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THE SPILLOVER EFFECTS OF MATERNITY LEAVE POLICY ON YOUNG WOMEN'S SCHOOLING CHOICES

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To my daughter, Isabel, in hope that my work makes the world a kinder place for you.

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ABSTRACT

While much research has been done on the effects of maternity leave on children's outcomes and maternal employment, less is known about the effects of such policies on choices that are made before one's childbearing years. This paper seeks to fill that gap by focusing on the effect of maternity leave on young women's post-secondary schooling choices. I use the introduction of paid maternity leave and extension of job protection in Norway in 1977, which created plausibly exogenous variation in access to leave, and compare the educational outcomes of the sisters of parents who gave birth immediately prior to and following the reform. I find that having a sibling with access to paid leave reduced the probability of completing a post-secondary degree by 2.75-4.1 percentage points, from a base of 31%, and increased the probability of initiating childbearing by age 25 by 4.9 percentage points, from a base of 46%. Finally, I find that the greatest changes in schooling and timing of fertility occurred among the sisters of women whose earnings five years post-birth benefited the most from the reform.

CHAPTER 1

THE SPILLOVER EFFECTS OF MATERNITY LEAVE POLICY ON YOUNG WOMEN'S SCHOOLING CHOICES

1.1 Introduction

Family leave policies have become increasingly common over the last 60 years: as of 2020, almost every country in the world mandates paid maternity leave following birth or adoption, and about half mandate paid leave for fathers. The stated objectives of such policies are varied and often multiple, such as supporting parent-child bonding, facilitating breastfeeding, and supporting women's labor force participation. Logically, then, much research has focused on the effects of family leave on children's outcomes and parents' labor supply and earnings following the birth of a child. In particular, the implementation of such reforms over the last 60 years has in numerous cases created quasi-exogenous variation in access to leave, conditional on giving birth within a small window around the time of the reform. As such, much research has taken advantage of this variation to inform us of the effects of unexpectedly having access to leave for one additional birth on outcomes that occur subsequent to the birth.

Less is known, however, about the impact of maternity leave policies on choices that are made before one's childbearing years. Maternity leave policies may affect women's long-term labor market and fertility outcomes by altering their investments in schooling, choice of field of study, occupation, labor force participation prior to childbearing, or initiation of childbearing. So far, little research has examined these anticipatory effects, due in part to the challenge of finding a satisfactory control group for comparison.

This paper seeks to fill that gap by focusing on the effect of the introduction of paid maternity leave and the extension of job protection on young women's post-secondary schooling choices. To do so, I consider a reform to the parental leave policy in Norway in 1977. Prior to the reform, Norway guaranteed 12 weeks of job protection to workers following the birth of a child, meaning a worker could take up to 12 weeks of unpaid leave and had the right to return to their same or a similar job. In mid-1977, Norway introduced 18 weeks of paid parental leave and increased job protection from 12 weeks to 1 year. Importantly, the policy applied to working parents who gave birth after July 1, 1977, creating plausibly exogenous variation in who had access to paid maternity leave. In this paper, I use administrative data to investigate whether having a sibling give birth shortly after the introduction of paid parental leave (relative to shortly before) had any effect on the likelihood of a young woman completing a post-secondary degree.

One can imagine several reasons why the introduction of universal paid maternity leave would affect young women's investments in post-secondary schooling. If paid leave and longer job protection increase women's job continuity and years in the labor force, then we would expect young women to make greater investments in their human capital if they anticipate paid leave will be available to them, since they will have more years to reap the returns on their investments. On the other hand, if one of the benefits of a post-secondary degree is that it facilitates re-entry after having left the workforce following the birth of a child, then an increase in job continuity brought about by paid leave and extended job protection could reduce the importance of a post-secondary degree for women. Paid maternity leave could also have numerous pro-fertility effects, making parenthood seem more attractive and inducing women to have more children or have children earlier than in the absence of such policies. Given the considerable overlap between women's childbearing and post-secondary schooling years, women starting families at a younger age could face difficulties in completing a degree. Of course, a combination of these forces could be at play, or women with different characteristics could respond in different ways: young women who are sure of their desire to have children but are unsure of whether they will work while their children are young could invest more in schooling if they anticipate being able to take paid leave and return to their job; whereas women who are sure of their desire to work but unsure of their fertility plans or timing may be induced to have more children or have children earlier, which could reduce their investments in schooling.

There are also several reasons why siblings of those who gave birth shortly after the introduction of paid leave might respond differently than siblings of those who gave birth shortly before. The existence of the reform may not have been widely known, especially among teenagers and young adults who were not considering having children in the short term. In this case, young adults whose siblings gave birth after the introduction, and therefore had access to paid leave, could have had a better understanding of the existence of the paid leave policy and its eligibility requirements. A lack of awareness about the existence of paid maternity leave would not be without precedent: according to a 2015 poll of voters in California, only 36% of voters were aware of California's Paid Family Leave policy, which had been in effect for eleven years (DiCamillo and Field, 2015).

Alternatively, even if the existence of the policy was well known, its effects on mothers' careers may not have been known at the time. For example, would the reform support mothers' transitions back to work, ensuring greater job continuity and labor force participation following the birth of a child; or would extended time out of work result in discrimination by employers or lower re-entry, and lower earnings in the years following the birth of a child? The answers to these questions may not have been widely known at the time, and so observing the experience of a close relative who was eligible for paid leave may have contributed to young adults' expectations.

The identification strategy employed in this paper relies on the quasi-random nature of date of birth within a small window of time. All working mothers giving birth after July 1, 1977 had universal access to four months of paid leave and a year of job protection. Since the reform was announced less than three months before the July 1 cutoff, all the mothers on either side of the cutoff were already pregnant at the time of the announcement.

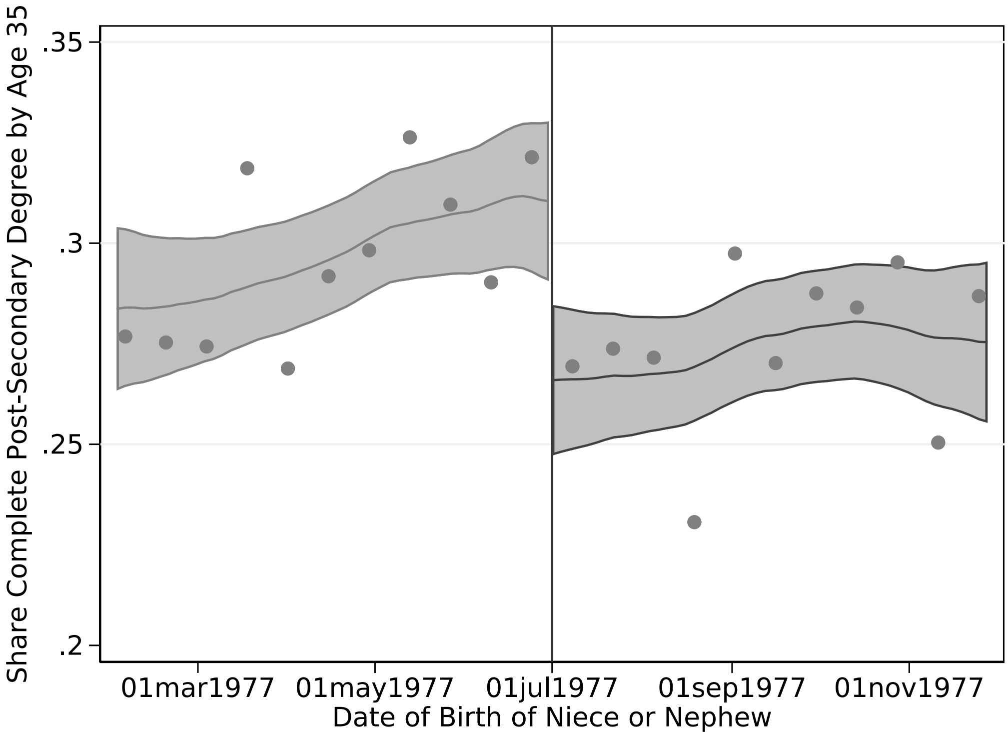
Furthermore, date of birth is not easily manipulated, and certainly not easily postponed (in order to gain eligibility). Therefore, within a sufficiently small window, whether a mother gave birth shortly before or shortly after July 1 should be close to random, and so the sisters of working women who had access to paid leave should be similar to the sisters of working women who did not on account of their child's date of birth. This means that any differences in schooling outcomes between these two groups should be attributable to having a sibling with access to paid maternity leave and extended job protection.

Figure 1.1 shows the share of young women who complete a post-secondary degree by age 35 (y-axis) by the date of birth of their niece or nephew (x-axis). Visual inspection of the fitted regression lines reveals a noticeable decrease in the share of women who complete a post-secondary degree by age 35 if her sister or sister-in-law gave birth after cutoff compared to before. The break in the locally linear fitted lines at July 1 is roughly 4 percentage points, from a base of approximately 31% immediately prior to the reform.

I confirm the finding of Figure 1.1 using regression discontinuity design with local linear and global linear trends and controlling for background characteristics. I next test for similar effects in non-reform years and among the sisters of parents who were ineligible for paid leave based on their earnings but gave birth in 1977, and find no effect in either group. Finally, I show that the fall in post-secondary completion that occurred among women with a niece or nephew born around the time of the reform is greater in magnitude than normal month-to-month variation in completed schooling around this time.

To better understand the mechanism driving the fall in post-secondary schooling, I look at several fertility and marriage outcomes. I find that women whose sisters were eligible for leave and gave birth after July 1 were 4.9 percentage points more likely to have their first child before age 25 (from a base of 46%) and 3 percentage points more likely to become a single mother (from a base of 25%). I interpret this to mean that the introduction of paid maternity leave and extended job protection brought about a change in the timing of

Figure 1.1: Share of Women with Post-Secondary Schooling by Age 35



Note: Gray dots indicated the share of women who complete a post-secondary degree by age 35, among those with a niece or nephew born during the indicated two week period. The light and dark gray lines indicate the estimated local linear trend in completed schooling. 95% confidence intervals for the local linear fit are shown in gray. The vertical line indicates July 1. Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time.

initiating childbearing. Mediation analysis shows that 14% of the decrease in post-secondary schooling can be attributed to the increased probability of having a child before age 25. Among women who were less than 19 at the time of the reform, this share rises to 36%.

I also find considerable heterogeneity in how the reform affected mothers' earnings five years post-birth. By looking at how the reform interacted with mothers' pre-reform characteristics, I estimate that approximately 20% of mothers experienced statistically significantly higher earnings as a result of the changes in the maternity leave policy, while approximately 20% experienced a decrease in earnings. Interestingly, I find that the sisters of women whose earnings increased because of the reform saw the biggest decrease in post-secondary schooling and the largest increase in the probability of having a child before age 25. In contrast, I find no change in post-secondary schooling or timing of fertility among the sisters of women whose earnings were likely depressed by the reform.

These results suggest two things. First, the effects of the reform on mothers were not well known at the time, and having a sibling with access to paid leave was informative for young women who were making important fertility and career decisions. Second, if young women believe that the introduction of paid leave will be beneficial to working mothers' earnings (or at least not harmful), some of them will be induced to have children at a younger age, to invest less in post-secondary schooling, or both.

This paper contributes to our understanding of the effect of parental leave reforms on women's careers and fertility decisions. Much previous research has focused on the effects of being exogenously assigned access to maternity leave on subsequent outcomes, such as time spent on leave, labor force participation and earnings, breastfeeding, and subsequent fertility. For the most part, this literature does not find that paid maternity leave policies of up to one year have much, if any, impact on mothers' medium- and long-run employment, earnings, or subsequent fertility.[1] In many of these papers, however, identification relies on studying women who were already pregnant and working at the time the reform was announced, and compares women who had access to leave with those who did not, based on the date they gave birth. The difficulty is that, for such women, many of their schooling, occupation, and fertility choices are already fixed by the time the policy is announced. Furthermore, both groups are often equally likely to have access to leave for any subsequent births, making the effect of the reform on fertility decisions or long-term labor market outcomes difficult to identify.

A small number of papers look at the effect of maternity leave policies on the labor

1. See for example, Baker and Milligan (2008a,b); Kluve and Tamm (2013); Bergemann and Riphahn (2015); Dahl et al. (2016). Some studies suggest that paid leaves in excess of one year have negative effects on women's earnings following their return to work, lasting for up to ten years (Schönberg and Ludsteck, 2014; Lequien, 2012; Lalive and Zweimüller, 2009).

market outcomes of women who were not pregnant at the time of the reform. Das and Polachek (2015) use a difference-in-difference technique to compare young women with men and older women in California and other states, and find that the labor force participation rate, the unemployment rate, and the duration of unemployment among young women rose following the introduction of paid family leave in California. Similarly, Thomas (2020) uses a difference-in-difference technique exploiting variation in state policy prior to the enactment of the Family Medical Leave Act, and finds that women hired after the FMLA was enacted were more likely to be employed but less likely to be promoted than women hired prior to the FMLA. While these papers are more similar to the current paper in that they look at the effect of the policy on all women's careers, not just those pregnant at the time of the reform, they do not consider the effects of the policy on women's investments in their careers.

