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The Long-Term Effect on Children of Increasing the Length of Parents' Birth-Related Leave*

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Abstract

The length of parents' birth-related leave varies across countries and has been subject of some debate. In this paper, I will focus on some potential benefits of leave. I investigate the long-term effects on children of increasing the length of parents' birth-related leave using a natural experiment from 1984 in Denmark when the leave length was increased quite suddenly by almost 50% from 14 to 20 weeks. Regression discontinuity design is used to identify the causal effect of the leave reform and to estimate whether there is a measurable, persistent effect on children's cognitive and educational outcomes at ages 15 and 21. A population sample of Danish children born in the months around implementation of the reform and a dataset with Danish PISA-2000 scores are used for the analysis. Results indicate that increasing parents' access to birth-related leave has no measurable effect on children's long-term cognitive outcomes.

JEL Classification: J13, J18, D13
Keywords: Maternity leave, parental leave, child outcomes

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1 Introduction

The length of parents' birth-related leave varies across countries. The Scandinavian countries are traditionally among those with the longest leave lengths whereas the U.S. for example has one of the shortest leave lengths. It is difficult to determine how the costs and benefits of parents' birth-related leave add up. This difficulty arises firstly since there are several different potential benefits of parental leave such as increased child health, increased maternal health, higher fertility, and higher long-run outcomes for the children and secondly since there are competing policies. Instead of increasing parental leave, day care could be provided.

In this paper, the focus is on investigating one of the potential benefits of a longer parental leave, namely long-term effects on children. I use a natural experiment from 1984 in Denmark to evaluate the effect of increasing parents' leave length by almost 50% from 14 to 20 weeks. Prior to the reform, Danish maternity leave was comparable in length to e.g. the U.S. today, where 12 weeks of (unpaid) leave is available for eligible employees. So, what is the effect of giving parents access to 6 weeks of parental leave on top of the existing 14 weeks of maternity leave? Is there an additional effect on children when maternity leave is already 14 weeks to start with? Children affected by the reform were born in 1984 and they are therefore 21 years old by the end of 2005. Using longitudinal data from 1984 to 2005 on this cohort, I address the reform's effect on high school enrollment, high school completion, and high school grade point averages (GPAs). For some of the children I also have access to PISA reading scores from OECD's PISA-2000 study. PISA reading scores provide an earlier outcome measure as reading abilities were tested when the children were 15 years old.

Denmark is an obvious candidate for investigating the effect of increasing leave length since women in Denmark generally participate actively in the labor market and therefore stand to benefit from a leave reform. As shown in Figure 1, the labor force participation for Danish women was high already in the 1980s, especially compared to American women. Because of the high labor force participation rate among women of childbearing age, access to publicly subsidized day care and maternity leave has been very important to Danish women throughout the period I investigate.[1] Further, since the female labor force participation rate is so high in Denmark, any observed effect of the reform is less likely to be tainted by self-selection issues. All women who give birth take at least some birth-related leave in Denmark[2] and most women take all the leave they are eligible to, as shown in Appendix A.

The method used to identify the causal effect of the leave reform is regression discontinuity (RD) design. A natural experiment is exploited to investigate whether eligibility to an almost 50% increase in the length of birth-related leave is beneficial

[1] Publicly subsidized day care and other childcare facilities are well established in Denmark and have been for more than a quarter century.

[2] According to the law, women are required to take at least two weeks leave right after childbirth. The exact rules about how much leave women can take and the compensation are described in Section 3.

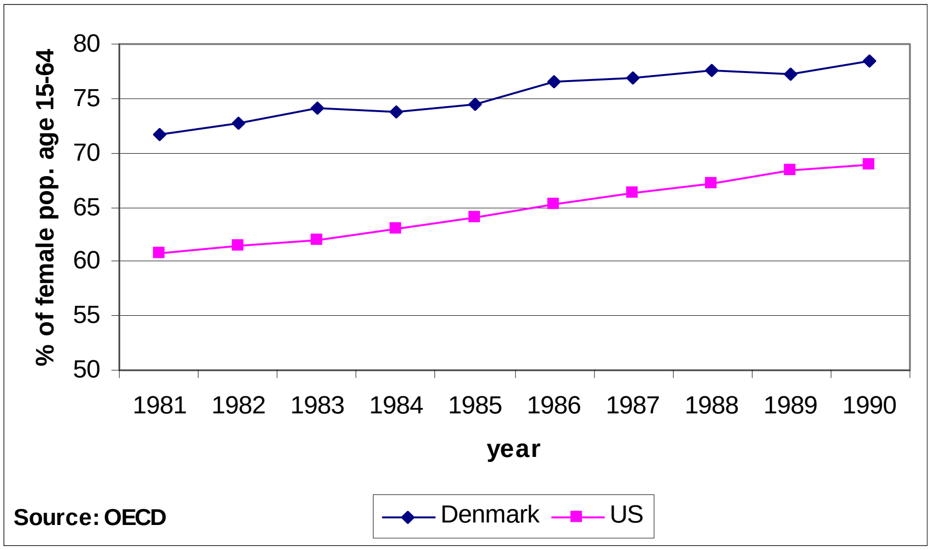

Figure 1: Female labor force participation in Denmark and the US, 1981-1990.

for children, i.e. has a measurable, positive impact on their long-term cognitive outcomes and enrollment in secondary education. It might be the case that an increase in the total birth-related leave will not have a long-term impact on children because the maternity leave was already long (more than 3 months) before the reform. It may also be the case that 6 weeks extra leave is too little to have a measurable long-run effect. Nonetheless, this is an empirical question which is answered by comparing a population sample of Danish children born in the months before the policy reform became effective with a population sample of Danish children born in the months after. In addition, a dataset with Danish PISA-2000 scores is used to address children's level of reading at the age of 15. These datasets are more thoroughly introduced in Section 5.

The contribution of this paper is threefold. First, long-term effects on children related to the length of maternity and parental leave are investigated. Usually, only shorter term outcomes are addressed due to data limitations. Second, policy recommendations for countries that do not have a long maternity or parental leave today can be provided from this study given institutional settings fairly similar to those in Denmark in the 1980s. Third, RD identifies the causal effect of extending birth-related leave by exploiting a natural experiment. This results in stronger identification than other studies in the literature. Further, RD has not previously been used to estimate the effect of the length of parents' birth-related leave on children's cognitive and educational outcomes.[3]

[3] An unpublished study using German data for the analysis does exist, though, see Dustmann and Schönberg (2007). Their paper and this study were both presented at the same session at the

In Section 2, a literature review is presented. In Section 3, Danish leave reforms during the 1980s are briefly summarized, and in Section 4 the method of regression discontinuity design is explained. Section 5 gives an introduction to the datasets used in the estimations, and the empirical results are presented in Section 6 along with a sensitivity analysis. Finally, the results are summarized and a brief discussion of policy implications is presented in Section 7.

2 Literature Review

Studies concerned with maternity and parental leave mostly focus on the effect of such leave on either maternal health, child health, maternal employment and wages, or fertility. A selection of these empirical studies is presented in this section as are some theoretical studies regarding child development and child outcomes. Finally, this section closes with a discussion of some important studies about or using regression discontinuity design.

A study using data from the U.S. investigates whether the length of maternity leave affects maternal health, see Chatterji and Markowitz (2005). They find that longer maternity leave reduces cases of depression among working mothers, i.e. there is a positive effect of a longer maternity leave, but the empirical analysis uses a sample of non-representative employed mothers. It is therefore not certain whether these results can be generalized. Other papers such as Ruhm (2000), Berger et al. (2005), and Baker and Milligan (2005, 2007) have focused on the relation between maternity leave and child health. Berger et al. (2005) find that there is a negative relation between mothers' return to work within the first 12 weeks after childbirth and child's health, the length of breast-feeding, and the child's behavioral problems, especially if the mother returns full-time. The sample used in Berger et al. (2005) is not representative of all births in the U.S. in the time period they focus on so results may not be generalized. Ruhm (2000) finds that more generous paid leave reduces deaths among infants and young children. Ruhm further suggests that there might be other positive effects of a generous leave scheme such as effects on child cognitive outcomes but he cannot directly address this issue using aggregated data for 16 European countries. Baker and Milligan (2005, 2007) use Canadian data and contrary to the other studies mentioned they do not find evidence that time at home affects infant health.

Baker and Milligan (2005) also look at the effect of maternity leave on women's labor supply. They find that introducing 17-18 weeks of mandated job-protected maternity leave does not increase the time mothers spend at home with their newborns because it apparently duplicates existing private arrangements. On the other hand, mandated leave lengths up to 70 weeks seem to increase mothers' time spent at home. Lalive and Zweimüller (2005) investigate the effect of a 1990 Austrian policy reform. They study the effect of parental leave on fertility and women's return-to-work in the private sector and find a big positive effect on fertility of a

ESPE conference 2007.

longer parental leave. It pays to have several children shortly after each other in Austria. Further, longer leave also increases women's time off work.

