Income Shocks, School Choice, and Long-Term Outcomes: Lessons from Child Allowances in Israel[*]

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Abstract

I study how a shock to household income during childhood affects educational decisions and long-term educational outcomes. For identification, I leverage a birthday cutoff rule in a reform to child allowances in Israel, which resulted in similar families receiving substantially different amounts of income. To improve statistical efficiency, I exploit variation in the shock's intensity by extending the recentered treatment approach in Borusyak and Hull (2021) to a regression discontinuity setting. For Jewish boys, losing USD 1,000 reduces the probability of matriculating high school by 2.3 percentage points (10%); however, I find limited evidence that the reduced child allowances affected other groups. This heterogeneity is explained by Jewish boys' sorting into lower-quality ultraorthodox schools that are unavailable for the other demographic groups. Transitioning to these ultraorthodox schools could appeal to parents since they provide amenities not found elsewhere, such as longer hours of care, meals, and transport. However, these schools are also among the worst performing in the country. My findings highlight parents' educational decisions as a mechanism driving the long-term consequences of income shocks during childhood.

1   Introduction

Access to resources during childhood is highly predictive of adulthood outcomes (Black and Devereux 2011). This relationship is not only correlational but causal. Recent literature has shown that even mild shocks to childhood circumstances—such as shocks to parental income, nutrition, and health conditions—can have outsized lifelong consequences (Almond et al. 2018). However, our understanding of the mechanisms that drive the long-term effects of income shocks is less complete.

Can parental educational choices explain the long-term effects of income shocks? Educational inputs during childhood, such as school and teacher quality, have been shown to affect children's adulthood outcomes (for example, Chetty et al. 2014 and Chetty et al. 2011). But parents' educational choices regarding these inputs might be constrained or influenced by factors such as demographic makeup or distance, that do not positively impact children's outcomes.[1] Therefore, shocks to parental income might affect educational decisions by changing preferences or the budget constraint. Indeed, the possibility that parents do not make the best decisions for their children is a key rationale for the public provision of education (Gruber 2015 p.309). However, empirical evidence on how sensitive educational choices are to household economic conditions is scant. To the extent that these choices are sensitive, there is even less evidence of how important they are in driving the long-term effects of income shocks during childhood.

I study the interaction between educational decision-making, educational outcomes, and income shocks. My setting is a negative income shock resulting from a cutback to the child-allowance system in Israel in 2003. The choice margin is the religious stream of the school. In Israel, parents of Jewish students can choose whether to send their child to the state-run system or the independently run Jewish ultraorthodox system. Choosing the latter might be attractive to parents facing a negative income shock since many ultraorthodox schools provide amenities and services such as meals, after-school care, and transportation that are not found elsewhere.[2] While attending ultraorthodox schools brings short-term benefits, these children might incur costs in the long run since the quality and quantity of instruction of secular subjects, such as math and English,

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[1]Hastings et al. (2007) estimate parental demand for schools and find that parents highly value proximity. Abdulkadiroğlu et al. (2020) investigate whether parents value school's effectiveness. They find that after controlling for peer quality, preferences are unrelated to school effectiveness.

[2]In addition, many ultraorthodox communities offer their members mutual insurance that includes interest-free loans, care for the sick and elderly, and in-kind loans (Berman 2000). Sending children, especially boys, to the community's school can signal commitment to that community.

is low in these schools. The neglect of secular subjects is more pronounced for boys than girls since boys' religious literacy is deemed more important than girls' and since women are, in many cases, the sole breadwinners in ultraorthodox households.

The income shock in this study comes from a reform to the Israeli child-allowance system in 2003.[3] Before the reform, the system was generous to large families since the benefit increased in child's parity. The reform aimed to make payments independent of the child's birth order. To ease the financial shocks, the new flat schedule only applied to children born after June 1, 2003. This cutoff meant that families that are otherwise similar received substantially different amounts of income from child allowances throughout their eligibility period. The average family in my sample with a child born right before the cutoff received USD 70 (in 2020 dollars) per month more for 10 years than a family with a child born right after the cutoff.

I employ a regression discontinuity design (RDD) that leverages the sharp discontinuity in child-allowance benefits. By using data from administrative files of the near-universe of students, I study how the income shock affected parents' educational choices and children's educational attainment. Educational-attainment outcomes include whether a student was awarded a high school matriculation certificate (a critical prerequisite for most postsecondary schooling and many entry-level positions in Israel), the quality of the matriculation certificate, and progress in the school system.

I find strong heterogeneity in the effect of the negative income shock. A shock of USD 1,000 to child allowances reduces Jewish boys' probability of matriculating high school by a statistically significant 2.3 percentage points (10%), but the effect on Arab boys and girls is small and statistically indistinguishable from zero. While the differences across groups are not statistically significant for matriculation, for other educational outcomes the heterogeneity persists and the effects are significantly larger for Jewish boys than the rest of the population. I show that this heterogeneity is explained by Jewish boys' sorting into lower-quality ultraorthodox schools that have no counterparts for other demographic groups.

In the second part of the analysis, I use an alternative empirical strategy that leverages my knowledge of individual-level exposure to the reform. Exposure to the reform varied across students since differences in child-allowance benefits across the cutoff were only substantial for high-

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[3]To the best of my knowledge, this is the first paper that studies the cutoff rule of the 2003 reform. The schedules in the 2003 reform were studied in Cohen et al. (2013) and Toledano et al. (2011) as part of time-series variations in child allowances in Israel. Both studies investigate the effect of child allowances on fertility and do not rely on the cutoff rule for identification.

parity births (higher than four) and parity was computed relative to siblings under 18 and thus changed over time. Therefore, exposure is a complex formula of demographic background characteristics, such as age spacing across siblings. Importantly, I can compute exposure at the individual level. I leverage my knowledge of individual-level exposure by extending the recentering approach introduced in Borusyak and Hull (2021) to the RDD setting. This approach adjusts for exposure without being contaminated by its nonrandom determinants. Adjusting for exposure is attractive since it leads to a better first-stage prediction that results in more precise estimates. The findings of this analysis yield a very similar pattern to the main findings but with standard errors that are 10% to 15% smaller.

