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ABSTRACT

Care or Cash? The Effect of Child Care Subsidies on Student Performance*

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.

JEL Classification: I1, J1

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1. Introduction

Many countries have implemented childcare subsidies in an effort to help families; in the United States, the government created the Child Care and Development Fund in 1996, which provides public funds for childcare assistance to low-income families. Despite the importance of the issue, little is known about the effect of childcare subsidies on parent and child outcomes. Research in this area has been limited because of the difficulty identifying 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 one cannot isolate the effect of price alone on later child 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 effect of childcare subsidies on parental behavior and the later academic achievement of children.

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 (2010b) has examined the effect of childcare subsidies in the United States on children's academic performance.[1] 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.[2] Our work complements this

[1] Also see Tekin (2005), Tekin (2007) and Blau and Tekin (2007).
[2] There is also a substantial literature looking at the effects of programs providing universal childcare. Herbst and Tekin (2010a) and Magnuson, Ruhm and Waldfogel (2007) find negative effects of universal childcare programs on children's performance, while Berlinski, Galiani and Manacorda (2008), Berlinski,

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

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 standard deviation. 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 standard deviation for those who receive the childcare subsidy.

Given this finding, we next investigate the mechanisms through which it is working. A childcare price subsidy may have a number of effects on the family. First, it may increase the attendance at formal childcare relative to less expensive and often lower-quality informal childcare. A lower childcare price could also reduce parental care (instead of informal care) and potentially increase parental labor supply. Alternatively, a subsidy could serve as a pure income transfer if demand for day care is inelastic. For any given gross income, families paying a lower price will have more disposable income than families paying the higher price.

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 a spot.[3] Also, as we describe later, parents are not informed about the income cutoffs that determine the childcare price unless their application for a childcare

Galiani and Gertler (2009), Fitzpatrick (2010), Havnes and Mogstad (2011) and Havnes and Mogstad (2010) find positive effects.

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

place has been successful. 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 significant effects on later academic performance, this suggests that early investments that increase disposable income may have long lasting effects.[4] Interestingly, and consistent with a disposable income explanation, we also find effects of the subsidy on the academic performance of older siblings.

Our research also contributes to a growing literature on the effect of family income on child outcomes. The results in this literature are mixed. Using a variety of identification strategies, Oreopoulos, Page and Stevens (2008), Dahl and Lochner (2011) and Milligan and Stabile (2007) find 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 instrumental variables, and Blau (1999) and Dooley and Stewart (2004) using fixed effects, find no or very small effects. Differences could be due to different identification strategies, data sources, countries and institutional settings.[5]

[4] 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).

[5] Dahl and Lochner (2011), argue that fixed effects (FE) estimators do not control for endogenous transitory shocks not directly related to family income and suffer from greater attenuation bias than OLS and Instrumental variables (IV), because family income is measured in differences rather than levels. Løken, et al. (2011) argue that differing estimates 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.

This paper advances our understanding along two dimensions. First, we are able to convincingly separate income effects from labor force participation; most of the identification strategies used in the existing literature 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 force participation or childcare utilization in the short-run, we are able to isolate what appears to be an income effect.[6] Second, given the recent literature suggesting the importance of investments early in a child's life (see Carneiro and Heckman, 2003 and Currie, 2009, for overviews), we are able to analyze the effects of shocks to income, through child care subsidies, when children are age 5, which is likely to be a critical period for human capital investment.

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

2. Institutional Background

Although the history of day care in Norway goes back a hundred years and the first law regulating day care was in 1953, there was almost no formal child care for children below age 7 (the school starting age, which was changed to 6 in 1997) in Norway until the mid seventies.[7] However, by the 1990s, the period we study, day care

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[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 change due to the subsidy.

[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, 2011). 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.

center coverage had risen to 60 % among 3-5 year olds and continued to increase throughout the period of study.[8]

There are two types of child care centers in Norway: public (municipality level) and private. In the early 1990s approximately 60 percent of the daycare centers were public. The private centers were typically owned by non-profit organizations like churches and cooperatives. However, both types of day care centers 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 is from the municipality and the rest is paid as fees by the parents. Most of the municipalities also subsidize private day care centers, 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 centers it is the municipality that pays the difference between a full fee and a reduced fee (the discontinuity we study); the day care centers 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 female labor force participation from the mid seventies onwards in Norway, creating excess demand for day care, and leading to

[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%.

[9] The Day Care Act ("Barnehageloven") gives nationwide standards along several areas for day care centers. 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 center. 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 centers. (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 centers across municipalities. In a recent survey, it was found that the share of formally qualified teachers in day care centers varied both across municipalities and within municipalities (Gulbrandsen and Winsvold, 2009). However, they did not report any differences across private and public centers.

rationing of access to day care centers. The allocation rules determining who got access are not transparent; nor is it clear whether there were different rules for private and public day care facilities. 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 to public day care centers (since both the state and the municipality provided subsidies).

Once the child is offered a place in childcare, the family is then informed that they can apply for the subsidy if they have family income below a certain level specified in the letter. While subsidy application is now online, in the 1990s, eligible families went to the municipality office to fill out the application form and document their income. If their income is below the cutoff, they received the subsidy. The relevant income measure is household income that includes income of the mother and, if applicable, her spouse/co-habiter.

The alternative to a formal day care center was the informal sector.[11] 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.[12]

[11] It was not until 2008 that Norway, through a change in the law, required 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).
[12] This was true for registered nannies (who paid income taxes) as well.

Mainly due to the availability of data, we will focus on childcare subsidies at age 5.[13] 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% of those who attended childcare at age 5 also attended formal child care at age 4. In addition, given the situation of excess demand, childcare decisions were likely determined prior to the granting of the subsidy.

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.[14] 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.

3. 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

[13] We have more observations for these cohorts as our data on income cutoffs and prices start 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 regression discontinuity (RD) design for identification, we need a large sample size to get enough observations around the discontinuity. So, while we would ideally like to study the effect of total childcare usage during childhood, we can only study childcare usage at age 5.
[14] See the report from the research institute of NOVA by Gulbrandsen and Winsvold (2009)

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. In this paper we focus on the first income cutoff in each municipality as, in municipalities with more than one income cutoff, the price differences at higher cutoffs are typically small.

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 for cheaper childcare jumps from 0 to 1 at the discontinuity. However, the take-up rates of childcare and the subsidy are below 100 %, and we have to take this into account when interpreting the estimates. 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 family income the year before ($fI_{i,m,t-1}$); if income was below a particular cutoff ($c_{m,t}$), 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 $c_{m,t}$.