This paper seeks to fill this gap in the literature on the effects of maternity leave policies on women's labor outcomes by exploring the effect of anticipating access to maternity leave on decisions that occur prior to pregnancy, and in particular on investments in human capital. In this sense, this paper is more closely related to a growing literature highlighting the responsiveness of schooling choices to changes in the returns to schooling, such as Abramitzky and Lavy (2014), who look at the responsiveness of investment in schooling to changes in redistribution schemes in Israel kibbutzim that increased the rate of return to education.[2]

This paper also contributes to a growing body of research on the effects of siblings' experiences on schooling outcomes using natural experiments. Using admissions thresholds in the United States, Chile, Sweden, and Croatia, Altmejd et al. (2020) find that older siblings' college and major choices can significantly influence their younger siblings' probability of enrolling and choice of college or major. Dustan (2018) uses randomness generated by Mexico City's school choice mechanism to show students prefer schools older siblings have attended, and Aguirre and Matta (2021) exploit discontinuous admission rules in Chile's post-secondary

2. See also Abramitzky et al. (2021), who look at the spillover effects of such reforms on the affected students' peers.

education system to reveal strong sibling spillovers in the choice of degrees and college institutions. Joensen and Nielsen (2018) use a pilot program in Denmark to show that younger siblings are most likely to choose math-science if their older sibling was exogenously induced to choose math-science. Finally, Dahl et al. (2020) use variation generated by admissions cutoffs in college field of study to find strong spillovers in field choices that depend on the gender mix of siblings and whether the field is gender conforming. To my knowledge, this is the first paper that uses quasi-exogenous variation to evaluate the impact of siblings' access to paid maternity leave and job protection on young women's schooling choices.

The remainder of the paper proceeds as follows. Section 1.2 describes the current state of parental leave policies around the world and in the U.S., and discusses what is known about their effects on mothers' labor market outcomes. Section 1.3 describes the data, and Section 1.4 outlines the empirical strategy. Section 1.5 provides new evidence on the effect of the 1977 reform in Norway on mothers' earnings. Section 1.6 presents the estimates of the effect of the reform on young women's schooling choices, timing of fertility, and marriage. Section 1.7 discusses the possible mechanisms behind the effect on schooling, and Section 1.8 concludes.

1.2 Maternity Leave Policies

1.2.1 Maternity Leave Policies around the World and in the U.S.

Globally, there is a wide variety of maternity leave policies. For this reason, when considering the large and growing literature on the impacts of such policies, it is useful to distinguish between several important features. Parental leave policies may be paid or unpaid. Unpaid policies may equivalently be referred to as job protection, meaning an eligible parent may take unpaid time off of work and return to the same or similar position. Paid policies vary in their replacement rates, that is, the share of salary or wages that are paid to a parent on leave, as well as in who pays the wages of parents on leave (e.g., government vs. employer).

Almost every country in the world currently mandates paid maternity leave (and just over half mandate paid leave for new fathers). The length of leave and replacement rate vary across countries: European and Central Asian countries have the highest median length of paid leave for mothers, and 421 days, while Middle Eastern and North African nations have the lowest median length of leave, at 70 days. Almost three-quarters of countries mandate full wage replacement for new mothers, while very few offer less than half. In about half of the world's countries with paid leave, the government pays the parent's replacements wages.

The United States is a notable exception among OECD countries in its parental leave policies, in that there is no nationwide, mandated paid parental leave. The Family and Medical Leave Act of 1993 (FMLA) enables eligible employees at covered employers to take up to 12 weeks of unpaid leave.[3] In addition to the FMLA, there is a patchwork of state legislation on parental leave. Currently 18 states offer unpaid leaves with fewer eligibility restrictions and, in some cases, slightly longer duration (Bailey et al., 2019). Paid leave is less common: only eight states and the District of Columbia have mandated paid family leave. Most recently, the Federal Employee Paid Leave Act, signed into law in December of 2019, provides eligible federal employees with 12 weeks of paid leave following the birth or adoption of a child.[4]

Given the overlapping leave policies and their differing eligibility requirements, it is not immediately obvious how widespread access to paid or unpaid parental leave actually is in the United States. According to the National Compensation Survey conducted by the Bureau of Labor Statistics, in 2016, 14% of civilian workers had access to paid family leave

3. Workers are eligible if they have worked for a covered employer for at least 1,250 hours within the previous 12 months. Covered employers are those that employ at least 50 employees within 75 miles of the work-site.

4. Eligibility requirements are that the birth, adoption, or placement occur on or after October 1, 2020; and the employee must be eligible for FMLA and have completed at least 12 months of federal service.

(Horowitz et al., 2017). Unpaid family leave was more common: 88% of civilian workers have access to unpaid leave, and 60% have access through the FMLA ((Horowitz et al., 2017; Klerman et al., 2012). Take-up of leave in the U.S., in particular unpaid leave, is also not immediately obvious and is itself the subject of some research. According to a 2017 Pew Research Center report, the median length of leave (paid or unpaid) among women following the birth of a child was 11 weeks, and among fathers the median leave was one week. There is considerable inequality in access to and use of leave: mothers with low household incomes tended to take considerably shorter leaves and were less likely to receive pay while on leave.[5] This may be tied to an important feature of unpaid leave policies: many parents that are eligible for unpaid leave cannot afford to take up their full leave. Among parents who took some parental leave, 56% reported taking less time than they needed or wanted to, and the most commonly cited reason was that they could not afford to lose more wages or salary.

At the national level, the current policy in the United States is remarkably similar to the policy in Norway prior to the 1977 reform studied in this paper: prior to the reform, working parents could take up to 12 weeks of unpaid leave. This makes the implications of the 1977 Norwegian reform of particular interest to U.S. policy decisions.

1.2.2 Existing Research on Effects of Maternity Leave Policies on Women's Labor Market Outcomes

There is a large body of research on the effects of parental leave policies on women's labor market outcomes, much of it using quasi-exogenous variation in access to leave to establish causal effects. Given the wide range of policies discussed above, it is important when discussing the evidence to distinguish between initial introductions and subsequent expansions

5. The median length of leave among mothers from households with incomes under $30,000 was six weeks, half the median of 12 weeks among mothers with household incomes of $75,000 or more. Among mothers that took leave, 62% of those with low household income received no pay, compared to 26% of those with high household income (Horowitz et al., 2017).

and between job protection (or unpaid leave) and paid leave.

Time Spent on Leave and Probability of Employment Immediately Following a Birth

In order to better understand the impact of parental leave policies on mother's careers, it is useful to first understand the effect of such policies on take-up of leave. As hinted at above, full take-up is not feasible for some mothers, especially in the case of unpaid leave. Even in the case of paid leave, parents may fear discrimination by their employers or skill atrophy. The effects on mothers' careers are also likely to depend on whether the introduction or extension of leave induces mothers who would have quit to instead stay employed and take leave, or whether it only impacts the length of leave.

Below I discuss a number of papers in greater detail, including their context and empirical strategy, but here I summarize the findings that, taken together, these papers suggest. First, there is some take-up of an initial expansion of unpaid leave among mothers, although the literature using the FMLA has not quantified the exact amount on account of limited information on eligibility and coverage. Second, relative to unpaid leave, paid leave has a significantly larger impact on length of leave among mothers. Furthermore, the changes in length of leave are greatest for socioeconomically disadvantaged groups, for whom unpaid leave may not be affordable. Third, there is mixed results on whether the initial introduction of unpaid or paid leave induces mothers to remain employed who would have otherwise quit, suggesting this may depend on the country studied. Fourth, expansions beyond modest entitlements (either paid or unpaid) do not appear to have large impacts on women's probability of remaining employed. However, expansions of paid leave do appear to increase average length of time on leave.

A number of papers look at the effect of initial introductions of unpaid leave on take-up using variation across states or provinces and over time in a difference-in-differences approach. Han et al. (2009) use variation in state laws over time and the introduction of the FMLA at the federal level, together with survey data from the June CPS fertility supplement and other months of the CPS. They find that mothers are 13 percent more likely to be on leave in the first month following a birth, 16 percent more likely in the second month, and 20 percent more likely in the third month, if they live in a state with a leave law, or after the introduction of FMLA, relative to mothers giving birth at a place or time with no leave policy. They find no differences in the probability of being employed, and interpret these findings to mean that the introduction of leave policies causes women to take more or longer leave, but does not induce women to stay employed if they would have otherwise left their employment. Unfortunately their data does not allow them to distinguish between covered or eligible mothers and non-covered or non-eligible mothers (recall that only 60% of workers are estimated to be covered and eligible under the FMLA, for example), nor do they distinguish between the effects of unpaid leave policy (such as the FMLA) or paid leave (via state temporary disability insurance programs).

In a similar research design, Baker and Milligan (2008a) empirically evaluate the impact of job protection policies using variation between Canadian provinces and across time in a difference-in-differences specification. In particular, they are able to compare initial expansions (increasing unpaid job protection from 0 to 17 or 18 weeks) with subsequent expansions (from 17 to 18 weeks to between 29 and 52 weeks).[6] The authors find that the introduction of modest entitlements induce mothers to take leave instead of quitting, while subsequent expansions induced mothers to substitute between working and being on leave, with no change in the probability of being employed.[7]

6. While the policy changes they evaluate were expansion in unpaid leave, it is important to note that some women were also eligible for employment insurance, which provided some income replacement during the unpaid leave.

7. In the case of both initial and subsequent policy changes, the authors find some evidence of increased job continuity, although job continuity is inferred from job tenure, and is thus an imperfect indicator of whether an employee returned to the same employer.

A number of papers providing evidence on the effect of initial introductions of paid leave exploit the enactment of California's Paid Family Leave policy (enacted in 2002, in effect beginning in 2004), which provides 60-70% of pre-birth wages for up to 8 weeks following the birth or adoption of a child. These papers often compare outcomes in California to those in other states, before and after the implementation of the policy, and sometimes add a third control group (e.g., another demographic group), and generally conclude that the introduction of paid leave has a significant effect on time spent on leave (relative to exclusively unpaid leave or job protection). For example, using data from the March CPS, Rossin-Slater et al. (2013) find that California's introduction of 8 weeks of paid family leave increased the use of maternity leave from three to six weeks on average.[8] Interestingly, they find evidence of larger growth for less advantaged groups, for whom unpaid leave (available to them prior to the California reform under the FMLA) was likely available but unaffordable. This is consistent with Han et al. (2009), who find that the introduction of unpaid leave under the FMLA increased leave the most among college-education or married mothers, who presumably were more likely to have the resources to take time off without pay following a birth.

Using an regression discontinuity design with child's date of birth and IRS tax data, Bailey et al. (2019) find an 18 percentage point increase in use of paid leave, but no effect on women's employment immediately following the birth.

Several papers using administrative data and reforms in Europe exploit variation in access to leave based on the child's date of birth. These papers often have the advantage of larger sample sizes and more detailed data on eligibility (especially relative to studies using the FMLA), but in some cases lack detailed data allowing the researcher to distinguish between being on unpaid leave and being out of the labor force. These papers also tend to focus more

8. The CA-PFL covers 60-70% of pre-birth wages, and does not provide job protection. Thus, some workers may have access to paid leave via CA-PFL but not job protection, if they are not FMLA-eligible or work at a covered employer.

on paid leave policies.

Carneiro et al. (2015) use the same reform discussed in this paper - the 1977 introduction of 18 weeks of paid leave and the extension of job protection to one year in Norway - and use data on earnings to infer unpaid time spent out of work. They find no difference in earnings post-birth between mothers who gave birth before and after the reform, suggesting that take-up of the paid leave was 100%, and the extension in job protection caused no change in unpaid leave.

Using administrative data from Denmark and a similar regression discontinuity design, Rasmussen (2010) found that the 1984 expansion from 14 to 20 weeks of paid leave resulted in higher maternal earnings and work experience in the five years after birth. This indicates that mothers substituted from either quitting or taking unpaid leave to taking paid leave, but we cannot be sure which one.

Using administrative data from Germany, Dustmann and Schönberg (2012) look at the effect of a series of expansions to both paid leave and job protection in between 1979 and 1992. Before the reforms, women were entitled to two months of job protection and two months of fully paid leave. By the end of 1992, job protection increased to three years, and paid leave to 18 months, allowing the authors to estimate the effect of expansions to leave, but not to distinguish between paid and unpaid leave. The authors exploit eligibility discontinuities in access to leave based on the child's date of birth, and find a short run reduction in the mother's probability of being at work, but no impact on maternal employment after the expiration of the leave period. This is consistent with Baker and Milligan's (2008) finding, suggesting that expansions beyond two months of leave induce mothers to substitute between returning to work and taking leave, but have little impact on labor force participation. While at first glance this may seem to conflict with the Rasmussen (2010) result, it is important to note that the lengths of paid leave in the post-reform period differ drastically (20 weeks in Denmark vs. 18 months in Germany), highlighting how the effects of marginal expansions in leave can vary significantly depending on whether the leave is more or less generous.

Dahl et al. (2016) measure the impact of subsequent expansions in paid leave in Norway between 1987 to 1992. They also use a regression discontinuity design, taking advantage of the birth cutoff date that determined eligibility. These expansions in paid leave - from 18 to 35 weeks - were not accompanied by any change in job protection, which remained at 12 months. The authors found complete take-up of paid leave, and, similar to Carneiro et al. (2015), no crowd-out of unpaid leave.

Women's Earnings and Employment Following a Birth

Given the above discussion, it is not surprising that research suggests that the effect of leave policies on mothers' earnings and employment varies considerably depending on the length of leave, whether it is paid, and whether it includes job protection. Rossin-Slater provides a comprehensive discussion of this literature, and so here I only summarize her conclusions and cite the referenced works. The first take-away is that short unpaid leave policies enable some women, usually more educated or higher earning, to take leave and return to employment, instead of quitting (Baum, 2003). Perhaps surprisingly, these policies have little impact on subsequent labor market outcomes (Waldfogel, 1999; Han et al., 2009).

The second take-away is that paid maternity leave policies of up to one year have either positive or zero impact on mothers' medium- and long-run employment and earnings, but that paid leave entitlements in excess of one year can negatively affect mothers' wages in the long-term. See for example, Baker and Milligan (2008a); Kluve and Tamm (2013); Bergemann and Riphahn (2015); Schönberg and Ludsteck (2014); Lequien (2012); Lalive and Zweimüller (2009); Dahl et al. (2016).