I make several contributions to the literature on investment in children. First, I contribute to the literature on the interaction between resource shocks during childhood and educational decisions. In developing countries, this literature has focused on how school attendance is affected by conditional cash transfers and other shocks to resources (for example, Parker and Todd 2017 and Shah and Steinberg 2017). In developed countries, the literature has mainly concentrated on how parental income affects decisions regarding postsecondary education (for example, Hilger 2016 and Manoli and Turner 2018). I extend this literature by looking at the sensitivity of parental choice to income shocks at earlier stages in school life.[4]

My second contribution relates to the literature on how children are impacted by the exposure to cash and near-cash programs. Going back to the first government-sponsored welfare program in the US, Aizer et al. (2016) find that children who were exposed to this benefit lived on average a year longer and obtained more years of schooling. The introduction of the food stamp program in the US provides another source for studying how changes to household resources impact children. Bailey et al. (2020) find that county-level availability of food stamps during childhood increases adulthood human capital and economic self-sufficiency. Finally, a substantial body of research documents how children's short- and long-term outcomes are affected by exposure during childhood to the earned-income tax credit (EITC) and similar child tax benefits. This research finds that even a modest increase in these benefits can substantially help children. Dahl and Lochner (2012), for instance, find that an additional USD 1,000 in the EITC improves performance on standardized

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[4]Even in places where education is publicly provided, parents make many choices at varying stages of their children's school life and there are growing calls to increase choice in the public system. While, in general, more competition improves schools' quality (see, for example, Campos and Kearns 2022), some evidence suggests that a voucher-based system can introduce lower-quality options (Abdulkadiroğlu et al. 2018). It is unclear, though, why and under which circumstances parents might enroll their children in an inferior-quality institution when a better alternative exists in their choice set.

tests by 0.06 standard deviations. Similarly, Milligan and Stabile (2011) find that child tax benefits in Canada improved a host of short-term educational outcomes, with the effect driven by boys. Bastian and Michelmore (2018) study the long-term effect of EITC exposure and find that an additional USD 1,000 increases early-adulthood income by 2.2%.[5]

I contribute to a small but growing strand of this literature that tries to isolate the effect of additional resources from that of altered incentives. In the EITC, for example, it is unclear whether the positive effects on children are coming from additional income or from the incentive that the EITC gives to parents to increase their labor supply. Understanding the importance of the role of additional income is critical to the design of new cash-transfer programs.[6] Barr et al. (2022) are able to isolate the EITC's income effect by comparing children born right before and right after January 1. Since only the former children were eligible for EITC in their first year of life and since parents of both groups faced the same work incentives, Barr et al.'s analysis identifies the income effect. They find that an additional USD 1,000 in the first year of life increases adulthood earning by 1% to 2% and that most of this effect can be explained by improvement in educational outcomes. As I show in the next section, my design is similar to theirs in that the variation I use identifies only an income effect, as it is not contaminated by differential incentives across the comparison groups.[7]

I also contribute to the literature on investment in children by providing the first estimates of the long-term effects of exposure to child allowances. In addition to not altering work incentives, child allowances are different from child tax benefits in that they are universal and are paid monthly. These properties imply that the findings from the EITC literature do not necessarily apply to child allowances. Even though child allowances exist in 14 developed countries and there are efforts to introduce them in the US,[8] there has been very little research on their impacts on children. One explanation for this scarcity is that the universal nature of child allowances makes it difficult to identify a plausible counterfactual, as all children are exposed to the same benefit. I overcome this

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[5]The EITC as also been found to improve maternal (Evans and Garthwaite 2014) and infant (Hoynes et al. 2015) health outcomes.

[6]A prominent example is Universal Basic Income programs which are gaining popularity among the public and policy makers (Hoynes and Rothstein 2019).

[7]Additional progress in estimating the income effect of saftey-net programs comes from evidence from baby bonuses. Borra et al. (2022) find no effect of the introduction of baby bonuses in Spain while de Gendre et al. (2021) find that baby bonuses in Australia improved health outcomes in the first two years of life.

[8]In the Biden administration's COVID-19 stimulus bill, child allowances were introduced temporarily through the refundable child tax credit. Even though this program was administered by the Internal Revenue Service, it was very similar to child allowances in that it was paid monthly and was unconditional on work. There were efforts to make this program permanent as part of the Build Back Better Act.

challenge by leveraging a reform that introduced two different schedules, where assignment to a schedule was based only on a child's birthday.[9]

This paper also contributes to the emerging literature on the interaction between policy and culture (Ashraf et al. 2020, La Ferrara and Milazzo 2017). Bau (2021), for example, shows that pension expansions reduce patrilocality and matrilocality practices. This paper shows that scaling back the formal safety net can increase reliance on the informal part of a dual education system. The transition to a religious schooling system could impact future religiosity and thus affect cultural norms (such as those regarding age of marriage, fertility, and political views).[10]

Finally, this paper shows how to extend recent innovations in the econometric literature on composite treatment to an RDD setting. Recent literature (for example, Abadie et al. 2019 and Coussens and Spiess 2021) has shown that ignoring first-stage heterogeneity in response to an exogenous instrument yields inefficient estimators. This literature proposes data-driven methods to estimate first-stage group-level heterogeneity. Such estimation is unnecessary in my setting, where heterogeneity in the first stage is a known formula of observed factors. Therefore, to leverage variation in treatment intensity to improve efficiency, I extend the recentered treatment approach introduced by Borusyak and Hull (2021) to an RDD setting. I do this by adopting a local randomization framework for an RDD (Cattaneo et al. 2015). The resulting standard errors are 10% to 15% smaller than those in the main RDD analysis. In other settings, the improvement in precision can be even more substantial. This method also yields natural falsification tests and thus serves as an additional robustness check.

2   Child Allowances and the Israeli Reform in 2003

Child allowances, or child cash benefits, are a type of cash benefit paid to families with children. This type of benefit exists in many developed countries and while the details change across countries, several features are common. First, the benefit is paid on a monthly basis. Second, it is

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[9]There is a limited body of research on how child allowances are affecting consumption and fertility. Kooreman (2000), Blow et al. (2012), and Raschke (2012) study how child allowances are spent in the Netherlands, the United Kingdom, and Germany, respectively. Cohen et al. (2013) and Toledano et al. (2011) study how fertility in Israel is affected by changes to child allowances, and Riphahn and Wiynck (2017) study how child allowances affect fertility in Germany.

[10]While I study the dual education system in Israel, other countries have an informal religious system in addition to the formal one. For example, madrasas in Indonesia (Bazzi et al. 2020) and Pakistan (Andrabi and Das 2005) and the Al-Azhar system in Egypt. In those cases, too, the religious system provides services and amenities that might appeal to families facing financial difficulties.

usually paid to the mother, regardless of her employment status. Third, the benefits are unconditional on work, and even in cases in which they are taxed, they phase out at relatively high levels of income. A study by the British Department for Work and Pensions found that 14 out of 22 developed countries included in the study had a child-allowance type of benefit (Bradshaw and Finch 2002).

These unique characteristics of child allowances make them different from child-related tax credits - a group of programs that have been more extensively studied. Since child allowances are unconditional on work, they do not provide an incentive to increase the labor supply as programs like the EITC do. In addition, child allowances have almost full take-up. Lastly, child allowances are paid monthly and not at the end of the tax year. These distinct characteristics of child allowances make it difficult to generalize from the much more developed literature on child-related tax credits. The unique characteristics of child allowances also make them difficult to study. The universal nature of the benefit implies that obtaining a valid control group that was exposed to a different level of the benefit is challenging.

