Does Money Matter?
The Effect of Child Care Subsidies on Academic Performance*

by

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PRELIMINIARY

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Abstract

Given the wide use of childcare subsidies across countries, it is surprising how little we know about the effect of these subsidies on children's longer run outcomes. Using a sharp discontinuity in the price of childcare in Norway, we are able to isolate the effects of childcare subsidies on both parental and student outcomes. We find very small and statistically insignificant effects of childcare subsidies on childcare utilization and parental labor force participation. Despite this, we find significant positive effect of the subsidies on children's academic performance in junior high school, suggesting the positive shock to disposable income provided by the subsidies may be helping to improve children's scholastic aptitude.

1. Introduction

Many countries have implemented childcare subsidies in an effort to help families; in United States, the government created the Child Care and Development Fund in 1996, which provides public funds for childcare assistance to low-income families. Given the importance of this issue, it may seem surprising that there is so little known about the effect of childcare subsidies on parents' and children's outcomes. Research in this area has been limited by the difficulty distinguishing the causal effect of childcare price on later outcomes. For example, higher childcare prices may be associated with better childcare or wealthier parents, in which case we are not able to isolate the effect of price alone on family outcomes. This paper uses recent data and a novel source of identifying variation--sharp discontinuities in the price of childcare by income in Norway--to identify the role of childcare subsidies on parental behavior and the later academic achievement of children.

A childcare price subsidy may have a number of effects on the family. First, it may increase the attendance at formal child care relative to less expensive and often lower-quality informal childcare. Simple price theory would predict that a decrease in the price of attending day care provides a positive response to demand for child care; as a result, using more high quality daycare may affect the children in a positive way depending on the alternatives. It could also crowd out parental care (instead of informal care) and potentially increase labor supply, in which case the effects on children would vary with the alternative quality of parental care.

Alternatively, a subsidy would serve as a pure income transfer if demand for day care is inelastic. Families paying the lower price will have more disposable income than

families paying the higher price given almost identical gross incomes. Increased income has been shown to affect children in a positive way, particularly for children with limited resources (Dahl and Lochner, 2008; Carneiro and Heckman, 2003; Currie, 2009; Løken, Mogstad and Wiswall, 2010). We are able to look at both parents' responses and children's later academic achievement directly to determine the effects of the childcare subsidy.

We find a significant positive effect of childcare subsidies at age 5 on children's junior high school academic performance. Being eligible for lower child care prices at age 5 increases the grade point average and the grade on an oral exam by around 0.30 of a grade point (on a scale of 1-6). Given that take-up of childcare is about 55-60 % for the sample around the discontinuity, this suggests an effect of about .40 of a grade point for those who receive the childcare subsidy.

Importantly, while we find large effects on student performance, we find no effect of these substantial childcare subsidies on the utilization of formal childcare. This is consistent with a situation of excess demand for day care; it is not the price that is important but the availability of spots, and even if parents would like to change their childcare use, there may not be any available spaces by the time they are made aware of their subsidy status.[1]

As a result, the childcare subsidy in Norway appears to have acted as a positive shock to disposable income in the family, and, through this mechanism, improved child outcomes. We estimate the effect on disposable income at age 5 to be around 10 % of yearly gross income for the families situated around the discontinuity. Given that we find

[1] Survey results strongly suggest that this was the case in Norway in the 1990s (Blix and Gulbrandsen, 2002)

significant effects on later academic performance, this suggests that early investments, by increasing disposable income, even short-term, may have long lasting effects.[2] Interestingly, and consistent with a disposable income explanation, we also find effects of the subsidy on the academic performance of older siblings.

The paper unfolds as follows: Section 2 reviews relevant literature. Section 3 gives the institutional background, while Section 4 presents the empirical strategy. Section 5 describes the data and Sections 6 and 7 presents results and robustness tests. Finally, Section 8 concludes.

2. Literature review

There are a number of papers that have examined the effect of childcare subsidies on female labor force participation, with the findings ranging from no effect to significant negative effects (See Blau, 2000, for a summary). More recently, work by Herbst and Tekin (2010) has examined the effect of childcare subsidies in the United States on children's academic performance.[3] They use a unique identification strategy, applying distance to the nearest social service agency that administers the subsidy application process as an instrument for subsidy receipt. They find small negative effects of subsidy receipt the year before kindergarten on kindergarten performance, although these negative effects have generally disappeared by third grade.[4] Our work complements this

[2] This relates to a large recent literature that argues that early investment in human capital matters (see for example Carneiro and Heckman, 2003 and Currie, 2009 for overviews).
[3] Also see Tekin (2005), Tekin (2007) and Blau and Tekin (2007).
[4] There is also a substantial literature looking at the effects of programs providing universal childcare. Herbst and Tekin (2008), and Magnuson, Ruhm and Waldfogel (2007) find negative effects of universal childcare programs, while Berlinski, Galiani and Manacorda (2008) Berlinski, Galiani and Gertler (2009) Fitzpatrick (2008) and Havnes and Mogstad (2009a) and Havnes and Mogstad (2009b) find positive effects.

existing literature, using a different and (arguably more exogenous) source of variation on a different population.

There is also a growing literature on the effect of family income on child outcomes, which relates to our main mechanism of more disposable income. The results in the literature are mixed. Using a variety of instrumental variable techniques, Oreopoulos, Page and Stevens (2008), Dahl and Lochner (2008) and Milligan and Stabile (2007) suggest some positive effects of family income on child outcomes, especially for poor families. This is supported by work by Duncan, Yeung, Brooks-Gunn and Smith (1998) and Levy and Duncan (2000) who apply family fixed effects methods. However, Shea (2000) and Løken (2010) using IV and Blau (1999) and Dooley and Stewart (2004) using FE, finds no or very small effects. Differences could be due to different data sources, countries and institutional settings.[5]

This paper advances our understanding along two dimensions. First, we are able to convincingly separate income effects from labor force participation; most of the instruments used in the literature and family fixed effects approaches are likely reflecting both family income changes and labor market participation (and, for young children, child care) responses. In our paper, given that we find no effect on labor participation or childcare utilization in the short-run, we are able to isolate what appears to be an income effect. The subsidy affects disposable income through lowering the price of child care.[6]

[5] Dahl and Lochner (2008), argue that FE estimators do not control for endogenous transitory shocks not directly related to family income and suffer from greater attenuation bias than OLS and IV, because family income is measured in differences rather than levels. Løken, et al. (2010) argue that differences might be due to the use of linear FE and IV estimators. Theory suggests an increasing, concave relationship between family income and child outcomes (Becker and Tomes , 1979) and different instruments might then capture different parts of the income distribution and therefore produce different effects.

[6] Unfortunately we do not have good data on hours of work. Since we are able to rule out effects on participation and changes in use of formal child care, it is unlikely that hours of work changes due to the subsidy,

Second, given the recent literature suggesting the importance of investments early in a child's life (see for example Carneiro and Heckman, 2003 and Currie, 2009, for overviews), we are able to analyse the effects of shocks to income, through child care subsides, when children are age 5, which is likely to be a critical period for human capital investment.

3. Institutional Background

Although the history of day care in Norway goes back a hundred years and the first law regulating day care goes back to 1953, there was almost no formal child care for children below age 7 (the school starting age until 1996) in Norway until the mid seventies.[7] However, by the 1990s, the period we study, the day care center coverage had almost doubled from its 1980 level to 60 % among 3-5 year olds and continued to increase throughout the period of study.[8]

There are two types of child care centres in Norway: public (municipality level) and private. In the early 1990s approximately 60 percent of the day care centers were public. The private centres were typically owned by non-profit organizations like churches and cooperatives. However, both types of day care centres are very similar in the way they operate. Around 40 percent of public day care costs are directly subsidized by the central government, up to one third from the municipality and the rest is paid as fees by the parents. Most of the municipalities also subsidize the private day care centres,

[7] At that time, a new law was passed that aimed at a large expansion in child care as a response to increasing labor force participation of women. The reform included subsidies to the municipalities that created incentives for municipalities to expand the sector either through own establishments or providing subsidies to private non-profit organizations (see Havnes and Mogstad, 2009a). Although this reform increased the coverage, it was still only 32% in 1980 among 3-5 year olds and 7 % among 1-2 year olds.
[8] There was also growth in the day care center coverage for 1-2 year olds but to a much lower level, between 15 % and 30 %.

but the subsidy may be lower than one third of the cost. Given the stringent national standards for childcare, there is likely little variation in quality across private and public centers.[9] For both private and public centres it is the municipality that pays the difference between a full fee and a reduced fee (the discontinuity we study); the day care centres are just subsidized more in cases with a reduced fee.[10] Means tested day care subsidies are decided at the municipality level. There was a tremendous expansion in the female labour participation from the mid seventies onwards in Norway, creating excess demand for day care, which led to a rationing of access to day cares. The allocation rule as to who got access, and whether there were different rules for private and public day care facilities, are not transparent. However, it is clear that children with special needs had priority, along with the children of single mothers (constituting 7-8 percent of children born) (OECD, 2009). Parents submitted a ranking of their preferred day care facilities to a central office in the municipality. This municipality level institution alone allocated children based on a variety of criteria; however, tenure in line was the most important. This rule was applied to both privately owned and for public day care centres (since both

[9] The Day Care Act ("Barnehageloven") gives nationwide standards along several areas for day care centres. There are national requirements concerning the education of the staff. For instance, the laws require that the manager and the pedagogical leader both have a college education (3 years), very similar to the education requirements for teachers. There are also strict requirements when it comes to playgrounds, playground facilities, and total area within the centre. The curriculum is centrally determined, with a strong focus on learning through social relationships both with other children and with adults in the day care centres. (OECD, 1999; Framework Plan ("Rammeplanen")).