Because the level of the cutoff varies by municipality and year, in all our analysis, we normalize family income by dividing it by the relevant cutoff income level in the municipality and subtracting one:

$$I_{i,m,t-1} = (fI_{i,m,t-1}/c_{m.t}) - 1$$

By construction, normalized family income ($I_{i,m,t-1}$) equals zero at the cutoff and takes on positive (negative) values above (below) the cutoff.[15]

For identification, we need to assume that income and other characteristics about the family vary continuously through the cutoff point; we verify this by comparing characteristics on either side of the cutoff. 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 to verify that the results are not driven by choice of smoothing parameters.[16] We use a paired-bootstrap 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 that imposes more structure but enables us to include individual and family controls in the equation. We estimate the following:

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

where $f\,(I_{i,m,t-1})$ is normalized family income the year before entering the equation in a flexible form, *x* is a vector of individual and family control variables, and λ is a vector

[15] We have also tried normalizing income by subtracting the cutoff level of income in the municipality and this leads to similar results.

[16] 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. We solve

$$\min_{\beta,\tau,\gamma} \sum_{i=1}^{N} K\left(\frac{I_{i,m,t-1}}{h}\right)(y_i - \eta - \beta(I_{i,m,t-1}) - \tau E_{i,m,t} - \gamma(I_{i,m,t-1})E_{i,m,t})^2$$

where *h* is the bandwidth and $E_{i,m,t} = 1\{I_{i,m,t-1} < 0\}$. The parameter of interest is estimated as $\hat{\alpha}_{RD} = \hat{\tau}$.

of cohort by municipality fixed effects.[17] 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 normalized income. We allow the slopes to be different on either side of the discontinuity. We also control for other pre-childcare parental characteristics in order to increase the precision of our estimates.

We will present our 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 scatterplots 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 that vary on each side of the discontinuity.

4. Data

We use administrative data covering the entire population of Norway. The analysis includes birth cohorts from 1986-1992 and links individuals to their parents through unique identifiers. We have information on parental characteristics such as

[17] 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, and family income prior to age 4 (measured as the average income when the child was ages 1-3). In addition, we include interactions of municipality dummies with cohort dummies.

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 childcare 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 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.[18] Our measure of disposable income is defined as family income minus the childcare price faced by the family.

Our measure of child care attendance is created from the information on tax deductions for child care expenses from parents' tax records which are available from 1993.[19] 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.[20] As a result, our definition of

[18] Almost all municipalities use previous year's tax sheet to determine subsidy eligibility. .As a robustness check, we have 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.

[19] This means that we do not have information on 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. However, it is important to include them due to earlier mentioned sample size issues.

[20] 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.

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. The Data Appendix and Appendix Table 1 contain details of exactly how childcare usage is inferred from the tax data.

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 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.

There are some missing observations in the data; we exclude the observations where parental background characteristics are missing. This reduces our sample from 448,198 observations to 367,836. We have tested that the main results on child outcomes are not sensitive to excluding these observations.

Our analysis is conducted on families that are located around the first price discontinuity in each municipality. We include families with income no more than 50% below or above the discontinuity, that is with normalized income between -0.5 and +0.5.[21] The results are generally not sensitive to this cut; however, the more observations

[21] 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

we include the further we move away from the discontinuity, while including fewer leads to less precise estimates.[22]

For children, we have information on performance in junior high school national exams and their grade point average (GPA) in 10th grade.[23] The grade point average is an average of the 10th grader's performance in all 12 graduating subjects.[24] The exam data are 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 graded. The written exam could be in either math, Norwegian or English, randomly assigned to schools[25] 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 middle school and the students are randomly allocated to subjects. These grades are important for high school admissions. The grades range from 1-6.[26]

Table 2 gives descriptive statistics for the total sample of children born between 1986 and 1992 as well as for our analysis sample. We see that the samples are very similar in terms of child characteristics such as age, gender, number of siblings and birth

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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.

[22] 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 who receive social security benefits and this also does not change any of the main results.

[23] In Norway children are aged 13-16 when attending junior high school.

[24] 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.

[25] 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 by cohort fixed effects.

[26] 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 it covers all subjects and averages 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.

order. However, in the analysis sample, parents tend to have fewer years of education and are more likely to be of non-Norwegian citizenship, highlighting the fact that the analysis 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 analysis sample. When comparing school performance of the total sample with our analysis sample, we see that children in the analysis sample tend to perform worse, with a mean GPA of 4 and 3.7, respectively.[27] This is not surprising as we know that children from low income families tend to perform worse in school.

5. Results

5.1. Testing for Income Manipulation

By communicating with the municipalities, we have verified that the cutoffs for the childcare subsidy are not also used for other social programs. Therefore, we are not worried that our discontinuity design could be picking up the effects of other welfare programs in addition to childcare. The remaining potential threat to the design is the possibility that families manipulate their income so as to locate strategically around the income cutoffs (Lee and Lemieux, 2009). There are several reasons why we think this is not a factor in this case:

1. The cutoff is based on the previous year's income but is unknown the year before the childcare subsidy is allocated; as a result, families cannot perfectly predict where the cutoff will be and adjust their income accordingly. However, a concern is that, in some

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

cases, the cutoff may be predictable. For example, of the 72 municipalities, 30 did not change cutoffs in our sample period, so the cutoff is easy to predict. The remaining 42 municipalities do change cutoffs and we have verified that none do so in an easily predictable fashion. For example, none of them increase the cutoff by the same percentage each year. Later (Table 4), we show that our estimates are robust to restricting the sample to only municipalities with large changes in cutoffs over time.

2. If there is income manipulation, it should show itself in a spike in the income distribution just below the cutoff. In Appendix Figure 1, we show the density of normalized family income for the analysis sample. We see that there is no evidence of income clustering below the cutoff.

3. Income manipulation would be a problem as it might imply that the unobserved characteristics of families would jump at the discontinuity. This implication is not testable but we can examine observed family characteristics. 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 left 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. The different columns correspond to different bandwidths. We see that there are no statistically significant differences between families when it comes to these characteristics, and this is robust across specifications. Appendix Figure 2 shows this graphically, presenting estimates with a rectangular kernel and bandwidth of .08 along with the associated 95% confidence intervals.[28]

[28] 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.

