School Enrolment and Mothers' Labour Supply: Evidence from a Regression Discontinuity Approach*

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Abstract

We analyse the impact on maternal employment of a universal school reform in Norway which lowered the school starting age from seven to six. We use a regression discontinuity approach exploiting exogenous variation in the compulsory school enrolment rule caused by the reform. Our results reveal positive short-term effects on labour supply (approximately 5 percentage points) and on earnings (about 12600/1350 NOK/Euro). Subgroup analyses show that the positive effects are much stronger for mothers with low wage potential, a group of mothers that were less likely to use formal childcare prior to the reform. The positive effects for this subgroup of mothers suggest that expanding child-care can be an effective tool for increasing labour supply of mothers that previously had relatively low labour market earnings potential.

Index terms: I21, J22

JEL codes: Labour supply, Mothers, school entry, regression discontinuity.

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1 Introduction

Public schooling not only contributes to the cognitive development of children, it may also have a positive impact on mothers' labour supply, through free and compulsory childcare. In this paper we exploit an exogenous change in the age for school enrolment to analyse the effect of subsidised and compulsory childcare on maternal employment. Prior to 1997, all pupils in Norway started compulsory school at the age of seven. In 1997 the age was lowered from seven to six. The intention of the reform was to provide all children an equally high quality early pedagogical environment. The reform implied a change in both the availability and price of childcare for children six years of age, and hence could potentially reduce the cost of labour market participation for mothers, especially among mothers who are low skilled and have low earning potential. Thus, we are interested in identifying the following: Is lack of free child care a barrier for mothers of school age children to seek and find work?

According to the neoclassical labour supply theory, a mother will adjust her labour supply such that she maximises the value of consumption and leisure (including childcare) subject to a budget constraint. At the extensive margin, the mother can choose between working (and purchasing childcare) or leisure (which implies taking care of the child herself). The impact of cheaper childcare on female labour supply is unambiguously positive for mothers that initially do not participate in the labour market. However, as shown in Gelbach (2002) and Fitzpatrick (2012), using this framework results in ambiguous effects of childcare subsidies on working hours for mothers already participating in the labour market. For mothers who before the reform worked longer than the number of hours provided by care, the reform comes with an income effect putting a downward pressure on labour supply. The Norwegian enrolment school reform of 1997 (REF97) provides a full-price subsidy for childcare for any mother working less than the length of the time the child is in day care. In the empirical analyses we will mainly focus on labour supply on the extensive margin, i.e, we will focus on the binary choice of employment or no-employment. For this group, the reform provides an economic incentive to enter the labour market.

Norway has, together with Denmark and Sweden, the highest female employment rate in the OECD (OECD 2015). Much of the increase in Norway occurred in the 1970s and 1980s (Statistics Norway 2012) and has stabilised since; for women aged 15-64, the employment rate has been around 73 to 74 per cent for the last 20 years. Female employment varies strongly with education; in 2015, the employment/population ratio for women 25-64 was 58 per cent among those with less than upper secondary education, 79 per cent among those with upper secondary education and 88 per cent among those with tertiary education(OECD 2015). The corresponding US percentages are 47, 67, and 79. The labour supply share of mothers' of six years old children in 1996 (the year prior to REF97) was 69 per cent, which is relatively high in comparative perspective. Together with high childcare coverage for five- to six-year-old children and strongly subsidised childcare prices, it is not obvious that such a public school enrolment reform will affect labour supply. We believe the Norwegian context is a difficult one to find effects of childcare, thus, positive effects provides useful information for policy makers in countries with lower childcare coverage.

Our paper relates to the quasi-experimental literature that seeks to analyse the effect of enrolling children in full-time or part-time education on the mothers' labour supply behaviour (Gelbach 2002; Schlosser 2007; Cascio 2009; Brewer and Crawford 2010; Berlinski and Galiani 2007; Berlinski, Galiani, and McEwan 2011; Fitzpatrick 2010, 2012; Havnes and Mogstad 2011). Havnes and Mogstad (2011) investigates the impact on maternal employment of a large expansion in childcare coverage in Norway during the 1970s, focusing on mothers of children aged 3 to 6. Using a difference-in-differences approach, they find no effect of the increased capacity on maternal employment and suggest that the new subsidized childcare may have crowded out informal childcare arrangements. A number of studies on US data find different results. Gelbach (2002) uses an IV-approach to find that free public school enrolment of five-year-olds has a statistically and economically significant effect on the labour market outcomes of mothers without younger children. Cascio (2009) uses data from the 1960s, 1970s and 1980s to analyse how state-funded kindergarten for five-year-olds affects maternal employment. These papers find that sin-

gle women with age-eligible children were highly responsive.

In terms of methods, our study is more in line with the recent papers of Berlinski, Galiani, and McEwan (2011), Goux and Maurin (2010) and Fitzpatrick (2012). Moreover, these papers study data from about the same time period as we do. Berlinski et al. (2011) utilize regional variation in compulsory education enrolment rules in Argentina that cut the year in half (June 30th). Their estimates suggest that preschool attendance of the youngest child in the household increases the mothers' labour supply. Goux and Maurin (2010) utilize regional variation in public availability for child care for two-year olds. They find a significant employment effect for lone mothers, but no effect for two-parent families. They explain the results in terms of the two-parent families having access to several alternative non-parental modes of childcare and suggest that pre-elementary school may crowd-out these non-parental alternatives. This finding is in line those in Fitzpatrick (2012), who finds that public school enrollment increases only the employment of single mothers without additional young children.

