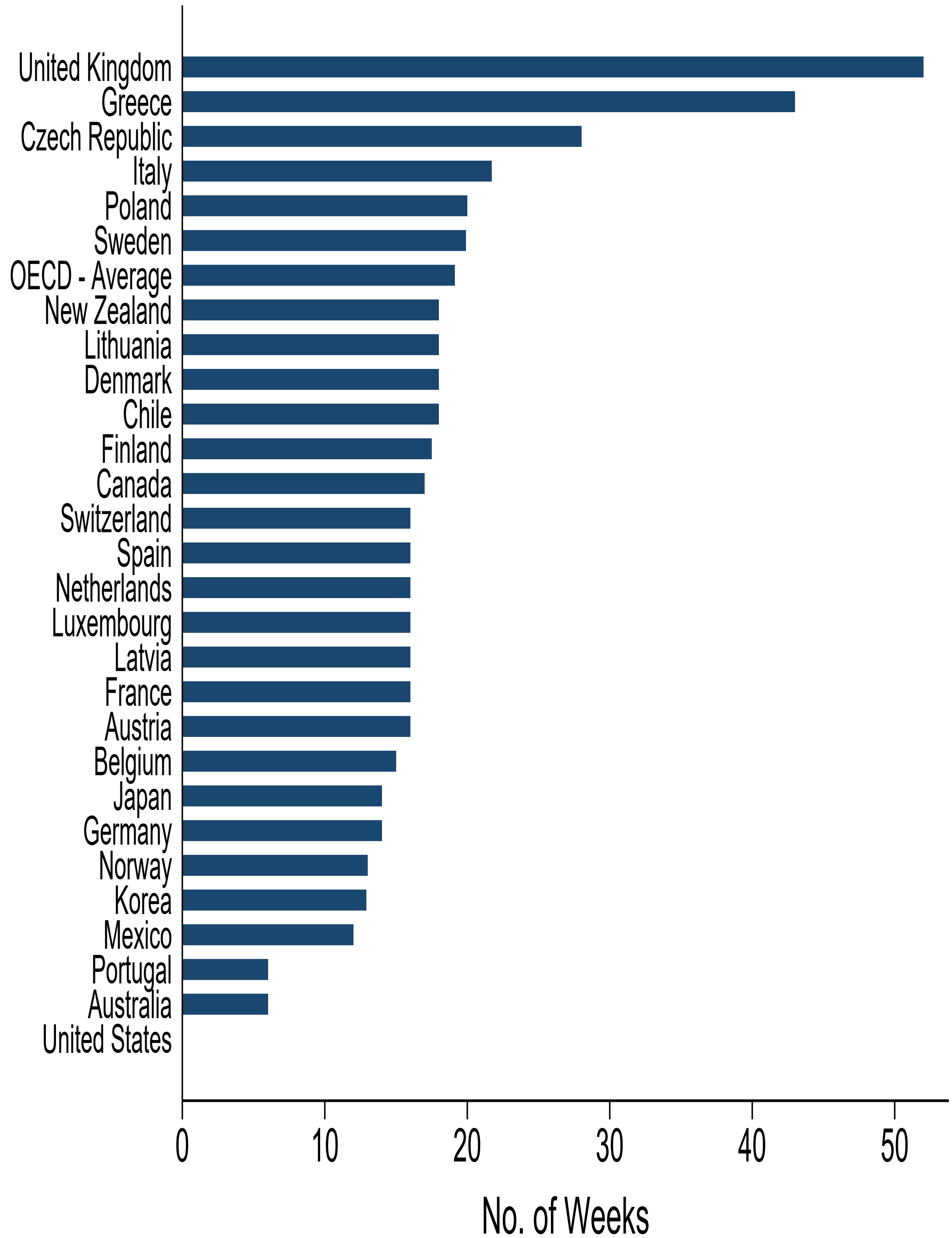
Todd, P.E., and K.I. Wolpin. 2003. "On the Specification and Estimation of the Production Function for Cognitive Achievement." The Economic Journal 113.

—. 2007. "The Production of Cognitive Achievement in Children: Home, School, and Racial Test Score Gaps." Journal of Human capital 1:91–136.

Twenge, J.M., W.K. Campbell, and C.A. Foster. 2003. "Parenthood and marital satisfaction: a meta-analytic review." Journal of marriage and family 65:574–583.

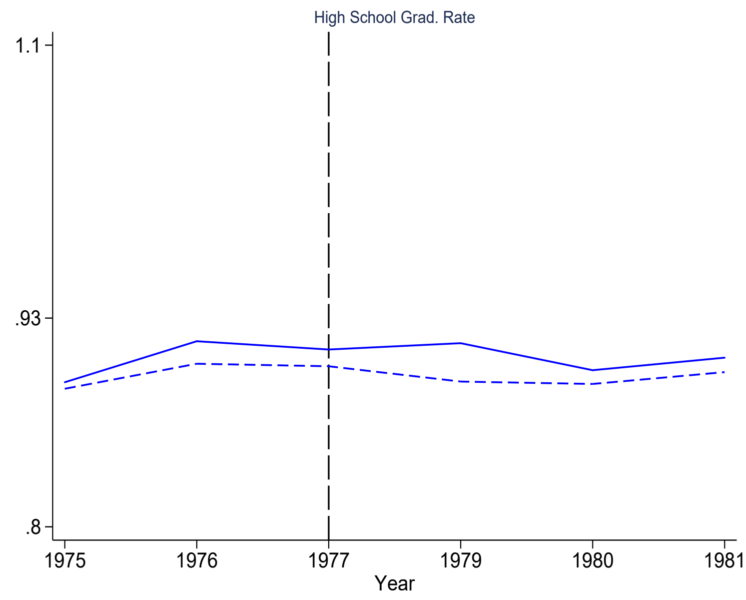
Waldfogel, J., E. Doran, and J. Pac. 2019. "Paid Family and Medical Leave Improves the Well-Being of Children and Families." Child Evidence Brief No. 5, Society for Research in Child Development, 1825 K Street, NW, Suite 325, Washington, DC 20006, July.