Studies trying to determine the effect of maternity (or parental) leave may suffer from weak identification of causal effects. The positive outcomes attributed to maternity leave often rest on the first stage relations between mandates of leave and mothers' labor supply decisions that are both empirically and, as explained later, theoretically ambiguous.

Instead of focusing on the direct effect on children of maternity and parental leave, many studies focus on an "inverse" effect, i.e. the effect on children of maternal employment. There is an extensive literature on this subject and the methods and results vary considerably. Baum (2003) finds that in the U.S. mothers' return to work within the first three months of the child's life in some cases results in lower cognitive test scores for the children.[4] On the other hand, he finds that increased family income resulting from mothers' market work has a positive effect on children's outcomes and therefore partially offsets the negative effects of maternal labor supply. Ruhm (2004) uses a U.S. dataset with detailed information on maternal, child, and household characteristics. No exogenous source of variation in parental employment is available in the study so the detailed information is needed to reduce possible selection bias due to parental employment. Ruhm finds a negative effect on children from early maternal employment, i.e. maternal employment in the first year after birth, though there seem to be partially offsetting effects if mothers continue working during the second and third year of the child's life. Ruhm (2005) finds that maternal employment has a negative effect on advantaged children but a positive effect on disadvantaged children. Cognitive outcomes and health (measured by weight) are used as outcome measures in this study. The type of childcare seems to be an important determinant of children's outcome along with maternal employment as shown by Gregg et al. (2005). They use data from the U.K. to investigate outcomes of children in the ages 4 to 7 and find that formal childcare substitution is better for children than informal childcare arrangements.

It is clear that the estimated effect on children of maternal employment ranges from negative to neutral or even positive. Short-term outcomes are most often investigated and the mixed results may be caused by selection since maternal employment among other things is endogenous.[5] Therefore, weak identification of the causal effect of maternal employment is a problem. Further, it is very difficult to control for heterogeneity in family or child characteristics correlated with parental job-holding. Therefore, reduced form estimates of child development functions often include but do not separately identify information on technological properties of the production function and characteristics of unobserved household preferences. Finally, non-representative samples tend to be used in empirical analyses.

[4] Baum (2003) also finds a negative effect from maternal work in the child's first year.

[5] Baker and Milligan (2005) suggest that maternity leave mandates may serve as a good instrument for maternal employment in the child's first year. Baum (2003) instruments for maternal labor supply using local labor market conditions.

Theoretical studies on child development and child outcomes include studies such as Becker and Tomes (1986), Ermisch and Francesconi (2000), Todd and Wolpin (2003), and Blau and Hagy (1998). These models suggest that parental market work may have both positive and negative effects on children. For example, parental time spent with children is expected to have a positive effect as it is a direct investment in children. Further, parental leisure time is also thought to have a positive effect on children as it decreases parents' stress level and thereby increases the quality of time spent with children. When both parents work in the market, it may have negative effects on the child since the child then has to be in non-parental childcare during most of the day. This can cause the mother (or father) child attachment to be weaker because of the daily separation and this potentially has long-term impacts as argued in Knudsen et al. (2006) and Heckman (2000). They find evidence of a strong relationship between early cognitive and non-cognitive learning and later outcomes and suggest that children should spend a lot of time with their parents, i.e. should not be in institutions when they are young. This is indirectly arguing that early maternal market work has a negative impact on children. On the other hand, the effect on children of non-parental childcare depends on the quality of the childcare so high quality childcare may be as good as or better for the children as parental care, see Esping-Andersen (2004). Parents' market work may have a positive effect on children through increased household income and may also make parents more satisfied because of a greater interaction with other adults. Because of these possible opposing effects of parental market work, one cannot from theory predict the sign of the effect of mothers' early market work or, alternatively, the effect of a longer maternity or parental leave.

As mentioned above, I use regression discontinuity design in the empirical analysis. The literature on regression discontinuity started with the study by Thistlethwaite and Campbell (1960) and has developed further since. An important contribution to the regression discontinuity literature is Hahn et al. (2001). They define and introduce sharp and fuzzy design and clarify which assumptions are needed for identification in a RD design. Recently, studies focusing on how to correct for specification error when using RD design have emerged, see Lee and Card (2006). The studies most similar to the study in this paper are Lalive and Zweimüller (2005) and Baker and Milligan (2005). Lalive and Zweimüller (2005) use an Austrian leave reform to investigate the effect of parental leave on maternal employment and fertility in a RD design. They focus only on mothers working in the private sector, though. Baker and Milligan (2005) use RD design when studying the effect of leave reforms introduced at the same time across Canadian provinces.

Finally, in an unpublished study Dustmann and Schönberg (2007) investigate effects of expansions of maternity leave in a German setting and also focus on long-term effects on children. Their study is similar to this study in using RD to find causal effects of leave reforms but their type of data and the reforms investigated are different. An advantage of their study is that they can evaluate three different reforms over a time span of 13 years. A disadvantage is that they only have data on children who have ever worked for pay by the end of 2004 when they evaluate the earliest reform from 1979. This means that children who are self-employed, civil servants, doing compulsory military service, or who are still full-time students and therefore have not entered the labor market yet, are not in the sample. So, the leave increase may have had a positive effect on schooling, but it cannot be disentangled from the selection effect. Further, the institutional settings in Germany are somewhat different than in Denmark. The female labor force participation rate is much lower in Germany than in Denmark and childcare is less readily accessible making viable alternatives to maternal care less.

So far, there are no published studies in the regression discontinuity literature investigating the effect of a policy reform on children's long-term outcomes. The use of RD design to estimate the effect of the length of parents' birth-related leave on children, estimating on the population of children born in the relevant period, and at the same time focusing on long-term outcomes are the main contributions of this study along with exploiting a natural experiment to give strong identification. Furthermore, the analysis is carried out in a country with high female labor force participation rate, also at the time of the reform, which makes it possible to extrapolate the findings to countries like the U.S. today.

3 Birth-Related Leave in Denmark

Maternity leave is defined here as birth-related leave solely for the mother. Parental leave, on the other hand, is birth-related leave to which both the mother and the father are eligible. The goal of government-mandated leave policies is to provide parental care to the child. Therefore, parents are not permitted to be on parental leave at the same time as this would shorten the cumulative time with parents.

Historically, women in Denmark have been entitled to some sort of maternity leave since 1901. At first, only women working in factories were entitled to maternity leave, but during the 20th century several maternity and parental leave reforms were implemented. A 1967 reform ensured almost all women in the labor force entitlement to maternity leave with economic compensation, see Borchorst (2003). During the 1970s women's labor force participation rose and it became the norm for men and women to provide jointly for the family. Therefore, political parties saw a potential for more votes if they suggested family-friendly policies. This led to a reform in 1980 which ensured mothers 14 weeks of maternity leave after birth with income dependent compensation. In addition, mothers were entitled to 4 weeks of leave before childbirth and could not be fired during pregnancy or the leave period. Fathers were not entitled to any birth-related leave, but a few fathers took birth-related leave and paid for it themselves. Maternity leave was compensated but depended on mother's income. The mother needed to be eligible for jobseekers allowance[6] and the compensation was at most 90% of the income from which the

[6] This implies that she is a wage earner or self-employed and has worked for pay and has a yearly income which entitles to at least 10% of the maximum amount of jobseekers allowance of DKK

jobseekers allowance is calculated. This implies that it is relatively more expensive for high-income parents to take leave. Parental leave was also compensated and income dependent and both parents had to be eligible for jobseekers allowance. In 1983-1985, the maximum compensation was DKK 2,008 (about $335) per week. The level of compensation for mothers increased gradually until October 1982, and after that remained on the same level until April 1986.

The association 'Women's Movement' among others argued throughout the 1980s that it generally was important to improve (increase) birth-related leave. The political parties broadly agreed that it was beneficial for infants to spend time with their parents. It was the general opinion that since both men and women worked in the market and provided for the family, fairness required that they both got access to birth-related leave. Therefore, 'the equal opportunities commission' argued that before increasing maternity leave further it was more important to ensure men access to birth-related leave. This was the starting point for the 1984 leave reform that is investigated in this study.

The 1984 reform extended the leave period from 14 to 20 weeks after childbirth, but designated these additional 6 weeks as parental leave. Furthermore, fathers were guaranteed 2 weeks of leave with compensation immediately after childbirth. Compensation rates were the same as before the reform, with a maximum benefit of DKK 2,008 per week. This more generous and even-handed policy reform was implemented on July 1st, 1984.