In this paper we utilize the strict adherence to calender years to decide entry to elementary school to estimate the labour supply effects using a regression discontinuity (RD) design. Compared to the differences-in-differences design employed by Havnes and Magstad's (2011) analysis of Norwegian data, RD builds on weaker assumptions and, importantly, the key assumptions (no manipulation and no discontinuity for relevant covariates) are directly testable Lee and Lemieux (2009). We employ recently developed, state-of-the-art, non-parametric RD-techniques to estimate the effect of the reform (Calonico, Cattaneo, and Titiunik 2014*b*).

The paper proceeds as follows: The next section presents the reform and the institutional settings in Norway, as well as other relevant reforms. Section 3 presents the data, the sample, and the variables. Section 4 presents the results, and section 5 provides the conclusion.

2 The reform and the institutional setting

The 1997 reform

The enrolment school reform of 1997 (REF97) is composed of two main parts: i) the age at which children must start school was changed from age seven to age six and ii) the length of compulsory school was prolonged from nine to ten years. It is the first part of the reform that we exploit. One of the main motivations for REF97 was to provide high-quality pedagogical training for young children.[1] By lowering the compulsory school enrolment age, all children aged six would receive equal educational opportunities, which would in turn diminish differences due to socioeconomic background.

One cohort of children in Norway consists of approximately 60000 children. The mothers of these children when they are six years old can be considered to be the gross number of mothers potentially affected by the reform. Of course, a large part of mothers of children aged six was already working prior to the reform. In our data, the labour supply share of mothers' of six years old children in 1996 (the year prior to the reform) was 69 per cent. This implies that the reform could potentially affect the labour supply decision of only a minority of mothers, but still quite a large minority.

The first cohort of pupils that had to abide by the reform started first grade in August of 1997. Enrolment rules follow the year of birth, so all children born in 1991 are affected. Parents can apply to the municipality to delay starting age by one year or start one year early on pedagogical and psychological grounds, but this is very rarely accepted. Thus, in practice, Norway practices very strict compulsory school enrollment rules based on year of birth. Furthermore, there is essentially no grade retention in Norway (Strøm 2004; Bedard and Dhuey 2006), so almost all pupils start at the same age in compulsory school and finish together. No other reforms were implemented simultaneously that might jeopardise our estimates.[2]

[1] Drange, Havnes, and Sandsør (2012) find no effects of the reform on pupils' long-term educational achievement in secondary school.

[2] Two other major reforms that might have influenced mothers labour supply were implemented in the 1990s. In 1998, the Cash-for-Care reform was implemented, offering a monthly lump-sum subsidy to families with one-year-old children that did not attend publicly subsidised childcare. This reform was implemented after the period we study in this paper. In 1993, the length of paid parental leave in

Childcare coverage and childcare costs

During the period we study the cost of a slot in a publicly subsidised childcare facility varied considerably between municipalities. Some municipalities had a flat rate, while the majority tied prices to household income. In 1996, the year preceding the reform, the average monthly childcare cost across all municipalities for a full-time slot was 1500 Norwegian kroner (NOK) for those with an annual household income less than 100000 NOK; 2250 NOK for households with less than 250000 in annual income and 2430 NOK for households with an annual income higher than 250000 NOK (Statistics Norway, 1996). As for 1997, monthly rates for a kindergarten slot varied from 1900-3600 NOK for families with 375000 NOK in annual income. For families with 250000 NOK in income payment varied from 1700 in the cheapest municipalities to 3200 NOK in the most expensive.[3]

Compulsory school, however, is free of charge, implying that the reform made public child care for six year olds free of charge during the academic year. The usual school week in elementary school is between 20-21 hours, with pupils attending the school that is closest to where they live. All municipalities have to provide Out-of-school facilities (SFO) for children in first to fourth grade, with prices which are affordable for most households and grants for those with insufficient means. SFOs are open 11 months a year on a a part-time (12 hrs) or full-time (25 hrs) basis. Since SFOs are on school grounds, children remain in the same place from about 7:30 or 8:00 until 16:30. Kindergartens (day care services) have the same opening hours. Thus, kindergartens and SFO enable both parents to work full-time.

Historically, slots for childcare have been rationed in Norway, but the accessibility of child care facilities has increased dramatically over the recent decades. Since 2005, there have been practically full coverage for children ages three and older. This means that childcare slots are today universally accessible. However, during the period under study, the access was still rationed. In our sample of mothers, 85 percent of those who gave

Norway was extended, but this was after the mothers in our sample gave birth. Below we further show that treatment and control groups are balanced on the probability of having older or younger children

[3]These numbers are calculated from a survey conducted by Statistics Norway covering approximately 30 per cent of the municipalities in Norway in the spring of 1997.

birth the year before the reform had their child in childcare when the child was six years of age (see Appendix Table A-1). The reform mechanically increased this number to 100 percent for the next cohort. These numbers nonetheless underpin the view that a large share of six-year-old children were already in day care prior to the reform. Consequently we might not expect the school reform to have large employment effects on most mothers. However, as the table shows, the increase is much larger for those who had low labour earnings prior to the reform.