[10] Although there are centrally described guidelines for staffing requirements, playground requirements etc, there is still some room for discretion on the part of the municipalities. For instance, it is the municipality that assesses the quality of the day care facilities; as a result, there may be differences in the quality of day care centres across municipalities. In a recent survey, it was found that the share of formally qualified teachers in day care centres varied both across municipalities and within municipalities (Gulbrandsen and Winsvold, 2009). However, they did not report any differences across private and public centres.

the state and the municipality provided subsidies), suggesting that the privately funded centres were not able to cream skim children.[11]

The alternative to a formal day care center was the informal sector.[12] This could either be play parks/groups run by nannies, or grandparents/relatives/friends. None of these informal arrangements received any subsidy from the municipality. They were also not subject to the same regulation by the municipality.[13]

In this paper we will focus on child care subsidies at age 5 mainly due to the availability of data.[14] However, given that we focus on age 5, the institutional setting provides us with a framework that suggests that the price subsidy at age 5 will work as a disposable income effect. At age 5, most children in our sample have already started child care at an earlier age (based on our own calculations we find that 86 % also attended formal child care at age 4). This means that given that it is random (around the discontinuity) whether you receive the subsidy at age 5, families have already made decisions about child care and labour supply before they receive the subsidy or not. There are very few families that withdraw children from child care at age 5 if they have already attended child care up to age 5. In addition, this is the time when child care coverage is at the highest, so most families with 5 year olds who want to use formal child care can do so

[11] It is a fact that the municipality owned day care centres has a slightly higher share of special needs children; to compensate, these centres received more resources in terms of extra teachers (OECD, 1999).
[12] It was not until 2008 that Norway, through a change in the law, which required the municipalities to have full formal child care coverage. A law from 1998 (the so-called "Cash for care" reform) gave parents the right to the state subsidy if they opted out of day care and stayed home with the child instead (see for instance Schone, 2004).
[13] This was true for registered nannies (who paid income taxes) as well.
[14] We have more observations for these cohorts as our data on income cutoffs and prices starts in 1991 and the last cohort with observations on educational outcomes is 1992 giving us for example only three cohorts of 1 year olds (1990-1992(, while we have seven cohorts of five year olds (1986-1992 in 1991-1997). As we rely on a RD design for identification, we need a large sample size to get enough observations around the discontinuity.

(excess demand is mostly affecting younger children, child care centers especially had limited access of spots for 1 and 2 year olds).

Table 1 shows information from a survey on the use of registered nannies and formal day care centers in the 1990s, in addition to labor supply of mothers.[15] We see that the labor supply of mothers with 3-5 year old children matches very well the total use of formal care – either registered nannies or daycare. For 1-2 year olds the match is not as good, as there is a higher preponderance of informal nanny use, especially for one year olds.[16]

4. Empirical strategy

The day care system in Norway is run at the municipality level (there are 435 municipalities) and the price is heavily subsidized for all (parents pay about 30 percent of the actual costs, on average). Some municipalities have a single price that is the same for all income groups, while others have multiple prices that depend on family income. In these municipalities, the pricing scheme takes the form of a step function with jumps in the price occurring at one or more levels of family income. These jumps suggest that there are discontinuities in the relationship between family income and the price of childcare. Assuming that other factors related to family income that affect child outcomes do not systematically change at the discontinuity points, we can identify childcare subsidy effects by comparing later outcomes of children whose family income was just less than a cutoff to those of children whose family income was just above a cutoff.

[15] See report from the research institute of NOVA by Gulbrandsen and Winsvold (2009)
[16] The excess care not accounted for by formal care is most likely grandparents and nannies in the informal market.

We use a regression discontinuity (RD) approach to estimate the effect of eligibility for lower child care prices. We have a sharp design since eligibility of cheaper child care jumps from 0 to 1 at the discontinuity. However, the take-up rates of child care and the subsidy is below 100 % and we have to take this into account when interpreting the estimates.[17] For family *i*, in municipality *m*, at time *t*, the eligibility for lower child care price ($E_{i,m,t}$) is a deterministic function of income the year before ($I_{i,m,t-1}$); if income was below a particular cutoff (*c*), the family received the extra subsidy and thereby paid a lower price. We can then estimate the effect of being eligible for a lower childcare price on child outcomes (*y*) by comparing families with incomes just below and above c.

For identification, we need to assume that income and other characteristics about the family vary continuously through the cutoff point. We then estimate the effect of the childcare subsidy by taking the difference of the boundary points of two regression functions of *y* on *I*, one for eligible families and one for ineligible families. We use local linear regression (LLR) as in Fan (1992), Hahn, Todd and Van der Klaauw (2001) and Porter (2003), using a rectangular kernel and different bandwidths[18] to verify that the results are not driven by choice of smoothing parameters.[19] We use paired-bootstrap

[17] Ideally, we would have liked to run an instrumental variable regression, however, we do not have a direct measure of disposable income which is our main mechanism. We will therefore run the intention to treat on the full sample and interpret the estimates based on known take-up rates of child care.

[18] We follow the recommendation in Lee and Lemieux (2009) to use only one kernel (rectangular) and rather focus on estimating the model with different bandwidths. We have, though, also tried different kernels without any significant changes to the main results.

[19] We solve $\min_{\alpha,\beta,\tau,\gamma} \sum_{i=1}^{N} K\left(\frac{I_i - c}{h}\right)(y_i - \eta - \beta(I_i - c) - \tau E_i - \gamma(I_i - c)E_i)^2$

where $E_{i,m,t} = 1\{I_{i,m,t-1} < c_{m,t}\}$ and $c_{m,t}$ is the income cut-off for municipality, m, at time, t. The parameter of interest is estimated as $\hat{\alpha}_{RD} = \hat{\tau}$.

percentile-T procedure with 2000 replications to estimate standard errors and verify our results implementing formulas from Porter (2003).

We also estimate a parametric specification, which enables us to include individual and family control variables in the equation. Then we use the entire discontinuity sample for identification (while the non-parametric specifications put more weight on observations close to the discontinuity). Note that, if the regression discontinuity approach is valid and the control variables are changing smoothly through the discontinuity, these control variables should not matter. We estimate the following:

$$y_{i,m,t} = \beta_o + \beta_1 1\{I_{i,m,t-1} < c_{m,t}\} + \beta_2 f_m(I_{i,m,t-1}) + \beta_3 x_i + \lambda_{mt} + \varepsilon_{i,m,t}, \qquad (1)$$

where $f_m(I_{i,m,t-1})$ is family income the year before entering the equation in a flexible form which is municipality-specific, *x* is a vector of individual and family control variables, and λ is a vector of cohort by municipality fixed effects.[20] We want to estimate β_1 which is the effect of being eligible for lower child care prices on children's outcomes.

As all our outcomes will typically vary with family income, and eligibility for cheaper childcare depends on income, we have to control for family income on each side of the discontinuities in a flexible way. We control for family income using a cubic, a quartic and a quintic function of income. We allow the slopes to be different at each side of the discontinuity. We also control for other pre-child care parental characteristics in order to increase the precision of our estimates.

[20] Control variables are parental age, parental citizenship, parental education when child is born, marital status when child is born, student and welfare recipient status of mother when child is age 4, family income prior to age 5, municipality fixed effects and cohort fixed effects. In addition, we include interactions of municipality dummies with cohort dummies.

We assume that households do not locate strategically around the income cutoffs. This is based on the notion that family income is unlikely to be perfectly manipulated around the cutoff. Families can try to locate below the cutoff but, since the cutoff is unknown the year before the child care subsidy is allocated, they cannot perfectly predict where the cutoff will be. Additionally, because cutoffs may change from year to year and because the income formulas are fairly complex (kindergartens themselves determine whether the family is eligible for cheaper child care), there is no reason to believe that parents can strategically adjust their income to locate just below the cutoff. [21] The absence of clumping around the cutoffs in the density of family income supports this assumption (see Appendix, Figure A1). In addition, balancing tests presented later support this assumption; the individual characteristics below and above the discontinuity are not significantly different.

We will present results both graphically and in tables. The figures will illustrate the nonparametric specification with rectangular kernel and bandwidth of .08. We will also show the 95 % confidence intervals and scatterplot with average outcomes for 60 income bins. Note that this is only to illustrate the pattern in the data; the nonparametric estimation uses all the observations to estimate the discontinuity. In the tables we will also present results with bandwidths of .06 and .10, in addition to parametric estimates with cubic, quartic and quintic family income controls varying on each sides of the discontinuity.