While July 1st was the official start date, there was a period of transition. Mothers who were already on a birth-related leave under the old rules on July 1st, 1984, were automatically eligible to receive the extended leave promised by the new reform, i.e. they had the right to 6 additional weeks of parental leave. Therefore, if the mother started her leave period less than 14 weeks before July 1st, 1984 (and was still on leave July 1st), she was eligible for 20 weeks of leave in total. If her leave period started more than 14 weeks before July 1st, 1984, she was only eligible for 14 weeks of leave after birth. The cutoff date for the reform was therefore 13 weeks and 6 days before July 1st, 1984. Mothers giving birth on or after March 26th, 1984, were in practice eligible for the extended leave.

A full picture of birth-related leave reforms in Denmark in the 1980s reveals that birth-related leave was further increased from July 1st, 1985. The total leave period after birth was at that point extended to 24 weeks of which the last 10 weeks were parental leave. The 1985 change was in fact the 2nd step of a 2 step reform that passed in late 1983. According to Borchorst (2003) the first proposal for the 1984/1985 leave reform was suggested in October 1983. After considerable debate and 23 proposals for revision, a slightly modified proposal was accepted in December

2,008 per week. Furthermore, within the last 4 weeks before the leave period she needs to have had a wage income of at least this size, been self-employed, or been unemployed but registered as job seeking at the public employment service.

If the mother is a housewife and has signed up for a voluntary insurance arrangement at least 10 months before she wants jobseekers allowance, she can get 4 weeks of jobseekers allowance when she is on birth-related leave.

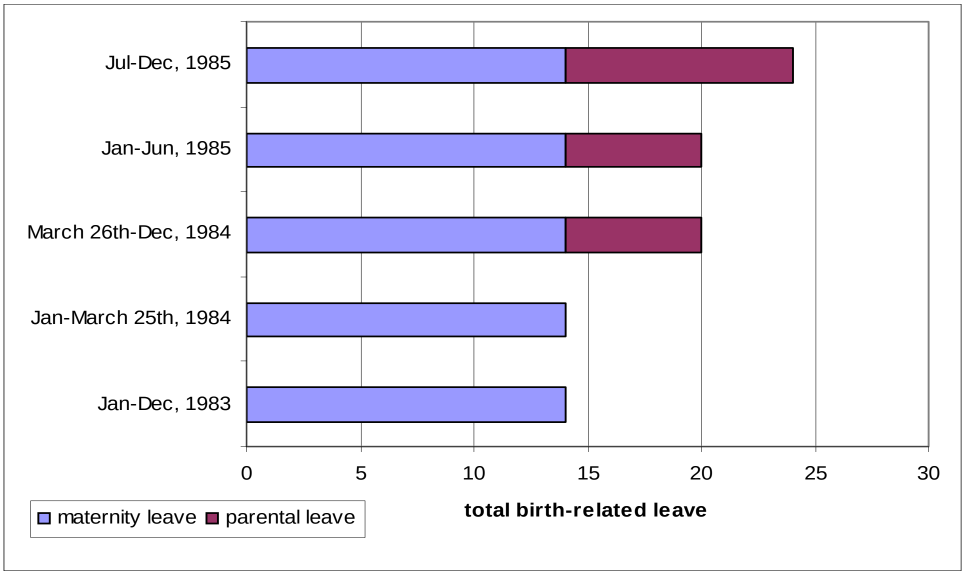

Figure 2: Total birth-related leave in Denmark from 1983 to 1985, maternity leave and parental leave separately.

1983. The process of accepting the reform was rather quick. Figure 2 illustrates the total leave length from 1983 to 1985.

For a parental leave to have any impact on children it is necessary for that leave to be taken. While it is not observable precisely how any leave time was spent, it is observable whether leave time was taken from work. The take-up rate for this reform was high as shown in Figure 3. There is a substantial change in the amount of leave taken by mothers of children born around the time the policy took effect. Indeed, this jump occurs on exactly the date the extended leave became available (March 26th, 1984, is time 0). The policy change therefore appears to have had the intended consequence – mothers took more birth-related leave. The jump in the amount of leave also matches expectations. Before the policy change, mothers were eligible for 18 weeks of leave (4 weeks before birth and 14 weeks after birth). In fact we observe them taking about 115 days on average, or a little less than 18 weeks. After the policy change, mothers were eligible for an additional 6 weeks of leave and we observe them taking about 40 additional days of leave, or just about 6 additional weeks. Appendix A provides further details regarding the take-up rate.

The reform created a natural experiment since children born before March 26th, 1984, had less "home time" with their mothers (or fathers)[7] than children born after March 26th, 1984. Children in the same school class therefore differ with respect to this, since almost all children in a school class are born in the same year. In

[7] In practice most of the leave is taken by mothers.

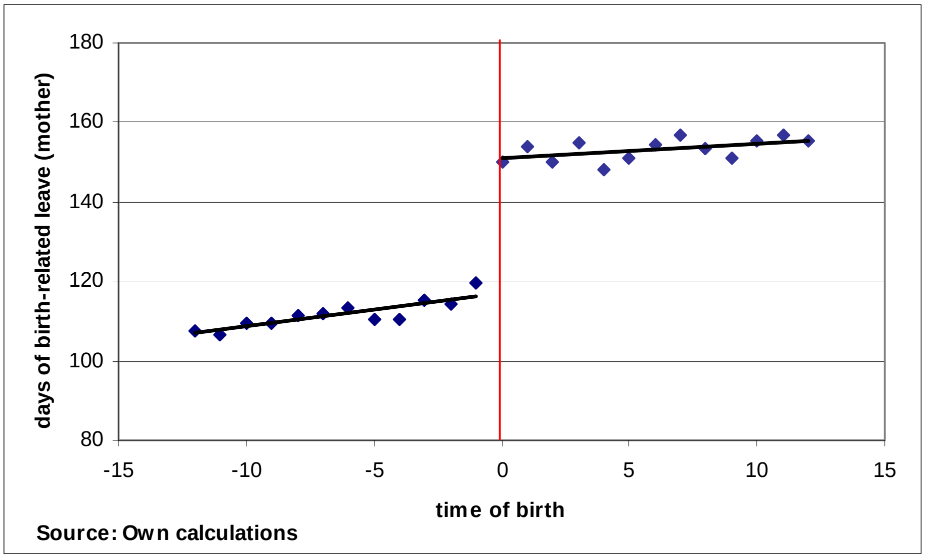

Figure 3: Days of birth-related leave taken by mothers shown by time of childbirth. The vertical line is at March 26th, 1984. Each dot represents the mean for all children born in a 5-day interval.

Denmark, school starts in August and all children 6 years old in the current year can start in school. A few children start early or late in school but the majority of children in a school class are born in the same year.[8] It is important for this study that the children have experienced similar environments during childhood since the focus is on long-term educational outcomes. I exploit this natural experiment in the identification strategy because it provides an exogenous source of variation in the amount of leave to which parents are eligible.

If anyone could anticipate the increase in leave length and change behavior according to it, there would be endogeneity problems and therefore problems with validity of the RD design. Selection on the basis of parents' deliberate choices can be ruled out for several reasons. First, biological limitations imply that it is impossible to delay childbirth if the birth is natural, i.e. not a caesarean section. Since I expect parents to prefer having their child born after March 26th, so as to be eligible to a longer leave period, I do not have to deal with selection issues with respect

[8]One might be concerned about whether Danish children born from March 26th to May 25th to a higher degree than children born from January 26th to March 25th postpone school entry with a year. If the weakest children postpone school entry, it leads to higher educational outcomes for them and upward biases the effect of the reform, see Puhani and Weber (2007). Postponing school entry does not seem to be a big issue, though. When investigating how many children have completed 8th grade in 1999, I find that 1.01% of children born before implementation of the reform have postponed school entry whereas 0.97% of children born after implementation of the reform have postponed school entry.