3 Empirical approach

Data and variables

The data consist of merged administrative registers encrypted to prevent identification of individuals and made available by Statistics Norway for research purposes. The starting point is a public demographic register with information on all births in Norway linked to information on the mother regarding employment, earnings, and education. The main sample consists of all women aged 18-40 (when they gave birth) who gave birth in the months October-December 1990 and January-March 1991.[4]

As the main dependent variable, we use a binary measure of labour supply. This takes the value 1 if the mother is registered as employed during the year of observation and 0 otherwise. This measure of labour supply is taken from individual information on spells of employment and wages from merged files from "The Employer-/Employee Register" ("Arbeidsgiver-/Arbeidstakerregisteret") administered by The Norwegian Labour and Welfare Service (NAV) and organised by Statistics Norway. To ensure reliability, we include only jobs spells that are also linked to a wage observation. The years of observation are 1996 (for those that gave birth in 1990) and 1997 (for those that gave birth in 1991). Since the two groups of mothers are evaluated in 1996 and 1997 respectively, we need to make sure that macroeconomic differences between these years are not driving our results. We conduct placebo analyses of fathers and mothers with older children to

[4]There are almost no missing data in the administrative registers. We use listwise deletion to handle the few missing observations.

deal with this concern. We describe these placebos below.

Identification strategy

We analyze the effect of free child care using a regression discontinuity (RD) approach. The RD design is made possible by the strict adherence to calender years to determine entry to elementary school. Mothers who gave birth on and after January 1, 1991, had access to free childcare the year the child turn six, while mothers who gave birth before this cut off date did not. The register data on the exact date mothers gave birth combined with the strict rule makes it possible to estimate the causal effect of free childcare for pre-school children.

We assume that each mother's labour supply the year the child turns six has two potential outcomes; $Y_i(1)$ which is the observed decision to participate if eligible to free childcare, and $Y_i(0)$ if not eligible. The causal effect of eligibility to free childcare, τ_i, is the difference between these two potential outcomes: $\tau_i = Y_i(1) - Y_i(0)$. Unfortunately, we can only observe one potential outcome for each mother i. If she gave birth after January 1, 1991, we observe $Y_i(1)$, if she gave birth before January 1, 1991, we observe $Y_i(0)$.

The key empirical challenge is to get plausible counterfactuals for the two scenarios. Using RD,[5] we estimate the relationship between distance to the eligibility date (Jan 1, 1991) and participation in the labour market when the child is six years of age, at each side of the eligibility date (Jan 1, 1991). These regressions give us the average potential outcome at each date; $E[Y_i(1)|X_i]$ for those who gave birth at or later than to Jan 1, 1991 and labour supply, and $E[Y_i(0)|X_i]$ for those giving birth prior to Jan 1, 1991 and thus not eligible to free childcare. On the eligibility date these two regression lines almost meet, and the distance between the observed curves at this point is often considered to be a good estimate of the treatment effect τ_i.

The assumption of τ_i being a good estimate of the treatment effect hinges on the ability of units to influence on what side of the cutoff they are located. Parents can obviously

[5]see Lee and Lemieux (2009) and Skovron and Titiunik (2015) for excellent introductions to RD.

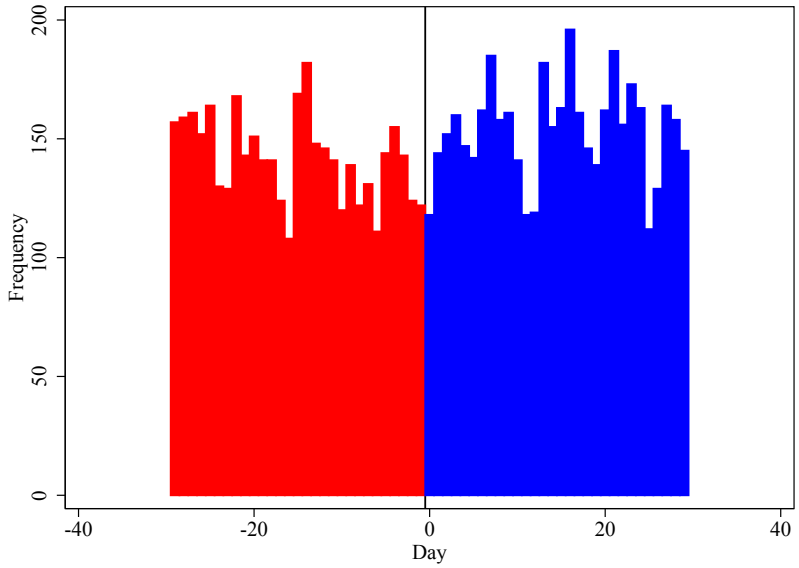
influence their location vis-a-vis the cutoff, however, close to the cutoff their ability to manipulate their location is limited. Moreover, when the mother got pregnant she was of course unaware of the future implementation of REF97, thus, any sorting around Jan 1, 1991 should be similar to calendar year sorting in other years. We empirically examine the importance of this type of sorting using a density test.