Since 1997 Israel has had a monthly child allowance program. The Israeli program was unique in that the benefit schedule increased steeply with child birth order (see Appendix Figure A1): from the program's inception in 1997 until 2003, the monthly allowance for the fourth child was four times larger than the monthly allowance for the first child. This unique pattern was a result of legislative pressures from ultraorthodox parties. The higher fertility rates of the ultraorthodox together with their lower incomes made them the main beneficiaries of this benefit schedule.

2.1 The 2003 Reform

In 2003 the Israeli government reformed the child-allowance program and dramatically slashed the benefits. The reform was part of a broader economic plan in response to a financial crisis. This plan was introduced by a new government following an election in early 2003. Importantly, ultraorthodox parties were not a part of the new government, and thus this reform was politically feasible. In addition to reducing public expenditure, an explicit goal of the reform was to encourage parents to rely on their labor income to support their families and to remove incentives to have more children to receive higher nonlabor income.[11]

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[11]The Israeli finance minister in 2003, Benjamin Netanyahu, who spearheaded this reform described the reform's goal in a radio interview: "The change is very dramatic, it means that we as a country cannot afford to spend money without account, even for a desirable, a very desirable goal like families with children. We need someone to support them and the first to support them should be the parents. Parents must take care of their children's well-being"

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The 2003 reform created two groups of children that were entitled to different benefit schedules: children who were born before June 1, 2003 (the cutoff), received a higher benefit than those born after the cutoff. Panel A in Figure 1 shows the two benefit schedules in August 2003. While both groups of children received a lower benefit compared to the 2002 schedule, children born before the cutoff (pre-cutoff Children) received more than those born after the cutoff (post-cutoff children). Importantly, the differential schedule only applied to parities three and higher. I focus my analysis on parities four and higher (high-order births) since for this group the differences were significant.

The distinction between pre- and post-cutoff children was long-lasting. While the original intention of the reform was that the two schedules would gradually converge into a single schedule with a constant benefit per child, subsequent changes in the political map prevented the continuation of the reform after 2006, and the difference in child allowance schedule for high-order births remained. Appendix Figure A2 depicts the difference in child allowances between pre- and post-cutoff children from 2003 through 2015.

From an economic standpoint two elements of the reform are important to highlight:

The 2003 Reform did not generate differentiated fertility incentives - Differences in child allowance amounts between families with high-order birth before and after the cutoff identify a pure income effect since incentives for an additional birth are the same for both groups (all future births are considered post-cutoff children for whom child allowances are independent of birth order).

The 2003 Reform affected the entire household (siblings included) - Even though families of high-order pre- and post-cutoff children received the same child allowances for their older children, total child allowances transfers differ. Panel B in Figure 1 illustrates that the reform was a shock at the household level. Therefore, older siblings too were exposed to the reform, and in my analysis I include older siblings of pre- and post-cutoff children. I call the child in the family who was born around the cutoff the "pivotal child".

(Calcalist 2015)

3 Data

The main data source is administrative records of the Israeli Ministry of Education (MOE). The data consist of the near-universe of children in Israel.[12] The data contain information on students' background characteristics, progress through the K-12 system, and performance on high school matriculation examinations. Importantly, the data include a unique parent identifier, which allows me to identify siblings, and an exact student birthday which allows me to identify the impacted population.

3.1 The Israeli Education System

Schools in Israel are divided into four groups (sometimes called sectors or streams): Jewish secular, Jewish religious, Jewish ultraorthodox (*haredi*), and Arab. As is evident from their names, schools are differentiated along religious and ethnic dimensions with substantial differences in the educational curricula. Parents have latitude in choosing between the different groups, but changing schools within a group is more complicated and depends on residency and capacity.

3.2 Educational Outcomes

This study evaluates how an income shock affects the following educational outcomes.

   High school matriculation certificate (*bagrut*) and certificate quality. My main outcome in this study is an indicator for matriculating high school, which is an important prerequisite for postsecondary education and a requirement for many entry-level positions in the labor market. Despite the marked differences across the different types of schools, the process of matriculating high school is quite standardized: starting in grade 10 students are tested in a series of centrally administered exams, each is associated with credit units. To be granted a matriculation certificate, one has to pass tests worth 20 credits or more.[13] To capture the effect of an income shock on matriculation certificate quality, I also look at following outcomes: (a) the number credit units for exams students passed; (b) indicators for obtaining more than the minimum required credits in

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[12]This is because even non-public schools in Israel receive some public funding which is based on the number of children in the institution and their socio-economic background (Lavy 2019). Thus, my data exclude a small group of children who attend homeschooling or children who attend schools that do not get any public funding.

[13]About 14 units are required; for instance, all students must be examined in at least 3 credits' worth of math and can choose to be tested in up to 5 credits' worth of exams in each subject. There are some differences in the requirements across school types. For instance, students in Arab schools are required to take a test in the Arabic language and the Hebrew language, while Jewish students are required to take a test in the Hebrew language and can choose to take a test in Arabic.

English and math (which I call advanced English and advanced Math). Matriculation outcomes are measured upon graduating from high school.

High school quality. Since school quality can be an important mediator of the effect of an income shock (see, for example, Gould et al. 2003 and Card et al. 2022), I examine whether the reform affected the high school quality a student attends. In this setting, where private schools are absent, changes in school quality can come either from moving between catchment areas or - for the Jewish population - switching between schools with different level of religiosity affiliations.[14] High school quality is defined as the average high school matriculation rate three years before a student enters the school. This variable is measured at the beginning of the 10th grade.

Progression in the K-12 system. I look at two outcomes related to school progression: grade repetition, which I define as ever repeating a grade by the 10th grade and ever attending the 10th grade.

3.3 Defining Exposure to Child Allowances

To get the first-stage effect of the reform on total exposure to child allowance income, I compute for each child their exposure to child allowances. While I do not directly observe the receipt of child allowances, they can be easily imputed since an allowance in any specific month is a known function of the number of children under the age of 18 in the household, their birth order, and their birthdays. This allowance function can, and indeed does, changes overtime.[15]

I define child-allowance exposure as the sum of monthly normalized child allowances payments from birth to the age at which the outcome is measured.[16] I normalize households' monthly payment by the number of children to make my exposure variable consistent with other work on income during childhood.[17] Formally, I define total exposure to child allowances by age a as

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[14]In some municipalities sorting to schools can also come from parental choice and students' educational achievements, but the predominant factors for high school assignment are residential location and religious affiliation.