[21] Yearly changes to the cutoffs vary between 2 and 9 %.

5. Data description

We use administrative data covering the entire population of Norway. The analysis includes birth cohorts 1986-1992 and links individuals to their parents through unique identifiers. We have information on parental characteristics such as parental age, educational attainment when the child was born, income, marital status, and citizenship. For children, we have grade point average and exam grades from junior high school. In addition, we match parents to their tax records, where we are able to observe whether parents take deductions for child care expenses; this allows us to identify whether a child attends formal child care. Lastly we have collected data from municipalities in Norway on childcare prices and family income cutoffs in the 1990s.

Family income is created by adding mothers' and fathers' earnings. Earnings are measured as total pension-qualifying earnings reported in the tax registry, starting from 1967. The earnings measure includes earnings for everybody in the labor force; labor earnings and all taxable welfare benefits including sick benefits, unemployment benefits and parental leave payments. This is the same income measure that municipalities use to determine whether families are eligible for cheaper child care.[22] Our measure of disposable income is defined as family income minus the childcare price faced by the family.

[22] Most municipalities use income from tax sheets for the year before to determine eligibility for lower prices. As mentioned before there is no overall information on how the different systems in municipalities work. We asked the municipalities a question on how they used the cutoffs and the previous year's tax sheet was the most common way to implement the cutoffs. As a robustness check we run the regressions for municipalities where we are certain that they used last year's tax sheet and find similar results (results available upon request). For identification it is also an advantage to use previous year's income as parents cannot manipulate the previous year's income based on today's cutoff. Of course having direct data from each municipality on the family's total income would be better but the register data is the closest we can get and, to the best of our knowledge, we classify the discontinuities correctly.

Our measure of child care attendance is created from the information on tax deductions for child care expenses from parents' tax records which is available from 1993.[23] The child care tax deduction was introduced in 1948; parents are allowed to deduct up to 25,000 NOK (USD 4,310) from taxes in one calendar year for the first child for formal childcare that takes place outside the home.[24] As a result, our definition of childcare excludes home care and informal care by grandparents and nannies. There is a significant amount of variation in tax deductions across families due both to different prices across municipalities and also different prices across income groups within municipalities. Our measure of childcare is an indicator for whether or not the child is attending formal childcare.[25]

Finally, we have collected data at the municipality level on the price system and actual prices of child care in the 1990s. If the municipality had variable prices across the income distribution we asked explicitly for the income cutoffs used by the municipality to determine eligibility for cheaper child care. We received information from 69 % of all the municipalities, including the ten largest municipalities. This gives us information on the price system for about 85 % of the total sample. Figure 1 provides a map of Norwegian municipalities showing that variable, flat, and unknown price municipalities are scattered across Norway.

[23] This means that we do not have child care attendance for our first two cohorts born in 1986 and 1987, however we have checked that all results on other outcomes are robust to excluding these two cohorts. We do want to include them though due to earlier mentioned sample size issues.
[24] The extra deduction for the second child is 5000NOK, for a total of 30,000NOK. The annual price of childcare is almost always below 25,000NOK per child, at least for the families we study around the discontinuity.
[25] By using information on family income and prices of childcare by family income and municipality, we could potentially obtain a more complete picture of child care. However, as we will use family income and prices as our identification strategy, it would be problematic to also use the same information to create the child care definition.

There are some missing observations in the data for different variables. This can for example be because children have not taken the exams in junior high school. There is also some missing observations on parents background characteristics, especially years of education and income. The main bulk of missing observations come from missing observations on immigrants. We exclude the observations when we have missing values on parental background characteristics. This reduces our sample from 448198 observations to367836. We have tested that the main results on child outcomes are not sensitive to excluding these observations.

Our main analysis will be conducted on families that are located around the first price discontinuity in each municipality. We include families with income within 50 % below and above the discontinuity.[26]The results are generally not sensitive to this cut; however, the more observations we include the further we move away from the discontinuity and the fewer we include the less precise estimates due to very few observations. We call our main sample the discontinuity sample. [27]

For children, we have information on junior high school national exams and grade point average.[28] The grade point average is an average of the 10th grader's performance in all 12 graduating subjects.[29] We also have the grades from written and oral exams that are administered in the final year of junior high school at the national level and are externally

[26] For example if the price discontinuity is NOK 100000, we include families with income between NOK 50000 and NOK 150000.The results are robust to including and excluding more families however as the discontinuity is at low levels of income we cannot move much further to the left of the discontinuity as zero income is binding from below.
[27] We have experimented with excluding families where mothers are students when the child is aged 4. This is because students might have different rules for child care and are not affected by the subsidies. Excluding students (about 5 % of sample) does not change the results. We have also experimented with excluding mothers on social security receipts and this does also not change any of the main results.
[28] In Norway children are aged 13-16 when attending junior high school.
[29] These consist of written and oral Norwegian, written and oral English, mathematics, nature and science, social science, religion, home economics, physical education, music and hand-craft.

graded. The written exam could be in either math, Norwegian or English, however everyone in the same cohort takes the same exam.[30] The students are informed of which exams they will take a couple of days before the exam date. The oral exam can be in any of the 12 subjects taught in the last year of high school and the students are randomly allocated to subjects. As junior high school is the last three out of 10 compulsory years of schooling, the grades obtained are important for potential attendance at different high schools. The grades range from 1-6.[31]

Table 2 gives descriptive statistics for the total sample of children born between 1986 and 1992 and our sub-sample of children from the same cohorts where families are located close to the price discontinuity (hereafter referred to as the "discontinuity sample"). We see that the samples are very similar when it comes to child characteristics such as age, gender, number of siblings and birth order. However, for the discontinuity sample, parents tend to have fewer years of education and are more likely to be of non-Norwegian citizenship, highlighting the fact that the income discontinuity sample is composed of individuals at the bottom end of the income distribution. About 80 % of the total sample is married or officially cohabiting in the year of birth of the child, while this number is 70 % for the discontinuity sample.[32] When comparing the school performance of the total sample with our discontinuity sample, we see that children in the discontinuity

[30] Our identification strategy with a balanced sample around the discontinuity will take out any differences in grading across cohorts, municipalities and schools (as they are also balanced around the discontinuity). In the parametric specification we control for municipality and cohort fixed effects.

[31] There are advantages and disadvantages with both measures. The exam grades are more variable as they are a one-time measure of skills. However, they are more comparable across cohorts and schools as they are graded externally. The grade point average is generally a better measure of long term skills as they cover all subjects and average over all grades; however it is also more subjective because it depends on teacher assessments. However, our identification strategy compares similar families just below and above the income cutoff who will, on average, have the same schooling environment so all three measures of academic performance should be valid.

[32] A more detailed distribution of grades for the grade point average and the written and oral exams is shown in Appendix Table 2.

sample tend to perform worse, with a mean GPA of 4 and 2.7, respectively. This is not surprising as we know that children of low income families tend to perform less well in school than children from high income families.

6. Results

6.1. Balancing tests

In order for our identification strategy to be valid, we need to assume that the differences in childcare subsidies on opposite sides of the discontinuity do not correspond to other observed and unobserved differences across families. The main assumption is that family income is unlikely to be perfectly manipulated around the cutoff (Lee and Lemieux, 2009). In the Appendix, Figure A1, we show the density of family income for the discontinuity sample. We see that there is no evidence of income clustering below the cutoff. To further support this assumption, we compare pre-subsidy characteristics for families on opposite sides of the discontinuity to verify that observable characteristics do not change at the discontinuity.

These results are presented in Appendix Table 3. The first part of the table shows the results for balancing tests on parent's educational attainment, age, citizenship and marital status at the birth year of the child.[33] The different columns correspond to different kernels and bandwidths. We see that there are no statistically significant differences between families when it comes to these characteristics, and this is robust to the different specifications. Appendix Figure 2 shows this graphically, presenting estimates with rectangular kernel and bandwidth of .08 in along with the associated 95%

[33] See Appendix Table 4, for additional balancing test on mother's student and welfare recipient status when the child is aged 4 and average family income prior to age 5 subsidy.

confidence intervals.[34] We see that even though there is a lot of variation in the data, represented by the average outcomes for 60 income bins, and therefore some small differences at the discontinuity, these differences are not even close to being statistically significant and the effects are quite small.

The second part of Appendix Table 3 presents the balancing tests using a parametric specification where the different columns correspond to different flexible controls for family income. Again we find no significant differences between families on either side of the cutoff in terms of background characteristics. This supports the idea that children's outcomes are most likely only being affected by the childcare subsidy itself, and not other differences.[35]

By using the regression discontinuity approach, we are implicitly assuming that the assignment of subsidy receipt is essentially random, conditional on observables. As a result, it should be the case that the probability of subsidy receipt prior to age 5 should be equal for both the treatment and control groups. Appendix Table 5 presents the estimates of the probability of being below the cutoff when the child was aged 1-4 for our discontinuity sample. Importantly, we see that there is no effect of being below the cutoff at ages prior to the subsidy (if anything you are a little less likely to have received the subsidy in earlier years). This further supports the validity of our approach.

[34] The values of the y-axis are created by always including plus/minus one standard deviation around the mean outcome in order to make the graphs comparable.