The right part of Appendix Table 3 presents the balancing tests using a parametric specification where the 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 provides support for the assumption that children's outcomes are most likely only being affected by the childcare subsidy itself, and not other differences.[29]

4. 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 is equal for both the treatment and control groups. Appendix Table 5 presents estimates of the probability of being below the cutoff when the child was aged 1-4. Importantly, we see that there is no effect of current subsidy receipt on the probability of being below the cutoff at ages prior to the subsidy; if anything, individuals who were eligible for the subsidy at age 5 are slightly less likely to have been eligible for the subsidy in earlier years so there is no evidence of serial income manipulators. This further supports our assumption that there is no strategic income manipulation.

5.2. Children's Outcomes

Table 3 presents the effect of childcare subsidies on children's grade point average and exam grades in junior high school. When analyzing these test scores, we standardize them to have mean zero and standard deviation of one so that the coefficients are comparable across regressions. Figure 2 presents the results for the standardized scores graphically, using a rectangular kernel and a bandwidth of 0.08. From the table,

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[29] Appendix Table 4 provides additional balancing tests on mother's student and welfare recipient status when the child is aged 4 and average family income prior to age 5. Once again, there are no significant differences by eligibility status.

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 childcare subsidy, this is an intention to treat effect. Given take-up of childcare for our analysis 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.[30] The right panel of Table 3 presents the estimates from the parametric specifications; we see that the results are positive and generally statistically significant.

From the graphs in figure 2 we can see that there is substantial variation in the data, and it is clear that the data points close to the discontinuity are driving our results. As there is a trend in child outcomes across family income, the more observations we include (the larger the bandwidth), the lower the estimates. It is reassuring, though, that the estimates are significant even in local linear regression specifications with a large bandwidth (.10)[31] and in parametric specifications that control for a quartic or quintic income polynomial.[32]

[30] 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.

[31] When we use the Imbens and Kalyanaraman (2009) method to determine the optimal bandwidth, the suggested bandwidth is between .2 and .3 depending on the outcomes. However, because family characteristics become quite different when we move only a small distance away from the discontinuity, we use a smaller bandwidth to isolate the effect at the discontinuity. When we do use a bandwidth of .3, we still estimate a statistically significant effect of subsidy eligibility on GPA and oral exam scores of about .1 (with a standard error of .05).

[32] To understand more about which children are most affected by the child care subsidy, we split the sample into different subgroups based on pre-childcare characteristics (mothers having 10 years of education or fewer when the child was born compared to more than 10 years, parents who were married/cohabiting when the child was born compared to not married/cohabiting, females compared to males and mothers with

5.2. Robustness Checks

To verify our findings, we run a number of robustness checks. A key concern is whether individuals can manipulate their income to be on the desirable side of the cutoff. As noted before, this is unlikely, given that we see no bunching at the favorable side of the income cutoff and there are no observable differences in characteristics on opposite sides of the discontinuity. As a further check, we divide our sample into those municipalities with no changes in cutoffs over time, those with small changes, and those with larger changes. Presumably, if individuals are manipulating their income, the effects should be driven by the more predictable, no- or small- change municipalities.[33] The first two columns of Table 4 show that this is not the case.

As a further check, 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 4 Columns 3 and 4 presents the results and the main effects come from municipalities with the largest price cuts (also see Appendix Figure 3 for the grade point average). We find essentially no effect for municipalities with small price jumps and large, significant effects for municipalities with larger price jumps.

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and without Norwegian citizenship status when the child was born). There is very little evidence of large differences by subgroup.

[33] We classify municipalities as large-change municipalities if the average change in the cutoff in the municipality over our period exceeds the median across municipalities. As such, this group excludes all municipalities that never change cutoffs and also excludes some municipalities that make small changes.

Finally, 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. And we see from Table 4, Columns 5 and 6, the effects are driven by municipalities with cutoffs at low levels of family income.

We also take 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 6 presents these results; it is reassuring to note that there is no effect on children's grades.

In Appendix Table 7, we present results when we move the cutoff plus and minus 5% from the true 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%.

5.4. 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 5 shows the effects of the childcare subsidy on various intermediate outcomes, and Figure 3 plots the variables using a bandwidth of 0.08.

Importantly, from the first row in Table 5, we see that there is no evidence of any effect of the childcare subsidy on childcare utilization. This finding is robust to a variety of specification tests.[34]

Despite this, it is clear that the price jumps, and hence disposable income falls, significantly at the discontinuity. From the next row in Table 5 we see that families below the discontinuity pay on average 9000 NOK (USD 1500) less for childcare per year. 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. We see no significant effects of the subsidy on mother's or father's labor supply. This suggests that there are no responses by the parents on the extensive margin and is consistent with no effects on child care utilization; the subsidy does not appear to affect time allocation between the labor 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 who work part time). When we study mother's and father's income at age 5 we see no significant effect on mother's income or father's income.

The results are similar when we estimate the parametric specifications, presented in the right panel of Table 5. We find no effect of the childcare subsidy on childcare attendance and mother's labor supply but 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.

[34] 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.

But could one year of higher disposable income have this large an effect on student's performance many years later? Given the availability of data on parental income and labor force participation throughout the child's school years, we are able to see if there are longer-run effects on family resources. Table 6 presents the results when we examine average parental income (total family income and then separately by mother and father), calculated as the average from the years when the child was between 6 and 15. We also examine parents' labor force participation, calculated as the total years of employment when the child was between the ages of 6 and 15. Importantly, we find that the childcare subsidy at age 5 appears to have a significant and substantial effect on household labor market experiences in later years. Average family income is significantly higher, as is father's labor force participation. These results are robust to all the specification tests described earlier.

It is also interesting to note that these effects appear to be driven by labor force experiences once the child has entered schooling. In the final rows of Table 6, we break the sample based on birth cohorts: those who started school at age 7 and those who started school at age 6. (The legal school starting age was changed in 1996 from 7 years of age to 6 years of age.) The labor market effects only appear once the child enters school. This means that there is a differential effect between those having received the subsidy and those not having received the subsidy when the parents no longer pay child care costs as the children enter free public schools. The effect of this seems to be long-lasting, persisting until the children are tested at age 15 and 16.