[15]Two critical variables in my data allow me to impute child allowances: exact birthday and parent identifiers. I assume that children with the same mother belong to the same household. This is of course not a perfect identifier of a household since students that live only with their father (or another adult) and students that live in the same household with other children from a different mother will be misclassified. However, my results are robust to defining a household by father.

[16]I adjust child allowance-payments to USD 2020.

[17]Most of the literature on cash transfers looks at either first birth (for example, Barr et al. 2022) or at settings with lower fertility (for example, Dahl and Lochner 2012). In this sample, the average number of children before the reform is 5.7, and the minimum number of children is 4. Therefore, comparing the dollar amount across studies is not straightforward. In addition, looking at total household child allowances income might not capture only access to more resources but also larger families. By normalizing by the number of siblings in 2003, I am neutralizing the role of family size in determining exposure to child allowances income.

follows:

$$CA_{ia} = \sum_{t=T_{age=0,i}}^{T_{age=a,i}} \frac{1}{children_{j(i),t}} \sum_{k=1}^{children_{j(i),t}} allowances_t(k, Bday_{j(i),k}) \tag{1}$$

Where i is an index for child, and $T_{age,i}$ is an operator that maps child i at age age to a calendar month-year. Thus, the first summation sums over all months in which child i was under the age of a (in most of the analysis $a = 18$ or 15). The variable $children_{j(i),t}$ is the number of children under the age of 18 living in household j in month t; $allowances_t$ is the per-child allowance-benefit schedule in month t, which depends on the child order k and, following the 2003 reform, also depends on the birthdays of all the children in the household ($Bday_{j(i),k}$). Note that if the child allowance formula was flat and constant over time and there was no differentiation by birthday, total child allowances exposure by age a was simply $12 \times a \times B$ (with B being the monthly benefit rate). However, the dependency in child order, the fact that order is with respect to number of children under 18 at a given time, the frequent changes in the formula, and the differentiation by birthday make child allowances exposure a highly nonlinear function in birth cohort, family size, birth order and birth date. The variation I exploit in this paper comes from variation in whether $Bday_{j(i),k}$ is before or after the cutoff date.

3.4 Sample Selection

I define the main analysis sample as all children from families with a high-order birth between January 1, 2003, and October 31, 2003. To avoid concerns about the reform affecting fertility decisions, I choose births that occurred no more than nine months after the public debate on the reform, which began in February 2003. To create a symmetric window around the cutoff date, I choose the start date to be January 1. Therefore, I have a five-month window around the cutoff day.

Importantly, my sample includes not only children born around the cutoff but their older siblings as well. In fact, my main analysis of high school matriculation outcomes includes *only* older siblings. The inclusion of older siblings is critical since pivotal children are not old enough to have matriculated high school during my sample period (2003 - 19). In addition, including siblings provides variation in age of experiencing the reform and also provides more variation in the treatment intensity.

3.5 Summary Statistics

Column 1 in Table 1 shows summary statistics for the main analysis sample which includes only children that turned 18 by 2019 (that is, includes only older siblings). For comparison I provide in Column 2 the weighted mean and standard deviation for the entire population of Israeli students, where weights are derived from the distribution of birth cohorts in the main analysis sample. As is expected, my sample analysis consists of students from disadvantaged backgrounds relative to the general population.

4   Identification Strategy - Regression Discontinuity Design

I identify the effect of an income shock during childhood on long-run educational outcomes by leveraging the cutoff date in the assignment to a child allowance schedule. Under the assumption that the relationship between the outcomes and birth date of the pivotal sibling is smooth, a discrete jump in the outcome at the cutoff identifies the reduced form effect of the program. Formally I estimate the following RDD model to estimate the reduced form effect of the reform:

$$y_{ia} = \beta_0 + \beta 1(day_{j(i)} \geq 0) + f(day_{j(i)}) + X_i\gamma + \varepsilon_i \tag{2}$$

Where y_{ia} is the educational outcome of interest for child i measured at age a and $day_{j(i)}$ is the distance of the pivotal child's birthday in family $j(i)$ from the cutoff day (the running variable). The coefficient of interest is β, which estimates the discontinuity in the outcome at the cutoff. To increase statistical precision I control for background covariates (X_i). Standard errors are clustered at the family level.

I follow the literature on RDD and model $f(day_{j(i)})$ as a local linear function with a differential slope at either side of the cutoff and use a uniform kernel. For ease of exposition I show the main results for a fixed window of 10 weeks on either side of the cutoff for all outcomes and subgroups. I do this since data-driven bandwidths are changing with samples and outcomes. I choose a 10-week window since this is the maximum bandwidth in the set of data-driven bandwidths, which are computed using Calonico et al. (2014).[18] In Appendix Table A1 I show that my results are not sensitive to using data-driven bandwidths. In Appendix Figure A5, I also show that my main result is stable over varying bandwidths.

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[18]In this I follow recent applications of RDD (such as Barr et al. 2022 and Deshpande and Mueller-Smith 2022) in settings where the running variable is date of birth.

To estimate the effect of a shock to family income on educational outcomes, I use the following two-stage model:

$$CA_{ia} = \alpha_0 + \alpha_1 1(day_{j(i)} \geq 0) + g(day_{j(i)}) + X_i \delta + \eta_i \tag{3}$$

$$y_{ia} = \theta CA_{ia} + h(day_{j(i)}) + X_i \psi + v_i \tag{4}$$

Where CA_{ia} is the per-child total allowance exposure by age a defined in equation 1.

The analyses below contain only one observation per child since most outcomes are measured at a certain age. My main set of outcomes concerns matriculation certificates, for which $a = 18$. For these outcomes pivotal children are excluded since they did not turn 18 by the end of my sample period. Another set of outcomes I analyze is markers of school progression, which are measured at the beginning of 10th grade ($a = 15$).

4.1 Evaluating the RDD assumptions

There are two main assumptions behind the causal-effect interpretation of the estimated discontinuity: no manipulation and smoothness of outcome. The first assumption states that there is no selection or manipulation around the cutoff day. In my setting this means that parents with a due date after the cutoff date do not try to induce birth such that they will receive the higher benefit schedule. If higher-SES parents are more aware of the policy and have more means and knowledge to manipulate the birthday, my estimates are likely to be biased upward. If parents with lower SES rely more on child allowances and are willing to go through the extreme measure of inducing labor to get the higher benefit, then my estimates will be likely downward biased.

I offer several pieces of supporting evidence that there is no selection around the cutoff. First, I show in Figure 2 that there is no bunching in the number of high-order births before the cutoff day. In addition, Appendix Figure A3 shows the results from a data-driven manipulation tests following Cattaneo et al. (2018). This approach too, does not reveal a potential sorting around the cutoff. Another way to relax the concern about manipulation is to remove observations right around the cutoff day (doughnut hole). As I show in the next section, my results are robust to removing doughnut hole of varying sizes.