The difficulty of interpreting the findings of the papers referenced here is that they mostly look at the effect of access to leave conditional on already being pregnant and having started one's career. Thus, they are not very informative about the effect of maternity leave policies on the wages of women who were induced to enter the labor force prior to childbearing, knowing that she would have access to job protection or paid leave. For example, Thomas (2020) develops a model in which the passage of family leave policy induces a change in the selection of women into the labor market, and tests the implications of this model using the passage of the FMLA. More generally, women who are not yet having children may respond in other ways to changes in maternity leave policy that regression discontinuities in date of birth cannot capture, such as choice of occupation, employer, number of hours, or fertility, but that are nonetheless important mechanisms by which maternity leave policies may support or hinder women's employment and earnings. My objective is to contribute to a broader understanding of how maternity leave policies may influence women's outcomes, by looking at how observing a sibling with access to maternity leave (and thus presumably having better information about one's own future access to leave) affects young women's schooling and fertility decisions.

1.3 Data

My analysis employs several administrative data sources maintained by Statistics Norway, which are linked through unique individual identifiers. The first data source is the population register, which contains demographic information such as date of birth, place of birth, sex, marital status, and current residence. Importantly, the population register includes the unique identifier of a person's mother and father, making it possible to link individuals to their siblings, nieces/nephews, and to measure fertility outcomes.

The second data source is the National Education Database, which contains data on individual's educational attainment and enrollment dating back to 1980.[9] The education database also includes each course of study (vocational or academic) taken by a person, with

9. The National Education Database is available on an annual basis beginning in 1980, including date of completion if after 1970. A snapshot of the population's highest education is also available as of October 1, 1970, but does not include date of completion.

start and end date, as well as the outcome (e.g., completed). The codes identifying course of study are detailed and consistent over time. Finally, data on income from wages and benefits is also available for every year beginning in 1967. These three data-sets can be linked using the unique personal identifiers.

I begin by identifying births in Norway in the months surrounding the introduction of the reform on July 1, 1977. I use the population register to link children to parents, and then to the parents' siblings. I also link the parents to their income data from 1976 to determine eligibility for maternity leave, and I link the parents' siblings to their fertility outcomes (observed through 2018), highest level of education completed by age 35, and labor market earnings. As placebo tests, I do the same for births from the surrounding years (1975 through 1978).

In order to be eligible for paid leave, parents had to be employed for 6 of the 10 months immediately prior to the birth, and had to have annual earnings above 10,000 Norwegian Kroner (equivalent to approximately US$5,700 in 2020). Unfortunately, detailed information on start and end dates of work around the time of the reform are not available. Therefore, I approximate the eligibility requirements using parents' labor earnings from 1976. I define a couple as being eligible for maternity leave if she earned at least 10,000 Kroner in the year prior to giving birth, as in Carneiro et al. (2015).

Since my focus is young adults for whom their siblings' experiences surrounding parenthood and maternity leave could have been potentially informative, I exclude from my baseline analytical sample all aunts and uncles who already have children of their own. Furthermore, since I am primarily interested in schooling choices, I exclude those over the age of 26, whose schooling choices are likely already completed. Presumably very young children are not old enough to observe and learn from their older siblings' experiences with parenthood, and so I also exclude siblings under the age of 8. Finally, I restrict my sample to the siblings of eligible parents, as defined above, and use the siblings of ineligible parents as a falsification exercise.

Table 1.1 presents descriptive statistics for the baseline sample. The average age in the sample is approximately 18.5. Women in the control group (with nieces and nephews born before July 1) are 2.5 months older on average than women in the treatment group. 92% of sisters studied are younger than the parent giving birth, and slightly more than half are a sibling of the mother. There is no significant difference in the average annual earnings of the eligible mother in the year before the reform. Between 91.8-92.9% of women in the sample over the age of 19 had positive labor earnings in the year prior to the reform, and there is no difference in the share with positive earnings or average annual earnings between the control and treatment group. Women whose sibling gave birth after the reform have lower average maternal education (measured as of 1980) than the control group. Since maternal education is a predictor of completed schooling, I include maternal education in the set of controls below.

The primary outcome of interest is whether the sibling of a parent completed any post-secondary education by age 35. Post-secondary education is education at the 14th class level and up, or, equivalently, anything that comes after the final year of upper secondary. This includes higher education (undergraduate level, graduate level, postgraduate level), as well as some technical or licensed vocational programs, as long as these are not part of upper secondary schooling.

1.4 Empirical Strategy

To determine the effects of the reform on mothers' employment outcomes and parents' siblings' schooling and fertility outcomes, I take advantage of the quasi-experimental variation in access to paid leave that was generated by the July 1 cutoff. The policy applied to all eligible parents who gave birth after July 1, 1977. The reform was announced on April 15, 1977, and became official on June 13, 1977, only shortly before it went into effect. Thus,

Table 1.1: Descriptive Statistics for Baseline Sample

Covariate	May-June	July-August	Difference
Own Age at Niece/Nephew's Birth	18.6	18.4	-0.21*
			(0.11)
Sibling's Age	24.7	24.4	-0.28***
			(0.09)
Younger Sibling	91.5	92.0	0.55
			(0.71)
Related to Mother	56.9	55.6	-1.27
			(1.29)
Income of Child's Mother in 1976	22,370.3	22,019.8	-350.55
			(254.91)
Share with Any Income 1976 (ages 20+)	92.9	91.8	-1.10
			(1.18)
Own Income 1976 (ages 20+)	18,597.2	18,676.6	79.41
			(583.69)
Own Mother's Educational Attainment (1980)			
Lower Secondary or Less	58.1	64.2	6.09***
			(1.28)
Upper Secondary, Basic	33.9	28.7	-5.15***
			(1.22)
Upper Secondary, Final	3.4	3.3	-0.08
			(0.48)
Any Post-Secondary	4.6	3.7	-0.85
			(0.52)

Note: Table shows descriptive statistics for the sisters of eligible parents of a child born between May-August of 1977, who were not already parents themselves, and were between ages 8 and 26 at the time. All earnings are shown in real 2015 USD.

the first women that were eligible to take paid maternity leave were already pregnant at the time the policy was announced. I use three slightly different specifications, described below, all of which rely on comparing the outcomes of the sisters of working parents who gave birth immediately following July 1, and therefore were eligible for paid leave, with the outcomes of sisters of working parents who gave birth immediately prior to July 1, who did not have access to paid leave. The key is that aunts of children born immediately before and after the reform should be similar, except that aunts of children born after the reform had a sister or sister-in-law with access to paid maternity leave.

Before discussing the empirical specifications, it is important to discuss the parameter of interest. The primary objective is to estimate the effect of having a sibling with access to universally mandated paid maternity leave and job protection of one year on an individual's completed schooling, which I will denote as β_1. I will also sometimes refer to this as the effect of the reform on siblings' completed schooling. It should be noted that this is slightly different from the effect of having a sibling who took paid maternity leave. First, it is possible that some employers offered unpaid or paid leave for their employees prior to the reform. Furthermore, the administrative data from this time period lacks any direct measure of how much paid leave mothers actually took after the reform (although as discussed below, there is evidence that take up of paid leave following the reform was close to 100% and taken almost exclusively by the mother). However, given these constraints, I cannot determine the effect of having a sibling who actually took paid leave. Rather, I am interested in the effect of having a sibling who had access to universal paid leave, since the existence and eligibility requirements of the policy would be the relevant information for a younger sister planning her schooling and fertility decisions.

If families that met the income eligibility requirement and had a child born in the months immediately prior to the introduction of paid leave were similar to eligible parents with a child born in immediately following July 1 in all dimensions except for the reform, then one way to estimate the effect of the reform is to simply compare mean schooling outcomes associated with births immediately prior to and following the birth, potentially controlling for relevant covariates, as in equation 1.1:

$$y_i = \beta_0 + \beta_1^{FE} D_i + \beta_2 X_i + \epsilon_i \tag{1.1}$$

where y_i is an indicator variable denoting whether person i completed a post-secondary degree by the age of 35; D_i is an indicator variable denoting whether i's niece or nephew was born on or after July 1st; and X_i denotes a vector of controls: fixed effects for i's year of birth, fixed effects for i's sibling's (the parent's) year of birth, and an indicator for whether i's mother completed at least basic upper secondary schooling (equivalent to 12th class level).[10] Throughout, I estimate equation 1.1 using births in the two months prior to and following the reform (May-August). Under the assumption that families that gave birth shortly before and shortly after the reform are otherwise similar, β_1^{FE} should be a consistent estimator of the treatment effect, β_1. I will refer to this specification as the fixed effect specification.

There are three reasons that one might be concerned that the characteristics of families may vary with the date of birth of a child. The first concern is that families may be able to precisely manipulate the date of birth of a child. In their paper, Carneiro et al. (2015) check for date-of-birth manipulations around the July 1, 1977 reform, and find that the number of births did not change in the days and weeks around the reform. They also check that the characteristics of mothers did not change immediately surrounding the reform. Furthermore, as discussed above, the policy change was announced shortly before the policy went into place, so all the women in the control and treatment groups were already pregnant when the reform was announced. Thus, it seems unlikely that families manipulated the date of birth of a child in order to become eligible for paid leave.

A second concern is that the date of birth of a child may have an impact on the outcome of interest. For example, the date at which children start school often determines at which age they begin school, which may influence their completed schooling and earnings as an adult. However, in this context, it seems unlikely that whether a niece or nephew's date of birth falls a few days earlier or later could impact an aunt's or uncle's outcomes in any way.

One final concern is that, even if families do not precisely manipulate the timing of birth for reasons related to the reform, given enough time to plan, families may be imprecisely timing the birth of a child for other reasons, and that different families may aim for different

10. In the baseline sample, between 58-65% of observations have a maternal education less than basic upper secondary, and between 28-34% have mother's highest completed education equal to basic upper secondary. Less than 10% have maternal education equal to final year of upper secondary or higher.

months of birth. For example, some families may prefer to have a child in summer than in winter. If families differ in whether or how they time the birth of a child, then this may create some differences in the characteristics of families having a baby in early 1977 compared to late 1977. As long as we assume that families cannot precisely manipulate the date of birth of a child, then families giving birth immediately before and after the reform should be similar. However, as we move away from the date of the reform, the characteristics of the families may become less similar.

To account for this, I also estimate the average treatment effect of the reform on parents' siblings using a regression discontinuity design with a local linear regression.[11] In particular, I estimate:

$$\min_{\beta_0,\beta_1^{LL},\beta_2,\beta_3,\beta_4} \sum_{i=1}^{N} K\left(\frac{z_i - c}{h}\right)\left[y_i - \beta_0 - \beta_1^{LL} D_i - \beta_2 \left(z_i - c\right) - \beta_3 D_i \left(z_i - c\right) - \beta_4 X_i\right]^2 \tag{1.2}$$

where z_i denotes the date of birth of i's niece or nephew, c is the cutoff of July 1st, and h denotes the bandwidth. A key decision in implementing a local linear estimator is the choice of bandwidth. To avoid an *ad hoc* approach, I use mean squared error optimal bandwidths (Imbens and Kalyanaraman, 2011; Calonico et al., 2014). Furthermore, I use the triangle kernel as it is has been shown to be boundary optimal (Cheng et al., 1997). The advantage of the regression discontinuity design is that it allows for the underlying characteristics of families to vary around the date of the reform, as long as they do so continuously, without any discrete jumps.

Another way to account for differences between the families is to simply allow for a linear trend in aunts' schooling outcomes as a function of date of birth, with a discontinuity at the

11. For a review of regression discontinuity designs see Van der Klaauw (2008); Imbens and Lemieux (2008); Lee and Lemieux (2010); DiNardo and Lee. See Hahn et al. (2001) and Porter (2003) for local linear estimation in a regression discontinuity design. For applications of regression discontinuity designs using local linear estimators, see Carneiro et al. (2015); Dahl et al. (2014, 2016), among others.

introduction of the reform:

$$y_i = \beta_0 + \beta_1^{GL} D_i + \beta_2 (z_i - c) + \beta_3 D_i (z_i - c) + \beta_4 X_i + \epsilon_i \tag{1.3}$$

Under the assumptions that families cannot precisely manipulate the date of birth of a child and the exact date of birth has no direct impact on an aunt's outcomes other than through the reform, then both the local and global linear RD estimators β_1^{LL} and β_2^{GL} consistently estimate the treatment effect β_1. In all specifications I report heteroskedasticity-robust standard errors (White, 1980).

As a robustness exercise, I perform several falsification exercises to test for date-of-birth effects. First, I estimate the same specifications described here using births from non-reform years (1975, 1978). I also estimate the same specifications described here using the siblings of non-eligible parents in 1977, the reform year. Finally, I test for treatment effects in every month during 1975-1978. I discuss these falsification exercises in greater detail in Section 1.6.

1.5 Effect on Mothers' Earnings

1.5.1 Leaving Taking

In 1977, Norway introduced 18 weeks of paid parental leave and increased job protection from 12 weeks to 1 year. In order to be eligible, parents had to be employed for 6 of the 10 months immediately prior to the birth, and had to have annual earnings above 10,000 Norwegian Kroner (equivalent to approximately US$5,700 in 2020). The 18 weeks of paid leave (paid at 100% of pre-birth wages) could be shared between the mother and the father, although six weeks were reserved exclusively for the mother.

Unfortunately, detailed data on womens' hours and employment is not available from this period, making it difficult to describe in detail the patterns in mothers' employment following a birth prior to the reform. Using data on earnings to infer unpaid time spent out of work, Carneiro et al. (2015) estimate a drop in mothers' earnings following birth equivalent to approximately 8 months on average prior to the reform. However, if mothers returned to work part-time following a birth, then this method of estimating unpaid leave will overstate the amount of time mothers spent out of the workforce.

Due to the data limitations described above, there is similarly no direct evidence on the take-up rate of leave among parents that were eligible for the reform. However, evidence from a few sources suggests that the take-up of the 18 weeks of paid leave was close to 100% and taken almost exclusively by the mother (Rønsen and Sundström, 2002). Rønsen and Sundström (1996) show that among mothers giving birth between 1968-88, almost no mothers took less than 4 months of leave. Furthermore, research on later expansions of paid leave in Norway show that the take-up among mothers was close to 100% (Carneiro et al., 2015; Dahl et al., 2016).