Given these puzzling effects, we wanted to verify that the results were in fact being driven by the childcare subsidy. To do so, we break the sample by childcare attendance at age 5, noting that there should be no effect for families who are eligible for the subsidy but do not actually receive it. (Given that we find no effect of the subsidy on childcare attendance, this is less problematic than it might ordinarily seem, as it may be reasonable to treat childcare attendance as exogenous to subsidy eligibility.) These results are presented in Table 7. As anticipated, we find the entire effect is driven by those enrolled in childcare. Finally, Appendix Table 8 shows that the results for family income between the ages of 6 and 15 display the same patterns; we observe effects for only those families who receive the childcare subsidy, we see larger effects for municipalities with larger price jumps, and larger effects for municipalities with low levels of family income.

In summary, we find that the childcare subsidy at age 5 leads to higher disposable income at that age but also higher family incomes at ages 6 to 15 (once the child has started school).[35] We are not sure why it has this effect. One possibility is that these low-income people are liquidity constrained and the extra disposable income allows them to move into self-employment.[36] Another possibility is that the extra income allows them to invest in human capital and they receive the payoff from in terms of higher earnings in later years. Unfortunately, we do not have the required data to test these hypotheses.

Assuming that the childcare subsidy affects school scores through its effects on family income, we can estimate the implied effect of income on scores. Given disposable income at age 5 increases by 11% due to the subsidy if the child is in childcare and the effect on test scores is about .4 of a standard deviation, this would imply that a 1%

[35] As shown in Appendix Table 4, there is no evidence that family income is higher for the eligibles before the child is aged 5.

[36] Blachflower and Oswald (1998) show that windfall gains are very important factors in enabling low-income people to make a transition to self-employment.

increase in family income at age 5 would increase scores by about .04 of a standard deviation. This would be a large effect of a once-off income shock and it is substantially larger than the short run effect reported in Dahl and Lochner (2011) where an increase in income of about 20 % from a tax credit increased test scores in math and reading test scores by about 6% of a standard deviation. However, subsidy-receivers in our case also have about 15% higher family income when the child is aged 6-15. So, this implies that a *permanent* 1% change in family income increases test scores by about .03 of a standard deviation.

Siblings

Finally, 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 8, we report effects for older siblings and see that there are tendencies towards positive effects although they are more imprecisely estimated due to smaller sample sizes. When we look at younger siblings, the estimates are too imprecisely estimated to draw any conclusions.

7. 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 effects of the subsidies on children's academic performance in junior high school, suggesting the positive shock to disposable income provided by the subsidies may help 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 move towards more income means tested subsidies may be beneficial for children. Our findings suggest that 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 early childhood. They also lead to greater labor force participation in later years and, therefore, a permanent positive shock to family income. 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