The second assumption is that in the absence of the reform the outcome would be continuous with respect to the running variable. This could be a concern if another policy also has the same

cutoff date. When discussing children and education, a natural candidate for such a confounding policy is school-entry cutoffs. Fortunately, June 1 is far away from the school-entry cutoffs, and as far as I am aware no other policy in Israel that has this specific date as a cutoff.

I provide several falsification tests of the identifying assumptions. First, I test whether there are discontinuities in predetermined demographic variables. This balance test, which estimates equation 2 with predetermined dependent variables and without controls, is presented in Table 2 and Appendix Figure A7. As expected, there are no statistically significant discontinuities and the p-value for the joint null hypothesis is 0.4. The second falsification test is to run my model on placebo cutoff days. In Appendix Figure A4 I construct the empirical cumulative distribution function (CDF) of all RDD estimates of placebo cutoff dates in a three-year range from March 1, 2000 to March 1, 2003. Only 0.5% of the placebo estimates are smaller than the estimated main effect at the true cutoff date. The figure also shows that there are no discontinuities around June 1 in previous years.

5   Results of the Regression-Discontinuity-Design Analysis

I start by estimating the reduced-form effect of the reform on my main outcome - obtaining a high school matriculation certificate. Figure 3 plots the residualized average matriculation (on the vertical axis) against the birth date of the pivotal child (on the horizontal axis). The running variable is recentered at the cutoff date, and bins are grouped by seven days. Linear lines of best fit are fitted on either side of the cutoff date. There is a noted discontinuous jump down in high school matriculation as we move across the cutoff date. The corresponding point estimate from estimating the RDD is -0.024 (SE 0.011). This point estimate represents a drop of 8% relative to the sample mean.

To interpret the effect of the reform, it is helpful to understand the first-stage effect on total child allowance-exposure. Figure 4 shows graphical evidence for the first stage by plotting total child-allowance exposure per child by age 18 ($CA_{ia=18}$) against the running variable. The corresponding point estimate from the RDD model in equation 3 is NIS -5,600 (USD 1,630). This roughly equals the monthly income of households in the bottom earning decile in Israel (CBS 2020). Note that this is exposure *per child*, and thus the reform reduced total *household* child-allowance income exposure by NIS 31,700 (USD 9,200) on average.[19] Given that the reform created on average

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[19] $-5,600 \times 5.66$

a monthly difference of NIS 250 (USD 72) between pre- and post-cutoff families, the household exposure shock implies that families were treated on average for about 10.5 years. Thus, my finding can also be interpreted as the effect of exposure to a negative shock to monthly income of NIS 250 (USD 72) for 10.5 years. This interpretation ignores the fact that exposure period and treatment size varied across students. In the next section I return to issues surrounding heterogeneous exposure to the reform.

The final step of the main analysis is to estimate the effect of a child-allowance income shock on high school matriculation. This is done with a 2SLS model in equation 4. The resulting point estimate of a shock of USD 1,000 in child-allowance exposure is -0.014 (SE = 0.0068). This estimate is strikingly close to a recent and similar study by Barr et al. (2022), who look at the effect of an additional EITC benefit as a result of being born just before January 1 (and therefore qualifying as a dependent in the first year of life). Their analysis finds that an additional USD 1,000 improves high school graduation rates, in a sample of North Carolina students, by 0.012 (SE = 0.0062) percentage points. While there are clear differences between the two settings, both this study and Barr et al. (2022) identify the effect of a pure cash transfer (that is, the incentive channel is shut down). One important difference that is worth highlighting is that this study looks at an exposure to a shock that accumulated over time, while Barr et al. (2022) look at a one-time shock that is experienced during the first year of life.

Table 3 summarizes the findings from the main analysis. Column 1 presents a version of the estimates without controls, and Column 2 presents estimates of the model that include background controls. The inclusion of controls improves precision significantly but also strengthens the effect of the reform. While both point estimates are not statistically different, the direction of the point estimates' movement when controls are added suggests that if there is selection, then it is in the direction that yields underestimates of the true effect. This is also suggested by the doughnut-hole analysis in Appendix Figure A6, where removing days just around the cutoff - when birthday manipulation is more likely - increases the absolute sign of the effect.

To better understand the mechanisms that drive the observed effect, I show in Table 4 the heterogeneity in the effect by sex and ethnicity. The reduced form estimated effect for Arabs is indistinguishable from zero, but for Jews is large and significant. The heterogeneity in the reduced-form effect could not be explained by heterogeneity in the first stage, as both groups seem to be equally affected by the reform. Among boys we observe larger point estimates than girls. Finally, the effect is stronger for Jewish boys than for non-Jewish groups. While the differences across

groups are not statistically different from each other, the point estimates might be suggestive of an underlying mechanism. An investigation of heterogeneity in other educational outcomes can help determine whether this is only statistical noise or a consistent pattern.

This heterogeneous pattern is persistent across outcomes. The first three columns in Table 5 show the reduced-form effect of the reform on markers of diploma quality: number of credits gained in high school matriculation exams and indicators for passing tests at an advanced proficiency level in math and English exams. For all three outcomes the effect is indistinguishable from zero for Arabs, stronger for boys than girls, and stronger for Jewish boys than non Jewish boys. The last three columns in Table 5 show the effect on school-progress outcomes measured by the beginning of grade 10 (age 15): high school quality, ever attending the 10th grade, and ever repeating a grade by 10th grade. Table 6 similarly show the 2SLS-RDD estimates for the same populations and outcomes. The results again show that Jewish boys are adversely affected by the reform, while there is no distinguishable effect for the other groups.

Another potential dimension of heterogeneity is age. There is a debate in the literature about whether additional resources and interventions are more productive in early childhood than in later childhood and adolescence (Heckman 2006 and Hendren and Sprung-Keyser 2020). By including older siblings who were exposed to the 2003 reform at different ages, my design allows analysis of how a negative shock's effect changes with age. Analyzing the reduced form effect of the reform by age might conflate differential exposure to the reform with differential treatment effect by age. Therefore, in Figure 5 I show the 2SLS estimate of a USD 1,000 shock to child-allowance income by age in 2003. Unfortunately, the estimates here are consistent with a decreasing, constant, and increasing returns with age.