1.5.2 Average Effect on Mother's Earnings Five Years Post-Birth

In order to understand the effects on siblings' schooling and fertility choices, it is important to first understand what, if any, effects the introduction of maternity leave had on mothers' careers and home lives. Table 1.2 shows the estimated effects on mothers' earnings and participation five years after the reform. Only mothers who earned more than 10,000 kroner in 1976 are included. As shown in Panel (a), relative to mothers who gave birth immediately before the reform, mothers who gave birth after the reform, and thus had universal access to paid leave, were equally likely to be employed five years later and appear to have similar earnings on average.[12] Of course, it is possible that any effects of paid leave on mothers' employment and earnings are shorter lived. In Panel (b), I look at the effect of the reform on

12. This is consistent with Carneiro et al. (2015), who estimate that the reform had no impact on maternal labor supply or earnings 5 years after the birth of a child. The authors also find no effect of the reform on completed fertility or divorce.

mothers' cumulative earnings over five years and on the number of years in which they have positive earnings as a proxy for cumulative work experience. I find no statistically or economically significant effect on either work experience or cumulative earnings. Furthermore, I do not find any evidence of statistically or economically significant effects on mothers' earnings, participation, or cumulative work experience 20 years later, as shown in Table A.1 in the Appendix.[13]

1.5.3 Heterogeneity in Effect on Mother's Earnings by Pre-Reform Characteristics

While I do not find any evidence that the introduction of paid maternity leave had any effect on mothers' earnings or participation on average, it is possible that this zero average effect may hide considerably heterogeneity in the treatment effect on mothers. To test whether the reform benefited some mothers and/or harmed others, I estimate the following fixed effect specification, in which I interact a treatment indicator with a vector of mothers' pre-reform characteristics:

$$y_i = \alpha_0 + \alpha_1 X_i + \alpha_2 D_i + \boldsymbol{\alpha}_3' D_i \boldsymbol{X}_i + \epsilon_i \tag{1.4}$$

where y_i denotes the mother's earnings five years post-reform, X_i denotes a vector of pre-reform characteristics, and D_i is an indicator variable equal to 1 if the mother gave birth after July 1, 1977, and 0 otherwise. In the vector of pre-reform characteristics, I include the mother's age at the birth, whether it was her first birth, her 1976 income and the income differential with her partner, her county of residence, her educational level and her broad field of study.[14] The estimates of the coefficients α_2 and α_3 are shown in Table A.2 in the

13. It is important to note, however, that mothers in the control groups would have been eligible for paid leave following any subsequent births. Thus, this result should not be interpreted as the effect on long term career outcomes of having paid leave following all births relative to never having access to paid leave.

14. Educational levels include lower secondary education or less, upper secondary education, or higher education. Broad field is an interaction between level (e.g., upper secondary, bachelors, masters) and field

Table 1.2: Effect of Introduction of Paid Maternity Leave on Mother's Short-Term Labor Market Outcomes

(a) Earnings and Participation 5 Years Later

	Any Annual Earnings		Real Annual Earnings (USD)	
	(1) FE	(2) GL	(3) FE	(4) GL
Child Born after July 1	-0.00881	-0.00731	-40.10	208.8
	(0.00939)	(0.0119)	(285.6)	(363.9)
Observations	7651	18658	7651	18658
Mean Outcome in Control Group	0.773	0.777	14336.0	14660.4

(b) Cumulative Earnings and Years of Participation 1978-1982

	Years with Positive Earnings		Cumulative Earnings	
	(1) FE	(2) GL	(3) FE	(4) GL
Child Born after July 1	-0.0215	-0.0306	-156.0	710.2
	(0.0359)	(0.0454)	(1186.9)	(1498.7)
Observations	7651	18658	7651	18658
Mean Outcome in Control Group	3.795	3.824	67433.6	69463.4

Heteroskedasticity-robust standard errors shown in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Table shows the estimated effects of having a child born after the introduction of maternity leave on mothers' labor market outcomes. The sample consists of all mothers who earned above 10,000 kroner in the year prior to the reform and who gave birth in the months surrounding the reform. Panel (a) shows the estimated effect on the probability of having any earnings and on real annual earnings in 1982, while panel (b) shows the estimated effect on the number of years with positive earnings between 1978-1982 and on cumulative real earnings during the same period. Columns (1) and (3) estimate the effects using the fixed effect specification, while columns (2) and (4) estimate the global linear regression discontinuity specification. All earnings are shown in real 2015 USD. Heteroskedasticity-robust standard errors shown in parentheses.

Appendix. The objective is simply to determine whether some groups of mothers - e.g., high

of study (e.g., health, welfare, and sport; general studies; business administration). I include indicator variables for the top 10 most common broad fields, and group the remaining fields (8.7% of mothers) into "Other". The most popular fields are lower secondary general subjects (22.2%), upper secondary business and administration (13.9%), upper secondary general subjects (13.9%), and upper secondary health, welfare, and sport (10.8%). Ideally, I would include the mother's 1976 occupation, but unfortunately data on occupation is not available from this period. For this reason, I include field of study as a proxy for occupation.

earnings, first-time mothers, women who studied business - benefited more or less from the introduction of maternity leave (in terms of earnings five years later). This may be the case, for example, if discrimination against mothers who take leave was greater in some sectors or regions than in others, or if skills depreciated faster with time out of the labor market in certain fields.

To test for the existence of heterogeneity in the treatment effect according to the mother's pre-reform characteristics, I conduct an F-test on the null hypothesis that the vector $\boldsymbol{\alpha}_3 = \mathbf{0}$, versus that alternative hypothesis that at least one element $\alpha_{j,3} \neq 0$. I am able to reject the null hypothesis at the 10% confidence level, with a p-value of 0.06.

Next, I define the predicted effect on mother's earnings as:

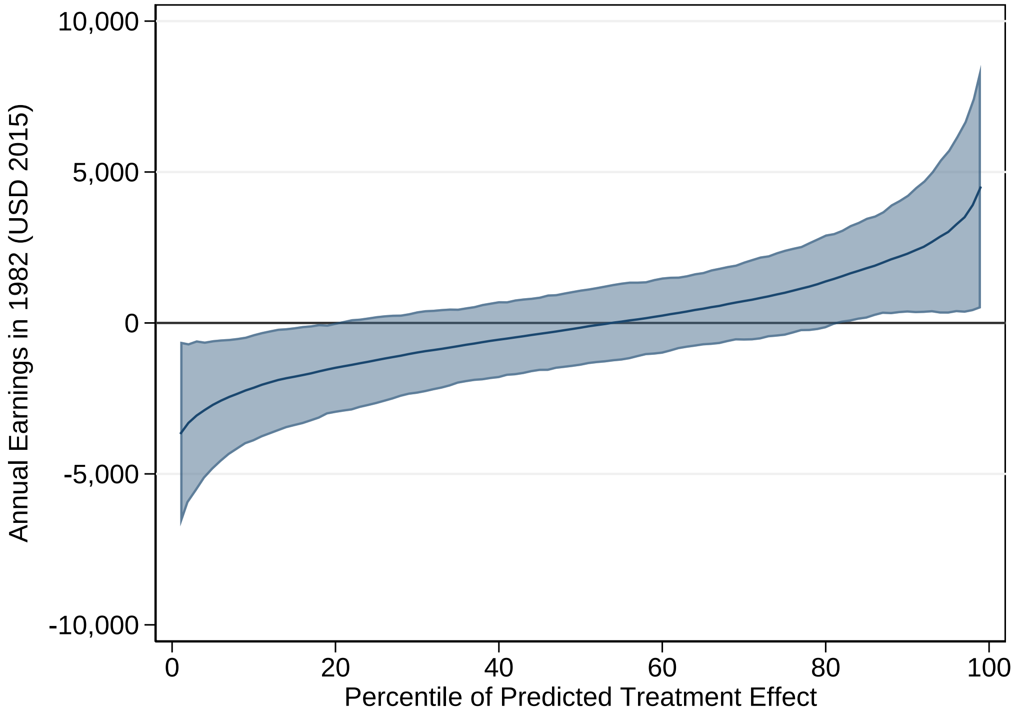
$$\hat{y}_{1i} - \hat{y}_{0i} = \hat{\alpha}_2 + \hat{\boldsymbol{\alpha}}_3' \boldsymbol{X}_i \tag{1.5}$$

Following Chernozhukov et al. (2018), I sort mothers based on their predicted effect, and graph the predicted effect on the mother's earnings (in dollars) as a function of her percentile of the predict effect distribution, shown in Figure 1.2. The shaded area represents the 95% confidence interval, which is calculated following the bootstrap algorithm described in Chernozhukov et al. (2018).[15] We can see that the median predicted effect is close to 0, and the predicted effects are fairly symmetric around the median. Approximately 20% of mothers are estimated to have experienced higher earnings because of the reform, with an estimated increase in annual earnings of USD 2,290 on average, while approximately 20% are estimated to have experienced lower earnings 5 years post-birth because of the reform, with an estimated decrease in earnings of USD 2,330 on average.

Mothers whose earnings are predicted to have benefited from the reform tended to have been higher earning pre-reform, have spouses with higher earnings, and be more likely to hold

15. Chernozhukov et al. (2018) derive a functional central limit theorem and a bootstrap function central limit theorem for the empirical sorted partial effects function (shown in this application in Figure 1.2).

Figure 1.2: Sorted Predicted Effect on Mother's Earnings

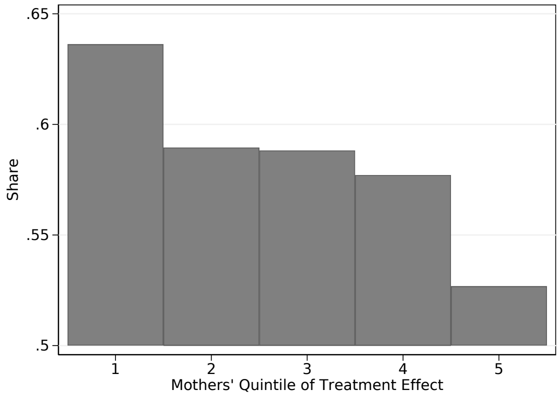


Note: Figure shows the predicted effect of the reform on mother's earnings five years later based on her pre-reform characteristics. The shaded region represents the 95% confidence interval, which is calculated using the bootstrap algorithm described in Chernozhukov et al. (2018).

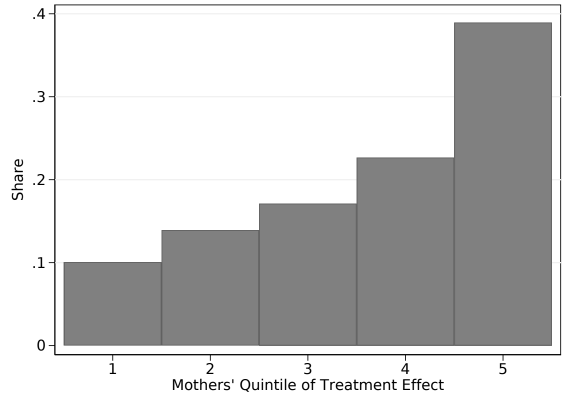
an undergraduate degree than mothers whose earnings are estimated to have been reduced by the reform, as shown in Figure 1.3. Mothers whose earnings were decreased by the reform are more likely to have been first time mothers, suggesting that additional time out of the labor market for women who have not previously returned to work following a birth may have a negative impact on their earnings.

As we will see below, this heterogeneity in the effect of the reform on mothers provides us with variation in the information that young women may have learned from their siblings' experiences. In particular, I can test whether the effect of having a sibling with access to universal paid leave varies depending on whether that sibling's earnings were likely benefited or harmed.

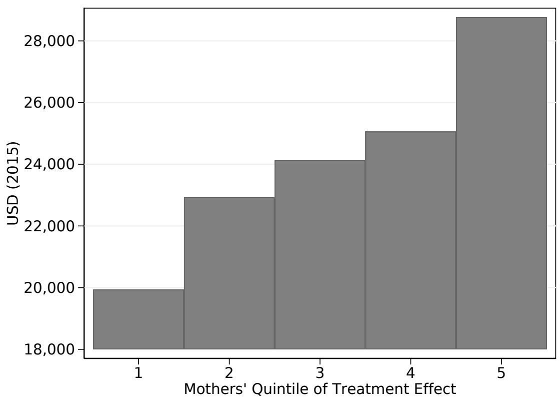
Figure 1.3: Characteristics of Mothers by Predicted Effect of Reform



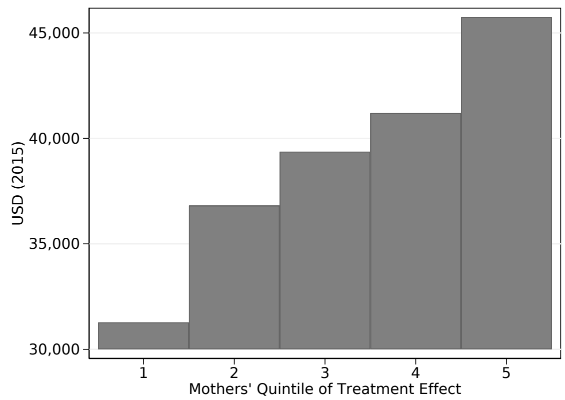
(a) First-Time Mother



(b) Undergraduate Degree or Higher



(c) Income in 1976



(d) Partner's Income in 1976

Note: Figures show the average pre-reform characteristics of mothers according to their quintile of predicted treatment effect.