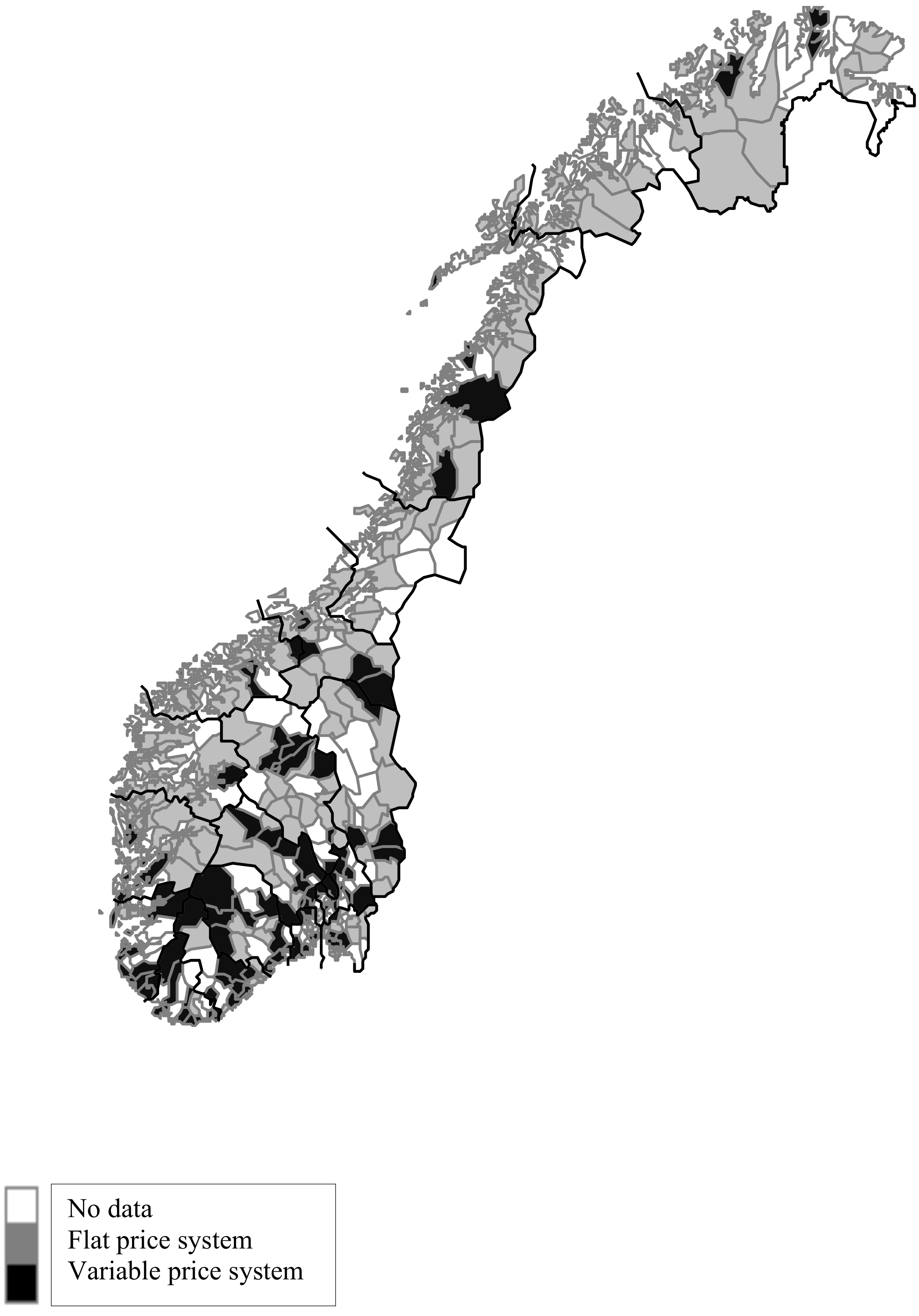
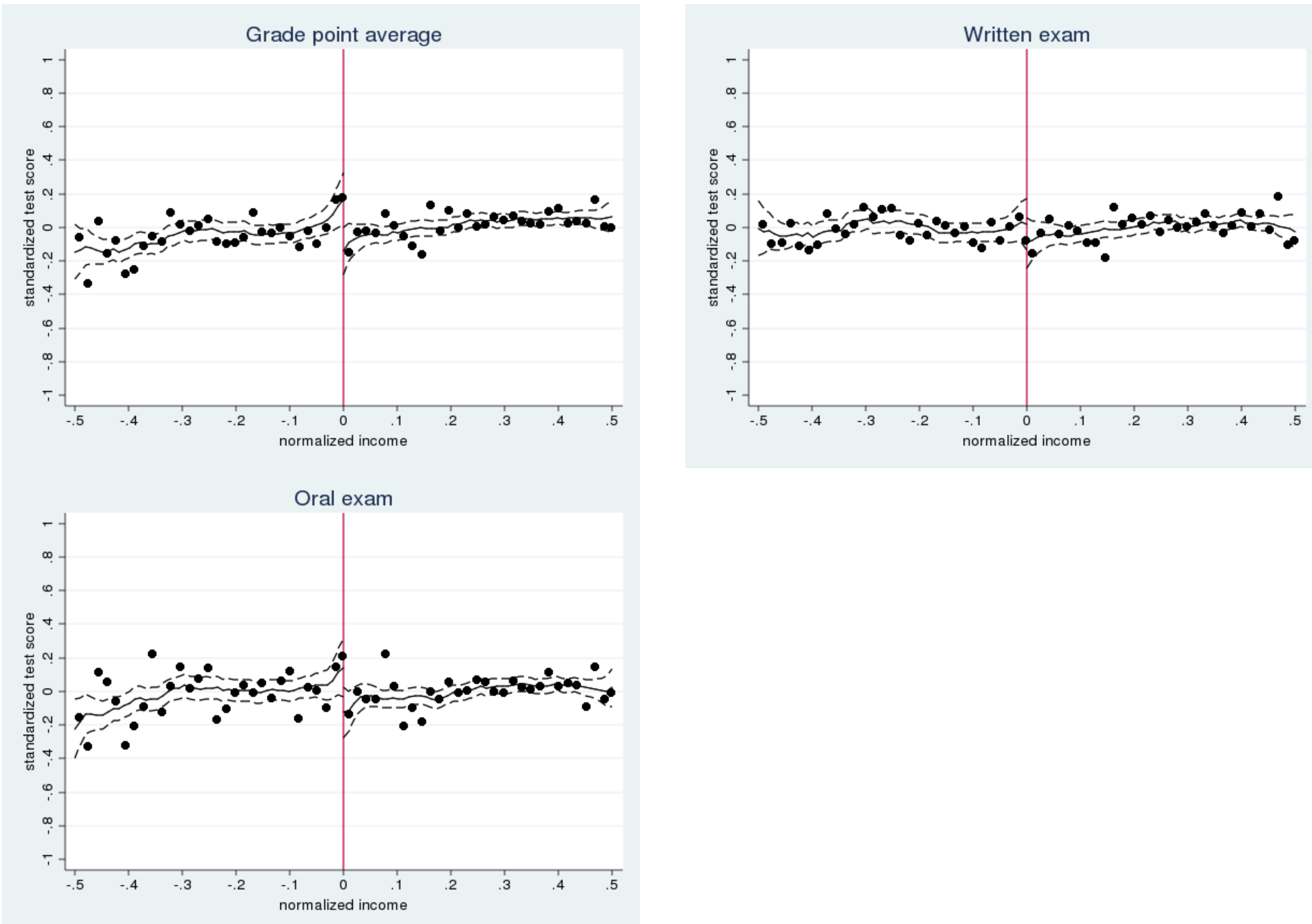
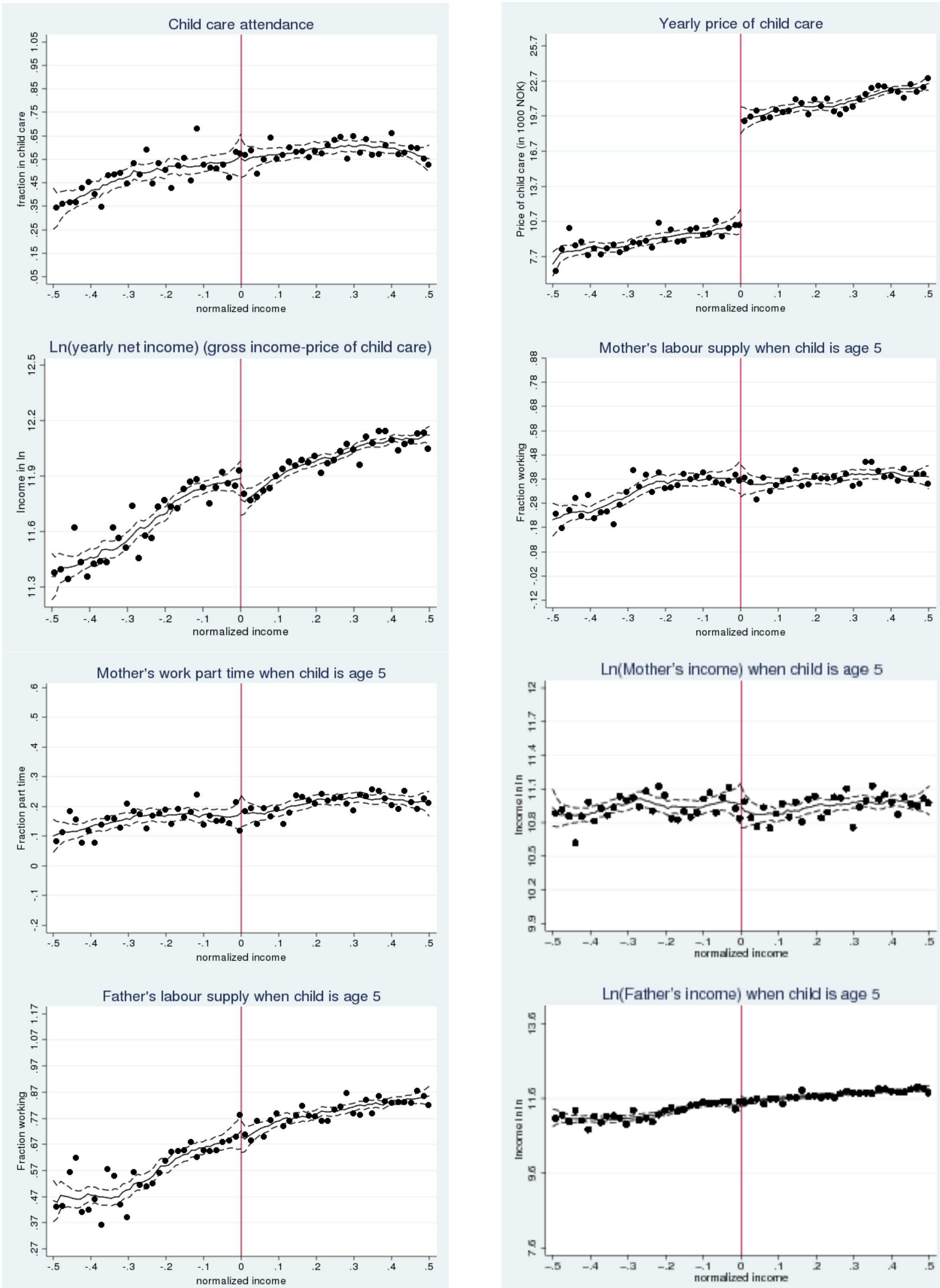