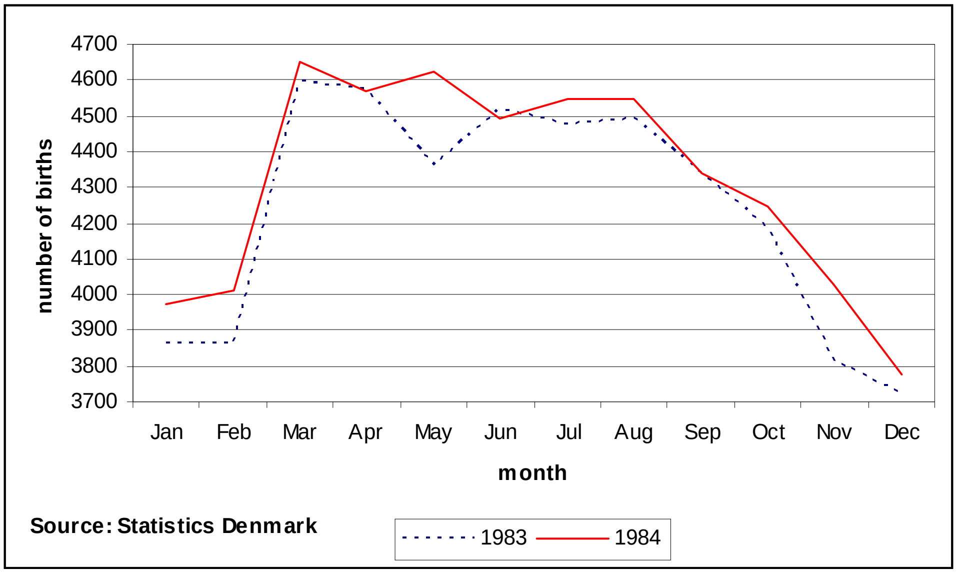

Figure 4: Births per month in Denmark in 1983 and 1984.

to planning or influencing the time of birth.[9] Further, since the legislation passed in December 1983, less than nine months before March 26th, 1984, parents could not anticipate and therefore plan according to the reform. Indeed the legislation was first proposed less than nine months before March 26th, 1984. By contrast, the 1985 increase should have been anticipated by parents and may possibly have affected their birth planning. Hence, the focus here is on the 1984 change alone.

To explore the selection issue further, I examined the number of births in Denmark in 1983 and 1984. Aggregate statistics show an increase, which could be indicative of selection on the basis of the leave reform. However, as shown in Figure 4 the 1984 increase is evenly spread throughout the year and not lumped only in the latter half of the year. Thus, it seems unlikely that the difference is a response to the leave reform. Figure 4 also shows that there is substantial seasonality to the birth pattern in Denmark. There are many more births between March and August than in the first two or last four months of the year. Further, the number of births in March and April seems to be almost equal in 1983 and 1984, and the number of births in February is clearly higher in 1984 than in 1983. Thus, I am confident that sample selection is not a problem.

[9] In 1984, the number of caesarean sections in Denmark was quite low. 6.5% of all births were "planned caesareans" meaning that they were planned more than 8 hours in advance. These 6.5% also include caesareans scheduled for medical reasons and recognized at least 8 hours in advance, see Sundhedsstyrelsen (2005).

4 Estimation Strategy

The classical problem when investigating the effect of a policy reform is that individuals have either been directly affected by the reform (been "treated") or not, but the same individual cannot be observed as treated and untreated at the same time. Several different methods can be used to take this problem into account and I will use regression discontinuity design to evaluate the effect of the 1984 leave reform. RD is a useful method for determining whether a program or treatment is effective when certain conditions are fulfilled.

4.1 Regression Discontinuity Design

The idea of RD is, for individual i, to determine the effect of a treatment, T_i, on an outcome, Y_i, where the treatment assignment function is discontinuous at the cutoff point B (here March 26th, 1984). Intuitively, I compare individuals very close to the discontinuity point so I expect them to be similar, except for the fact that they have been exposed to different treatments. That is, their values of the underlying targeting variable are just below or just above the discontinuity point but apart from that they have experienced identical environments. The average treatment effect is therefore estimated by comparing average outcome values of those individuals just above and just below B and the treatment effect is identified exactly at B. To ensure identification, a sample "close" to the discontinuity point must be used and I use 2 months on either side of the discontinuity point in the main analysis and do robustness checks using only 1 month. This is slightly different from Lalive and Zweimüller (2005) who use 1 month on either side of the discontinuity point in their main analysis of the Austrian leave reform.

What is unique in the RD design is the way individuals are allocated to different groups based solely on a cutoff criterion. Therefore, individuals in different groups do not have to be identical given "pre-program" indicators as in a randomized experiment. It may for example be the case that individuals are allocated to different groups based on their health or based on a test score. It is assumed that in the absence of the "program" (policy change) the pre-post relationship would be equivalent for the two groups. In this study, the cutoff criterion is determined on the basis of a birth date so here I will actually also expect individuals to be equivalent on pre-program indicators. All individuals born before the cutoff value (March 26th, 1984) are assigned to the control group whereas individuals born after the cutoff value are assigned to the treatment group. Treatment is parents' entitlement to the extra 6 weeks of parental leave following childbirth.

When RD design is well implemented, as I argue it is in this study, inferences are comparable to conclusions from randomized experiments. I.e. the policy change investigated results in the same effect on children's cognitive outcomes and educational attainment whether using random experiments or RD in the analysis.

There are two types of regression discontinuity designs, sharp design and fuzzy design, see Hahn et al. (2001). Under sharp design, treatment is a discontinuous but deterministic function, $f(\cdot)$, of some "forcing variable", b_i, where b_i takes on a continuum of values. If treatment assignment is not a deterministic function of b_i, i.e. there are additional variables unobserved to the researcher that determine assignment to treatment, then it is a so-called fuzzy design. In this study, a deterministic treatment assignment function can be set up as

$$T_i = f(b_i) = 1(b_i \geq \textit{March 26th, 1984}),$$

where b_i is the date of birth. I therefore have a sharp design with discontinuity point at $B = \textit{March 26th, 1984}$. Mothers are predicted to take as much leave as possible, i.e. under treatment (the child is born after the discontinuity point) mothers take 20 weeks of leave after birth, otherwise they take 14 weeks of leave.

The pre-post relationship is well known and therefore I can model it correctly. This along with the fact that I do not have any spurious discontinuity in the pre-post relationship at the cutoff point[10] ensures the strength and validity of the RD design.

The following assumptions are necessary for implementation of the RD design:

Assumption 1 The limits

$$T^+ \equiv \lim_{b \to B^+} E\left[T_i | b_i = b\right] \quad \text{and} \quad T^- \equiv \lim_{b \to B^-} E\left[T_i | b_i = b\right]$$

exist and are *not* equal.

Assumption 2 $E\left[\textit{outcome absent treatment} | b_i = b\right]$ is continuous in $b = B$.

Assumption 3 $E\left[\textit{treatment effect} | b_i = b\right]$ regarded as a function of b is continuous at B.

Discontinuity of treatment at B is ensured because Assumption 1 is fulfilled. Policy implementation is uniform to all recipients, i.e. they all receive the same entitlement to leave because the policy reform is universal and based on date of childbirth. This is confirmed in Figure 3. Assumption 2 ensures that the pre-reform distribution is continuous which is crucial for identification of the treatment effect because it ensures that the average treatment effect is similar for individuals with values of b_i close to B. Finally, Assumption 3 generalizes the identification strategy to include heterogenous treatment effects instead of only constant treatment effects.

Further, if I assume that T_i is independent of the treatment effect conditional on b_i close to B, then the average treatment effect at B (under sharp design) is non-parametrically identified as

$$y^+ - y^-,$$

where $y^+ \equiv \lim_{b \to B^+} E\left[Y_i | b_i = b\right]$ and $y^- \equiv \lim_{b \to B^-} E\left[Y_i | b_i = b\right]$, see Hahn et al. (2001). The treatment effect is consistently estimated given consistent estimators of y^+ and

[10] There were no other major changes in Denmark at the same time as the reform was implemented.

y^-. The (weak) conditional independence assumption ensures that individuals do not select into treatment on the basis of their anticipated gains from treatment which is important for the internal validity of the RD design.

In this study, I have a sufficient number of prereform values in the comparison group to enable adequate estimation of the true relationship for that group.[11] Apart from the probability of treatment, individuals on either side of the cutoff point experience almost identical environments. Both groups come from a single continuous prereform distribution, division between groups is determined only by the cutoff, and there are no other major changes in Denmark at this point in time. I therefore do not have to worry about possible spurious discontinuities in the pre-post relationship coinciding with the cutoff point.[12]

4.1.1 Empirical Model Specification

To make sure that the statistical model is correctly specified, I visually examine the pre-post reform relationship in the outcome variables to determine whether there is any visually discernible discontinuity at the cutoff. Two general types of discontinuities are possible: the outcome level may change (called a main effect) and/or there may be a change in the outcome measure over time (a slope or interaction effect). Figures 5, 6, and 7 depict the average value of the different outcome measures used in the analysis as a function of the child's birth date. The figures show that the pre-post distribution is fairly linear and therefore it makes sense to use RD design for identifying the effect of the reform. There might be a small discontinuity in high school GPA according to Figure 6 and potentially a change in slope of the PISA reading score according to Figure 7. There is a lot of noise in the PISA reading score, though. According to Figures 5, 6, and 7 the effect of treatment does not seem to be large.