1.6 Effect on Sisters' Schooling and Fertility

1.6.1 Completed Schooling

Pooled Sample

Table 1.3 shows the estimates of the average effect of having a sibling give birth after the introduction of paid leave on completed schooling. As discussed above, I include only women ages 8-26 years old who were childless at the time, and whose sister or sister-in-law met the income eligibility requirement.[16] After controlling for maternal education, year of birth fixed effects, and fixed effects for sibling's age, I estimate that the reform reduced the share of young women that complete a post-secondary degree or higher by age 35 by between 2.75-4.1 percentage points, from a base of 30-31% in the control group.

Figure 1.1 shows how the share of women who complete post-secondary schooling varies by the date of birth of their niece or nephew. The gray dots show the average in each two week bin, the solid line denotes the local linear estimate, and the gray shaded region denotes the 95% confidence intervals. The vertical line denotes the introduction of the reform, so that the births to the right of the vertical line had the option of paid leave. While there is some variance in the two-week average on either side of the reform, overall there appears to be a downward shift in completed schooling following the introduction of the reform.

In Section 1.6.1 below, I perform the same regression analyses using births from non-reform years and, separately, siblings of ineligible parents as a falsification exercise, and find no similar effect to the one estimated here. In the appendix, I probe the stability of my baseline estimates to alternative specifications. I find that the global linear specification is robust to the choice of estimation window (see Table A.5). I also test for effects on the share of women who complete a bachelors degree (see Table A.6), and find that the effects are negative and similar in magnitude relative to the baseline, although noisier. Finally, I test for any effect on the post-secondary schooling among brothers of men and women who had a baby around the time of the reform, and find no effect (see Table A.7).

Schooling Response by Quintile of Effect on Mother's Earnings

In Section 1.5, we saw that the finding of zero average effect was hiding considerably heterogeneity in how the reform was affecting mothers' earnings over the first few years, with approximately 20% of mothers benefiting from the reform and an equal share being harmed.

16. In all specifications, I include women whose sister or sister-in-law gave birth after July 1. I find no difference in the effect on schooling according to whether the woman is related to the mother or the father.

Table 1.3: Difference in Share of Women with Post-Secondary Schooling by Age 35

	(1) Fixed Effect	(2) Global Linear R.D.	(3) Local Linear R.D.
Niece/Nephew Born After July 1	-0.0275**	-0.0324**	-0.0410*
	(0.0117)	(0.0150)	(0.0243)
Observations	5545	13086	15517
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.311	0.297	0.313

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. Column (1) includes all births in May-August of 1977, column (2) includes all births within 150 days of the reform. In column (3), the bandwidth for the local linear regression is 67 days. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

This begs the question of whether all young women with knowledge of the existence of the reform responded similarly, or whether the effect of the reform on sisters' schooling varied across subgroups or even according to the mother's experience. For example, sisters and sisters-in-law may have learned different things about how the reform would affect women's careers. Other pre-reform characteristics may have interacted with the reform in different ways: for example, siblings that reside closer to one another may have a better understanding of each others' experiences with parenthood.

I begin by looking at whether the effect on completed schooling varied with the effect on the mother's earnings, using the estimated sorted effects presented in Section 1.5. Table 1.4 shows the estimated effect on the share of women who complete a post-secondary degree by age 35 of having a sister or sister-in-law give birth after the introduction of paid leave, by quintile of the estimated effect on the sister's earnings five years after giving birth (bottom 20%, 20th-80th percentiles, top 20%). There is no statistically significant effect on completed schooling among women whose sisters' earnings were decreased or unaffected by the reform (below the 80th percentile). In contrast, there is a very large and negative decrease in completed post-secondary schooling among women whose sisters' earnings increased with the reform (-8 percentage points). In other words, the reduction in completed schooling among sisters of women giving birth around the time of the reform is entirely driven by the sisters (and sisters-in-law) of women whose earnings five years post-birth were increased by having access to paid leave. Women whose sisters' earnings were harmed by the introduction of leave did not respond with any change in their completed post-secondary schooling.

The result in Table 1.4 suggests that young women in the process of making important life decisions are observing their siblings' experiences and using these observations to form expectations, preferences, or both. Presumably all the women with a niece or nephew born after July 1 were aware of the introduction of paid leave, regardless of whether their sister was harmed by or benefited from the reform. Yet their response to the reform appears to have been quite different, suggesting that there was information that was relevant for young women's career decisions beyond simply the existence of paid leave. In particular, the effect of paid maternity leave on mothers' careers may have been unknown or private information, and so having a close relative take paid leave, and then observing their experiences in the following years, may have been an informative event.

Table 1.5 further explores this possibility by looking at whether the effect on completed schooling varied with the siblings' geographic proximity. Roughly half of the women in the sample resided in the same municipality as their sibling. As shown in Column (3), the reduction in completed schooling is most pronounced among women who resided in the same municipality as their sibling at the time of the birth, with a reduction in the share completing post-secondary schooling of 4.6 percentage points from a base of 26%.[17] The effect among sisters not residing in the same municipality is also negative but smaller in magnitude, and I cannot reject the null hypothesis that it is equal to zero nor that it is different from the

17. Here, I define a woman as residing in the same municipality as her sibling if she lives in the municipality where the child was born. Since almost all mothers and fathers reside in the municipality where their child was born, the coefficients reported in Table 1.5 are nearly identical if I instead use the residence of the woman's sibling.

Table 1.4: Effect on Sister's Schooling by Percentile of Mother's Predicted Treatment Effect

	Full Sample	By Mom's Sorted Predicted Effect		
	(1)	(2)	(3)	(4)
	Full Sample	Bottom 20%	20th-80th	Top 20%
Niece/Nephew Born After July 1	0.0166	0.0118	-0.0192	-0.0807**
	(0.0291)	(0.0294)	(0.0159)	(0.0297)
Born after 7/1 x Mom in 20th-80th Percentile	-0.0370			
	(0.0332)			
Born after 7/1 x Mom in Top 20%	-0.0947**			
	(0.0413)			
Controls	Yes	Yes	Yes	Yes
Observations	4831	984	2910	937
Mean Outcome in Control Group		0.320	0.287	0.360

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

effect of those living in the same municipality. However, overall, the results of Tables 1.4 and 1.5 strongly suggest that the effect of the reform on working mothers was not perfectly understood at the time, and witnessing a mothers' experience following the introduction of paid leave was an important source of information for young women as they made important investments in their future careers.

Placebo Results

Before discussing why the introduction of paid maternity leave could have a negative effect on young women's completed schooling, it is important to consider any possible threats to identification. In particular, could the empirical strategy actually be picking up some spurious correlation between a niece or nephew's date of birth and one's own completed schooling? There are three potential causes for concern, which I discuss and test for below.

Table 1.5: Effect on Schooling by Geographical Proximity to Sibling

	(1) Full Sample	(2) Different Municipality	(3) Same Municipality
Niece/Nephew Born After July 1	-0.0438**	-0.0236	-0.0457**
	(0.0211)	(0.0212)	(0.0211)
Born after 7/1 x Different Municipality	0.0184		
	(0.0299)		
Controls	Yes	Yes	Yes
Observations	13069	6791	6278
Mean Outcome in Control Group		0.331	0.259

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Coefficients are estimated using the global linear regression discontinuity specification, and include different slopes on either side of the reform data for each sub-group. Heteroskedasticity-robust standard errors shown in parentheses.

The first concern, discussed in Section 1.4, is that there may be some seasonality in births, meaning that parents that give birth in the months preceding July may be different from those that give birth in July and the following months. If this is the case, then their siblings may be different as well. One way to test for this type of seasonality is to test for any effect of having a niece or nephew born after July 1 in the surrounding years. This would be informative about any differences between those who give birth in the first versus second half of the year, and that are constant across years. I test for any effect in 1975 and 1978, using all the same selection criteria as in the baseline sample.[18]

A second possible concern is that the introduction of paid parental leave was accompanied

18. As in Carneiro et al. (2015), I exclude 1976 from this exercise because of an abortion reform that was passed in early 1976, making it easier to obtain an abortion in the first 12 weeks of pregnancy. The first cohort that would have been potentially affected by the abortion reform was born in July 1976, creating the potential for differences in the characteristics of families that give birth immediately prior to and following July 1976.

by other policy changes affecting parents. If this were the case, then the empirical strategy could be picking up the effect of some reform other than the paid parental leave, and testing for differences in the surrounding years would not be informative about differences between the treatment and control groups in 1977. To test for any differences between families giving birth before and after July 1, 1977, other than the introduction of paid leave, I estimate whether there was any effect of having a niece or nephew born after July 1 among the sisters of parents who did not meet the income requirement for maternity leave. Primarily, these are the sisters and sisters-in-law of women who did not work in the year prior to giving birth, and so would not have been eligible for leave. If there were no other policies affecting parents enacted at this time, these parents should not have been differentially affected by the reform, and so their should be no spillover effect on their sisters' educational attainment.

Table 1.6 shows the placebo estimates from the global linear specification.[19] Column (1) shows the estimated placebo effect among the sisters of ineligible parents, while columns (2) and (3) show the estimated placebo effect from 1975 and 1978 among sisters parents who were or would have been eligible based on their prior earnings. There is no statistically significant effect of having a niece or nephew born after July 1 among sisters of ineligible parents in 1977 (estimated coefficient of 0.01). There is also no evidence of a difference in schooling outcomes among the aunts of children born before and after July 1, 1975 (estimated coefficient of 0.006). There does appear to be an *increase* in schooling of 2.5 percentage points (significant at the 10% significance level) among aunts of children born after July 1, 1978 compared to before, and so, if anything, suggests that any seasonality effects would have

19. Throughout the placebo analysis, I will focus on the global linear specification for simplicity. As shown in Table 1.3, the three specifications produce estimated average effects on sisters' schooling that are roughly similar in magnitude, with the local linear RD estimate being greater (in absolute value) than the global linear RD estimate, which is in turn greater than fixed effect estimate. One can see, however, that the local linear RD estimator is less precise. Furthermore, visual inspection of Figure 1.1 reveals that the local linear estimated trends appear to be roughly globally linear on either side of the cutoff. For this reason and for ease of exposition, I will focus on the global linear RD estimates for the remaining results, since these appear to be more precise and not qualitatively different. However, I have include the estimates from the other two specifications in the Appendix.

resulted in an increase in educational attainment in response to the reform, not a decrease, as reported in Table 1.3.

Table 1.6: Placebo Tests: Difference in Share of Women with Post-Secondary Schooling by Age 35

	(1) Non-Eligible	(2) 1975	(3) 1978
Niece/Nephew Born After July 1	0.0116	-0.00635	0.0250*
	(0.0148)	(0.0147)	(0.0145)
Observations	11908	12719	14188
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.235	0.264	0.291

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Column (1) shows the change in completed schooling around the reform's introduction (July 1, 1977), among sisters of parents who were *not* eligible for maternity leave based on their prior year's earnings. Columns (2) and (3) show the estimated effect of having a niece or nephew born after July 1 in the years in which there was no change in the maternity leave policy, among sisters of parents who were (or would have been) eligible for leave based on their prior year's income. The sample includes sisters who were not already parents themselves and were between 8 and 26 at the time. All columns estimate the global linear regression discontinuity specification and control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Figure 1.4 shows the two-week averages of the share of women who complete post-secondary schooling and the local linear regression plots for each of the three placebo groups. Upon visual inspection, we can see that no group shows the same downward shift in the share of women who complete post-secondary schooling following July 1.

The third potential concern is that the relationship between niece or nephew's date of birth and one's own schooling outcomes is very noisy, and my estimates have simply detected random month-to-month variation. To test this, I estimate 43 placebo effects, one for each month between March 1975 and November 1978, by redefining the start of the reform to be the first of the month. I then comparing the outcomes of siblings of eligible parents who gave birth prior to and following the placebo reform. Figure 1.5 shows the resulting distribution of estimated placebo effects. The estimated treatment effect for July 1, 1977 is denoted by the dark gray vertical line. As we can see, the change in completed schooling surrounding the introduction of paid leave lies in the lower tail of the distribution. Of the 43 estimated placebo effects, only 3 are smaller than the estimated treatment effect.

In summary, it seems that the estimated negative effect on women's completed schooling is greater than what one would expect in the absence of a reform. In particular, the change in women's outcomes is greater than what one would expect from normal month-to-month variation. Furthermore, there is no strong evidence of seasonality that needs to be differenced out from the treatment effect in 1977, nor of any other policy that would have differentially affected families giving birth after July 1 of 1977 relative to those giving birth in the prior months.

1.6.2 Timing of Fertility and Single Parenthood

Before discussing the possible mechanisms behind the fall in post-secondary schooling among those whose sister or sister in law had access to paid leave, it is first useful to explore what other outcomes may have been affected, to get a more complete picture of the effect of the reform on the parents' sisters.

Pooled Sample

Table 1.7 shows the estimated effect of having a sibling with access to paid leave on various fertility outcomes and the probability of being a single parent. Here I have show the estimated coefficients from the fixed effect specification. The estimated coefficients from the global linear regression discontinuity specification, shown in Table A.10 in the Appendix, are similar in magnitude and statistical significance. Column (1) shows the effect on the probability of having a child by age 25 among women who were 18 and under in 1977. Relative to women whose sister or sister-in-law gave birth immediately prior to the reform, women whose sister gave birth after the reform were 4.9 percentage points more likely to have a child by the age of 25, from a base of 46% in the control group. Column (2) shows the probability of being a single parent, where single parenthood is defined as (i) having a child by the age of 45, and (ii) being single - as opposed to married, separated, divorced, or partnered (an official status in the data) - at the time of the first birth. Women who do not have children by age 45 are included and coded as never being a single parent. Here, I return to the full sample (ages 8-26 in 1977). I estimate that having a sibling give birth after the reform increased the probability of being a single parent by 3 percentage points, from a base of 25% in the control group.