[35] In addition, we have run results (available upon request) on disposable income, and price of child care in the years before age 5 (from birth to age 4). We do not observe differences across the discontinuity in these variables which support our assumption that it is random whether families receive the subsidy or not at age 5. In other words, there is no evidence that potential prior subsides (at ages before 5) affect the probability of being around the cutoff at age 5.

6.2. Children's Outcomes

Table 3 presents the effect of childcare subsidies on children's grade point average and exam grades in junior high school, while Figure 2 presents these results graphically, with the standardized variables for row 3 with a bandwidth of 0.08. From the table, we can see that there is a statistically significant positive effect of subsidy receipt on children's grade point average of about .30 of a standard deviation. As not everyone is in childcare and hence affected by the child care subsidy, this will be an intention to treat effect. Given take up of child care for our discontinuity sample is about 55%-60 %, this means that the effect for the treated is even larger – around .40 of a standard deviation.

For both written and oral exams we see positive effects of the subsidy. However, while the effect on the oral exam is about .25 of a standard deviation, the effect is smaller and insignificant for the written exam.[36] The second part of Table 3 presents the estimates from the parametric specifications; we see that the results are positive and generally statistically significant.

From the graph we can see that there is substantial variation in the data, and the data points close to the discontinuity are giving us most of the identification in children's grades. We also observe that children tend to perform better to the left than to the right if we move a little bit further away from the discontinuity, with some exceptions. As there is a trend in child outcomes across family income, the more observations we include (larger bandwidth, parametric specification), the lower the estimates. It is reassuring,

[36] One possible explanation for these results is that the oral exam and grade point average (consisting for the most part of grades from assessment in class over the year) capture outcomes that are more correlated with behavioral and non-cognitive outcomes, while the written exam is more correlated with cognitive behavior.

though, that the estimates are significant in the extreme specifications with a large bandwidth (.10) or five income polynomials (parametric).

6.3. Mechanisms

Given the observed effect of the childcare subsidy on children's junior high school performance, the next question becomes what factors are driving these effects. The first part of Table 4 shows the effects of the childcare subsidy on various intermediate outcomes, and Figure 3 shows the standardized variables for row two with a bandwidth of 0.08.

There is no evidence of any effect of the childcare subsidy on child care utilization. If we use the alternative, simple, childcare measure the effect is even closer to zero. This finding is robust to a variety of specification tests.[37]

Despite this, it is clear that the price, and hence disposable income, jumps significantly at the discontinuity. From the next row in Table 4 we see that families below the discontinuity pay on average 9000 NOK (USD 1500) less for child care. Taking the difference between income at age 5 and the price of child care (in natural logarithms) we see that families below the cutoff have on average 11 % more yearly disposable income when the child is age 5.

We next study whether the subsidy affects parental labor supply and income in order to understand the results. We see no significant effects of the subsidy on mother's or father's labor supply. This indicates that there are no responses by the parents on the extensive margin and is consistent with no effects on child care utilization which means

[37] We do not have information on child care deductions at age 5 for all cohorts. We have checked that all the main results hold if we exclude the cohorts where we do not have child care information.

that the subsidy does not affect the time use between the labour market and care for the children. We also look at mother's part time work when the child is aged 5 and see no effect (there are almost no fathers that work part time). When we study mother's and father's income at age 5 we see no effect on mother's income or father's income, although the effect are slightly positive which explains why the net income effect slightly exceeds the subsidy.

These results are the same when we estimate the parametric specifications, presented in the second part of Table 4. This confirms the non-parametric results with no effect of the childcare subsidy on childcare attendance and mother's labor supply but with large effects on net income, suggesting that most of the positive findings for children can be attributed to higher disposable income for families receiving the subsidy.

Given that we are arguing that the significant effects of the childcare subsidies on children's academic achievement are being driven by changes in disposable income at age 5, it should be the case that municipalities with smaller jumps in prices should also experience smaller changes in children's performance. As a test, we split municipalities into those with small jumps in price (where there is little effect on disposable income) and those with larger jumps in price. Table 5 present the results and the main effects come from municipalities with the largest price cuts (also see Figure 3 in appendix for the grade point average). It is basically no effect for municipalities with small price jumps and large, significant effects for municipalities with larger price jumps. It is also very interesting to see that the effect on ln net income is much larger and more significant for the high price jump municipalities, which shows that it is really the subsidy that drives this effect. There is no effect on net income when the subsidy is very low.

When it comes to municipalities with different cutoffs across family income we might expect the effect to be larger for municipalities where the cutoff is at lower levels of family income, since this is consistent with the literature finding larger effects for the most disadvantaged families. Ad we see from Table 5, row three and four, the effects are driven by municipalities with cutoffs at low levels of family income. And, interestingly, the subsidies are also larger for this group and the net income effect is large and significant. This supports the fact that it is disposable income that is the most likely mechanism for child outcomes.

Given that it seems the most likely mechanism is disposable income during childhood, an important check of our results would be to look at the effects of the subsidy on the other children in the family; if the subsidy is, in fact, increasing disposable income for the family, then all children should benefit and not just the child that generates the subsidy. In Table 6, we report effects for older siblings and see that there are tendencies towards positive effects although more imprecisely estimated due to smaller sample sizes. When we look at younger siblings, the estimates are too imprecisely estimated to draw any conclusions.

6.4. Placebo tests using flat price municipalities and moving the cutoffs

To verify our findings, we run a number of specification checks. The first takes advantage of the fact that some municipalities have no variation in the price of childcare; municipalities with a flat price system do not give us variation across family income to identify an effect of differences in childcare prices across income. However, as a placebo test, we assign the flat price municipalities the average cutoff of the variable price

municipalities to check whether there are any systematic differences across child outcomes for our cohorts that are unrelated to the price discontinuity. (We should observe no effect of this "placebo" discontinuity on any outcomes.) Appendix Table 7 presents these results; it is reassuring to note that there is no effect on children's grades.

In Appendix Table 8 we present results when we move the cutoff plus and minus 5 % from original cutoff and estimate the effects using these placebo cutoffs; again, it is reassuring to see no effects on any of the outcomes whether we use plus or minus 5 %.

8. Conclusion

Given the wide use of childcare subsidies across countries, it is surprising how little we know about the effect of these subsidies on children's longer run outcomes. Using a sharp discontinuity in the price of childcare in Norway, we are able to isolate the effects of childcare subsidies on both parental and student outcomes. We find very small and statistically insignificant effects of childcare subsidies on childcare utilization and parental labor force participation. Despite this, we find significant positive effect of the subsidies on children's academic performance in junior high school, suggesting the positive shock to disposable income provided by the subsidies may be helping to improve children's scholastic aptitude. Policy recommendations based on the results in this paper point towards increasing disposable income for low income families. Norway subsidizes child care with NOK 28 billion (USD 4,5 billion) yearly and most of these subsidies are universal. A direction towards more income means tested subsidies may be beneficial for the children. The child care subsidy in Norway for 5 year olds work as an in-kind transfer providing families with more disposable income for a period of the early childhood. We

cannot rule out that subsides targeted at other ages or in other settings might give different parental responses. A general lesson to learn from all research on family policies is that we need to understand parental responses to the reforms before we can understand the underlying effects and mechanisms.

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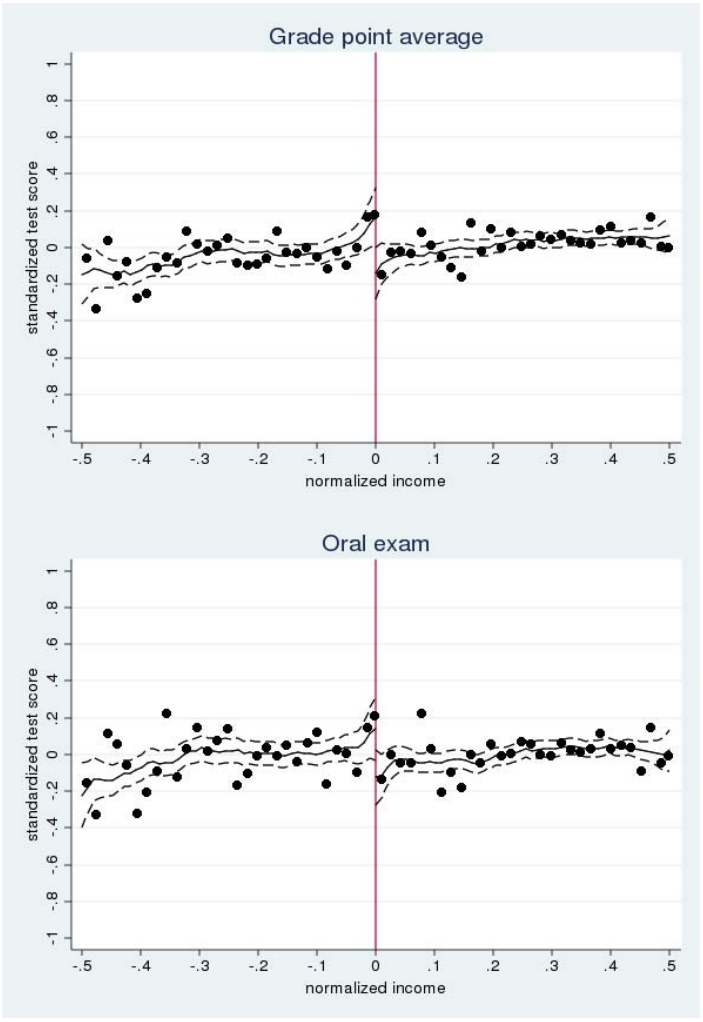
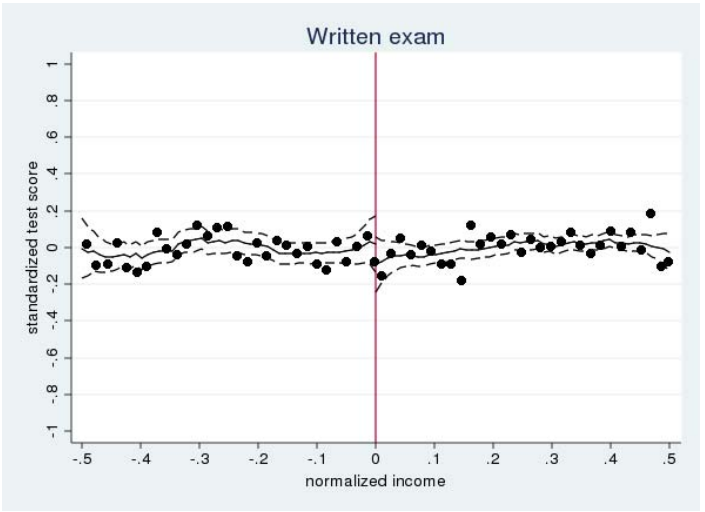
Figure 1
Map of Norway with information on price systems across municipalities