Figure 1: Paid maternity leave in OECD countries

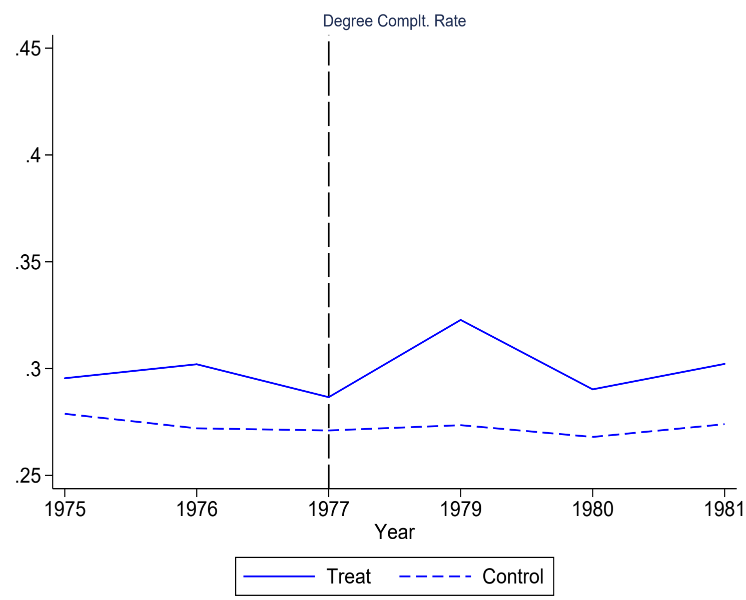


Notes: We plot the availability of national paid leave available to (expectant) mothers among select OECD countries in 2016. The total length of leave includes paid maternity leave and paid parental and paid home care leave that are available to mothers before and after child birth. We extract data from the OECD website: http://www.oecd.org/gender/data/length-of-maternity-leave-parental-leave-and-paid-father-specific-leave.htm.

Figure 2: Average educational outcomes by birth year and treatment status



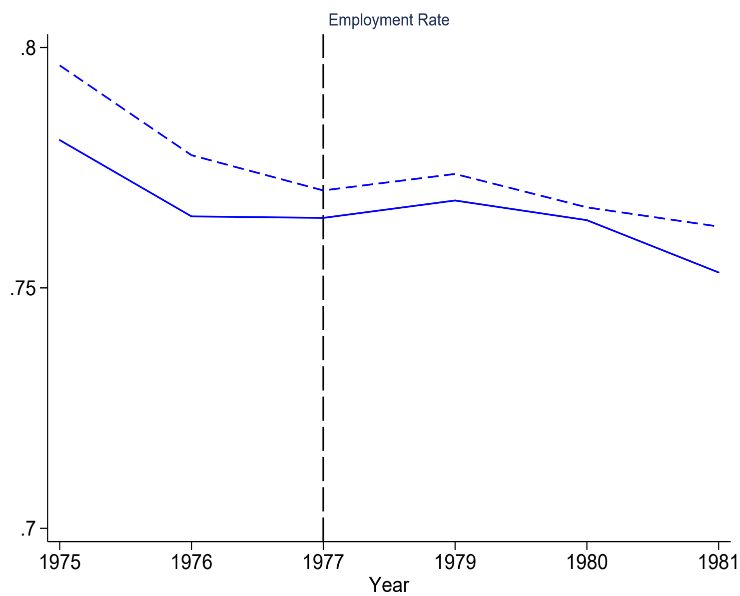
(a)



(b)

Notes: Figure (a) plots the average high school graduation rate by birth-year and treatment status. Figure (b) plots the average college degree completion rate by birth-year and treatment status. The solid line represents the treatment group (individuals who were born in Hawaii, California, Rhode Island, New Jersey and New York). The dashed line represent the control group (individuals who were born in the remaining states, except Colorado).

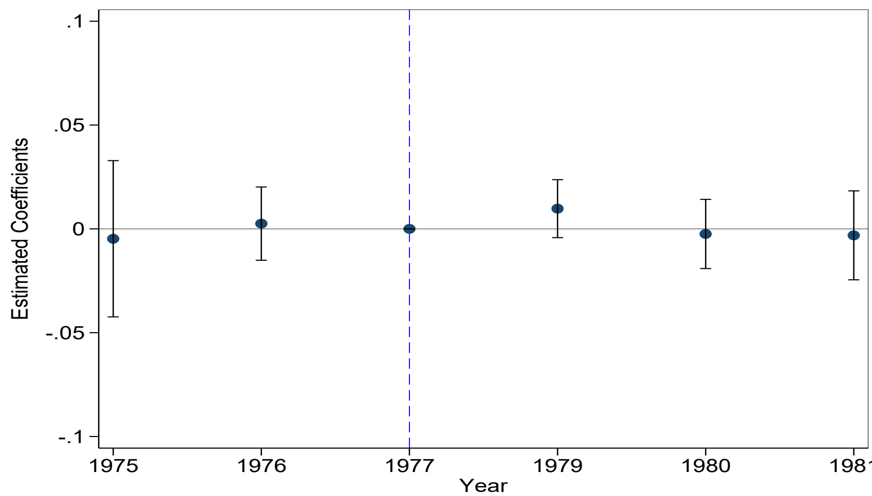
Figure 3: Average employment by birth year and treatment status