Based on the visual inspection of Figures 5, 6, and 7, I set up a simple estimation equation as

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 T_i + \varepsilon_i, \tag{1}$$

where Y is the outcome variable, X is time of birth (normalized to 0 at March 26th, 1984), T is the dummy coded treatment variable, and ε is the error term. The parameter of interest is β_2 which measures the main effect of the reform, i.e. the vertical discontinuity at the cutoff point. Since it is difficult to determine from the graphs whether an interaction term between X and T should be added or whether higher order polynomials of the forcing variable, X, should be added, I begin with an empirical model as simple as possible and experiment with different specifications in the sensitivity analysis.

[11] In the data description in Section 5 it is shown that the comparison group consists of about 9,000 observations in the population sample and 650 observations in the PISA dataset.

[12] In Appendix B different covariates are graphed by the child's birth date. It is clear from the graphs that there are no jumps or apparent differences between parents with children born on either side of the cutoff point. Also, ethnicity of the children on either side of the cutoff point is very similar.

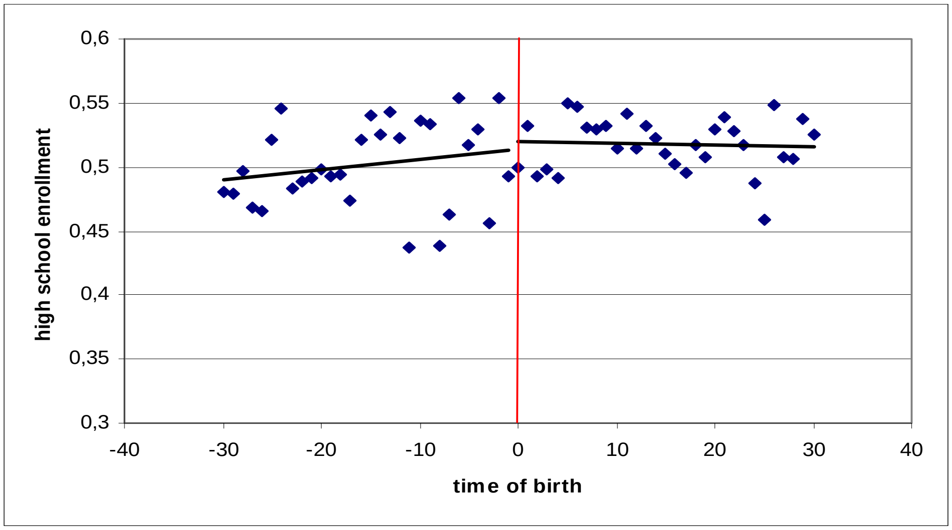

Figure 5: 1984 born children's high school enrollment in 2005 by time of birth. Mean values are calculated using 2-day intervals. The vertical line is at March 26th, 1984.

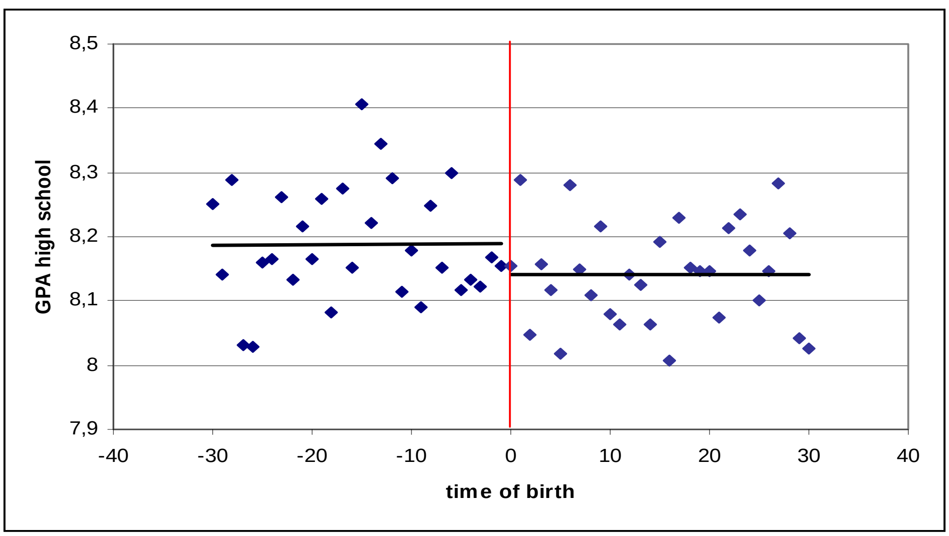

Figure 6: 1984 born children's GPA in 2005 by time of birth. Mean values are calculated using 2-day intervals. The vertical line is at March 26th, 1984.

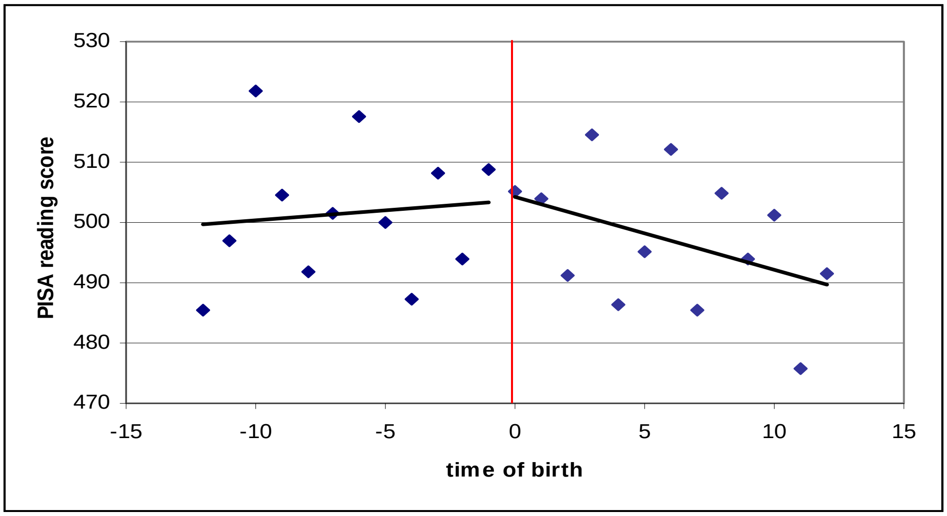

Figure 7: Children's PISA reading score in 2000 by time of birth. Mean values are calculated for 5-day intervals. The vertical line is at March 26th, 1984.

5 Data

Estimations are based on two data sources. The first dataset is an administrative register dataset consisting of the entire population of Danish children born from January to May 1983 and January to May 1984. This dataset was obtained from Statistics Denmark for the sole purpose of this paper. The second dataset is the Danish PISA-2000 subsample in which PISA reading scores for the children are combined with register information from Danish administrative registers. This dataset has also been provided by Statistics Denmark. The two datasets are presented in the following sections.

5.1 Population Sample

In 1984, 51,800 children were born in Denmark, about 4,000 were born in January and February and 4,600 in March, April, and May. These children are the focus of this study. In the Danish tax and income registers created by Statistics Denmark, these individuals and their parents are followed on a yearly basis from 1984 to 2005 if they have not left the country or died. The registers provide information on the parents' and children's marital status, residence, education, income, wage, labor market activities, etc. This includes information on the children's completed education in 2005 when they are 21 years of age. Some Danish children take an optional 10th grade before enrolling in high school and they may therefore still be enrolled in high school at the age of 21. It is also very popular among the Danish youth to take a sabbatical year between high school and college. Thus,

	controls		treated	
	mean	std.dev	mean	std.dev
control group, month 1	0.50	0.50		
control group, month 2	0.50	0.50		
treatment group, month 1			0.51	0.50
treatment group, month 2			0.49	0.50
days of leave (mother)	111.73*	42.87	153.07	57.19
gender (1: boys, 2: girls)	1.49	0.50	1.49	0.50
Danish origin	0.90	0.31	0.89	0.31
GPA from high school	8.19*	1.00	8.14	1.00
high school	0.50*	0.50	0.52	0.50
high school, mother	0.27	0.44	0.27	0.44
high school, father	0.23	0.42	0.23	0.42
work experience (years), mother	5.55	3.63	5.64	3.52
work experience (years), father	7.76	3.89	7.82	3.82
annual wage income (DKK), mother	75,822*	53,773	78,773	52,657
annual wage income (DKK), father	119,835*	72,593	123,586	84,702
obs	9,053		10,028	

*: Significantly different from the mean for the treatment group at a 5% level.

Table 1: Means for the control and treatment group from the full sample.

higher educational goals may not be clear when the children are 21 years old. I will therefore focus on high school enrollment and high school completion, which in short will be referred to as high school enrollment.

In addition to information about children born from January to May 1984, I have a population sample of children born from January to May 1983. This dataset is used for a sensitivity analysis, e.g. to perform a differences-in-differences analysis. In what follows, focus is on the main sample which consists of children born from January 26th to May 25th, 1984.