Table 1.7: Effects on Fertility and Single Parenthood

	(1) Any Children by Age 25*	(2) Single Parent	(3) Any Children by Age 45	(4) Number of Children by 45
Niece/Nephew Born After July 1	0.0491**	0.0302**	-0.00565	0.0154
	(0.0180)	(0.0112)	(0.00913)	(0.0335)
Controls	Yes	Yes	Yes	Yes
Observations	3059	5706	5745	5745
Mean Outcome in Control Group	0.461	0.254	0.861	2.029

Heteroskedasticity-robust standard errors shown in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves. The sample in column (1) is restricted to those under age 19 at the time of the reform, whereas columns (2)-(4) includes those between 8 and 26 years old. Single parent is defined as being single at the time of first birth, and women who never have children are included as never being a single parent. All columns use the fixed effect regression discontinuity specification, and control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling. Heteroskedasticity-robust standard errors shown in parentheses.

Columns (3) and (4) look at whether having a sibling with access to paid leave had any effect on completed schooling. I find no economically or statistically significant effect on the share of women that have any children by age 45 nor on the average number of children by age 45.

It is important to interpret the results in columns (3) and (4) with a degree of caution. The lack of a detected effect in columns (3) and (4) should not be interpreted as proof that the introduction of paid maternity leave and extension of job protection had no effect on the probability of ever having a child or the total number of children. It is possible that both the control and treatment groups (women whose siblings gave birth before and after July 1) eventually learned of the existence of the leave reform and responded in the same way, simply at different times. (Note that completed fertility is measured at age 45.) Rather, drawing on the results for all four outcomes, I interpret these results as indicating that *early awareness* of the existence of guaranteed paid maternity leave (before the initiation of childbearing) induced women to begin having children earlier than they otherwise would have.

Fertility Response by Quintile of Effect on Mother's Earnings

Tables 1.8 and 1.9 show the estimated effects on select fertility and marriage outcomes by quintile of the estimated effect on the sister's earnings five years after giving birth (bottom 20%, 20th-80th percentiles, top 20%). Table 1.8 shows the predicted change in the share of women who had a child by age 25. As above, the sample includes only women who were 18 or under at the time of the reform and not already parents themselves. I find that the sisters of women who gave birth after the reform and are predicted to have been most harmed by the introduction of paid leave were not were not significantly more likely to have a child by the age of 25 than the sisters of similar women who gave birth before the reform. Recall that this group also showed no change in completed schooling. In contract, the sisters of women who gave birth after the reform and are predicted to have benefited from the introduction of paid leave were 13 percentage points more likely to have had a child by age 25 than the sisters of similar women who gave birth before the reform. The sisters of women who were neither significantly harmed nor benefited from the reform (20th - 80th percentiles) fell somewhere in the middle: among these women, those whose sister gave birth after the return were 4 percentage points more likely to have a child by age 25, although the estimate is not statistically significantly different from zero.

Table 1.8: Effect on Sister's Probability of Having Child by Age 25 by Percentile of Mother's Predicted Treatment Effect

	Full Sample	By Mom's Sorted Predicted Effect		
	(1)	(2)	(3)	(4)
	Full Sample	Bottom 20%	20th-80th	Top 20%
Niece/Nephew Born After July 1	0.0126	0.0101	0.0402	0.131**
	(0.0432)	(0.0437)	(0.0245)	(0.0459)
Born after 7/1 x Mom in 20th-80th Percentile	0.0245			
	(0.0496)			
Born after 7/1 x Mom in Top 20%	0.126**			
	(0.0623)			
Controls	Yes	Yes	Yes	Yes
Observations	2663	517	1660	486
Mean Outcome in Control Group		0.442	0.488	0.391

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 18 at the time. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Table 1.9 shows the effect of having a sibling give birth after the introduction of paid maternity leave on the probability of being a single parent, among women ages 8-26 at the time of the reform who were not already parents themselves, by quintile of the mother's treatment effect. Again, I find that women whose sisters' earnings were likely harmed by the reform and gave birth after July 1 were no more likely to become a single parent than similar women whose sister gave birth before July 1. However, women whose sisters' earnings were not harmed by the reform *or* benefited from the reform were between 4.4-4.6 percentage points more likely to become a single parent.

Table 1.9: Effect on Sister's Probability of Being a Single Parent by Percentile of Mother's Predicted Treatment Effect

	Full Sample	By Mom's Sorted Predicted Effect		
	(1)	(2)	(3)	(4)
	Full Sample	Bottom 20%	20th-80th	Top 20%
Niece/Nephew Born After July 1	0.0135	0.00942	0.0459**	0.0441
	(0.0260)	(0.0260)	(0.0158)	(0.0270)
Born after 7/1 x Mom in 20th-80th Percentile	0.0317			
	(0.0304)			
Born after 7/1 x Mom in Top 20%	0.0337			
	(0.0373)			
Controls	Yes	Yes	Yes	Yes
Observations	4969	1012	2997	960
Mean Outcome in Control Group		0.231	0.271	0.238

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Being a single parent is defined as (i) having a child by the age of 45, and (ii) being single - as opposed to married, separated, divorced, or partnered (an official status in the data) - at the time of the first birth. Women who do not have children by age 45 are included and coded as never being a single parent. Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

In summary, it appears that having a sibling with access to paid leave induced younger women to have children earlier, with no overall effect on completed fertility or the probability of having children by age 45. Furthermore, among women of all ages, having a sister or sister-in-law give birth after the reform increased the probability of becoming a single parent. Interestingly, the changes in the timing of first birth and in the probability of becoming a single parent are concentrated among those whose sister was predicted to have benefited from the reform (in terms of her earnings five years post-reform). In contrast, women whose sister had a baby around the time of the reform and whose earnings were likely harmed by the reform showed no change in their probability of having a child by age 25 nor of becoming a single parent.

1.7 Mechanisms

In order to better understand how paid maternity leave and similar policies aimed at supporting families and workers could influence young adults' schooling choices in other contexts, we must understand the mechanism behind the reduction in post-secondary schooling following the 1977 reform. Ideally, we may also learn something about how young adults form expectations and preferences, and how they use the experiences of those around them when making life decisions as important as schooling, the timing of fertility, and marriage.

The main schooling results discussed in Section 1.6 beg two important questions. First, why did the introduction of paid maternity leave reduce post-secondary schooling for *any* young women? In other words, what is the link between the existence of paid maternity leave and completed schooling? The second question is why did the sisters of those who gave birth after the introduction of paid leave respond *differently* than the sisters of those who gave birth before and therefore did not have access to paid leave? In particular, since I condition on not already being a parent oneself at the time, both the women in the control group and the treatment group (those whose sibling gave birth before vs. after July 1) would have access to paid leave if and when they eventually began having children. Below, I discuss possible answers to these two questions. While I am not able to conclusively rule out or prove some possibilities, I provide further evidence for a relationship between paid leave and job protection, fertility, and schooling. I also discuss how the heterogeneous effects of the reform on mothers' earnings and the differential response by their sisters illustrate the role of siblings' experiences in how young adults form expectations about the future.

1.7.1 Paid Maternity Leave and Women's Completed Schooling

In this section, I discuss possible explanations for why the existence of paid maternity leave could reduce post-secondary schooling among women who have not yet begun to have children.

One explanation for the decrease in completed schooling is that the introduction of paid maternity leave made motherhood seem more appealing or affordable to young women. In Section 1.6.2, I show that having a sibling with access to paid leave increased the probability of having a child by age 25 by almost 5 percentage points from a base of 46%, while increasing the probability of being a single mother by approximately 3 percentage points from a base of 25%. Both of these results suggest women "shifted up" the initiation of childbearing in response to the introduction of paid maternity leave.

Of course, a change in the timing of first birth alone does not explain a reduction in women's completed schooling. After all, there is considerable overlap in the early childbearing and late education years: as of 1981, the median age of women completing a post-secondary degree in Norway was 24.5, while the median age of mothers at first birth was 23.8.[20] Nevertheless, the arrival of a woman's first child usually marked the end of her schooling: among mothers giving birth for the first time in 1975 (two years before the reform), only 13% would go on to complete a degree within the next ten years. Furthermore, as shown below, there is a strong correlation between having one's first child before age 25 and lower post-secondary schooling.

An important feature of the reform could further explain the link between increased fertility before age 25 and reduced post-secondary schooling surrounding the reform: in order to be eligible for paid maternity leave, a mother had to be working for 6 of the 10 months immediately prior to the birth, and had to have annual earnings in excess of 10,000 kroner

20. I report the age distribution of graduates in 1981 since this is the first year of highly reliable data for graduation date.

(equivalent to approximately US$5,700 in 2020). In other words, the work requirement for paid leave would have increased the opportunity cost of being enrolled relative to working for women who were on the brink of childbearing.

To quantify how much of the reduction in schooling can be explained by the increase in fertility before age 25, I follow Heckman et al. (2013) in applying mediation analysis. This analysis allows me to disentangle the direct effect of having a sibling with access to paid leave on completed schooling, from the indirect effect on schooling arising from the effect on the timing of fertility. For simplicity, I suppress the individual i index. As above, let D denote whether a woman had a sibling with access to paid parental leave. Let Y_1 denote the potential schooling outcome of a woman if her sibling had access to parental leave, and Y_0 denote the potential schooling outcome if her sibling did not. My analysis is based on the following linear model:

$$Y_d = \tau_d + \alpha_d \theta_d + \boldsymbol{X}' \beta_d + \epsilon_d \tag{1.6}$$

where τ_d is a treatment-specific intercept, θ_d is the potential fertility outcome (whether or not the woman gave birth before age 25) if she receives treatment d, $\boldsymbol{X}$ is a vector of pre-treatment variables (own year of birth fixed effects, sibling's year of birth fixed effects, and own mother's completed schooling), and ϵ_d is an error term. There are a few important things to note. First, this model assumes the pre-treatment controls, $\boldsymbol{X}$, cannot be affected by the treatment, although their effect on schooling (β_d) may vary with treatment status. Similarly, the effect of early fertility on schooling (α_d) can be affected by the treatment. Finally, we should think of the error term ϵ_d being comprised of two components: a pure error term that is uncorrelated with $\boldsymbol{X}$ and θ, plus the unmeasured mediators, or any other intermediate outcomes that were affected by having a sibling with access to paid leave and that also play a role in completed schooling. The key identifying assumption is that these unmeasured mediators are uncorrelated with both the controls ($\boldsymbol{X}$) and early fertility (θ) for all values of the treatment variable. This assumption allows us to identify the parameters of the above model (Heckman and Pinto, 2015).

The last step is to specify a model θ_d:

$$\theta_d = \mu_0 + \boldsymbol{X}'\mu_1 + \mu_2 d + \eta \tag{1.7}$$

(Note this is the same empirical specification I estimated in Section 1.6.2.)

For simplicity of exposition, let us assume for now that $\alpha_1 = \alpha_0 = \alpha$ and $\beta_1 = \beta_0 = \beta$, that is, that the effect of early fertility and covariates on schooling does not depend on the treatment variable. Let us substitute the model of fertility (equation 1.7) into the model of schooling (equation 1.6):

$$Y_d = \tau_0 + \tau d + \alpha\left(\mu_0 + \boldsymbol{X}'\mu_1 + \mu_2 d + \eta\right) + \boldsymbol{X}'\beta + \epsilon_d$$

Then we can decompose the average treatment effect associated with having a sibling with access to paid maternity leave as the sum of the direct effect on schooling and the indirect effect associated with the change in early fertility:

$$E\left[Y_1 - Y_0\right] = \underbrace{\tau}_{\text{Direct Effect}} + \underbrace{\alpha\mu_2}_{\text{Indirect Effect}} \tag{1.8}$$

Under the assumption that $\alpha_1 = \alpha_0 = \alpha$ and $\beta_1 = \beta_0 = \beta$, I estimate the parameters needed for the decomposition by first estimating the following model by OLS:[21]

$$Y = \tau_0 + \tau D + \alpha\theta + \boldsymbol{X}'\beta + \epsilon \tag{1.9}$$

21. Alternatively, I can consider the case $\alpha_0 \neq \alpha_1$ and $\beta_0 \neq \beta_1$ by simply interacting the indicator variable for fertility and the vector of controls with the treatment variable and estimating by OLS:

$$Y = \tau_0 + \tau D + \alpha_0\theta + (\alpha_1 - \alpha_0)\,\theta D + \boldsymbol{X}'\beta_0 + \boldsymbol{X}'\,(\beta_1 - \beta_0)\,D + \epsilon$$

The final step is simply to estimate the model of fertility (equation 1.7) by OLS.

The estimated coefficients from equation (1.9) are shown in Tables A.11 and A.12 in the Appendix. There is a strong negative correlation between early fertility and completed schooling: compared to women born in the same year, and controlling for maternal education, treatment status, and siblings' age, women who have their first child before the age of 25 are approximately 20 percentage points less likely to complete a post-secondary degree. The p-value on the F-test that $\alpha_1 - \alpha_0 = \beta_1 - \beta_0 = 0$ is 0.14 (in the full sample) and 0.22 (for those 18 years old and younger). For this reason, below I have shown only the results that impose the assumption that $\alpha_1 = \alpha_0 = \alpha$ and $\beta_1 = \beta_0 = \beta$, although I have included the full results in the Appendix.

Table 1.10 shows the results of the mediation analysis for both the full sample (ages 8-26) and for younger women (ages 8-18). I find that the reform's effect on the share of women having children before age 25 is responsible for 14% of the reduction in post-secondary schooling for the full sample, and for 36% of the reduction in schooling among those 18 years old or less at the time of the reform.

Table 1.10: Share of Change in Schooling Attributable to Changes in Timing of Fertility

	Direct Share	Indirect Share
All	0.86	0.14
$<$ 19 Years Old	0.64	0.36

Note: Table shows the share of the average effect of having a sibling with access to paid leave on completed schooling that can be attributed to changes in the share of women having a child before age 25 ("Indirect Share"). The direct and indirect shares are calculated according to equation (1.8), and are estimated for the full sample (childless sisters ages 8-26 of eligible parents giving birth in May-August of 1977) and for just those under age 19 at the time.