5.1 Interpreting The Results

What can explain the high level of heterogeneity in the reform's effect? Why would Jewish boys be adversely impacted by the reform while Arab students are not impacted strongly? Heterogeneity in the literature is usually explained by differences in the first stage (that is, different groups had different exposure to a reform) or by differences in the income levels across groups, which make the effect of an additional dollar more critical for more disadvantaged groups.[20] In this setting, there

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[20]Barr et al. (2022), for example, find that additional EITC exposure had a weaker impact on Hispanic students. They explain that finding by the lower take-up rate of the EITC among the Hispanic population. Another example is Bastian and Michelmore (2018) who find that exposure to EITC had a stronger effect for more disadvantaged students for whom additional resources could have a greater impact.

are no first-stage differences across groups. While I do not observe income, parental schooling in my sample is higher for Jews, suggesting that income levels for the Arab population are lower; so we expect a negative income shock to be more disruptive for Arab students. This is the opposite of what we observe.

Therefore, I hypothesize that the institutional differences in the Israeli education system between the Arab and Jewish sectors are responsible for the apparent pattern. Specifically, I claim that the reform induced Jewish parents to enroll their children in the ultraorthodox system. And thus, the long-run effect we observe on educational outcomes could be explained by the fact that ultraorthodox schools have almost-zero matriculation rates for boys. This is because schools in the ultraorthodox sector are single sex and schools for boys especially focus on religious instruction at the expense of core subjects (Kingsbury 2020). In addition, despite the ultraorthodox schools' receipt of government funding, central supervision on of their educational curricula is lax (Barak-Corren and Perry-Hazan 2022).

Why would parents send their children to ultraorthodox schools in the face of a negative income shock? There are two main reasons why this might be the case. First, some ultraorthodox schools provide additional amenities and services such as subsidized after-school care, meals, and transportation (both of which are uncommon in Israeli public schools).[21] In fact, some educational scholars in Israel were concerned that these amenities would lure families into the ultraorthodox system (Schiffer 1999). However, in the aggregate data, this concern never materialized (Blass and Bleikh 2016). Second, some ultraorthodox communities provide a community-level informal safety-net that provides members with donations or low-interest loans. Belonging to these communities requires signaling commitment to the community's values; sending children to the community's school can strengthen the connection to the community (Berman 2000).

To test this hypothesis, I use the RDD model to estimate the reform's effect on enrolling in the ultraorthodox system. Columns 1 and 2 in Table 8 show the reduced-form effect of enrolling in an ultraorthodox middle or high school, respectively, for all Jewish students and boys and girls separately. While the point estimates are positive, they are not significant. In Column 3, I look at the probability of attending a high school with a zero matriculation rate. Ninety-seven percent of

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[21]For subsidized after-school care, see an example of after-school rates in Jerusalem, which are determined by the student's institution's religious affiliation: https://tinyurl.com/bd2mx698.

A piece of anecdotal evidence on transportation is a directive by the Israeli attorney general to stop government funding to a network of ultraorthodox schools because of concerns that these funds are used for transporting non-ultraorthdox students to the network schools that encourage religious observance (https://web.archive.org/web/20160314210821/https://news.walla.co.il/item/880602 [in Hebrew]).

schools included in this definition are ultraorthodox, but only eighty percent of ultraorthodox high schools are included in this definition. In other words, Column 3 purges ultra-orthodox schools that are present in Column 2 but have some students taking matriculation exams. This variable captures the lowest-quality ultraorthodox schools. The reform's effect on this variable is significant and is driven by boys.

Another test for this hypothesis is to search for heterogeneity by pre-reform family size. If the reform's negative shock induced parents to enroll their children in ultraorthodox institutions, we would expect this effect to be driven by Jews who would not have done so in the absence of the reform. In other words, we would expect the effect to be concentrated among non-ultraorthodox Jews. Unfortunately, I do not have good pre-reform indicators for religiousness. I try to overcome this by estimating the effect by the number of siblings in 2003 (that is, the birth order of the pivotal child). Since ultraorthodoxy is correlated with large family, we would expect the effect to be driven by smaller families. This is only a suggestive exercise because family size in 2003 can be correlated with other important dimensions of heterogeneity, most notably parental age. Table 7 shows the 2SLS estimate separately for families with a parity-four birth and families with parity-five and higher pivotal birth. Indeed, the effect is concentrated in the smaller families. Interestingly, in the Arab population the point estimates are also larger in the smaller families, although both estimates are statistically not different from zero.

6   An Alternative Empirical Strategy: Using Variation in Exposure to the Reform

The empirical analysis so far has ignored the fact that different students had differential exposure to the reform. In this section, I leverage variation in treatment intensity to identify the causal effect of the reform. The purpose of this section is twofold. First, to provide a robustness test for the main results in Section 5, and second, to improve statistical precision. Using treatment intensity as a robustness test or leveraging it to gain precision is not uncommon in the literature. However, since treatment intensity is continuous and depends nonlinearly on demographic characteristics, standard approaches, such as estimating the model on high- and low-exposure samples, do not readily apply.[22]

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[22]The idea of leveraging variation in the first stage to improve precision is not new. In studies that look at natural experiments that arise from policy changes, for example, some demographic groups are often excluded from the analysis if the policy had little effect on them. For instance, in the literature that identifies the return to schooling

In this section, I leverage variation in treatment intensity by implementing the recentering approach introduced in Borusyak and Hull (2021, henceforth BH). I start by illustrating the heterogeneity in first-stage exposure in my data. Since this is the first implementation of BH in an RDD setting, I then explain how I extend BH to this setting by adopting the local-randomization approach to RDD (Cattaneo et al. 2015). Next, I show supporting evidence for the identifying assumptions. Finally, I conclude this section with the results of the intensity-based analysis and a comparison to the main RDD findings.

6.1 Nonrandom Exposure to the Child-Allowance Reform

To illustrate the variation in treatment exposure, it will be helpful to start with an example. Consider a set of two families, each with four children with the exact same distribution of birthdays except that the pivotal child in family A was born before the cutoff while the pivotal child in family B was born after the cutoff. Following the 2003 reform, family A will receive additional monthly child allowances. But for how long? That will depend on the age of the firstborn since the child's order is computed with respect to the number of children under 18 in the household and since the reform only generated differences for parities four and higher. Another critical source of exposure variation is the order of birth of the pivotal child. This is because the additional child allowance for the fifth child (for pre- versus post-cutoff children) is higher than that for the fourth child.[23] Another complication is the frequent variations in the formula schedule (displayed in Appendix Figure A2). These time-series changes imply that exposure changes nonlinearly with spacing.[24]

The histogram in Figure 6 shows that all these factors amount to a substantial variation in exposure to the reform. The figure shows the distribution in the treatment size for the control group (pre-cutoff families). I define treatment size by the difference between the realized $CA_{ia=18}$ and a counterfactual child allowance in which the pivotal child in family $j(i)$ would have been born on the

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from compulsory schooling legislation, Black students are often excluded from the analysis since they are weakly affected by changes in compulsory-schooling laws (see for example Lleras-Muney 2005). Recent work (for example, Abadie et al. 2019, Coussens and Spiess 2021, Borusyak and Hull 2021) has studied the statistical consequence of such first-stage-based sample selection and developed methods to optimally leverage variation in the first stage.