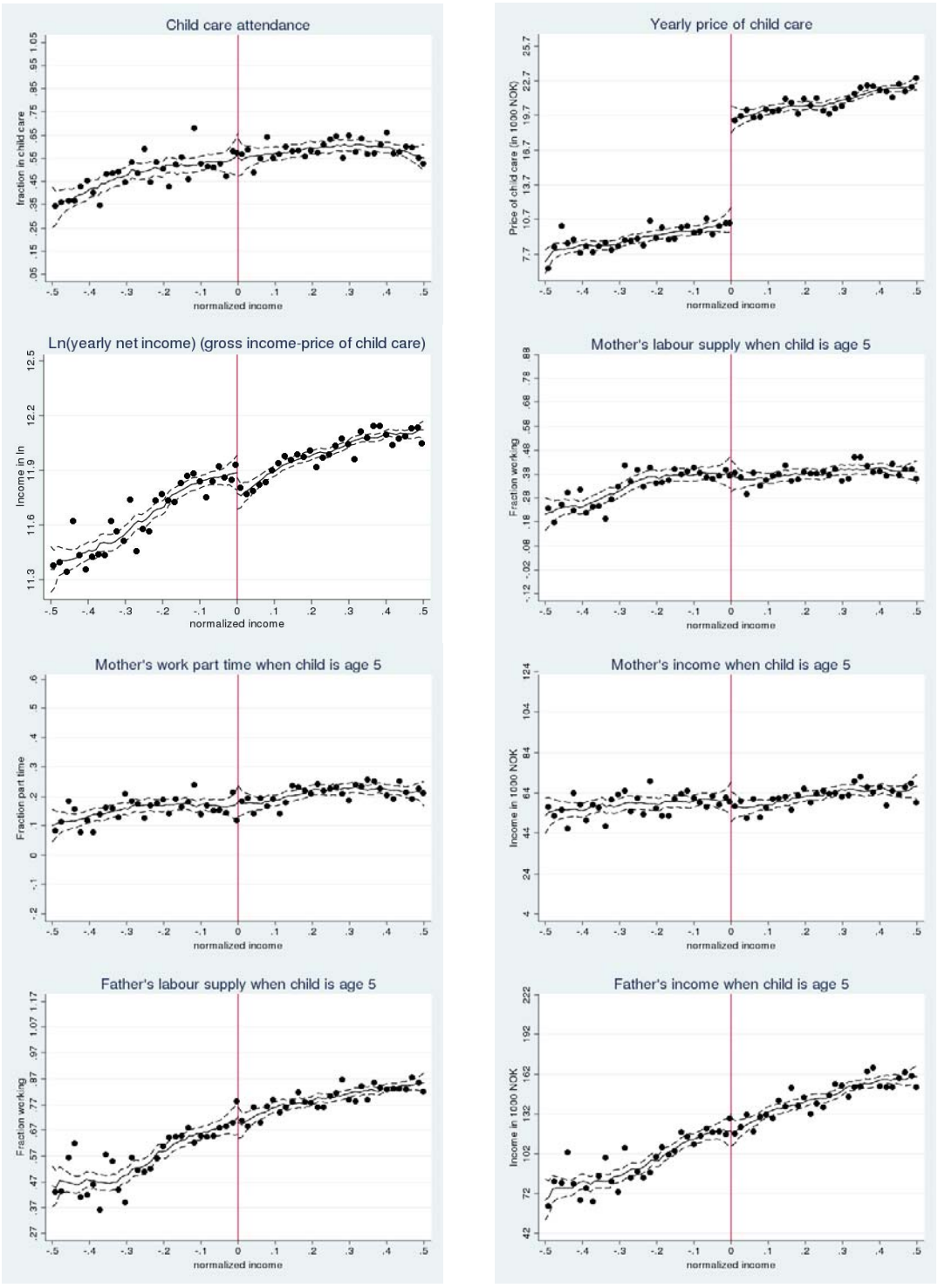
Figure 2
Effect of Childcare Subsidy on Children's Junior High Academic Performance





Note: the solid line is the local linear regression with a rectangular kernel and bandwidth .08. The dashed lines are 95 % confidence intervals. The scatter plot is the average standardized outcome for 60 income bins.

Figure 3
Mechanisms



Note: the solid line is the local linear regression with a rectangular kernel and bandwidth .08. The dashed lines are 95 % confidence intervals. The scatter plot is the average standardized outcome for 60 income bins.

Table 1
The Use of Day Care and Mother's Labor Supply
3-5 Year Old Children
1992 and 1998

Year	1992	1998
Nannies (%)	13	8
Nannies and day care (%)	64	77
Mother Work Full time (%)	32	38
Mother Work Part time (%)	35	41
Mother work Total (%)	66	79

Source: NOVA

.

Table 2
Descriptive Statistics

	Total Sample	Discontinuity Sample
Age in 2006	18.9 (2.0) [367,836]	19.1 (2.0) [10,770]
Female	.49 (.50) [367,836]	.49 (.50) [10,770]
Number of siblings	1.8 (1.1) [367,836]	1.9 (1.4) [10,770]
Birth order	1.9 (1.0) [367,836]	1.9 (1.1) [10,770]
Mother's education at birth of child	11.9 (3.4) [367,836]	10.2 (4.3) [10,770]
Father's education at birth of child	11.8 (3.5) [367,836]	9.8 (4.4) [10,770]
Mother's age at birth of child	27.9 (5.0) [367,836]	26.1 (5.4) [10,770]
Father's age at birth of child	30.7 (5.6) [367,836]	29.3 (6.4) [10,770]
Mother non-Norwegian citizen at birth of child	.05 (.21) [367,836]	.17 (.38) [10,770]
Father non-Norwegian citizen at birth of child	.05 (.21) [367,836]	.18 (.39) [10,770]
Married/cohabiting at birth of child	.8 (.40) [367,836]	.72 (.45) [10,770]
Grade point average (scale: 1-6)	4.0 (.82) [359,339]	3.7 (.85) [10,238]
Grade written exam (scale: 1-6)	3.5 (1.1) [344,271]	3.1 (1.1) [9,572]
Grade oral exam (scale: 1-6)	4.3 (1.2) [318,783]	3.9 (1.2) [8,823]

Table 3
Effect of Childcare Subsidy on Children's Junior High Academic Performance

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5 polynomials	N
Grade point average	.310*** (.120)	.310*** (.110)	.259*** (.097)	.134* (.081)	.259*** (.102)	.271** (.124)	10238
Written exam	.150 (.128)	.112 (.115)	.156 (.098)	.056 (.086)	.140 (.107)	.092 (.129)	9572
Oral exam	.296** (.139)	.273*** (.105)	.260*** (.100)	.099 (.087)	.178* (.110)	.227* (.133)	8823

Columns 1-3 report the coefficients from an RD regression running local linear regression with a rectangular kernel and different bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income allowing the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, marital status, at birth of child, and pre-child care family income, mothers welfare status and mothers student status at age 4 of the chil

Table 4
Mechanisms

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5 polynomials	N
Child care attendance	-.019	.013	.013	-.038	-.034	-.047	7477
	(.073)	(.063)	(.055)	(.048)	(.061)	(.073)	
Price of child care (in NOK)	-8370***	-8514***	-8760***	-9550***	-8961***	-8492***	10770
	(949)	(799)	(720)	(292)	(386)	(489)	
Ln(net income) (gross income-price of child care)	.034	.119*	.152***	.099**	.073	.115*	10770
	(.076)	(.070)	(.060)	(.048)	(.061)	(072)	
Mother's labor supply	-.025	.019	.013	.010	.015	.038	10770
	(.058)	(.051)	(.046)	(.038)	(.049)	(.059)	
Mother work part time	-.011	-.003	-.009	-.013	-.023	-.022	10770
	(.046)	(.040)	(.035)	(.032)	(.040)	(.048)	
Mother's income	368	4521	2506	3736	-742	312	10770
	(6984)	(5963)	(5264)	(4467)	(5718)	(6811)	
Father's labour supply	.017	.027	.039	-.011	-.005	.058	10770
	(.055)	(.048)	(.041)	(.036)	(.045)	(.054)	
Father's income	-1148	2447	4858	2352	499	6075	10770
	(9579)	(8187)	(7229)	(5959)	(7547)	(9144)	

Columns 1-3 report the coefficients from an RD regression running local linear regression with a rectangular kernel and different bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income allowing the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, marital status, at birth of child, and pre-child care family income, mother's welfare status and mothers student status at age 4 of the child.