In sum, the introduction of paid maternity leave could have made motherhood seem more appealing or affordable, inducing some women to begin having children earlier than they otherwise would have. This is consistent with the increase in the share of women having children before age 25 and in the share of women becoming single mothers reported in Section 1.6.2. This, together with an increase in the opportunity cost of enrollment as a result of the work requirement for paid maternity leave, could explain the notable reduction in post-secondary schooling associated with having a sibling give birth after the introduction of the reform.

1.7.2 Spillover Effects on Siblings

One important question remains regarding the results discussed in Section 1.6: why would sisters of parents giving birth after the reform respond differently than sisters of parents giving birth before the reform? Put differently, if the introduction of paid maternity leave made motherhood seem more affordable or appealing, why would the control and treatment groups not respond in the same way, given that both would be eligible for paid leave in the future? Here, I discuss two possibilities and the evidence for each.

The first possible explanation is that the existence of the reform that introduced paid maternity leave was not well known, in particular among young women and teenagers who were not considering having children in the near future. There is survey evidence to suggest that maternity leave legislation is not highly salient to the general population, albeit from a different context: according to a 2015 poll of voters in California, only 36% of voters were aware of California's Paid Family Leave policy, which had been in effect for eleven years (DiCamillo and Field, 2015).

If the existence of the leave reform was not widely known, then young women whose siblings gave birth after the introduction of the reform would have a better understanding of the options for paid leave as well as the eligibility requirements. Recall from Table 1.5 that women who resided in the same municipality as their sibling reduced the probability of completing a post-secondary degree by almost twice as much as women who resided in a different municipality from their sister. If sisters living close by speak more often and share more details, then siblings living closer would presumably have a better understanding of the paid leave policy.

A second possible explanation is that the effect of the reform on women's careers was not yet known at the time. For example, it may not have been known whether women would face discrimination as a result of taking a longer leave, resulting in lower earnings and promotions after returning; or whether women would be less likely to return to work after taking more time away. Alternatively, paid maternity leave and extended job protection could have increased job continuity, increasing the share of new mothers that return to work and increasing their earnings following the birth of a child. In either case, if the effect of paid maternity leave on mothers' careers was not generally understood at the time of the reform or was private information for a few years, then having a close relative take paid leave and observing their experiences may have been an informative event for young women. In fact, as shown in Section 1.5, the effect on earnings five years post-reform varied considerably across mothers, with roughly one-fifth of mothers experiencing a significant decline in earnings, and one-fifth experiencing a significant increase.

Accordingly, the response to having a sibling give birth after the introduction of paid leave varied considerably across young women. As shown in Table 1.4, sisters and sisters-in-law of women who were predicted to have benefited from the reform decreased their post-secondary schooling by the most, whereas the sisters of women whose earnings were predicted to have been decreased by the reform showed no decrease in their post-secondary schooling (and in fact, I estimate a small increase in the share that complete a post-secondary degree, although the effect is not statistically significant). Recall the sisters of mothers who benefited from the reform also showed the greatest change in the timing of fertility, with an 13 percentage point increase in the share having a child before age 25. Together, these results strongly suggest that women who anticipated that the reform would benefit working mothers were the ones who made the greatest changes in their fertility and schooling decisions.

Ultimately, the evidence presented in this paper does not definitively disentangle these two possibilities: that the existence of the reform itself was not well-known among young women who were not yet having children, or that the effects of the reform on mothers were not well known. Of course, these two possibilities are not mutually exclusive: there may have been a lack of knowledge about both the existence of the reform and its effects on mothers' careers, and having a sibling give birth after the introduction of the reform may have been informative for young women in both regards. In either case, the differential response of young women to the introduction of paid leave according to whether their sister gave birth immediately before or after the reform highlights the significant role that siblings' experiences play in forming young adults' expectations, preferences, or both, and subsequently in their fertility and schooling decisions.

1.8 Conclusion

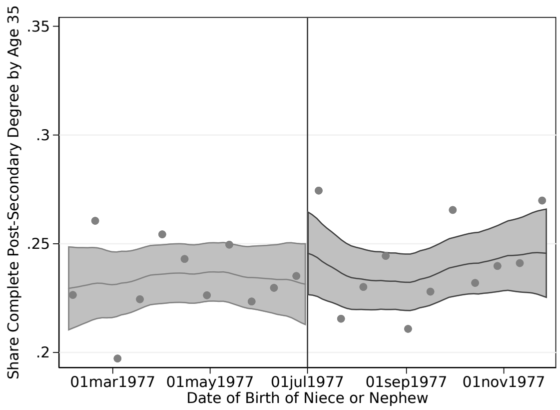
This paper uses administrative data and quasi-experimental variation in access to paid maternity leave and extended job protection to study the effect of maternity leave on young women's schooling choices. By comparing the sisters of women who gave birth immediately prior to and following the introduction of a maternity leave reform in Norway, I shows that the sisters of women who had access to universal paid maternity leave and extended job protection were less likely to complete a post-secondary degree, and more likely to have a child before age 25 and to be a single mother. Mediation analysis shows that 14-36% of the decrease in post-secondary schooling can be attributed to the increased probability of having a child before age 25. I interpret this as evidence that expecting to have access to paid maternity leave and a long period of job protection made motherhood seem more attractive or affordable, inducing women to have children at a younger age. The change in the timing of childbearing, coupled with the work requirements for paid leave, likely interfered with or discouraged post-secondary schooling.

I also find that while the reform appears to have had zero effect on average on mothers' earnings 5 years post-birth, there was considerable heterogeneity in how the reform impacted mothers, with approximately 20% of mothers having statistically significantly higher earnings because of the reform and approximately 20% having significantly lower earnings as a result. The effect on mothers is highly predictive of the change in their sisters' schooling, with the greatest changes in completed schooling and timing of fertility among the sisters of women whose earnings benefited from the reform, and no change among the sisters of women whose earnings were harmed. This finding suggests that the effects of the reform on mothers' careers were not well-known at the time, and so young women used their sisters' experiences with leave to inform their beliefs about working motherhood and leave taking.

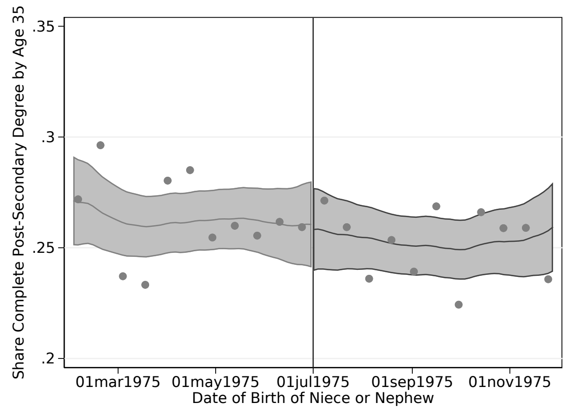
These findings highlight the role of siblings' experiences in young adults' formation of expectations and preferences. Previous literature has illustrated how an older sibling's choice of college or field of study affects a younger sibling's schooling choices. In many of these studies, the proposed mechanism is that older siblings' experiences allow younger siblings to gather information that would have been otherwise difficult to come by. In this context, having a sibling give birth after the introduction of paid leave and extended job protection may have provided young women with additional information about the existence of the policies and their effect on women's careers.

More generally, this paper attempts to contribute to our knowledge of how young adults, and young women in particular, form expectations and preferences about future fertility and work, and how these beliefs enter into their investments in schooling.

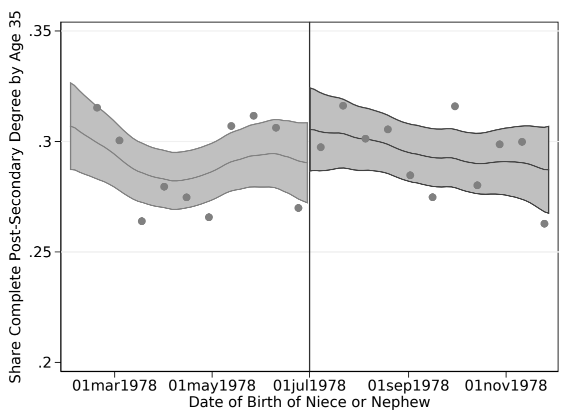
Figure 1.4: Share of Women with Post-Secondary Schooling by Age 35



(a) 1977 - Sisters of Ineligible Parents



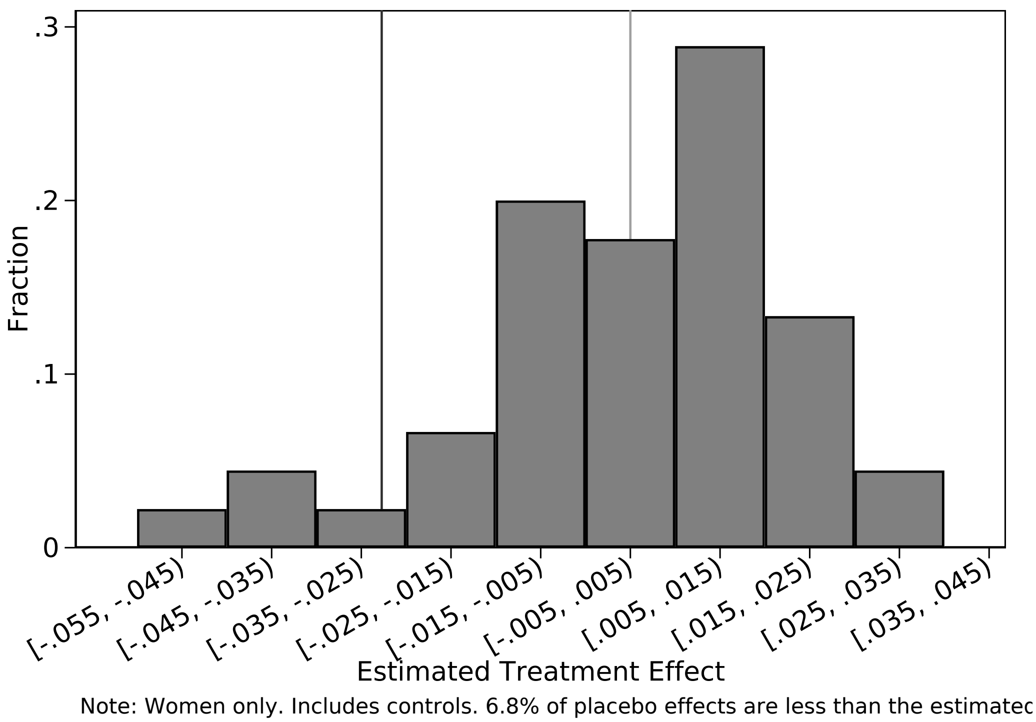
(b) 1975 - Sisters of Eligible Parents



(c) 1978 - Sisters of Eligible Parents

Note: Gray dots indicated the share of women who complete a post-secondary degree by age 35, among those with a niece or nephew born during the indicated two week period. In all panels, I restrict the same to sisters who were not already parents themselves and were between 8 and 26 at the time. The light and dark gray lines indicate the estimated local linear trend in completed schooling. 95% confidence intervals for the local linear fit are shown in gray. The vertical line indicates July 1.

Figure 1.5: Histogram of Placebo Effects on Women's Schooling using Global Linear RD Specification



Note: Figure shows the distribution of estimated placebo effects on women's completed schooling, using the global linear regression discontinuity specification. Of the 43 placebo effects, only 3 (6.8%) are smaller than the estimated effect on July 1, 1977, which is indicated with the dark gray line.

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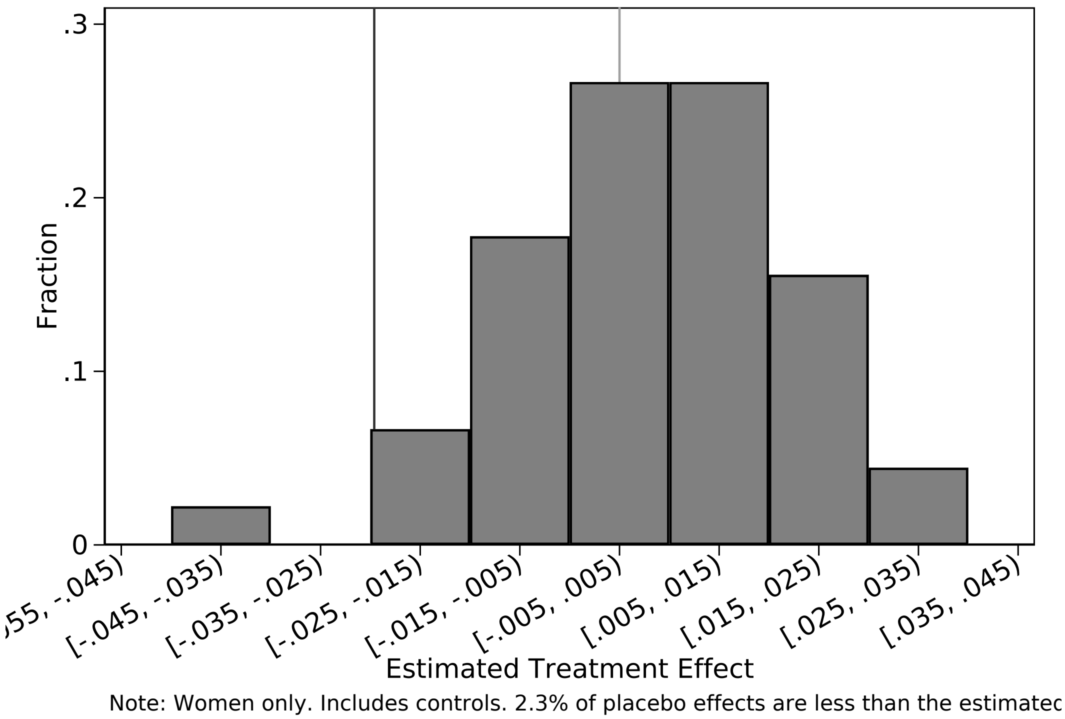
APPENDIX A

ADDITIONAL TABLES AND FIGURES

Figure A.1: Histogram of Placebo Effects on Women's Schooling using Fixed Effect Specification



Note: Figure shows the distribution of estimated placebo effects on women's completed schooling, using the fixed effect specification (with controls). Of the 43 placebo effects, only 1 (2.3%) are smaller than the estimated effect on July 1, 1977, which is indicated with the dark gray line.