Table 1 shows mean values for selected variables for the children and their parents. The children are grouped in control and treatment groups, where the control group includes children born from January 26th to March 25th, and the treatment group consists of children born from March 26th to May 25th, 1984. The mean values for children are from 2005 and the values for parents are from 1983, i.e. the year before the child's birth.

It is clear from Table 1 that, based on the covariates, the two groups are almost identical. The high number of observations, however, makes it possible to identify a few statistically significant differences. One anticipated difference between the groups is mothers' days of leave. Mothers in the treatment group are expected to have taken significantly more leave than mothers in the control group. In addition, I find that mothers and fathers in the control group have significantly lower annual wage income compared to treatment group mothers and fathers. Control group children therefore have a slightly weaker socio-economic background than children in the treatment group. The outcome variable high school GPA is higher for control group children whereas high school enrollment is higher for treatment group children.[13]

Given the substantial similarities between the two groups, I attribute any potential effect of treatment directly to the leave reform despite the difference in parents' annual wage income.

5.2 Danish PISA-2000 Sample

PISA is short for the OECD's "Programme for International Student Assessment". In the year of 2000, a similar battery of tests was administered to 15-year-old children in 32 countries, most of them OECD countries. The PISA-2000 study focused on children's reading abilities but also tested some of the children in mathematics and science. The reading score is used as the outcome measure in this study since all children were tested in reading.[14] Test scores from PISA tests are normalized to an OECD mean of 500 and a standard deviation of 100. Denmark scored below average in reading with a mean of 497 and a standard deviation of 98. By comparison, Finland had the highest score of 546 in reading and a standard deviation of 89. For more information on the PISA-2000 study, see OECD (2002).

Children participating in the PISA study are equipped with identifiers to combine PISA information with register data. This provides information on child and parents from childbirth to the year 2005 (2006 for some variables). Parental information is from the year of childbirth whereas it was from the year before childbirth in the population sample. 4242 children participated in the Danish PISA test in 2000. Of these, about 300 to 350 children were born in each of the months January to May, 1984.

The PISA sample is a subsample of the population sample and therefore it has fewer observations. Mean values are shown in Table 2 and again control and treatment groups are almost similar. Only the length of mothers' leave is significantly different between the control and treatment groups. Both mothers' and fathers' work experience and annual wage income seem to be a bit lower for the treatment group. Also, child high school GPA and PISA reading score are a bit lower for the treatment group but high school enrollment is higher. None of these differences are statistically significant at a 5% level, though.

[13]In Denmark, GPAs from high school range from 0 to 13 with 8 as the middle grade. 6 is equivalent to passing the exam.

[14]WLE scores are used since this is not a cross-country comparison. The WLE score is calculated by the ACER institute in Australia which is responsible for all OECD's PISA analyses. WLE scores are not simple observed test statistics but instead they are predictions based on estimated models for all countries participating in the analysis under study. In regressions and for statistical tests, I use the PISA test scores *as if* they were the observed test scores. This is the only practical solution for performing statistical analyses using this type of data and it is the same method used in other Danish and international studies. For a critical discussion of the PISA measures, see Allerup (2005).

	controls		treated	
	mean	std.dev	mean	std.dev
control group, month 1	0.51	0.50		
control group, month 2	0.49	0.50		
treatment group, month 1			0.51	0.50
treatment group, month 2			0.49	0.50
days of leave (mother)	112.20*	41.23	155.81	54.96
gender (1: boys, 2: girls)	1.50	0.50	1.49	0.50
Danish origin	0.95	0.21	0.94	0.24
PISA reading score	501.46	98.34	497.92	97.69
GPA from high school	8.23	1.03	8.19	1.00
high school	0.55	0.50	0.56	0.50
high school, mother	0.31	0.46	0.33	0.47
high school, father	0.25	0.44	0.26	0.44
work experience (years), mother	6.14	3.84	6.02	3.74
work experience (years), father	8.24	3.98	8.16	3.99
annual wage income (DKK), mother	60,262	54,041	57,507	50,453
annual wage income (DKK), father	129,633	76,487	127,307	75,607
obs	639		702	

*: Significantly different from the mean for the treatment group at a 5% level.

Table 2: Means for the control and treatment groups from the PISA sample.

Compared to the population sample, the PISA sample seems to be positively selected. In general, children in the PISA sample have higher outcomes and stronger background but some differences result from using information about parents a year later than for the population sample. This leads to a higher parental work experience, for example. A higher fraction of PISA children are of Danish origin, but parents' annual wage income is lower than in the population sample. For mothers, wage income is probably lower in part because they spend some of the year on leave and only receive a fraction of their usual income.

6 Empirical Evidence

In this section, the empirical analysis is presented. I test the following hypothesis using the two different datasets:

Hypothesis An increase in the length of parents' total birth-related leave is beneficial for children, i.e. has a measurable, positive impact on their long-term cognitive outcomes. Furthermore, it has a positive impact on enrollment in secondary education.

The alternative is that an increase in the length of parents' total birth-related leave will not have a (long-term) impact on children. A possible reason for the

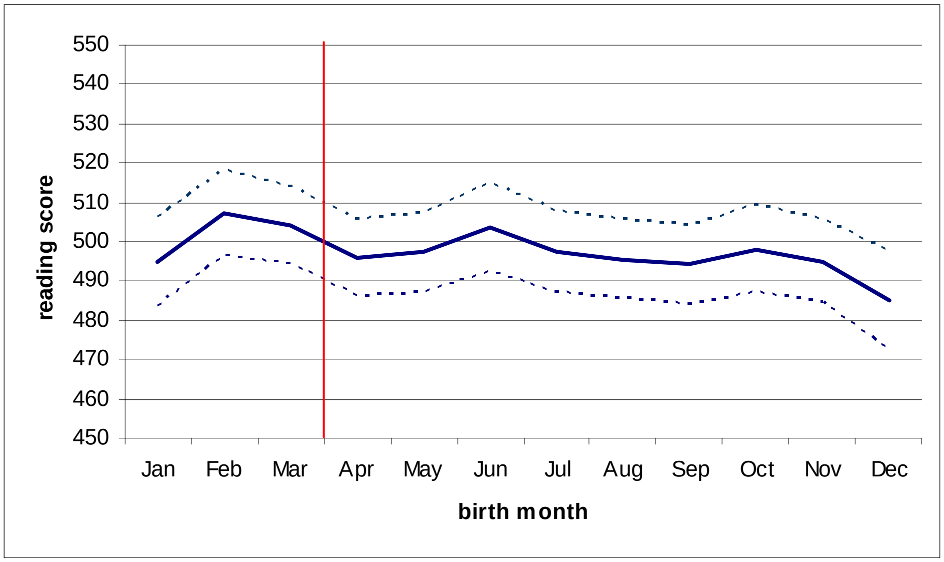

Figure 8: PISA-2000 reading scores for Danish children born in 1984. The vertical line shows April 1st, 1984, i.e. (almost) the cutoff point for treatment or not.

alternative hypothesis to dominate is that maternity leave was already quite long (more than 3 months) before the reform was implemented. Also, an increase in leave length of "only" 6 weeks may not make a big enough difference in relation to the 21 years that follow.

I expect to find support for the hypothesis since many empirical studies within both the economics, sociology, and psychology literatures suggest a positive effect on children from time spent with parents during early childhood, see e.g. Knudsen et al. (2006). Descriptive evidence presented in Tables 1 and 2 suggests a possible neutral or even negative relationship between access to longer birth-related leave and the child's cognitive outcome. The mean value of high school GPA in the population sample is lower for the treatment group and the mean is significantly different from the control group's mean. The PISA reading score seems to be smaller for the treatment group according to Table 2 and Figure 8 but the difference is not statistically significant. High school enrollment is significantly lower for the control group so the descriptive results point in different directions.

Figures 5, 6, and 7 in Section 4 show children's high school enrollment, high school GPA, and PISA reading score, respectively, by time of birth and calculated as means for all individuals born in 2-day intervals (5-day intervals for the PISA sample). The vertical line shows March 26th, 1984, i.e. the point in time when a discontinuity should be observable if the reform has an effect on long-term outcomes. Based on these figures, the effect of the reform seems to be modest at most which supports the evidence from the mean tables. In the next section, I estimate more complex models using regression discontinuity design where controls for being in the treatment group and for date of birth are included.

6.1 Estimation Results

Table 3 shows results from the RD estimation using the population sample. Two outcome measures are investigated; whether the child has completed high school as of 2005 or is still enrolled in high school (both of these will be referred to as high school enrollment), and the child's high school GPA if the child has completed high school. The results suggest that access to longer parental leave has no long-term effect neither on high school enrollment nor on high school GPA as the coefficient to the treatment dummy is insignificant in both specifications. The large sample sizes (19,000 and 8,700) naturally act to increase the likelihood of rejecting any coefficient value, making these results even more striking.