Table 5
Effect of Childcare Subsidy on Children's Junior High Academic Performance
By dimensions: cutoffs and prices
Regression Discontinuity Results
Rectangular Kernel, .08 Bandwidth

Subgroups	Municipalities with large price jumps	Municipalities with smaller price jumps	Municipalities with cutoff at low levels of family income	Municipalities with cutoff at higher levels of family income
Takeup rates of child care	.55	.55	.48	.59
Children's outcomes:				
Grade point average	.441***	-.007	.448***	.224
	(.135)	(.205)	(.174)	(.145)
	[7148]	[3090]	[3706]	[6528]
Written exam	.186	-.101	.292*	.006
	(.129)	(.195)	(.177)	(.140)
	[6677]	[2895]	[3491]	[6071]
Oral exam	.297**	.216	.310*	.239
	(.136)	(.218)	(.182)	(.157)
	[6116]	[2707]	[3115]	[5710]
Mechanisms:				
Child care attendance	.046	-.089	-.015	.020
	(.072)	(.124)	(.104)	(.079)
	[5394]	[2083]	[2565]	[4915]
Price of child care (in NOK)	-10144***	-3187**	-10522***	-6969***
	(898)	(1462)	(1027)	(1110)
Net income (gross income-price of child care) (in NOK)	.189***	-.064	.217*	.077
	(.077)	(.133)	(.127)	(.071)
Mother's labour supply	.025	.012	-.010	.042
	(.060)	(.095)	(.078)	(.067)
Mother work part time	.032	-.086	.047	-.032
	(.046)	(.075)	(.052)	(.056)
Mother's income	2994	9064	-1908	9376
	(6871)	(11250)	(8384)	(8075)
Father's labour supply	.032	.008	.099	-.016
	(.057)	(.084)	(.079)	(.058)
Father's income	7751	-10082	18628*	-8797
	(10284)	(14166)	(11126)	(11124)
N	7530	3234	3954	6825

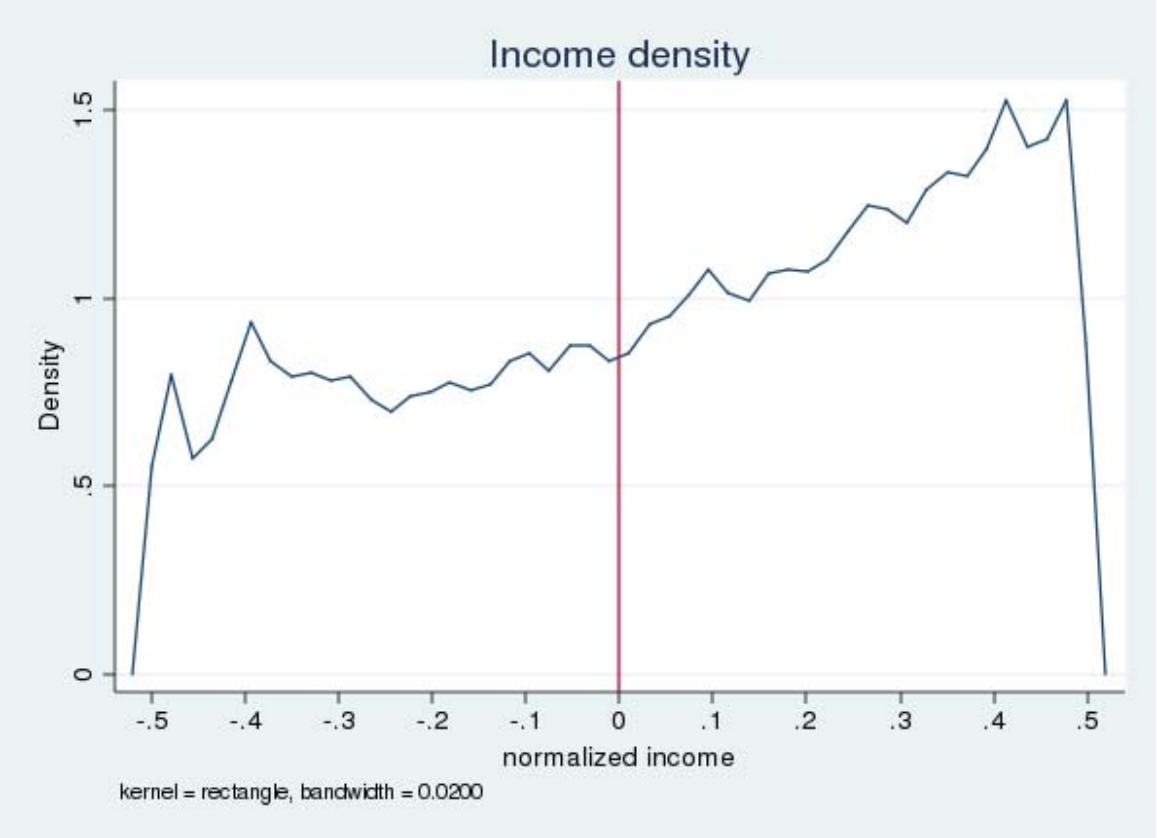
This table reports the coefficients from an RD regression running local linear regression with a rectangular kernel and bandwidth of 0,08 on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications.

Table 6
Effect of Childcare Subsidy on (closest) older Sibling's Junior High Academic Performance

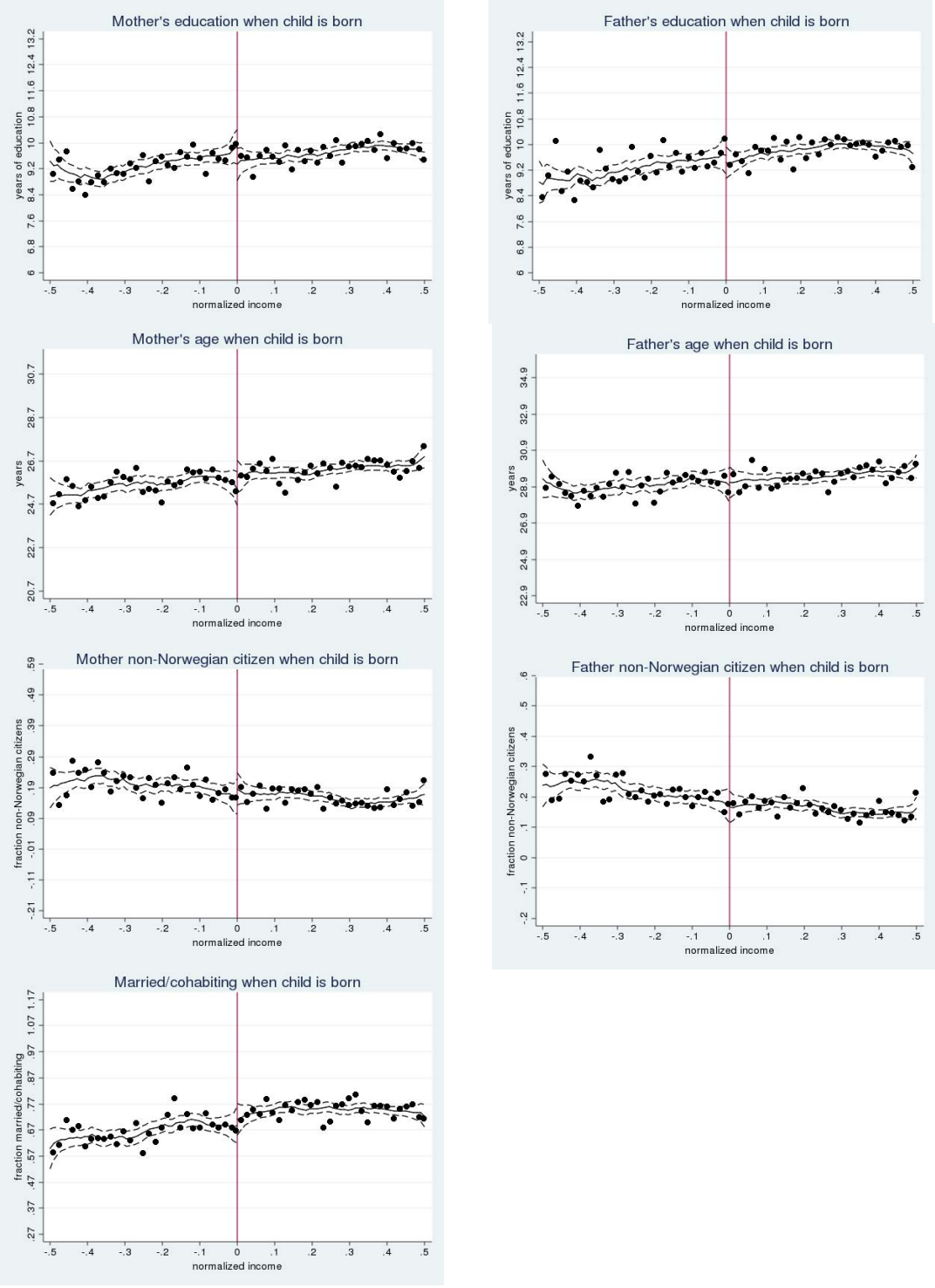
	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	2 polynomials	3 polynomials	4 polynomials	N
Grade point average	.172	.329*	.296*	.097	.214	.005	2556
	(.211)	(.189)	(.175)	(.165)	(.204)	(.248)	
Written exam	.111	.274	.297*	.162	.362*	.105	2404
	(.237)	(.212)	(.180)	(.179)	(.220)	(.260)	
Oral exam	.331	.396*	.331*	.004	.431*	.318	2193
	(.258)	(.221)	(.187)	(.185)	(.236)	(.281)	