Table A.1: Effect of Introduction of Paid Maternity Leave on Mother's Long-Term Labor Market Outcomes

(a) Earnings and Participation 20 Years Later

	Any Annual Earnings		Real Annual Earnings (USD)	
	(1) FE	(2) GL	(3) FE	(4) GL
Child Born after July 1	0.000795	0.00235	14.92	126.5
	(0.00622)	(0.00790)	(391.2)	(510.2)
Observations	7651	18658	7651	18658
Mean Outcome in Control Group	0.918	0.920	33801.5	33826.5

Heteroskedasticity-robust standard errors shown in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .001$

(b) Cumulative Earnings and Years of Participation 1978-1997

	Years with Positive Earnings		Cumulative Earnings	
	(1) FE	(2) GL	(3) FE	(4) GL
Child Born after July 1	-0.0632	-0.0292	-3110.6	-8.956
	(0.0922)	(0.117)	(4849.4)	(6194.3)
Observations	7651	18658	7651	18658
Mean Outcome in Control Group	17.03	17.09	447199.0	450925.1

Heteroskedasticity-robust standard errors shown in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Table shows the estimated effects of having a child born after the introduction of maternity leave on mothers' labor market outcomes. The sample consists of all mothers who earned above 10,000 kroner in the year prior to the reform and who gave birth in the months surrounding the reform. Panel (a) shows the estimated effect on the probability of having any earnings and on real annual earnings in 1982, while panel (b) shows the estimated effect on the number of years with positive earnings between 1978-1982 and on cumulative real earnings during the same period. Columns (1) and (3) estimate the effects using the fixed effect specification, while columns (2) and (4) estimate the global linear regression discontinuity specification. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.2: Effect of Introduction of Paid Maternity Leave on Mother's Earnings in 1982 by Pre-Reform Characteristics

	Income 1982
Treatment (Child Born after July 1)	401.8
	(2324.1)
First Birth × Treatment	-485.2
	(729.9)
Age at Birth × Treatment	-111.6
	(77.70)
Upper Secondary Education or Higher × Treatment	3226.9
	(4716.4)
Higher Education × Treatment	1097.0
	(2209.7)
Income in 1976 × Treatment	0.0636**
	(0.0271)
Income Differential with Partner × Treatment	0.0288**
	(0.0140)
County (Omitted: Oslo × Treatment)	
Rogaland × Treatment	659.1
	(1188.9)
Møre og Romsdal × Treatment	-143.0
	(1329.6)
Nordland× Treatment	178.9
	(1447.4)

Viken× Treatment	-223.6
	(1055.5)
Innlandet × Treatment	1297.3
	(1343.2)
Vestfold og Telemark × Treatment	624.8
	(1436.9)
Agder × Treatment	-599.9
	(1361.7)
Vestland × Treatment	2697.2**
	(1136.6)
Trøndelag × Treatment	790.2
	(1234.9)
Troms og Finnmark × Treatment	1055.9
	(1507.3)
Degree/Field (Omitted: Lower Secondary, General Education × Treatment)	
Other Fields/Degrees × Treatment	-5599.4
	(4817.6)
Upper Secondary, Basic: General Fields × Treatment	-4277.9
	(4804.1)
Upper Secondary, Basic: Humanities & Arts × Treatment	-3953.4
	(4942.5)
Upper Secondary, Basic: Business & Administration × Treatment	-4504.6
	(4739.0)
Upper Secondary, Basic: Health, Welfare & Sport × Treatment	-5822.5
	(4806.1)

Upper Secondary, Basic: Transport, Communic., Safety × Treat.	-714.6
	(5212.4)
Upper Secondary, Final Year: General Subjects × Treatment	-5425.2
	(4890.7)
Post-Secondary: Business and Administration × Treatment	-1964.8
	(5220.8)
Undergraduate: Teacher Training × Treatment	-5601.8
	(5202.2)
Undergraduate: Health, Welfare & Sport × Treatment	-3349.0
	(5179.0)
Observations	7651
P-value on F-test	0.0600

Standard errors in parentheses

*$p < .1$, ** $p < .05$, *** $p < .001$

Note: Table shows the OLS estimates of α_2 and α_3 from equation 1.4. The dependent variable is the mother's earnings in 1982 (in 2015 USD). The last row contains the p-value from the F-test of the null hypothesis that the vector $\boldsymbol{\alpha}_3 = 0$ versus the alternative hypothesis that at least one interaction term has a coefficient $\alpha_{j3} \neq 0$.

Table A.3: Differences between Mothers in Top and Bottom Quintiles of SPE Distribution

	Difference	LL	UL
First Birth	-0.11	-0.47	0.25
Mom's Age at Birth	0.01	-3.31	3.32
Upper Secondary Educ. or Higher	-0.22	-0.46	0.03
Undergraduate or Higher	0.29	-0.06	0.64
Resides in Oslo	-0.09	-0.38	0.21
Income in 1976	8830	-1206	18866
Partner's Income in 1976	14472	1928	27015

Note: Column (1) shows the differences in characteristics between mothers who most benefited from the reform (top 20% of predicted effects) and mothers who were most harmed by the reform (bottom 20% of predicted effects). Column (1) shows the difference in the mean values of the covariate, while columns (2) and (3) provide the lower and upper limits of the 95% confidence interval, as calculated using the bootstrapping algorithm described in Chernozhukov et al. (2018).

Table A.4: Heterogeneity in Effect of Reform on Schooling by Age in 1977

	(1) All Ages	(2) 18 and Under	(3) Over 18
Niece/Nephew Born After July 1	-0.0420**	-0.0433**	-0.0194
	(0.0198)	(0.0198)	(0.0228)
Born after 7/1 x Over 18 in 1977	0.0198		
	(0.0302)		
Controls	Yes	Yes	Yes
Observations	13086	6968	6118
Mean Outcome in Control Group		0.256	0.343

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Age is as of the date of birth of the niece or nephew. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.5: Sensitivity of Estimated Effect on Women's Completed Schooling to Estimation Window

	(1) 90 Days	(2) 120 Days	(3) 150 Days
Niece/Nephew Born After July 1	-0.0439**	-0.0333**	-0.0324**
	(0.0192)	(0.0167)	(0.0150)
Observations	8134	10641	13086
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.303	0.302	0.297

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. Each column estimates the global linear regression discontinuity equation using a different estimation window. For example, column (1) estimates equation (1.3) using births occurring 90 days before and after July 1, 1977. All columns control for the uncle's year of birth, the parent's year of birth, and the uncle's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.6: Difference in Share of Women with Bachelor's Degree by Age 35

	(1) Fixed Effect	(2) Global Linear R.D.	(3) Local Linear R.D.
Niece/Nephew Born After July 1	-0.0183	-0.0255*	-0.0336
	(0.0114)	(0.0145)	(0.0225)
Observations	5545	13086	15517
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.281	0.267	0.280

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. Column (1) includes all births in May-August of 1977, column (2) includes all births within 150 days of the reform. In column (3), the bandwidth for the local linear regression is 67 days. All columns control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.7: Difference in Share of Men with Post-Secondary Schooling by Age 35

	(1) Fixed Effect	(2) Global Linear R.D.	(3) Local Linear R.D.
Niece/Nephew Born After July 1	-0.00316	-0.0106	-0.0380
	(0.0106)	(0.0135)	(0.0247)
Observations	6684	16083	19008
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.288	0.279	0.294

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes brothers of eligible parents of a child born in 1977, who were not already parents themselves, and were between 8 and 26 at the time. Column (1) includes all births in May-August of 1977, column (2) includes all births within 150 days of the reform. In column (3), the bandwidth for the local linear regression is 52 days. All columns control for the uncle's year of birth, the parent's year of birth, and the uncle's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.8: Placebo Tests: Difference in Share of Women with Post-Secondary Schooling by Age 35

	(1) Non-Eligible	(2) 1975	(3) 1978
Niece/Nephew Born After July 1	0.0111	-0.00570	0.0194*
	(0.0116)	(0.0115)	(0.0113)
Observations	5063	5307	5922
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.236	0.263	0.295

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Column (1) shows the change in completed schooling around the reform's introduction (July 1, 1977), among sisters of parents who were *not* eligible for maternity leave based on their prior year's earnings. Columns (2) and (3) show the estimated effect of having a niece or nephew born after July 1 in the years in which there was no change in the maternity leave policy, among sisters of parents who were (or would have been) eligible for leave based on their prior year's income. The sample includes sisters who were not already parents themselves and were between 8 and 26 at the time. All columns estimate the fixed effect specification and control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.9: Placebo Tests: Difference in Share of Women with Post-Secondary Schooling by Age 35

	(1)	(2)	(3)
	Non-Eligible	1975	1978
Niece/Nephew Born After July 1	0.0700**	-0.00896	0.0442
	(0.0288)	(0.0239)	(0.0274)
Observations	14260	15091	16729
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.229	0.262	0.294

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Column (1) shows the change in completed schooling around the reform's introduction (July 1, 1977), among sisters of parents who were *not* eligible for maternity leave based on their prior year's earnings. Columns (2) and (3) show the estimated effect of having a niece or nephew born after July 1 in the years in which there was no change in the maternity leave policy, among sisters of parents who were (or would have been) eligible for leave based on their prior year's income. The sample includes sisters who were not already parents themselves and were between 8 and 26 at the time. All columns estimate the local linear regression discontinuity specification, with the bandwidth chosen optimally, and control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling as controls. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.10: Effects on Fertility and Single Parenthood

	(1) Any Children by Age 25*	(2) Single Parent	(3) Any Children by Age 45	(4) Number of Children by 45
Niece/Nephew Born After July 1	0.0395*	0.0253*	-0.00771	0.0267
	(0.0228)	(0.0144)	(0.0116)	(0.0416)
Controls	Yes	Yes	Yes	Yes
Observations	7174	13468	13572	13572
Mean Outcome in Control Group	0.412	0.265	0.858	2.026

Heteroskedasticity-robust standard errors shown in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Sample includes sisters of eligible parents of a child born in 1977, who were not already parents themselves. The sample in column (1) is restricted to those under age 19 at the time of the reform, whereas columns (2)-(4) includes those between 8 and 26 years old. Single parent is defined as being single at the time of first birth, and women who never have children are included as never being a single parent. All columns use the global linear regression discontinuity specification, and control for the aunt's year of birth, the parent's year of birth, and the aunt's mother's completed schooling. Heteroskedasticity-robust standard errors shown in parentheses.

Table A.11: Estimated Model of Early Fertility and Completed Schooling (All Ages)

	(1)	(2)	(3)	(4)
	$\alpha_0 = \alpha_1, \beta_0 = \beta_1$	$\alpha_0 \neq \alpha_1, \beta_0 = \beta_1$	$\alpha_0 = \alpha_1, \beta_0 \neq \beta_1$	$\alpha_0 \neq \alpha_1, \beta_0 \neq \beta_1$
Any Child by Age 25	-0.195***	-0.204***	-0.197***	-0.199***
	(0.0123)	(0.0172)	(0.0124)	(0.0178)
Treatment	-0.0232**	-0.0304**	-0.0999	-0.101
	(0.0114)	(0.0149)	(0.139)	(0.139)
Any Child by 25 × Treatment		0.0176		0.00562
		(0.0232)		(0.0247)
Controls	Yes	Yes	Yes	Yes
Controls × Treatment	No	No	Yes	Yes
N	5544	5544	5544	5544

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Treatment refers to having a niece or nephew born after July 1, 1977.

Table A.12: Estimated Model of Early Fertility and Completed Schooling (18 Years Old and Younger)

	(1) $\alpha_0 = \alpha_1, \beta_0 = \beta_1$	(2) $\alpha_0 \neq \alpha_1, \beta_0 = \beta_1$	(3) $\alpha_0 = \alpha_1, \beta_0 \neq \beta_1$	(4) $\alpha_0 \neq \alpha_1, \beta_0 \neq \beta_1$
Any Child by Age 25	-0.215***	-0.224***	-0.217***	-0.220***
	(0.0152)	(0.0219)	(0.0153)	(0.0223)
Treatment	-0.0191	-0.0271	-0.0821	-0.0855
	(0.0151)	(0.0212)	(0.158)	(0.159)
Any Child by 25 × Treatment		0.0161		0.00592
		(0.0300)		(0.0306)
Controls	Yes	Yes	Yes	Yes
Controls × Treatment	No	No	Yes	Yes
N	2981	2981	2981	2981

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .001$

Note: Treatment refers to having a niece or nephew born after July 1, 1977.

Table A.13: Share of Change in Schooling Attributable to Changes in Timing of Fertility

	All Ages		< 19 Years Old	
	Direct Share	Indirect Share	Direct Share	Indirect Share
$\alpha = \beta = 0$	0.86	0.14	0.64	0.36
$\beta = 0$	0.86	0.14	0.63	0.37
$\alpha = 0$	0.87	0.13	0.74	0.26
$\alpha \neq 0, \beta \neq 0$	0.86	0.14	0.73	0.27

Table A.14: Labor Market Outcomes at Age 35

	(1)	(2)	(3)
	Employed	Annual Earnings	Ln(Annual Earnings)
Niece/Nephew Born After July 1	-0.0110	-855.2	-0.0696*
	(0.0116)	(814.5)	(0.0360)
Observations	13572	13572	11730
Controls	Yes	Yes	Yes
Mean Outcome in Control Group	0.862	26408.0	10.03

Heteroskedasticity-robust standard errors shown in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .001$