Figure 2
Effect of Childcare Subsidy on Children's Junior High Academic Performance



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: Report from the research institute of NOVA by Gulbrandsen and Winsvold (2009)

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Table 2
Descriptive Statistics

	Total Sample	Analysis 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: Type of polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	N
Grade point average	.310***	.310***	.259***	.134*	.259***	.271**	10238
	(.120)	(.110)	(.097)	(.081)	(.102)	(.124)	
Written exam	.150	.112	.156	.056	.140	.092	9572
	(.128)	(.115)	(.098)	(.086)	(.107)	(.129)	
Oral exam	.296**	.273***	.260***	.099	.178*	.227*	8823
	(.139)	(.105)	(.100)	(.087)	(.110)	(.133)	

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. The parametric specifications include cohort*municipality fixed effects and different flexible specifications of income that allow the trends in income to differ on each side of the discontinuity. In addition, we control for parents' age, education, citizenship, marital status at birth of child, pre-childcare family income, and mother's welfare and student status when the child was aged 4.

Table 4
Robustness Tests: Effect of Childcare Subsidy on Children's Junior High Academic Performance

	No or small changes in cutoffs 1993-1997	Big changes in cutoffs 1993-1997	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
Take-up rate of childcare	.57	.55	.55	.55	.48	.59
Grade point average	.228*	.285**	.441***	-.007	.448***	.224
	(.137)	(.119)	(.135)	(.205)	(.174)	(.145)
	[4555]	[5683]	[7148]	[3090]	[3706]	[6528]
Written exam	.069	.136	.186	-.101	.292*	.006
	(.161)	(.171)	(.129)	(.195)	(.177)	(.140)
	[4252]	[5320]	[6677]	[2895]	[3491]	[6071]
Oral exam	.319	.329*	.297**	.216	.310*	.239
	(.212)	(.197)	(.136)	(.218)	(.182)	(.157)
	[3991]	[4832]	[6116]	[2707]	[3115]	[5710]

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 5
Mechanisms

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	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)	
Ln(Mother's income)	-.096	.047	.026	.125	.122	-.022	8043
	(.155)	(.133)	(.121)	(.101)	(.129)	(.155)	
Father's labor supply	.017	.027	.039	-.011	-.005	.058	10770
	(.055)	(.048)	(.041)	(.036)	(.045)	(.054)	
Father's income	-.024	-.135	-.058	-.040	-.121	-.172	9812
	(.132)	(.113)	(.098)	(.081)	(.104)	(.127)	

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 6
Long Term Effects on Family Income

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	N
Ln(Annuity of family income) age 6-15							
	.115*	.171***	.184***	.160***	.126***	.179***	10636
	(.063)	(.057)	(.050)	(.040)	(.049)	(.059)	
Ln(Family income) age 6, cohorts starting school age 7 (86-90)	.017	.030	.004	-.002	.053	.036	7669
	(.096).	(.082)	(.071)	(.058)	(.074)	(.093)	
Ln(Family income age 6), cohorts starting school age 6 (91+92)	.009	.235**	.235**	.139	.151	.214*	3016
	(.139)	(.120)	(.106)	(.088)	(.106)	(.123)	
Ln(Family income) age 7, cohorts starting school age 7 (86-90)	.071	.153**	.140**	.114**	.137**	.182**	7642
	(.083).	(.071)	(.066)	(.056)	(071.)	(.085)	
Ln(Family income age 7), cohorts starting school age 6 (91+92)	.040	.145	.166	.079	.057	.182	3019
	(.168)	(.136)	(.118)	(.098)	(.125)	(.150)	

Columns 2-4 report the coefficients from an RD regression running local linear regression with 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.

Table 7
Effect of Childcare Subsidy on Children's Junior High Academic Performance by Child care status

Subgroups	Attend child care at age 5	Do not attend child care at age 5
Takeup rates of child care	1	0
GPA	.327**	.205
	(.166)	(.171)
N	3939	3175
Written exam	.349**	-.028
	(.170)	(.183)
N	3666	3164
Oral exam	.401*	.135
	(.211)	(.222)
N	3523	2758