Columns 1-3 report the coefficients from an RD regression running local linear regression with a rectangular kernel and different bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income allowing the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, marital status, at birth of child, and pre-child care family income, mother's welfare status and mothers student status at age 4 of the child. Note that since we have missing on some of these variables (see Table 4) the number of observations will be lower in the parametric specification.

Appendix Figure 1:
Income density around discontinuity

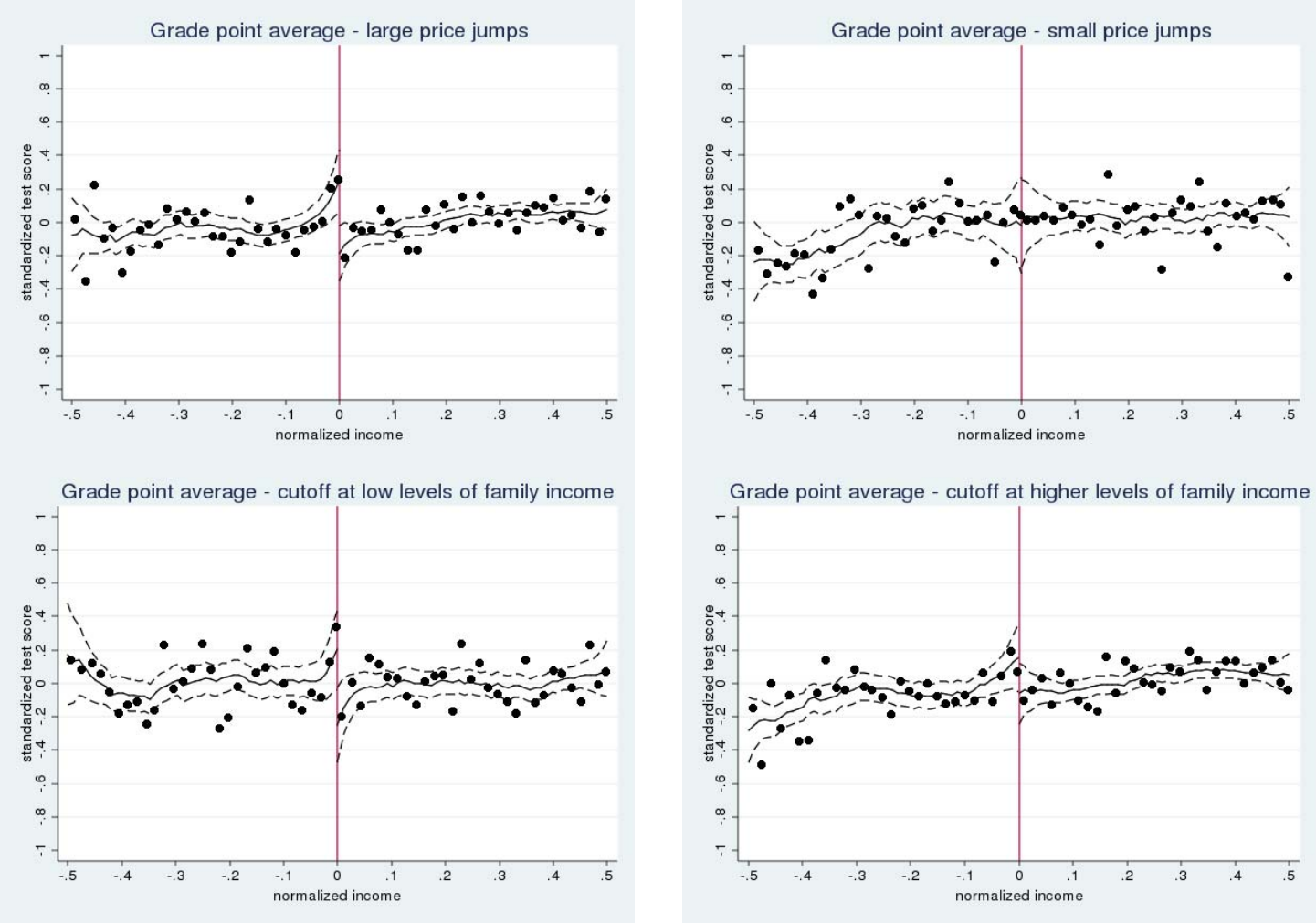


Appendix Figure 2
Balancing Tests



Note: the solid line is the local linear regression with a rectangular kernel and bandwidth .08. The dashed lines are 95 % confidence intervals. The scatter plot is the average standardized outcome for 60 income bins.

Appendix Figure 3
Results by price jumps and cutoffs



Note: the solid line is the local linear regression with a rectangular kernel and bandwidth .06. The dashed lines are 95 % confidence intervals. The scatter plot is the average standardized outcome for 60 income bins.

Additional Data description

Our main challenge with the tax deduction data is the presence of siblings. If parents take a tax deduction for childcare, we cannot observe which child (or children) attended childcare. In one-child families or multi-child families where only one child is of preschool age, there is no problem, as any deduction has to relate to that child.[38] In multi-child families, we use the age of all children to determine which children are likely covered by childcare. If the family has two children of pre-school age and has a tax deduction, we assume the older child is in childcare, as it would not make sense for mothers to stay home with older children and send younger ones to formal child care. We also can look at changes in the deduction over time to determine when the second child likely entered childcare. As we know the sibling reduction in specific municipalities, we can use this to define a minimum increase in deductions that relates to a new child entering childcare.[39]

Given that this is the only dataset of its kind in Norway, we are not able to verify the accuracy of our childcare numbers. However, we can aggregate our variable to the

[38] Parents can deduct care expenses from taxes until a child is aged 10 years old. During the first year of a child's life, parents are eligible for parental leave entitlements. Hence, we define pre-school age as aged 1-10.

[39] We will illustrate with an example using a two-child family with one child in our analysis sample having an older sibling of preschool age. Say we study child care at age 5 and the older sibling is aged 7. A positive tax deduction could then relate to the older sibling instead of the younger one. We then do the following: starting at age 1 (age 3 for the older sibling) we study tax deduction for all subsequent years (2-5 for younger sibling, 4-7 for older sibling). If there is no increase in tax deduction the subsequent years, the younger sibling started child care at age 1. If there is an increase in tax deductions of at least the cost of having an additional child in child care (the sibling reduction is on average 50 % so the increased costs is then also 50 %) in one of the subsequent years, the younger sibling started child care in the year of the boom in tax deductions. Our definition then assumes the following: if you start child care at a given age you do not drop out of child care again and if we do not observe any future increases in costs of child care and the family has a positive deduction, you attend child care from age 1. For families with three or more children of pre-school age, we use the same procedure except we separate by both siblings younger (then the child (who is the oldest one) is in child care), one older and one younger (then we use same method as for having an older sibling in two-child families) and both older (then we compare the costs (tax deduction) of adding a third child in child care).

national level and compare this to the national statistics for formal child care. As can be seen in Table 1 in the appendix, it is reassuring that we find aggregate numbers that are very close to the national statistics. As a robustness check, we will also conduct analysis using an alternative simple definition of childcare attendance where we define a dummy for attendance if we see a positive child care deduction in the data. This will tend to overstate child care usage as the tax deduction may relate to a different child in the family. However; note that the difference between the simple definition and the definition by family size is only minor. This rationalizes the focus on subsidies at age 5 since children age 5 are very likely to be the family's main user of formal child care.[40]

[40] The mother is unlikely to be home with five year olds and send younger children to child care however she is also unlikely to be home with a five year old and then send children of school age to after-school care (when they could instead come home to the mother).