[23]Note that the role of birth spacing and order of pivotal child interact: when the pivotal child order is four, the critical spacing for exposure is the interval between the firstborn and the fourthborn. When the order of pivotal child is five, then the critical interval is that between the secondborn and fifthborn.

[24]To see that, consider again the set of families with a fourth-order pivotal child. If the firstborns are 17, then the treated family will receive an additional NIS 380 per month for a year. If the firstborns are 16, then the treated family will receive an additional NIS 380 in the first year and an additional NIS 290 per month in the second year until the firstborns turn 18. If the firstborns are 15, however, then the treated family will receive additional NIS 380 per month, and NIS 290 per month for the first and second years, respectively, but for the third year they will receive only an additional NIS 210 per month.

other side of the cutoff. The mean of this variable is NIS 5,400 (USD 1,600), which is reassuringly close to the estimated first-stage effect of the reform in the main RDD analysis. However, there is considerable variation in treatment size: students in the third quartile, for example, received a treatment 1.5 times larger than students in the first quartile.

6.2 Identification

After establishing that there is variation in the reform exposure, I am now interested in how to exploit this variation to identify the causal effect of an income shock. In a simplified version of differential exposure in which only one group is affected by the reform, I would simply carry out my analysis separately on each group and expect the effect to be driven only by the affected group. However, exposure in my setting is determined by continuous variables in a highly nonlinear fashion. In this section, I use the recentering approach introduced in BH to leverage the differential exposure to the reform. The idea behind BH is to combine variation in the exogenous shock (which in this case is whether the birth of the pivotal child is before or after the cutoff) with variation in endogenous exposure to the shock (which is a result of the the child allowances rules in Israel over the years) to better predict the endogenous variable (income from child allowances shock) while only being identified from the exogenous shock.

Formally, we are interested in the following structural relationship:

$$y_{ia} = \alpha + \theta^{BH} CA_{ia} + \varepsilon_{ia} \tag{5}$$

We can write CA_{ia} in equation 5 as a function of endogenous and exogenous components:

$$CA_{ia} = f(X_{ia}, 1(Bday_{j(i),k=p(j)} \geq 0)) = f(X_{ia}, g_i)$$

Where f is a known function that is derived from child-allowance rules over the years; X_{ia} is a set of demographic variables such as birth cohort, birth order, and family birth spacing that determine exposure to child allowances; and $Bday_{j(i),k=p(j)}$ is the birthday of the pivotal child in family $j(i)$ centered around the cutoff date. For ease of exposition, I define $g_i = 1(Bday_{j(i),k=p(j)} \geq 0)$.

In general, OLS estimation of equation 5 suffers from omitted variable bias since child allowance exposure might be correlated with predictors of educational outcome (that is $\mathbb{E}(CA_{ia}\varepsilon_{ia}) \neq 0$). BH show that if g_i is exogenous, in the sense that it is as good as random with a known distri-

bution and satisfies the exclusion restriction,[25] the following holds:

$$\mathbb{E}((CA_{ia} - \mu_{ia})\varepsilon_{ia}) = 0 \tag{6}$$

Where $\mu_{ia} = \mathbb{E}(CA_{ia}|X_i)$ is the expected child allowances conditional on X_i, and the expectation is over realizations of the shock. We call this variable "expected treatment." Thus, equation 6 implies that instrumenting CA_{ia} in equation 5 with recentered child allowances, defined as $\tilde{CA}_{ia} = CA_{ia} - \mu_{ia}$, will purge omitted variable bias.

6.2.1 Implementing the BH Approach

Implementing the BH approach requires three steps: identifying an exogenous shock, determining a data generating process (DGP) for the shock, and calculating $\tilde{CA}_{ia}$. I ground the assumption that g_i is an exogenous shock in the local randomization approach to RDD (Cattaneo et al. 2015). The local randomization approach builds on the idea that "near the cutoff, the RD design can be interpreted as a randomized experiment" (Cattaneo and Titiunik 2022). Specifically, it requires the existence of a window in which the assignment mechanism of the running variable is (a) as-good-as random in the sense that the distribution of the running variable is the same for all units (b) the running variable is excluded from affecting the outcome equation (Assumption 1 in Cattaneo et al. 2015). Note that if the running variable satisfies these conditions, g_i, which is a transformation of the running variable also satisfies them. Therefore, under the local randomization approach, g_i satisfies the exogeneity notion in BH (Assumption 1 in BH).

We are now left with the question: what is the window around the cutoff in which birthday can be viewed as exogenous? The main approach in the literature is to take a window in which pre-determined variables are balanced. Another approach is to use knowledge about the relationship between the running variable and the outcome in the years prior to the reform and examine the seasonal gradient. Since the local randomization approach essentially imposes a flat relationship between the outcome and the running variable (see an illustration in Figure 1 in Cattaneo and Titiunik 2022), in Appendix Figures A8 to A9 I probe into the birth seasonality in Israel in search of the size of a window in which this relationship is flat. The evidence across outcomes and samples suggests that seasonality is not very volatile, and that June 1 is in a relatively flat region of the seasonal variation. Therefore I take a three weeks window around the cutoff. Balance tests,

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[25]Exclusion in this case implies that g_i affects the outcome of interest only thorough its effect on on CA_{ia}. As I discuss in the next subsection, this assumption is plausible in a small window around the cutoff.

which are described in the next section, also support the choice of a three-week window.

Next, to compute the expected child allowances, μ_{ia}, I need to postulate the shock's DGP. For the main estimate I assume that there is a 50% chance of having the pivotal birth before the cutoff and 50% chance of having it after the cutoff.[26] Formally, μ_i is computed as follows:

$$\mu_{ia} = \frac{1}{2}(f(X_{ia}, g_i = 0)) + \frac{1}{2}(f(X_{ia}, g_i = 1)) \tag{7}$$

And recentered child allowances are simply $C\tilde{A}_{ia} = CA_{ia} - \mu_{ia}$. In what follows, I estimate equation 5 with a 2SLS model in which I instrument CA_{ia} with recentered $C\tilde{A}_{ia}$.