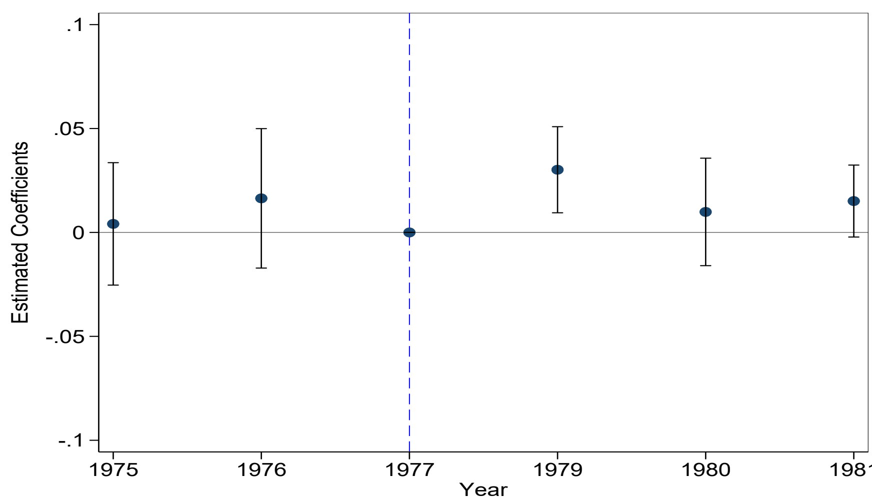
Notes: We plot the average employment rate by birth-year and treatment status. The solid line represents the treatment group (individuals who were born in Hawaii, California, Rhode Island, New Jersey and New York). The dashed line represent the control group (individuals who were born in the remaining states, except Colorado).

Figure 4: Event study estimates

Panel A: High school

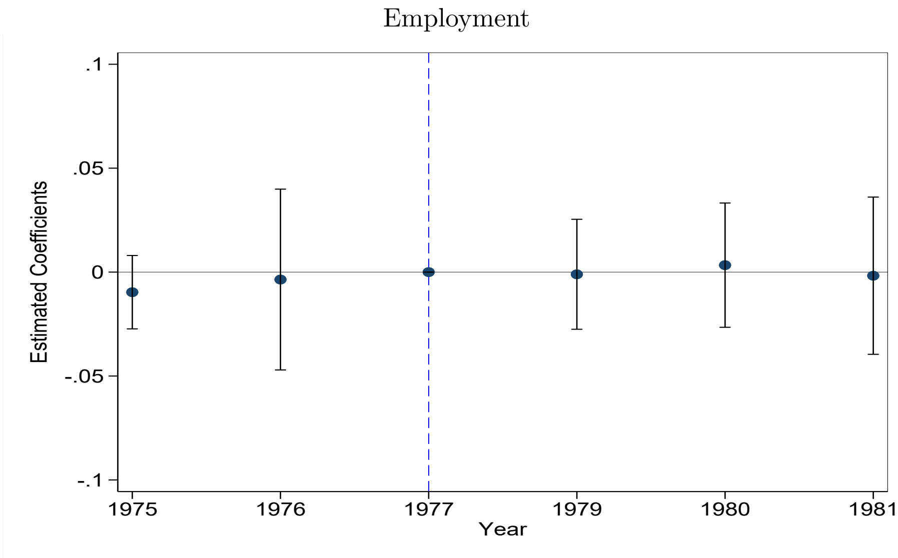


Panel B: College degree



Notes: This figure plots the event study estimates with the 95-percent confidence intervals. All estimates are relative to 1977. As explained in the text, our analytical sample does not include 1978 birth cohorts. Panel A presents the estimates on high school graduation and panel B for college degree attainment. Standard errors are clustered at the state-of-birth level.

Figure 5: Event study estimates

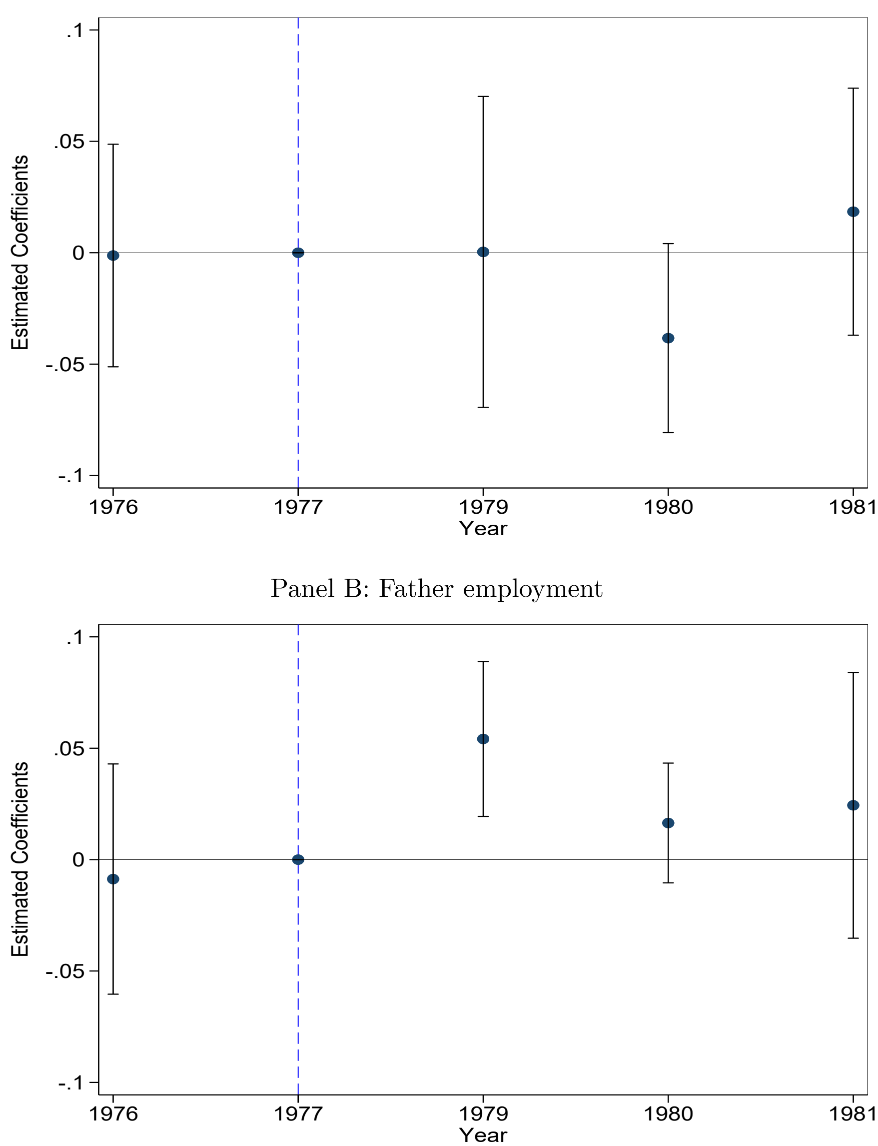


Notes: This figure plots the event study estimates with the 95-percent confidence intervals. All estimates are relative to 1977. As explained in the text, our analytical sample does not include 1978 birth cohorts. Standard errors are clustered at the state-of-birth level.