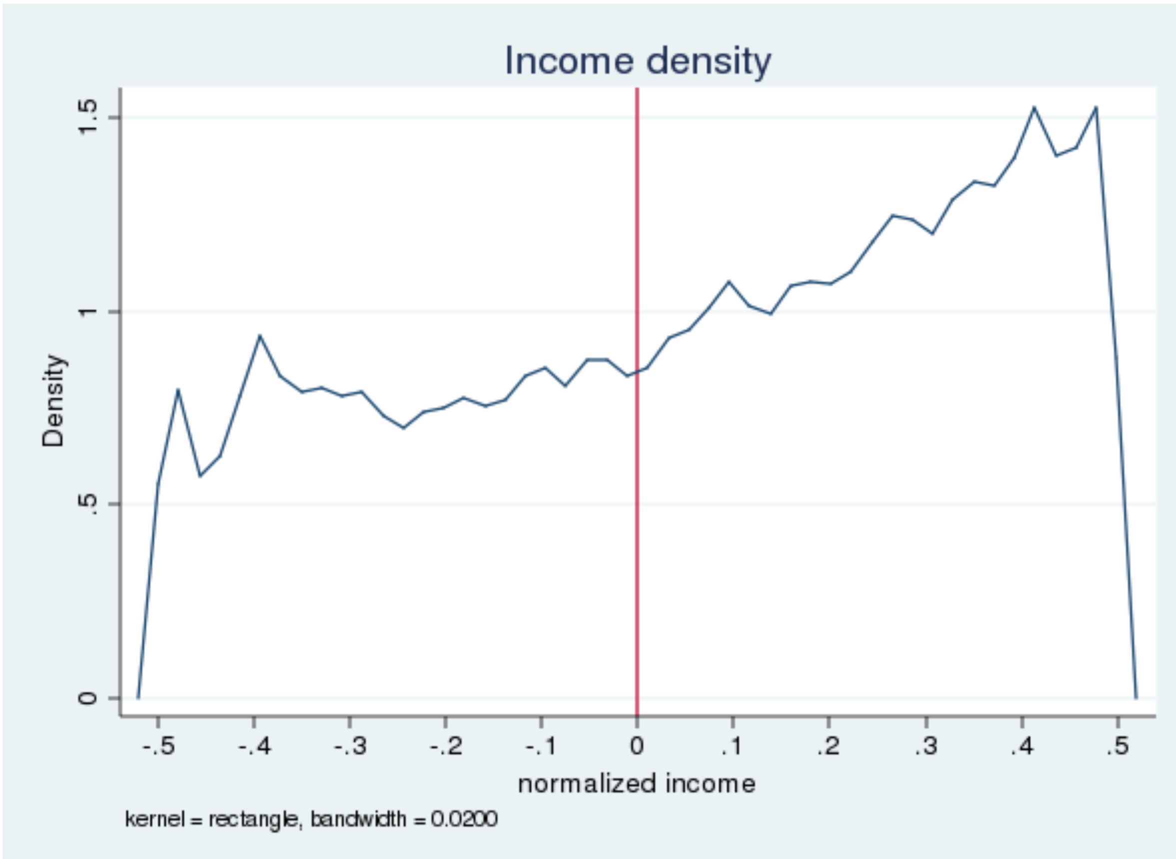
This table reports the coefficients from an RD regression running local linear regression with a rectangular kernel and bandwidth of 0.08 on each side of the discontinuity and taking the difference between the outcomes to the left and right of the discontinuity. The standard errors are obtained using percentile-T bootstrapping with 2000 replications. Note that we do not have child care data for the children born in 1986 and 1987.

Table 8
Effect of Childcare Subsidy on (closest) older Sibling's Junior High Academic Performance

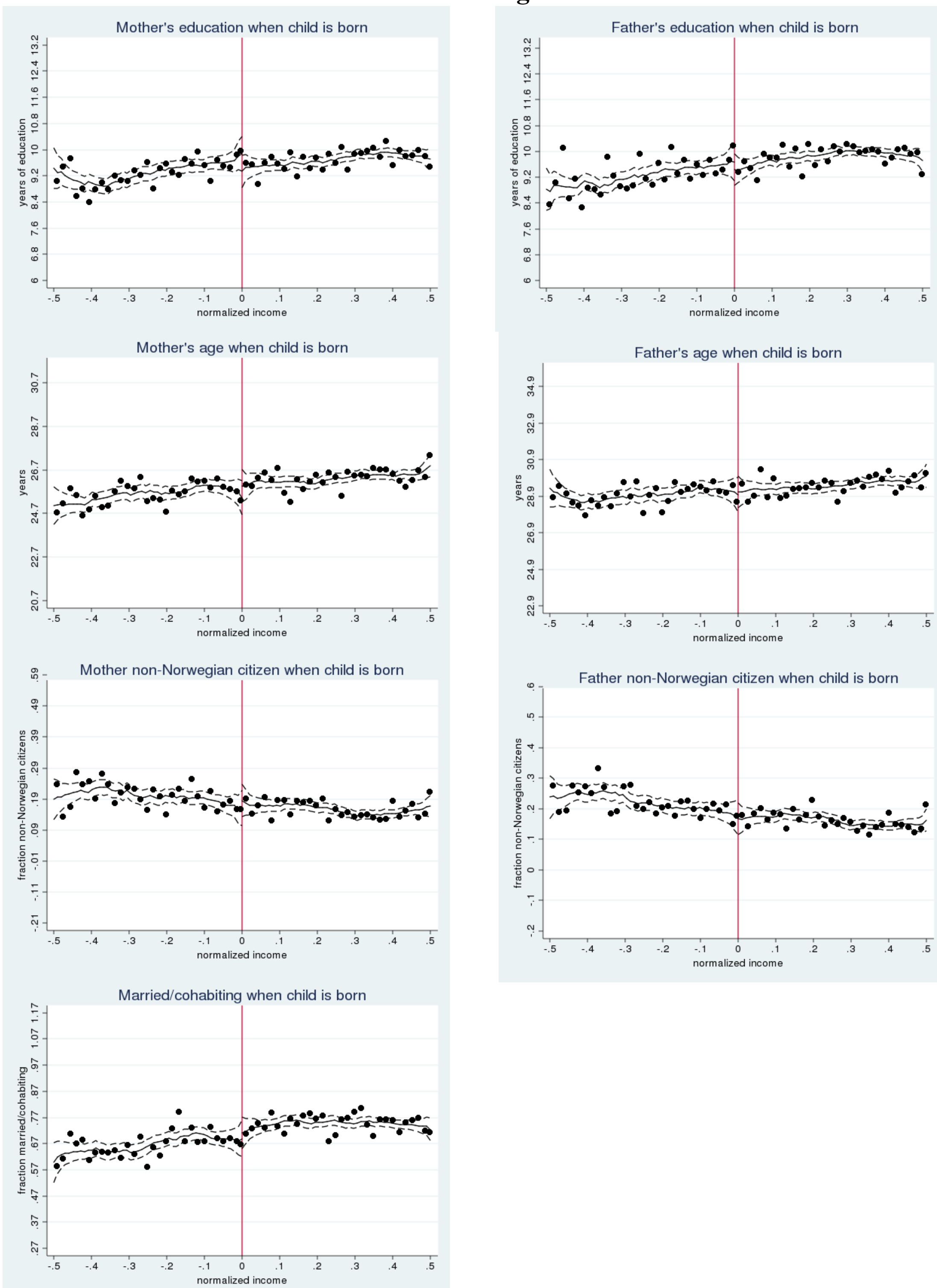
	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	N
Grade point average	.172 (.211)	.329* (.189)	.296* (.175)	.097 (.165)	.214 (.204)	.005 (.248)	2556
Written exam	.111 (.237)	.274 (.212)	.297* (.180)	.162 (.179)	.362* (.220)	.105 (.260)	2404
Oral exam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

Columns 2-4 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 that allow the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, and marital status at birth of child, and pre-childcare family income, mother's welfare status and mothers student status at age 4 of the child.