Estimation

We estimate τ_i using a local polynomial RD. Local polynomial RD is preferred to global RD because in the latter observations far away from the cut-off might influence the estimate of τ_i (Gelman and Imbens 2014). The most important decision to make is to select the bandwidth, which in our case refers to how many days around the cut off date we should include when estimating τ_i. While the precision of the treatment estimate increases with the size of the bandwidth, the bias in the estimate might increase with larger bandwidths since it will often be more difficult to correctly model the regression slopes at each side of the cut off. If the slopes are incorrectly estimated, the treatment effect will be biased. We follow recent recommendations and developments in RD methodology and select the optimal bandwidth using a data-driven, non-parametric approach (Skovron and Titiunik 2015). In our main analyses, we rely on the Calonico, Cattaneo, and Titiunik (2014*b*; 2014*a*) approach to select the bandwidth, but we also report results using the Imbens and Kalyanaraman (2012) approach as a robustness check.

A second decision is to choose the polynomial order. This decision is important as wrong functional forms will bias the treatment estimate. The bandwidth selection procedures select the optimal bandwidth given the chosen polynomial order, thus results should not be very sensitive to the choice of polynomials. Following recent advice on the dangers of over-fitting when using more than two polynomials (Gelman and Imbens 2014; Skovron and Titiunik 2015), we rely on linear or quadratic specifications. A third decision regards the choice of kernel function, which determines how heavy we choose to weight observations closer to the cut-off. We rely on a triangular Kernel, which is the standard choice in the literature (Skovron and Titiunik 2015, 14). Using a triangular kernel implies

Figure 1: Number of children born each day around Jan 1, 1991



that the further away from the cut-off, the less weight we put on the observations.

4 Empirical Results

Before presenting the treatment effect we study the validity of the design. Figure 1 plots the number of children born each day around the cut off date of Jan 1, 1991. We expect the figure to look similar at both sides of the cut off. The figure shows that the number of births is lower than average on Jan 1, but similar to the number of births on the two last days of 1990. The dips in the numbers of births are Saturdays, Sundays, or holidays when no planned births occurred. The number of births increases on Jan 2, 1991, probably because births are again planned for from this date. Frandsens's (2013) density test of whether there is a jump in the density at the cut-off has a p-value of .30.

Next we run the analysis on a set of pre-determined covariates for which we should not see substantial "treatment effects" in the form of discontinuities in their distributions at the cut-off. We report these results in Table 1 and a set of graphs in the appendix (Figure A-1). Descriptive statistics are included in the Appendix. Reassuringly, we find no large or significant treatment effects on these covariates. The figures show that for

Table 1: RD on pre-determined covariates. Local polynomial analysis. Optimal bandwidths selected using the approach of Calonico, Cattaneo, and Titiunik (2014*b*). N=27769.

Covariate	Bandwidth	Treatment coefficient	Robust p-val
Age	33	-.242	.32
Compulsory school	24	-.006	.95
Secondary education	32	006	.93
University/college	25	-.006	.62
Married	34	.014	.51
Native	27	.000	.89
Has older children	39	-.008	.82
Has younger children	36	-.014	.61
Unemployment rate (county)	36	-.020	.54

Table 2: RD of free child care the year the child turn six and mothers' labour supply. N=27769.

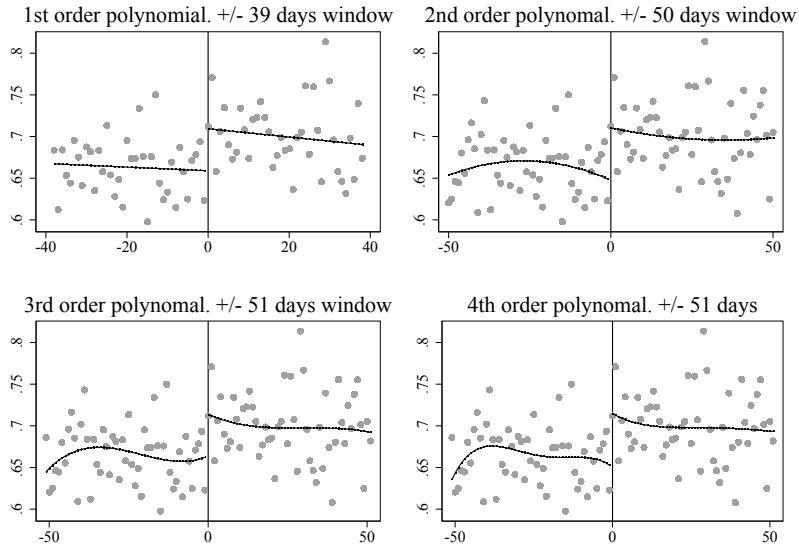
Polynomial order	Bandwidth	Method	Treatment coefficient	Robust p-val
1	39	CCT	.051	.02
2	50	CCT	.056	.06
3	51	CCT	.055	.13
1	78	CCT	.045	.01
1	53	IK	.050	.01

some variables there are small but insignificant shifts, and in a few figures the estimated trends are different at the two sides of the cut off. However, in sum these findings suggest that there are no sudden shifts in the characteristics of mothers at the cut off, i.e. if we see a shift in labour supply at the cut off it is unlikely to be due to shifts in the composition of mothers.