Figure 6: Event study estimates for mechanism: employment

Panel A: Mother employment

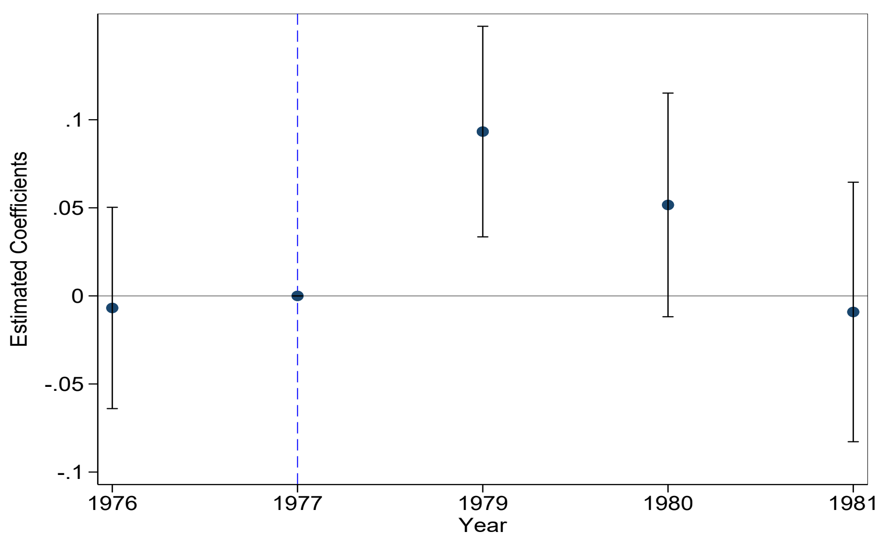
Panel B: Father employment



Notes: This figure plots the event study estimates, with the 95-percent confidence intervals, based on the Current Population Survey (CPS) data. All estimates are relative to 1977. As explained in the text, our analytical sample excludes the 1978 birth cohort (specifically, the representative parents of that cohort) due to the partial implementation of paid leave. Additionally, we exclude the sample related to the 1975 birth cohort (specifically, the representative parents of that cohort) from the analysis as state identification is unavailable in the CPS data. Standard errors are clustered at the state-of-birth level.

Figure 7: Event study estimates for mechanism: family income

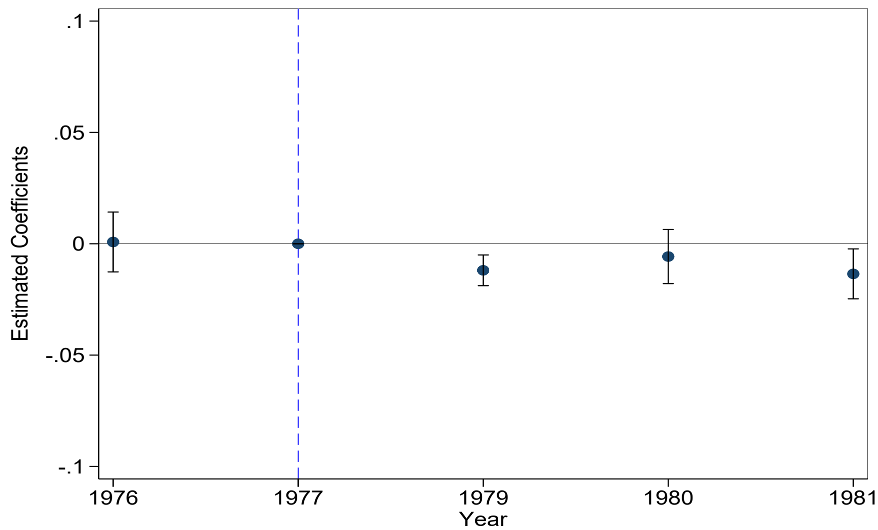
Panel A: Family income



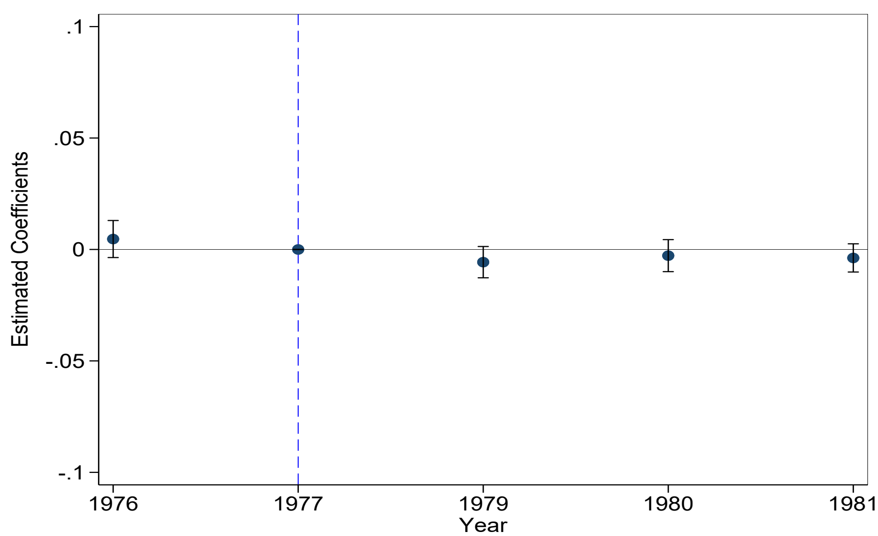
Notes: This figure plots the event study estimates, with the 95-percent confidence intervals, based on the Current Population Survey (CPS) data. All estimates are relative to 1977. As explained in the text, our analytical sample excludes the 1978 birth cohort (specifically, the representative parents of that cohort) due to the partial implementation of paid leave. Additionally, we exclude the sample related to the 1975 birth cohort (specifically, the representative parents of that cohort) from the analysis as state identification is unavailable in the CPS data. Standard errors are clustered at the state level.