Appendix Figure 1:
Income density around discontinuity

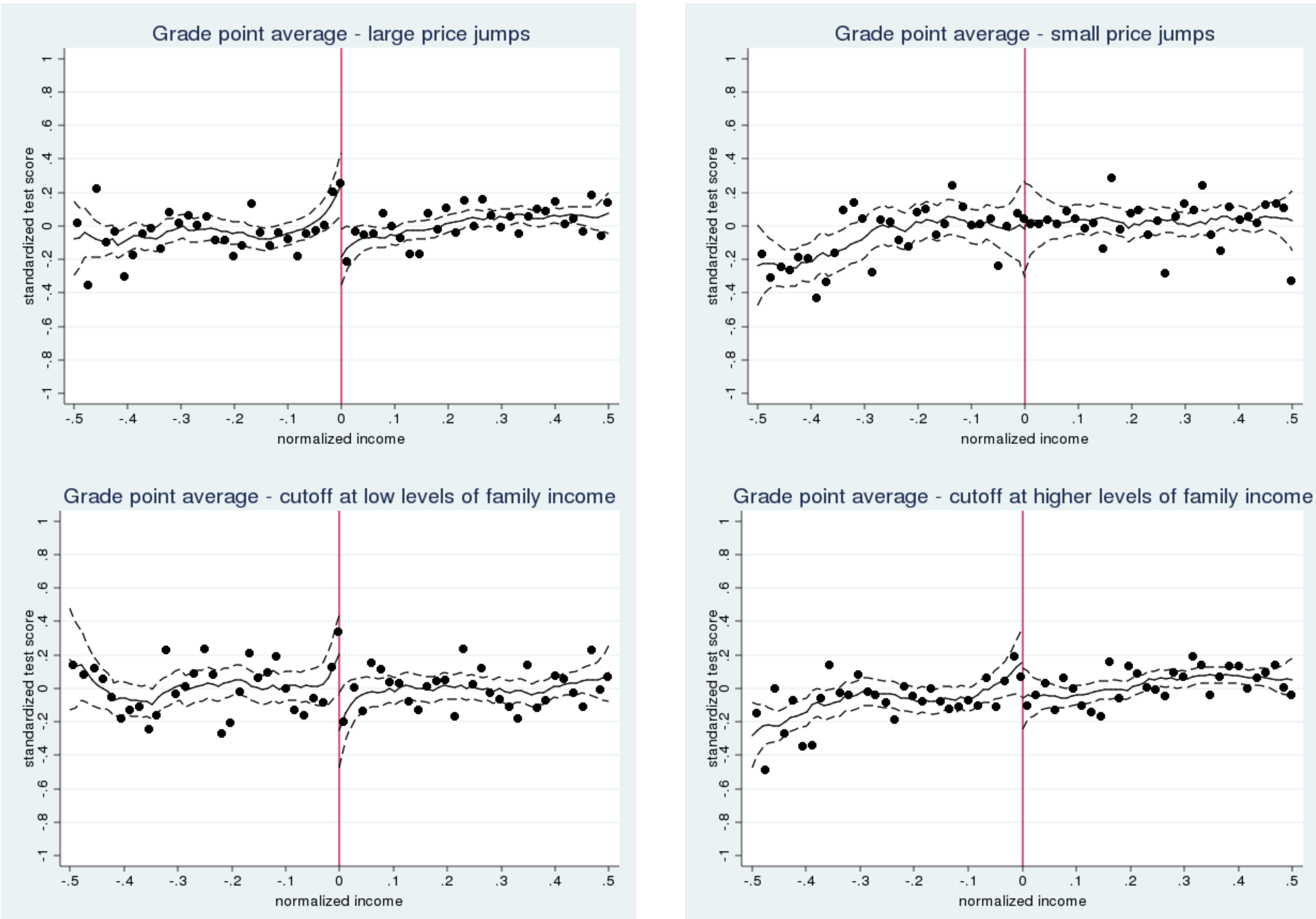


Appendix Figure 2
Balancing Tests



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.[37] 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.[38]

[37] 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.

[38] 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 deductions for all subsequent years (2-5 for the younger sibling, 4-7 for the older sibling). If there is no increase in the tax deduction in 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 increase 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).

Given that this is the only dataset of its kind in Norway, we are not able to verify the accuracy of our childcare numbers.[39] However, we can aggregate our variable to the 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 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]

[39] 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 use family income and prices as our identification strategy, it would be problematic to also use the same information to create the child care variable.

[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			
	.81	.83	.84
	(.39)	(.38)	(.37)

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

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

Appendix Table 3
Balancing Tests

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	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 2-4 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 that allow the trends in income to vary on each side of the discontinuity.

Appendix Table 4
Additional Balancing Tests

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	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 2-4 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 years prior to subsidy at age 5

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	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 2-4 report the coefficients from an RD regression running local linear regression with 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.

Appendix Table 6
Placebo – flat price municipalities

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	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 2-4 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 that allow the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, and marital status at birth of child, and pre-child care family income, mother's welfare status and mother's student status at age 4 of the child.

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Appendix Table 7
Placebo – moving cutoffs +/- 5 % from original cutoffs

	Kernel: rectangular			Parametric: Type of Polynomial			
Bandwidth	.06	.08	.10	Cubic	Quartic	Quintic	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 2-4 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 that allow the trends in income to vary on each side of the discontinuity. In addition we control for parents age, education, citizenship, and marital status at birth of child, and pre-childcare family income, mother's welfare status and mother's student status at age 4 of the child.

Appendix Table 8
Effect of Childcare Subsidy on Ln family income child aged 6-15
By Subgroups
Regression Discontinuity Results
Rectangular Kernel, .08 Bandwidth

Subgroups	Attend child care at age 5	Do not attend child care at age 5	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
Take-up rates of child care	1	0	.55	.55	.48	.59
Ln(annuity family income) child aged 6-15	.264***	.120	.202***	.098	.279***	.095
	(.077)	(.105)	(.076)	(.090)	(.087)	(.069)
N	4079	3300	7426	3210	3859	6780

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.