We now turn to the analysis of labour supply. The estimates are reported in Table 2 and presented graphically in Figure 2. The estimated treatment effect is about 5 percentage points in the main specification (the top row), where we estimate the treatment effect using the Calonico, Cattaneo, and Titiunik (CCT) approach to select the optimal bandwidth. The employment rate in the control group is 67 percent, so we consider the effect to be sizeable.

The next two rows present the treatment effect when we add polynomials. As evident, the treatment effect is very close to the main estimate. The standard errors of the

Figure 2: Treatment effects. Optimal bandwidths selected using the approach of Calonico, Cattaneo, and Titiunik (2014*b*)



estimates are, however, somewhat sensitive to the number of polynomials. In the fourth row we show that the treatment estimate reduces slightly to about 4.5 percentage points when we follow the advice of doubling the optimal bandwidth as a robustness check.

Finally, in the bottom row we show that the Imbens and Kalyanaraman approach to determine the optimal bandwidth produces the same point estimate as the CCT approach. Thus, the 5 percentage points treatment estimate is robust to small changes in the model specification.

In the Appendix we present two additional analyses. First we study whether the probability of working more than 15 hours a week increases at the cut point. Ideally, we would have studied a continuous variable of labour supply, but the administrative registers do not include such fine-tuned information. We include those with no employment in these analyses, because we do not want to condition our outcome on a clearly endogenous variable. The results, presented in Table A-4 show a positive treatment effect on this outcome.

Second, we present treatment effects on total annual labour earnings (in 1997 NOK). Again we include those without earnings to avoid conditioning on employment. The

results (Table A-5) show that the reform increased earnings in the treatment group by about 13000 NOK, a quite large effect in view of mean earnings of 108000 NOK in the control group. In relative terms, the size of this effect is on par with the size of the employment effects. We find it reassuring that we find similar effects using different measures of labour supply. As for employment, the treatment estimate does not differ much across specification, but again it is less precisely estimated when we add polynomials.

5 Placebo analyses

To increase the credibility that the results presented above are caused by the reform we present a set of placebo analyses. First we analyze the effects for fathers. If we find an effect on the fathers' labour supply it suggests that the main results probably pick up the effect of a positive macroeconomic shock, since it is the mothers who mostly stay at home to take care of small children. Reassuringly, we find no effect of childcare on the fathers' labour supply in Table 3. The estimated treatment effect on fathers is -.008, p=.52 (26 days bandwidth), i.e. substantively and statistically insignificant. Moreover, the negative coefficient suggest that, if anything, the treatment effects estimated above are downward rather than upward biased. For earnings, the effect is 2240 NOK, p=.97 (31 days bandwidth).

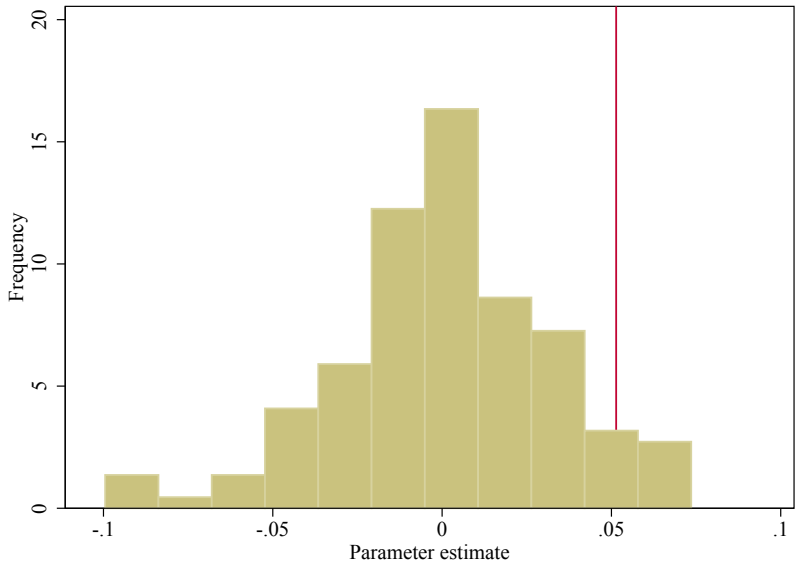
Next we conduct the analyses on mothers with children which are one year older than in our sample, i.e. a group that started school one year earlier. We should not expect to see a treatment effect on these mothers unless our main results are picking up something other than the reform. Again we find a small, negative, and insignificant effect (-.006, p=.65, 35 days bandwidth). We get small, negative, and insignificant effects also for earnings (-2272, p=.49, 34 days bandwidth).