Figure 8: Event study estimates for mechanism: divorce and fertility

Panel A: Fertility



Panel B: Divorce



Notes: This figure plots the event study estimates, with the 95-percent confidence intervals, based on the Current Population Survey (CPS) data. All estimates are relative to 1977. As explained in the text, our analytical sample excludes the 1978 birth cohort (specifically, the representative parents of that cohort) due to the partial implementation of paid leave. Additionally, we exclude the sample related to the 1975 birth cohort (specifically, the representative parents of that cohort) from the analysis as state identification is unavailable in the CPS data. Standard errors are clustered at the state level.

Table 1: Summary statistics

	Treatment		Control		
	Mean	Sd	Mean	Sd	Normalized difference
Panel A: Pre-treatment period					
High school	0.905	0.293	0.896	0.305	0.022
College degree	0.295	0.456	0.274	0.446	0.032
Employment	0.770	0.421	0.781	0.413	-0.019
Female	0.503	0.500	0.515	0.500	-0.016
Hispanic	0.241	0.428	0.065	0.247	0.356
White	0.575	0.494	0.762	0.426	-0.287
Black	0.115	0.319	0.142	0.349	-0.056
Other	0.069	0.254	0.031	0.173	0.124
No. of observations	6460		25,442		
Panel B: Post-treatment Period					
High School	0.905	0.293	0.892	0.310	0.031
College degree	0.304	0.460	0.272	0.445	0.051
Employment	0.761	0.426	0.768	0.422	-0.010
Female	0.506	0.500	0.509	0.500	-0.004
Hispanic	0.239	0.426	0.073	0.261	0.331
White	0.568	0.495	0.742	0.437	-0.263
Black	0.110	0.313	0.151	0.358	-0.085
Other	0.082	0.275	0.034	0.180	0.148
No. of observations	17,261		64,188		

Notes: We present summary statistics using the 2001-2007 American Community Survey data. Panel A presents the statistics for those who were born during the pre-treatment period (1975-1977) and panel B for those who were born during the post-treatment period (1979-1981).

Table 2: Pre-treatment trend analysis

	High school	College degree	Employed
	(1)	(2)	(3)
Panel A: Continuous trend			
Trend*Treatment	0.003	-0.001	0.005
	(0.009)	(0.007)	(0.005)
Trend	0.007**	-0.003	-0.013***
	(0.003)	(0.004)	(0.003)
Panel B: Discrete trend			
Year 1975*Treatment	-0.006	0.002	-0.010
	(0.019)	(0.015)	(0.010)
Year 1976*Treatment	0.004	0.017	-0.007
	(0.009)	(0.016)	(0.022)
Joint test of significance	0.192	0.742	0.488
	(0.826)	(0.482)	(0.617)
Panel C: Placebo test with an artificial treatment year			
Post 1976*Treatment	0.008	0.006	0.006
	(0.019)	(0.012)	(0.011)
Controls	No	No	No
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
No. of observations		31,902	

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Panel A does not include individual controls. We limit the analysis for the pre-treatment period (from 1975 to 1977). Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: Effects on educational and labor market outcomes

	High school	College degree	Employed
	(1)	(2)	(3)
Panel A: Without control variables			
Post 1978*Treatment	0.004	0.016**	0.005
	(0.003)	(0.007)	(0.011)
Controls	No	No	No
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
Panel B: With control variables			
Post 1978*Treatment	0.002	0.011**	0.005
	(0.003)	(0.005)	(0.010)
Controls	Yes	Yes	Yes
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
Mean of dep. var.	0.896	0.279	0.772
No. of observations		113,351	

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Panel A presents the results without individual controls. Panel B uses full set of controls, as explained in the manuscript. Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4: Effects on educational and labor market outcomes: adding state-level control and state-specific trends

	High school	College degree	Employed	High school	College degree	Employed	High school	College degree	Employed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post 1978*Treatment	0.000	0.013**	-0.001	0.006	0.031*	-0.015	0.009	0.031**	-0.011
	(0.003)	(0.006)	(0.010)	(0.011)	(0.015)	(0.009)	(0.006)	(0.012)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-of-Birth Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Unemployment Rates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Linear Trend	No	No	No	Yes	Yes	Yes	No	No	No
State-specific Quadratic Trend	No	No	No	No	No	No	Yes	Yes	Yes
No. of observations					113,351				

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Columns 1-3 present the results that further add the unemployment rate as a control variable in Equation (1). Columns 4-6 present the results that controls for birth-specific linear trends and columns 7-9 for quadratic trends. Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: Heterogeneous effects by race

	High school	College degree	Employed
	(1)	(2)	(3)
Panel A: White sample			
Post 1978*Treatment	0.004	0.012	0.009
	(0.006)	(0.008)	(0.011)
No. of observations		85,528	
Panel B: Black sample			
Post 1978*Treatment	-0.009	0.038**	-0.001
	(0.026)	(0.018)	(0.016)
No. of observations		12,351	
Panel C: Hispanic sample			
Post 1978*Treatment	-0.024	-0.010	-0.006
	(0.017)	(0.015)	(0.017)
No. of observations		10,393	
Controls	No	No	No
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 6: Effects on parents' employment, family income, divorce, and fertility