Appendix Table 1
Average Childcare Coverage
Various Definitions – total sample

	Average Childcare Coverage (%)		
Age of Child	**3**	**4**	**5**
Sample Definition: Simple			
	.75	.76	.77
	(.43)	(.43)	(.42)
Sample Definition: Adjusted for Family Size	.68	.70	.72
	(.47)	(.46)	(.49)
Municipality Definition			
	.65	.65	.70
	(.018)	(.016)	(.015)
Mothers Labour Participation Income>2G (G=xxxx)	.81	.83	.84
	(.39)	(.38)	(.37)

Appendix Table 2
Distribution of Student Performance
Total Sample and Discontinuity Sample

	1	1-2	2-3	3-4	4-5	5-6
GPA						
Total [N=359,339]	.0	1.0	12.9	34.6	42.4	9.2
Discontinuity [N=10,238]	.0	2.4	22.1	39.1	31.6	4.8
Written Exam						
Total [N=344,271]	1.8	16.7	32.0	31.7	15.4	2.5
Discontinuity [N=9,572]	4.1	24.1	34.7	26.0	9.5	1.6
Oral Exam						
Total [N=318,783]	.3	6.3	19.6	28.8	28.2	16.8
Discontinuity [N=8,823]	.6	11.0	25.9	29.8	22.0	10.7

Appendix Table 3
Balancing Tests

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5polynomials	N
Mother's education at birth of child	.157 (.447)	.304 (.372)	.356 (.332)	.038 (.300)	.251 (.375)	.193 (.454)	10770
Father's education at birth of child	.320 (.432)	.250 (.368)	.375 (.334)	.121 (.309)	.236 (.372)	.373 (.442)	10770
Mother's age at birth of child	-.615 (.626)	-.475 (.522)	-.211 (.484)	-.318 (.434)	-.709 (.545)	-.371 (.660)	10770
Father's age at birth of child	-.165 (.762)	-.053 (.662)	.035 (.584)	-.222 (.484)	-.074 (.661)	-.081 (.792)	10770
Mother non-Norwegian citizen at birth of child	-.021 (.046)	-.027 (.040)	-.011 (.035)	.024 (.029)	-.014 (.036)	-.018 (.044)	10770
Father non-Norwegian citizen at birth of child	.005 (.046)	.004 (.039)	.011 (.034)	.032 (.030)	.023 (.038)	.012 (.045)	10770
Parents married at birth of child	-.032 (.055)	-.021 (.048)	-.041 (.042)	-.049 (.037)	-.062 (.047)	-.032 (.057)	10770

Columns 1-3 report the coefficients from an RD regression running local linear regression with a rectangular kernel and different bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income allowing the trends in income to vary on each side of the discontinuity.

Appendix Table 4
Additional Balancing Tests

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5polynomials	N
Mother is on welfare when child is 4	.022 (.043)	.052 (.036)	.037 (.034)	.032 (.030)	.049 (.038)	.070 (.044)	10770
Mother is a student when child is 4	-.022 (.039)	-.018 (.034)	.020 (.030)	.001 (.028)	.023 (.035)	.026 (.042)	10770
Average family income when child is aged 0-3	.075 (.073)	.040 (.082)	.070 (.059)	.045 (.051)	.013 (.064)	.070 (.081)	10770

Columns 1-4 report the coefficients from an RD regression running local linear regression with different kernels and bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income.

Appendix Table 5
Additional Balancing Tests: probability of being below cutoffs in the year prior to subsidy at age 5

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5polynomials	N
Probability of being below cutoff at age 1	-.003 (.104)	-.068 (.088)	.036 (.078)	-.029 (.062)	-.029 (.076)	-.009 (.093)	3578
Probability of being below cutoff at age 2	-.039 (.088)	-.057 (.075)	-.004 (.067)	.008 (.047)	-.014 (.060)	-.052 (.072)	5103
Probability of being below cutoff at age 3	-.029 (.079)	-.048 (.066)	-.010 (.060)	.034 (.041)	.012 (.053)	-.040 (.066)	6676
Probability of being below cutoff at age 4	-.065 (.067)	-.091 (.058)	-.056 (.049)	.035 (.038)	-.021 (.049)	-.024 (.060)	8650

Columns 1-4 report the coefficients from an RD regression running local linear regression with different kernels and bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income.

Subgroup analysis

To understand more about which children are most affected by the child care subsidy, we split the sample in different subgroups based on pre-child care characteristics. Appendix Table 6 reports the results using a rectangular kernel with bandwidth 0,08 for the following subgroups: mothers having 10 years or fewer or education when child was born compared to more than 10 years, parents who were married/cohabiting when child was born compared to not married/cohabiting, females compared to males and mothers with and without Norwegian citizenship status when child is born.

We see that the effect is largely driven by mothers with more than 10 years of education, although it is important to note that the effect is only statistically different when looking at the effect of the subsidy on oral exam performance. Note that the takeup rate of child care is much lower for families where mothers have 10 years or lower education, suggesting that the effects are not directly comparable. It is also worth noting that we already focus on low income families so the mothers with more than 10 years of education are still a disadvantaged group compared to the total population. We see very few differences across marital/cohabitation status, also in takeup rates of child care. For gender, we observe quite similar effects, with the effects for girls being stronger for oral exam. Overall, there do not seem to be large difference by subgroups.

Appendix Table 6
Effect of Childcare Subsidy on Children's Junior High Academic Performance
By Subgroups
Regression Discontinuity Results
Rectangular Kernel, .08 Bandwidth

Subgroups	Mother 10 years or lower education	Mother more than 10 years of education	Married /cohabiting when child is born	Not married /cohabiting when child is born	Females	Males	Mother non-Norwegian citizen at birth of child	Mother Norwegian citizen at birth of child
Takeup rates of child care	.44	.65	.54	.57	.54	.56	.39	.59
GPA	.205	.339**	.337***	.252	.238*	.341**	.331	.306**
	(.151)	(.156)	(.135)	(.201)	(.142)	(.156)	(.267)	(.121)
N	[4870]	[5368]	[7365]	[2873]	[5010]	[5228]	[1677]	[8561]
Written exam	.046	.109	.174	-.022	.095	.127	.100	.114
	(.154)	(.153)	(.139)	(.200)	(.163)	(.156)	(.255)	(.125)
N	[4476]	[5096]	[6927]	[2645]	[4716]	[4856]	[1587]	[7985]
Oral exam	-.023	.460***	.321**	.161	.345**	.158	.224	.282**
	(.163)	(.164)	(.142)	(.218)	(.158)	(.171)	(.274)	(.135)
N	[4167]	[4656]	[6402]	[2421]	[4334]	[4489]	[1451]	[7372]

This table reports the coefficients from an RD regression running local linear regression with a rectangular kernel and bandwidth of 0,08 on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications.

Appendix Table 7
Placebo – flat price municipalities

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5 polynomials	N
Grade point average	-.052 (.113)	-.030 (.098)	-.039 (.086)	.068 (.094)	-.105 (.118)	.025 (.142)	8128
Written exam	-.140 (.151)	-.145 (.135)	-.098 (.117)	-.021 (.102)	-.145 (.128)	-.040 (.156)	7592
Oral exam	-.030 (.178)	-.023 (.149)	-.023 (.136)	-.102 (.106)	-.029 (.132)	-.108 (.160)	7023

Columns 1-3 report the coefficients from an RD regression running local linear regression with a rectangular kernel and different bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income allowing the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, marital status, at birth of child, and pre-child care family income, mothers welfare status and mothers student status at age 4 of the child.

Appendix Table 8
Placebo – moving cutoffs +/- 5 % from original cutoffs

	Kernel: rectangular			Parametric			
Bandwidth	.06	.08	.10	3 polynomials	4 polynomials	5 polynomials	N
Cutoff -5% from original cutoff							
Grade point average	-.033	-.060	-.047	.048	-.002	-.114	10367
	(.121)	(.100)	(.090)	(.071)	(.077)	(.113)	
Written exam	.098	.020	-.017	.060	.009	-.121	9572
	(.130)	(.109)	(.097)	(.090)	(.075)	(.112)	
Oral exam	-.117	-.167	-.132	.074	.027	-.080	8823
	(.126)	(.109)	(.097)	(.072)	(.073)	(.109)	
Cutoff +5% from original cutoff							
Grade point average	.035	-.047	-.055	.100	.117	.103	10367
	(.122)	(.107)	(.098)	(.072)	(.087)	(.099)	
Written exam	-.018	-.073	-.040	.075	.054	.060	9572
	(.127)	(.113)	(.101)	(.071)	(.089)	(.092)	
Oral exam	-.040	-.054	-.051	.067	.011	.266	8823
	(.126)	(.114)	(.104)	(.067)	(.091)	(.355)	

Columns 1-3 report the coefficients from an RD regression running local linear regression with a rectangular kernel and different bandwidths on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Columns 5-7 report coefficients from a parametric specification with cohort*municipality fixed effects and different flexible specifications of income allowing the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, marital status, at birth of child, and pre-child care family income, mothers welfare status and mothers student status at age 4 of the child