Finally, we run 140 placebo analyses, moving the cut off one day at the time +/-70 days away from the true cut off of Jan 1, 1991. The treatment estimates from this exercise are presented in Figure 3. Although we find a couple of treatment effects more extreme than the true effect, our estimate is clearly in the tail of the distribution. Moreover, the mean of the estimates is 0, suggesting that the null hypothesis of a zero difference at the

Table 3: RD of free child care the year the child turn six and labour supply.

Polynomial order	Bandwidth	Method	Treatment coefficient	Robust p-val
Fathers. N=25579.				
1	26	CCT	-.008	.52
2	32	CCT	-.022	.38
3	48	CCT	-.027	.38
1	78	CCT	.009	.66
1	44	IK	.005	.98
Mothers with one year older children. N=27411.				
1	35	CCT	-.006	.65
2	36	CCT	-.021	.45
3	52	CCT	-.024	.45
1	70	CCT	.009	.74
1	50	IK	.002	.70

Figure 3: The distribution of placebo treatment effects. The line identifies the true treatment effect.



cut off is a meaningful hypothesis.

6 Treatment heterogeneity

Next we investigate whether free childcare has heterogeneous effects depending on prior labour earnings. We split the sample according to whether the mother in 1994 had a

yearly labour earnings above or below 2 G,[6] which in 1994 amounted to 76,000 NOK, and compare the coefficients across the groups (descriptive statistics are reported in the Appendix). In the group of mothers with labour earnings below 2G we also include mothers with no labour earnings, i.e., this group consists of mothers with low and no labour earnings, as we want to measure changes on both the intensive and extensive margin. Having no labour earnings does not imply that the mother had no other incomes (for example from unemployment benefits), but she did not participate on the labour market. The motivation behind this categorization is to distinguish between mothers that had (below 2 G) and mothers that had not much (above 2 G) extra labour to offer.[7] We find no indications of a discontinuity in the proportion with low earnings in 1994 at our cut-off if we consider low-earnings as a pre-determined covariate and run an RD with low earnings as the dependent variable (τ_i=.016, p=.46).

Statistics from our data (Table A-1) show that about 96 percent of those with pre-earnings above 2G had their child in formal childcare when the child was five, compared to approximately 68 percent among those with pre-earnings below 2G. Since most of the previous high wage earners had their child in childcare prior to the reform and thus were not affected by the reform, we expect low earnings mothers to drive the treatment effect. Stronger responsiveness to child care costs among low-earnings mothers have been documented in previous research (e.g. Mason and Kuhlthau 1992; Baum 2002).[8]

The results are presented in Table 4 and Figure 4. The results are indisputable and in line with our priors: The treatment effect is solely driven by low earnings mothers. There is a huge effect on mothers with low previous earnings, while there is no effect on high earnings mothers. Consistent with the employment effects, we find that the earnings effect is driven by low earnings mothers (see Table A-5 in the Appendix).

Finally, we have also estimated the models separately for those with low and high edu-

[6]G is the basic level of support in the Norwegian Social Security System.

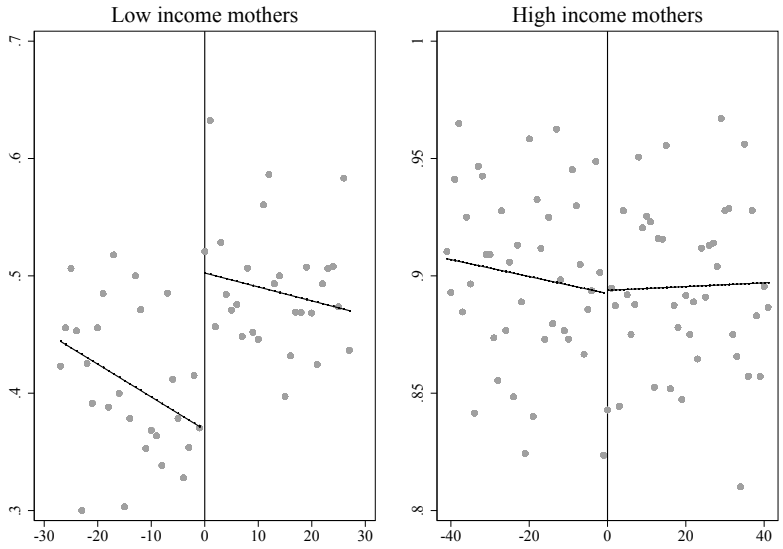
[7]We choose 1994 to allow some distance to the years of observation.

[8]Oppenheim Mason and Kuhlthau (1992) analyse employment barriers for sample of Detroit-area mothers of preschool-aged children. The results suggest that policies to increase the supply of child care or to lower its cost could increase female labor supply by a substantial fraction. Baum (2002) use a hazard framework to examine a mother's decisions about work and hours of work after childbirth. The focus is on low-income mothers with infants. The results showed that child care costs are a barrier to work that is larger for low-income mothers than for non-low-income mothers.

Table 4: RD of free child care the year the child turn six and mothers' labour supply. Sample split by mothers' previous earnings.