Table 4 shows results from the RD estimation using the PISA sample. An extra outcome variable is analyzed here, namely the child's PISA reading score. Again, the treatment variable has no significant effect on child outcomes. The sample sizes are somewhat smaller in the PISA sample than in the population sample but this is not likely to cause the insignificant coefficients. A more likely reason is that there are no measurable long-term effects on these educational outcomes since this was also suggested in Table 3 when using the population sample of children.

Dep.var.: High school enrollment	coefficient	t-statistic
treatment	0.0052	0.37
date of birth	0.0002	0.78
constant	0.5070	62.54
obs	19,081	
Dep.var.: GPA (high school)		
treatment	-0.0459	-1.09
date of birth	-0.0001	-0.08
constant	8.1892	344.78
obs	8,706	

Table 3: Regression discontinuity estimates for the population sample. Dependent variables are high school enrollment and high school GPA in 2005.

Dep.var.: High school enrollment	coefficient	t-statistic
treatment	0.0554	1.03
date of birth	-0.0009	-1.13
constant	0.5262	17.26
obs	1,341	
Dep.var.: GPA (high school)		
treatment	0.0778	0.51
date of birth	-0.0020	-0.92
constant	8.1685	95.15
obs	706	
Dep.var.: PISA reading score		
treatment	2.9914	0.29
date of birth	-0.1093	-0.71
constant	498.18	85.15
obs	1,341	

Table 4: Regression discontinuity estimates for the PISA sample. Dependent variables are high school enrollment in 2006, high school GPA in 2006, and PISA reading score in 2000.

6.2 Sensitivity Analysis

To verify these results and to make sure that the estimated model is correctly specified, I have conducted a variety of sensitivity anlyses. I have tested several alternative specifications. I have also estimated the model on different subsamples of the main sample to check the robustness of the results. All results described below are related to an analysis on the population sample since the PISA sample is just a subsample of the population sample.

In some of the alternative model specifications, interaction terms between date of birth and treatment are included. In others, higher order polynomials in the forcing variable, i.e. the date of birth, are included. These alternative specifications do not significantly improve the fit of the model. It is not appropriate to include 2nd (or higher) order polynomials in the forcing variable or to include interaction terms. Thus, the empirical model set up in Equation (1) appears to be the appropriate specification.[15]

I have further tested the model by looking for a treatment effect in various sample subsets. These specifications generally confirm the results shown in Tables 3 and 4. The results are substantially the same when the sample is restricted to include only those children whose mothers participated in the labor force and when the sample is restricted by gender of the child, parents' occupation, parents' level of education (completed high school or not), parents' employment status (part-time or full-time), or by parents' level of income. No treatment effect is apparent either when focusing

[15] These results are available from the author upon request.

Dep.var.: High school enrollment	coeff.	t-stat.	coeff.	t-stat.
born in 1984 (0/1)	0.0052	0.69	-0.0121	-1.53
treatment group (0/1)	0.0176	2.41	0.0112	1.48
born in 1984*treatment group	-0.0027	-0.26	0.0021	0.20
constant	0.4970	94.35	0.3428	16.96
controls†	no		yes	
obs	37,823		29,208	
Dep.var.: GPA (high school)				
born in 1984 (0/1)	0.0046	0.21	-0.0096	-0.41
treatment group (0/1)	-0.0286	-1.33	-0.0139	-0.63
born in 1984*treatment group	-0.0204	-0.67	-0.0220	-0.70
constant	8.1862	524.37	7.5024	130.23
controls†	no		yes	
obs	17,481		14,860	

†: parental work experience, annual wage income, level of education, and child's ethnicity.

Table 5: Differences-in-differences estimates for the population sample. Dependent variables are high school enrollment in 2005 and high school GPA in 2005.

on children growing up in single parent households or when the sample is reduced to consist only of children born in a one month interval on either side of March 26th instead of the two month interval used for the main results. In no case is a significant treatment effect observed.[16]

Finally, a differences-in-differences analysis using a population sample of children born between January 26th and May 25th in the years 1983 and 1984 is conducted. The control group is defined to be children born January 26th to March 25th, and the treatment group is children born March 26th to May 25th. In differences-in-differences estimates the coefficient of interest is the coefficient to the interaction term between year of birth and being in the treatment group. Table 5 confirms that using a differences-in-differences analysis there is not generally a long-term effect on children from parental eligibility to 6 weeks of parental leave on top of 14 weeks of maternity leave. This result persists when including a variety of family background information. Including these covariates, the significance of the coefficient to the treatment dummy disappears.

Generally, the sensitivity analysis reveals that the RD results are very robust.

7 Conclusion and Policy Implications

The length of parents' birth-related leave varies across countries with the Scandinavian countries among those with the longest leave lengths and the U.S. with one

[16]Results for specifications by single parent household, parents' educational level, and child's gender are shown in Appendix C.

of the shortest leave lengths. In this paper, the focus is on investigating one of the potential benefits of a longer parental leave length, namely the long-term effects of such leaves on children's outcomes. I use a natural experiment from 1984 in Denmark to evaluate the effect of increasing parents' leave length by almost 50% from 14 to 20 weeks. Selection was not an issue as the female labor force participation rate was high and the take-up rate of leave was also high.

Based on a population sample of children born two months on each side of the implementation of the reform, I find no evidence of a long-term effect on children when regression discontinuity design is used to estimate potential effects. Neither when using differences-in-differences do I find any effects. Whether the outcome measure is reading score at age 15, high school enrollment, or high school grade point average at the age of 21, there does not seem to be an effect.

I therefore conclude that there is no support for the hypothesis of a positive effect on children of increasing parents' access to birth-related leave from 14 to 20 weeks. On the other hand, I do not find negative effects on the children either, meaning that children are not harmed in the long-run when their parents have access to longer leave length. The results also imply that children are not harmed if their parents (mothers) work when the children are between 14 and 20 weeks old compared to if they only work when the child is older than 20 weeks. This is a valuable lesson as well.

An interesting question is *why* the leave reform does not seem to have an effect on children's long-term outcomes. Several explanations seem reasonable. First, day care may remove any differences between parental care and non-parental care for children in the ages 14 to 20 weeks. In Denmark a publicly subsidized day care system is well established, even for very young children. This was also true in the 1980s, see Esping-Andersen (2004). Further, since some children stay home longer with their parents as a result of the reform, there are in general fewer children in day care facilities and this may increase the quality for the remaining children, i.e. the children whose parents did not have access to longer leave. There may therefore be a positive externality on the children whose parents did not have access to the longer leave period.

Second, the leave length was 14 weeks, i.e. more than 3 months, before the reform. A period of 14 weeks of leave is comparable to what is available in e.g. the U.S. today[17] and may be enough for giving the child a good and positive "start" in life. Even though longer leave prolongs breastfeeding as shown by Baker and Milligan (2007) for Canada, they do not find an increase in child health associated with the longer period of breastfeeding. Therefore, even though mothers spend more time with their young children, it may not translate to higher long-term child outcomes.

In summary, the results seem to show that in the long-run children do not

[17] The U.S. policy is 12 weeks of leave given certain eligibility constraints. One of the U.S. constraints is very different from those in Denmark, namely the constraint regarding number of employees. Such a constraint does not exist in Denmark.

benefit but also are not harmed by mothers working when the children are 3 months old and further on. The analysis is carried out in a country with a high female labor force participation rate, also at the time of the reform. Results from this study can therefore inform other countries which today have similar labor force participation rates and comparable institutional settings as those in Denmark in 1984. For example, the female labor force participation rate in the U.S. today is very similar to the level in Denmark in 1984 according to OECD's Labor Force Statistics. Furthermore, the leave length in Denmark before the reform was almost the same as the leave length in the U.S. today. Therefore, the findings from this study might be extrapolated to the U.S. Of course one important difference between Denmark 1984 and the U.S. 2007 is in whether the leave is paid or not. The Danish leave being paid does not entail the same tradeoff between income and time that a change in the U.S. policy would. Nor does it result in as uncertain an impact on child outcomes. Theoretically, if the household has both the time and the money, then the child should be better off.

In this study focus has solely been on long-term child outcomes. No effect was found but longer parental leave might instead influence short-term outcomes as for example child health, maternal health, or maternal employment. I leave these questions for further research.

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A Appendix

The average weeks of leave for parents with children born in 1984 are unfortunately not available from Statistics Denmark separately for children born before and after March 26th. A yearly average for all child-related leaves in 1984 is available but is not very informative when a leave reform was implemented during the year. Therefore, since there was another leave reform in 1985 where the available amount of leave was higher than in 1984 *and* information is available for children born on either side of the reform, I here take a look at the 1985 reform.