	Maternal employ. (1)	Husband employ. (2)	Fam income (3)	Divorce (4)	Fertility (5)
Post*Treat	-0.0060	0.0354***	0.0480**	-0.0108**	-0.0064***
	(0.0196)	(0.0127)	(0.0199)	(0.0048)	(0.0023)
No. of observations	12,160	10,186	12,001	174,187	174,187

Notes: In columns 1-3, we estimate the short-term effects of paid leave on mothers' employment, husbands' employment, and family income. We use an indicator for employment status that is equal to one if employed (columns 1-2) and the log of family income (column 3). In columns 4-5, we present the estimates of the effect of paid leave on the likelihood of a woman being divorced and having a child less than one year old, respectively. The sample is restricted to individuals aged 18 to 45 years. Columns 1-3 further restrict the sample to those individuals with a child less than one year old. Each column in each panel presents the results from a separate regression. Standard errors are clustered at the state level. The results are obtained using a regression of the form $y_{ist} = \beta_0 + \beta_1 Post78 * Treat + \varsigma_s + \lambda_t + \delta X_i + \epsilon_{ist}$. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix

Table A1: Marginal effects on educational and labor market outcomes (probit models)

	High school	College degree	Employed
	(1)	(2)	(3)
Post 1978*Treatment	0.002	0.011**	0.005
	(0.003)	(0.005)	(0.010)
Controls	Yes	Yes	Yes
State-of-birth dummies	Yes	Yes	Yes
Birth-year dummies	Yes	Yes	Yes
No. of observations		113,351	

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Using a probit model, we estimate each outcome variable on state-of-birth fixed effects, birth-year fixed effects, an interaction term between these two, and full controls, as explained in the manuscript. The estimates here represent the average marginal effects. Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A2: Including Colorado and the 1978 cohort

	High school	College degree	Employed
	(1)	(2)	(3)
Panel A: Including Colorado			
Post 1978*Treatment	0.0009	0.0103*	0.0038
	(0.0030)	(0.0055)	(0.0101)
No. of Obs		114,938	
Panel B: Including the 1978 cohort			
Post 1978*Treatment	-0.0000	0.0118*	0.0052
	(0.0038)	(0.0062)	(0.0098)
No. of observations		126,136	
Controls	Yes	Yes	Yes
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Panel A presents the results, including Colorado in the control group. Panel B adds the 1978 cohort to the baseline analytical sample. Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A3: Exclusion of southern states and analysis extension to 2019

	Excluding southern states			Extending analysis to 2019		
	High school (1)	College degree (2)	Employed (3)	High school (1)	College degree (2)	Employed (3)
Panel A: Without control variables						
Post 1978*Treatment	0.002	0.018**	0.001	-0.0032	0.0112**	0.0037
	(0.004)	(0.007)	(0.011)	(0.0040)	(0.0055)	(0.0085)
Controls	No	No	No	No	No	No
State-of-birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: With control variables						
Post 1978*Treatment	-0.001	0.012*	0.000	-0.0030	0.0097**	0.0040
	(0.004)	(0.006)	(0.011)	(0.0042)	(0.0048)	(0.0084)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-of-birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		76,719			477,465	

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. The left panel excludes Southern States, according to the U.S. Census Bureau, which are Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. The right panel extends the analysis to include post-Recession years through 2019. Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A4: Event study and dynamic treatment effects

	High school	College degree	Employed
	(1)	(2)	(3)
Year 1975*Treatment	-0.005	0.004	-0.010
	(0.019)	(0.015)	(0.009)
Year 1976*Treatment	0.003	0.016	-0.004
	(0.009)	(0.017)	(0.022)
Year 1979*Treatment	0.010	0.030***	-0.001
	(0.007)	(0.010)	(0.013)
Year 1980*Treatment	-0.002	0.010	0.003
	(0.008)	(0.013)	(0.015)
Year 1981*Treatment	-0.003	0.015*	-0.002
	(0.011)	(0.009)	(0.019)
Joint significance test of pre-treatment trends	0.14	0.76	0.08
p-value	p=.707	p=0.388	p=0.773
Controls	Yes	Yes	Yes
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
No. of observations		113,351	

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. We interact between the treatment variable and year fixed effects. We drop the year 1978 when the policy came into effect. We use the year 1977 (the last year of the pre-treatment period) as the reference year. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A5: Event study and dynamic treatment effects: including the 1978 cohort

	High school	College degree	Employed
	(1)	(2)	(3)
Year 1975*Treatment	-0.0068	0.0029	-0.0108
	(0.0188)	(0.0147)	(0.0088)
Year 1976*Treatment	0.0010	0.0142	-0.0051
	(0.0087)	(0.0167)	(0.0217)
Year 1978*Treatment	-0.0055	0.0230	0.0045
	(0.0152)	(0.0139)	(0.0138)
Year 1979*Treatment	0.0071	0.0274**	-0.0031
	(0.0074)	(0.0106)	(0.0133)
Year 1980*Treatment	-0.0037	0.0080	0.0017
	(0.0084)	(0.0130)	(0.0150)
Year 1981*Treatment	-0.0048	0.0131	-0.0031
	(0.0108)	(0.0087)	(0.0188)
Controls	Yes	Yes	Yes
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. We interact between the treatment variable and year fixed effects. We use the year 1977 (the last year of the pre-treatment period) as the reference year. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A6: Event study effects on parents' employment, family income, divorce, and fertility

	Maternal employ. (1)	Husband employ. (2)	Fam income (3)	Divorce (4)	Fertility (5)
Year 1976*Treatment	-0.0013	-0.0087	-0.0068	0.0047	0.0008
	(0.0249)	(0.0257)	(0.0284)	(0.0041)	(0.0067)
Year 1979*Treatment	0.0004	0.0542***	0.0933***	-0.0057	-0.0119***
	(0.0347)	(0.0173)	(0.0297)	(0.0035)	(0.0034)
Year 1980*Treatment	-0.0383*	0.0164	0.0516	-0.0028	-0.0057
	(0.0211)	(0.0134)	(0.0316)	(0.0036)	(0.0060)
Year 1981*Treatment	0.0184	0.0244	-0.0091	-0.0038	-0.0135**
	(0.0276)	(0.0297)	(0.0367)	(0.0031)	(0.0056)
No. of observations	12,160	10,186	12,001	174,187	174,187