Polynomial order	Bandwidth	Method	Treatment coefficient	Robust p-val
Low labour earnings. N=13037				
1	27	CCT	.147	.00
2	46	CCT	.157	.00
3	54	CCT	.165	.01
1	54	CCT	.112	.00
1	35	IK	.136	.00
High labour earnings. N=14732				
1	41	CCT	-.007	.68
2	41	CCT	-.012	.74
3	47	CCT	-.005	.91
1	82	CCT	-.003	.75
1	67	IK	-.004	.55

Figure 4: Treatment effects according to previous labour earnings. Optimal bandwidths selected using the approach of Calonico, Cattaneo, and Titiunik (2014*b*).



cation. These estimates point in the same direction of a stronger effect among those with low education, however, the differences between the groups are much smaller. Moreover, the treatment estimates show more variation across the models so the results do not allow us to conclude that there is heterogeneity according to level of education.

7 Conclusion

In this paper, we present evidence of the impact of free childcare on mothers' labour supply. In 1997, the statutory school starting age in Norway was lowered from seven to six. We use this change in the compulsory school enrolment rule to analyse the effect of changes in the price of public childcare on mothers' labour supply. We take advantage of the strict adherence to calendar years to decide entry to elementary school to estimate the labour supply effects using a regression discontinuity (RD) design. Compared to DD or IV-designs, used by the majority of studies within the field, RD builds on weaker assumptions and, importantly, the key assumptions are directly testable (Lee and Lemieux 2009).

In the main analysis we find a positive effect of lower compulsory school enrolment age on mothers' labour supply at the extensive margin. The effect is estimated to approximately 5 percentage points, which is fairly stable across different specifications. Compared to the mean employment rate in the control group (67 percent), this represents an increase of 7 per cent, which we consider to be a sizeable effect. Results from placebo analyses, where we run 140 placebo analyses moving the cut off one day at the time, analyses of father's labour supply, as well as analyses of the labour supply of mothers with older children, strengthen the interpretation that we do identify the effect of the school age reform.

Heterogeneity analyses further show that the positive effect is driven by mothers with low pre-earnings. No effects are found for mothers with higher wage potential. These results suggest that offering free childcare for six-year-old children is not an effective means to increase the labour supply of mothers in general, but might spur the labour supply among women with low labour market attachment. These results are likely explained by

high initial female labour market participation among Norwegian mothers and relatively cheap and widely available childcare. For most mothers, the school reform implied that the child went to school instead of kindergarten. The positive labour supply effects are found for mothers who were less likely to have their child in childcare at age five. The labour supply among this group of mothers is relatively low, and consequently they have extra labour supply to offer.

What is the relevance of our results? The low income group of mothers, for which we find an effect, constitute less than half of the full sample of mothers potentially affected by the reform. Still, this is a non-trivial sized group. Furthermore, considering likely positive impacts of higher maternal labour supply on family economics, as well as on the offspring's wellbeing and future development, we argue that it is important to increase this group's employment. For instance, one might argue that the availability of almost free childcare should be provided when the children are even younger than the six year olds we study in this paper.

One should be cautious in extrapolating results across time and different institutional settings, but our results are in line with recent European studies using data for the period after the turn of the millenium (Bettendorf, Jongen, and Muller 2015; Givord and Marbot 2015). Still, our results warrants some further comments as they seem to be in contrast to results from, for example, Havnes and Mogstad (2011), who also use Norwegian data but find no impact. They propose that formal childcare facilities crowded out informal childcare modes as an explanation for their null findings. Such a crowd-out mechanism will diminish the impact on the maternal labor supply. Since the period covered by their study, the existence of informal markets has become almost negligible, and so has the likelihood of a crowding out mechanism. Another reason for the differences in results might be that the children are of a different age. Social norms question whether the gains for small children from child care, particularly long hours, are important. Hence, mothers may be more hesitant to put infants and toddlers in care than a 6 year-old. Finally, the group of mothers is different, with different intentions to work. Low-income mothers in the 1970s might have been less inclined to work regardless of the availability of child care

due to opportunities and norms which were very different in the late 1990s.

Moreover, the stronger results for relatively low attachment women is in line with results from recent studies (Cascio 2009; Goux and Maurin 2010; Fitzpatrick 2012). Fitzpatrick (2012), for example, reports positive labour supply estimates in the US, but limited to single mothers. She argues that the small effects for the larger group of women are because the population of mothers has changed, and so has the population at the margin. Similarly, Heim (2007) reports falling labour supply elasticities of women over time, which are in line with these results. Still, the positive effects for a non-trivial part of the group of mothers in our sample, suggest that expanding child-care is an effective tool for increasing labour supply of mothers that previously had a relatively low labour market earnings potential.

This result may be given some final thoughts, including some further contextual interpretations. As mentioned earlier, in an international context, Norway has a relatively generous welfare system, with a strong social safety net. In such a setting one might expect early school start to have less of an impact on maternal labour supply for mothers with potentially low earnings capacity in the labour market. However, wages in Norway are compressed, and they are especially compressed at the bottom of the distribution, creating relative high wages in this part of the wage distribution. The high wages at the bottom increase low wage mothers' economic incentives to work in response to reduced child-care costs. Moreover, Norway is also a country with high gender equality. If female participation in the labour market is a shared norm by many, such norm may add to the economic incentives to work, and thereby contribute to a positive impact of lower child care costs on maternal labour supply.[9]

[9]We interpret the increase in labour supply as a response to changes in childcare costs and availability. One might argue that school start provides an "institutional signal" for when mothers should return to work, which might explain part of the effects we find. Still, believing in the natural experiment we are exploiting, we argue that the effect we find mainly reflect impacts of reduced child care costs and increased capacity.