Table 6 shows the average weeks of birth-related leave for men and women in Denmark in 1985. Before July 1st, 1985, almost all women took the 20 weeks of leave they were entitled to after birth and after July 1st almost all of them took the 24 weeks they were entitled to. The same pattern probably holds for mothers of children born in 1984 since the available amount of leave in 1984 is less than in 1985. This also seems to be confirmed by Figure 3.

Men*

Weeks after birth	Jan-Jun	Weeks after birth	Jul-Dec
1-2	1.8	1-2	1.9
15-20	3.4	15-24	4.5
21-	3.5	25-	4.6

Women**

Avr. weeks of leave	Jan-Jun	Jul-Dec
Total	24.5	29.4
After birth	19.3	23.2

* Excluding self-employed men. 95% of the men took less than 3 weeks of leave in 1985.

** Excluding self-employed women.

Source: Statistics Denmark: Statistiske Efterretninger, 1987.

Table 6: Average weeks of birth-related leave for men and women in Denmark in 1985.

B Appendix

Some covariates are graphed in Figures 9, 10, and 11 by birth intervals of 5 days. These figures are based on the population sample and they show that there are no obvious jumps or discontinuities at the point corresponding to March 26th, 1984 (March 26th, 1984, is marked with a vertical line). Therefore, parents of children born March 26th, 1984, or later do not seem to be different than parents of children born earlier than March 26th. Figure 11 also shows that there are no obvious differences between ethnic background for children in control and treatment groups.

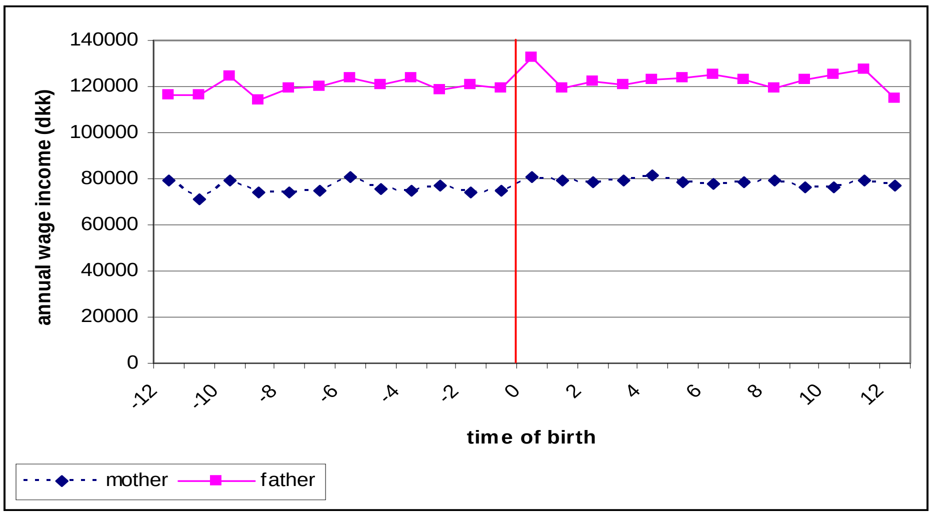

Figure 9: Annual wage income (DKK) for parents measured the year before childbirth, i.e. in 1983. Mean values are calculated using 5-day intervals.

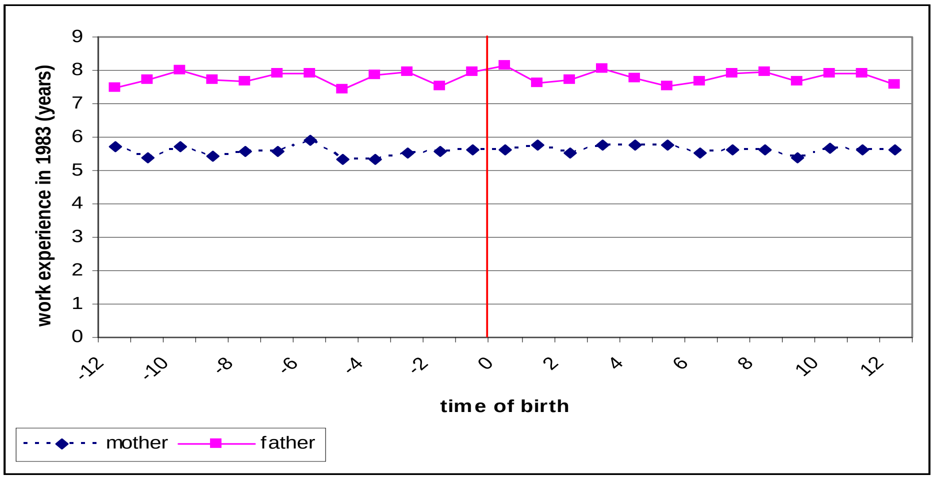

Figure 10: Work experience for parents measured in the year before childbirth, i.e. 1983. Mean values are calculated using 5-day intervals.

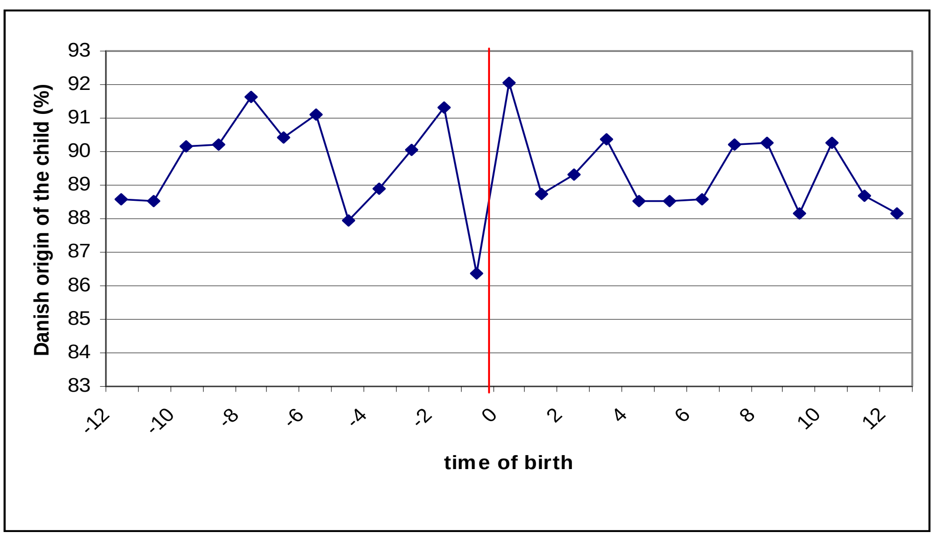

Figure 11: Ethnicity of the child. Mean values are calculated using 5-day intervals.

C Appendix

Dep.var.	High sch. enrollment		GPA (high school)	
	coefficient	t-statistic	coefficient	t-statistic
treatment	-0.0502	-0.91	0.3457	1.53
date of birth	0.0008	1.02	-0.0044	-1.39
constant	0.3591	11.31	7.8994	62.08
obs	1,162		335	

Table 7: Regression discontinuity estimates for the population sample when the child grows up in a single parent household.

Dep.var.	High sch. enrollment		GPA (high school)	
	coefficient	t-statistic	coefficient	t-statistic
treatment	0.0015	0.06	-0.1080	-1.61
date of birth	-0.0002	-0.43	0.0006	0.59
constant	0.7570	55.69	8.4973	226.55
obs	5,016		3,515	

Table 8: Regression discontinuity estimates for the population sample when the child's mother has completed high school.

Dep.var.	High sch. enrollment		GPA (high school)	
	coefficient	t-statistic	coefficient	t-statistic
treatment	0.0072	0.29	-0.0358	-0.49
date of birth	0.0001	0.26	0.0000	-0.03
constant	0.7993	55.67	8.4665	203.88
obs	3,845		2,863	

Table 9: Regression discontinuity estimates for the population sample when the child's father has completed high school.

Dep.var.	High sch. enrollment		GPA (high school)	
	coefficient	t-statistic	coefficient	t-statistic
treatment	0.0155	0.79	-0.0362	-0.54
date of birth	-0.0001	-0.23	-0.0005	-0.53
constant	0.4104	36.68	8.1541	215.38
obs	9,744		3,615	

Table 10: Regression discontinuity estimates for the population sample of boys.

Dep.var.	High sch. enrollment		GPA (high school)	
	coefficient	t-statistic	coefficient	t-statistic
treatment	-0.0016	-0.08	-0.0508	-0.94
date of birth	0.0004	1.27	0.0002	0.3
constant	0.6058	53.61	8.2130	269.59
obs	9,337		5,091	

Table 11: Regression discontinuity estimates for the population sample of girls.

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