Notes: The table presents the event study effects based on the Current Population Survey (CPS) data. All estimates are relative to 1977. As explained in the text, our analytical sample excludes the 1978 birth cohort (specifically, the representative parents of that cohort) due to the partial implementation of paid leave. Additionally, we exclude the sample related to the 1975 birth cohort (specifically, the representative parents of that cohort) from the analysis as state identification is unavailable in the CPS data. In columns 1-3, we estimate the short-term effects of paid leave on mothers' employment, husbands' employment, and family income. We use an indicator for employment status that is equal to one if employed (columns 1-2) and the log of family income (column 3). In columns 4-5, we present the estimates of the effect of paid leave on the likelihood of a woman being divorced and having a child less than one year old, respectively. The sample is restricted to individuals aged 18 to 45 years. Columns 1-3 further restrict the sample to those individuals with a child less than one year old. Each column in each panel presents the results from a separate regression. Standard errors are clustered at the state level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix B:

Table B1: Pre-treatment trend analysis

	High school	College degree	Employed
	(1)	(2)	(3)
Panel A: Continuous trend			
Trend*Treatment	0.003	-0.002	0.005
	(0.009)	(0.007)	(0.004)
Trend	0.008***	-0.002	-0.013***
	(0.003)	(0.004)	(0.003)
Panel B: Discrete trend			
Year 1975*Treatment	-0.005	0.004	-0.010
	(0.019)	(0.015)	(0.009)
Year 1976*Treatment	0.002	0.016	-0.004
	(0.009)	(0.017)	(0.021)
Joint test of significance	0.072	0.554	0.690
	(0.931)	(0.578)	(0.507)
Panel C: Placebo test with an artificial treatment year			
Post 1976*Treatment	0.006	0.004	0.008
	(0.019)	(0.012)	(0.012)
Controls	Yes	Yes	Yes
State-of-birth fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
No. of observations		31,902	

Notes: The dependent variables are high school graduate status, a four-year college degree attainment, and employment status. Panel A does not include individual controls. We limit the analysis for the pre-treatment period (from 1975 to 1977). Standard errors are clustered at the state-of-birth level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

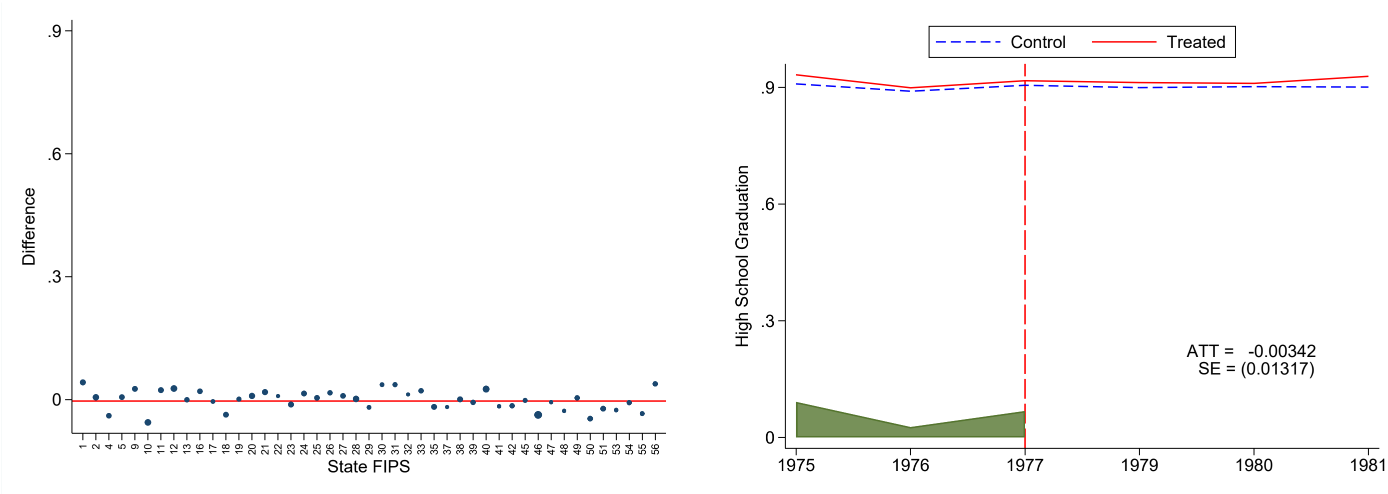
Table B2: Effects on parents' employment, family income, divorce, and fertility

	Maternal employ. (1)	Husband employ. (2)	Fam income (3)	Divorce (4)	Fertility (5)
Panel A: White					
Post*Treat	-0.0144	0.0600***	0.0551***	-0.0119**	-0.0028
	(0.0153)	(0.0134)	(0.0173)	(0.0058)	(0.0025)
No. of observations	8,555	7,660	8,460	130,486	130,486
Panel B: Black					
Post*Treat	-0.0243	-0.0578	0.1570**	0.0119	-0.0207
	(0.0404)	(0.1298)	(0.0607)	(0.0083)	(0.0135)
No. of observations	1,268	588	1,242	17,716	17,716
Panel C: Hispanic					
Post*Treat	0.0156	0.0058	-0.0341	-0.0238***	-0.0098*
	(0.0375)	(0.0192)	(0.0860)	(0.0074)	(0.0057)
No. of observations	2,337	1,938	2,299	25,985	25,985