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Appendix

Table A-1: Trend in utilization of childcare among children born in 1990 and 1991

	All mothers			
	Age of child			
	3	4	5	6
Born in 1991	78.7	81.2	83.3	100
Born in 1990	78.0	80,3	82,4	84.6
	Low wage mothers			
	Age of child			
	3	4	5	6
Born in 1991	59.5	64.2	68.8	100
Born in 1990	58.9	63.0	67.6	72.3
	High wage mothers			
	Age of child			
	3	4	5	6
Born in 1991	95.5	96.1	96.0	100
Born in 1990	95.6	96.2	96.0	96.0

Note: We construct a proxy for childcare attendance using information from parents' tax records on tax deductions for formal childcare expenses. The annual tax deduction is up to 25000 NOK for one child and 30000 NOK for two children. To identify whether a child is enrolled in childcare, we construct a binary variable equal to 1 if the child's parents claimed a tax deduction for the cost of external childcare costs, and 0 otherwise. Low (high) wage mothers are mothers with yearly labour earnings below (above) 2 G in 1994.

Table A-2: Descriptive statistics broken down by treatment status

	Treatment Mean (SD)	Control Mean (SD)
Labour supply	0.704	0.673
	(0.457)	(0.469)
Earnings	114,828.747	108,238.397
	(96,524.367)	(94,794.842)
Age	34.305	34.424
	(4.841)	(4.864)
Comp. edu	0.319	0.325
	(0.466)	(0.468)
Sec. edu	0.414	0.409
	(0.493)	(0.492)
University edu	0.256	0.252
	(0.436)	(0.434)
Married	0.679	0.660
	(0.467)	(0.474)
Native	0.931	0.928
	(0.254)	(0.258)
Has older kids	0.580	0.555
	(0.493)	(0.497)
Has younger kids	0.525	0.539
	(0.499)	(0.498)
County unemp	3.258	3.275
	(0.717)	(0.706)
Observations	14,559	13,210

Table A-3: Descriptive statistics broken down by subgroup and treatment status

	High earnings		Low earnings	
	Treatment Mean (SD)	Control Mean (SD)	Treatment Mean (SD)	Control Mean (SD)
Labour supply	0.898	0.904	0.482	0.414
	(0.303)	(0.295)	(0.500)	(0.493)
Age	35.09	35.20	33.41	33.55
	(4.459)	(4.544)	(5.095)	(5.060)
Comp. edu	0.208	0.220	0.446	0.442
	(0.406)	(0.414)	(0.497)	(0.497)
Sec. edu	0.426	0.423	0.400	0.393
	(0.494)	(0.494)	(0.490)	(0.489)
University edu	0.364	0.353	0.138	0.139
	(0.481)	(0.478)	(0.339)	(0.346)
Married	0.773	0.759	0.737	0.733
	(0.419)	(0.427)	(0.440)	(0.442)
Native	0.961	0.962	0.897	0.891
	(0.194)	(0.192)	(0.304)	(0.312)
Has older kids	0.548	0.521	0.618	0.594
	(0.498)	(0.500)	(0.486)	(0.491)
Has younger kids	0.529	0.535	0.521	0.545
	(0.499)	(0.499)	(0.500)	(0.498)
County unemp	3.244	3.264	3.274	3.286
	(0.744)	(0.725)	(0.683)	(0.684)
Observations	7,755	6,977	6,804	6,233

Table A-4: RD of free child care the year the child turn six and mothers' labour supply. Dependent variable is whether the mother works more than 15 hours per week. N=27229.

Polynomial order	Bandwidth	Method	Treatment coefficient	Robust p-val
1	28	CCT	.053	.07
2	34	CCT	.067	.05
3	40	CCT	.076	.09
1	56	CCT	.054	.08
1	50	IK	.051	.08

Table A-5: RD of free child care the year the child turn six and mothers' annual labour earnings.

Polynomial order	Bandwidth	Method	Treatment coefficient	Robust p-val
Earnings. N=27769				
1	31	CCT	12665	.02
2	37	CCT	13001	.05
3	46	CCT	13551	.10
1	62	CCT	13307	.01
1	68	IK	13334	.02
Low income. N=13037				
1	29	CCT	20961	.00
2	39	CCT	22427	.00
3	45	CCT	23955	.01
1	54	CCT	18681	.00
1	38	IK	20580	.00
High income. N=14732				
1	37	CCT	8740	.14
2	47	CCT	9505	.17
3	42	CCT	10441	.30
1	82	CCT	7962	.04
1	56	IK	8980	.21

Figure A-1: RD on pre-determined covariates. Optimal bandwidths (CCT)