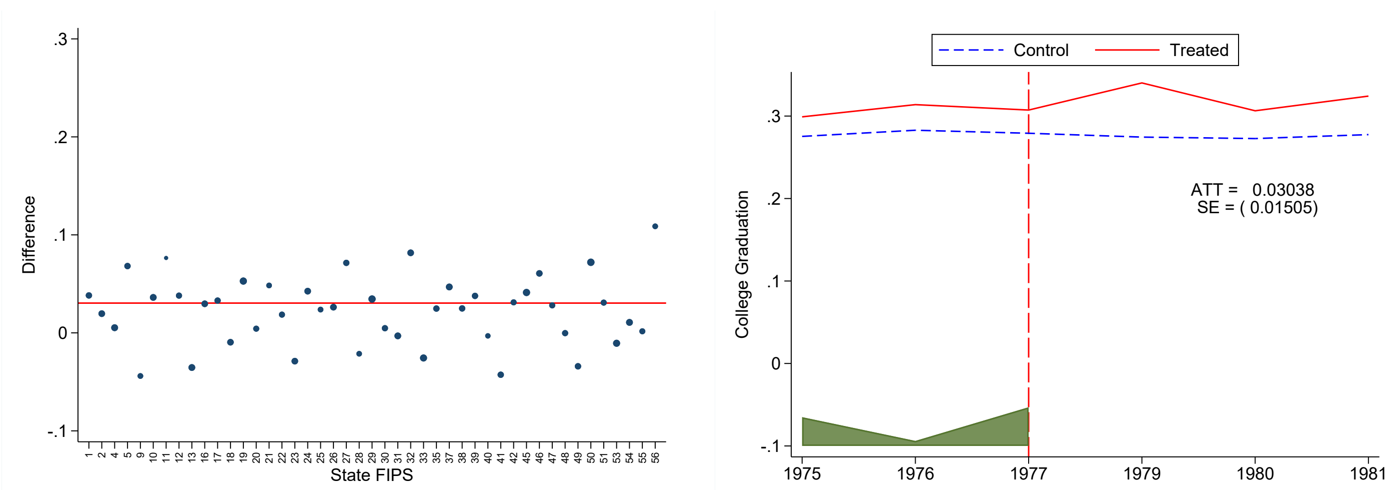
Notes: In Columns 1-3, we estimate the short-term effects of paid leave on mothers' employment, husbands' employment, and family income. We use an indicator for employment status that is equal to one if employed (columns 1-2) and the log of family income (column 3). In columns 4-5, we present the estimates of the effect of paid leave on the likelihood of a woman being divorced and having a child less than one year old, respectively. The sample is restricted to individuals aged 18 to 45 years. Columns 1-3 further restrict the sample to those individuals with a child less than one year old. Each column in each panel presents the results from a separate regression. Standard errors are clustered at the state level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure B1: Synthetic DID

Panel A: High school



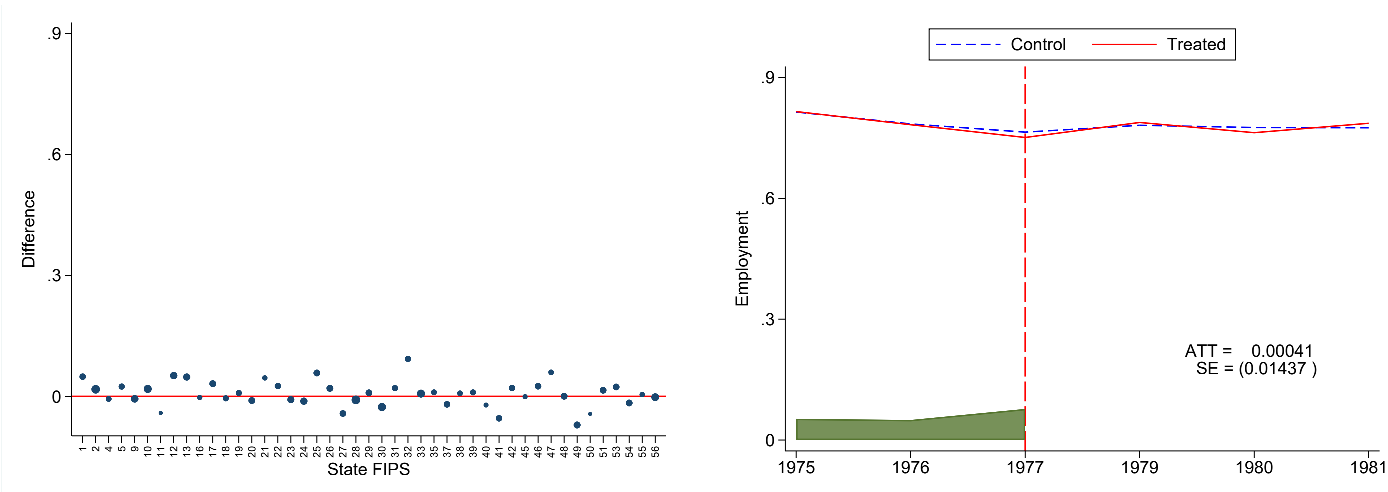
Panel B: College graduation



Notes: We employ the synthetic difference-in-differences (SDID) model to calculate the trends in outcomes in the treated five states versus the weighted average of control states. This is presented in the figures on the right side, along with the year-specific weights at the bottom of the figures. The estimated average treatment effects on the treated (ATTs) and corresponding standard errors are displayed within the figures. The figures on the left show state-by-state adjusted outcome differences, indicated by horizontal lines, with the dots representing the weighted average of these differences. The numbers on the x-axis correspond to state FIPS codes.

Figure B2: Synthetic DID

Employment



Notes: We employ the synthetic difference-in-differences (SDID) model to calculate the trends in outcomes in the treated five states versus the weighted average of control states. This is presented in the figure on the right side, along with the year-specific weights at the bottom of the figure. The estimated average treatment effect on the treated (ATT) and corresponding standard error are displayed within the figure. The figure on the left show state-by-state adjusted outcome differences, indicated by a horizontal line, with the dots representing the weighted average of these differences. The numbers on the x-axis correspond to state FIPS codes.