6.2.2 Explaining Changes in Precision

Since one of the purposes of using this alternative method is leveraging variation in treatment intensity to get more precise estimates, it is worth discussing the sources for differences in precision between this approach and the main RDD approach. First, recentering allows for a more powerful first stage. To see that, consider a group of never-takers for which a pivotal birth on either side of the cutoff will result in very little change in CA_{ia}.[27] However, in the first-stage RDD equation their predicted CA_{ia} will be larger by USD 1,600 (the coefficient in the first stage) if the pivotal birth is pre-cutoff. In the BH framework, on the other hand, there will be no differences between predicted CA_{ia} for pre- and post-cutoff pivotal births. This is because either realization of CA_{ia} will fall close to μ_{ia}, and thus the recentered Child Allowance will be zero for this group of never-takers regardless of the realization of the shock. In other words, we can think of BH here as removing never-takers from the sample.[28] Similar precision gains might be achievable by interacting the instrument in the RDD approach with background controls. However, weak-instrument issues might arise in the presence of many instruments and a nonlinear relationship. Implementing BH by adopting the local randomization approach can also potentially make precision worse. This is because the sample in this approach is smaller than the one in the main RDD analysis. The smaller sample size results from the fact that the exogeneity assumption holds only in a small window

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[26]While the main source of uncertainty is coming from uncertainty in birthday, another source of uncertainty is in the cutoff day itself. In fact, in an early version on the legislation for the 2003 Reform the cutoff day was May 1st. Because the legislation process was not completed in time, the cutoff date was push back to June 1st.

[27]In my setting this can happen if the firstborn in a family is almost 18 when his younger fourth-parity sibling is born. In this case the pivotal child will spend very little time as a high parity child.

[28]For an example of how removing non-compilers reduces the variance of the 2SLS estimator see Coussens and Spiess (2021).

around the cutoff.

6.3 Validating the Model Assumptions

The main assumptions in this framework are that the shock is indeed exogenous and that the DGP process is not misspecified. While these assumptions can not be tested directly, there are tests that will provide some confidence that the recentering approach removes the bias from nonrandom shock exposure.

Panel A in Figure 7 plots the joint distribution of child allowances and family size in 2003. This figure shows a clear correlation between the two variables. The positive correlation between child-allowance exposure and family size is unsurprising given the Israeli child-allowance schedule that favors larger families. Panel B in Figure 7, on the other hand, plots the joint distribution of recentered child allowances against family size in 2003. This figure shows clearly that the correlation between these two variables is zero.

Another helpful exercise is to plot recentered child allowances versus μ_{ia}. This is plotted in Figure 8. The symmetric pattern across the x-axis tells us that conditional on μ_{ia}, exposure to treatment is balanced. If the DGP was misspecified, we would expect the observations to not necessarily be symmetric around the x-axis.

The last test is to check more formally that the correlation between child allowances and observables is removed after recentering around μ_{ia}. Column 1 in Table 9 shows an OLS regression where the dependent variable is child allowances and the control variables are listed in the rows. This shows us that background covariates are strong predictors of child allowances exposure. Column 2 in Table 9, shows the same regression except that the dependent variable is recentered child allowances. After recentering, the association between child allowances and background controls disappears and I cannot reject the null hypothesis that the regression coefficients are jointly zero.

6.4 Results

I first present results of estimating equation 5 with high school matriculation as the dependent variable. Column 1 in Table 10 shows the coefficient on child-allowance exposure in the entire sample without controlling for any confounding factors. The point estimate is negative, implying that additional child allowances are associated with lower high school matriculation rates. This is unsurprising since the unadjusted child allowances are correlated with background variables that are negatively correlated with educational attainment (such as family size). Next, Column 2

22

shows results from estimating the same specification in a three-week window around the cutoff. The result remains the same within a short window around the cutoff. Column 3 adds control variables to the specification in Column 2. Here, the point estimate sign flips and is now positive, implying that controlling for background characteristics removes some of the correlation between child allowances and confounding factors.

The findings from the recentering framework adopted in this section are presented in Columns 4 and 5 in Table 10. Column 4 shows the coefficient from regressing child allowances on high school matriculation, where child allowances are instruments with $\tilde{CA}_{ia}$. The estimate from this regression is now positive and significant and has the same order of magnitude as the 2SLS-RD estimate in Section 5. In the remainder of this section I present results from estimating this specification. Finally, Column 5 shows that instrumenting child allowances without additional controls yields a similar result.

Table 11 shows results from the recentering framework for various educational outcomes and subgroups. The pattern that emerges here is almost the same as in the 2SLS-RD estimation: effects indistinguishable from zero for Arabs and girls and a significant and strong effect for Jewish boys across outcomes. Table 12 shows the effect on attending an ultraorthodox school and Figure 9 shows age heterogeneity of the effect of child allowance on high school matriculation. The results from both of these analyses are in line with the results from the main RDD analysis: a significant effect on sorting to low-preforming ultraorthodox high schools that is driven by boys; and a heterogeneity in age that cannot be ruled out to be constant. While in theory this exercise could have yielded large gains in precision, in practice the gains that were achieved are mild (on the order of 10%-15%).

To conclude, in this section, I used variation in treatment intensity to identify the effect of Child Allowance income. The first goal of this section - a robustness test of the main estimates - was achieved. However, improvements in statistical precision - the second goal of this section - were only mild. These mild improvements may be a result of the fact that the sample size is one-third of that in the RDD analysis. In other settings, implementing the BH approach in an RDD setting may result in more considerable improvements to statistical precision.

7 Conclusions

In this study, I showed that faced with a negative income shock, families send their children to lower-quality religious schools that provide amenities and services that can mitigate the effect of the lost income. I showed that this decision is the primary mechanism for the income shock's long-term impact on children. The causal identification of an income shock came from leveraging a unique cutoff rule in an Israeli reform that cut back on child allowances.

There are several caveats to these results. First, I study a population that heavily relied on income from child allowances before the reform. It is unclear whether the result would hold for different types of income shocks. Second, I only look at educational outcomes. The reform may have affected other margins as well. Indeed, in a follow-up study, Gershoni et al. (2022) find, using the same identification strategy, that the reform reduced total fertility for Arab mothers but had no effect on Jewish mothers. This result highlights the importance of heterogeneity in preferences and alternatives when assessing the impact of resource shocks.

From a policy perspective, the findings in this paper show that the 2003 reform had at least two unintended consequences. First, the reform's main goal was to cut back public expenditure. The finding that children were less likely to matriculate high school suggests that their future earnings were impacted as well and, as a result, government income from taxes was also affected. A back-of-the-envelope calculation puts the marginal value of public funds (MVPF) of this program at 1.07.[29] This MVPF is relatively low; Hendren and Sprung-Keyser (2020) find that many intervention during childhood have MVPFs that exceed 2 and in many cases reach infinity (that is they pay for themselves). This low value might be even lower if the reform had effects on labor supply and fertility in the direction that policy makers intended. Therefore, the reform's direct effects on individuals' educational attainment, taken by themselves, do not justify reversing this policy.

Beyond the direct effect on children's lifetime earnings, there is the question of children's long-run values and behaviors. It is unclear whether sending children to ultraorthodox schools has

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[29]The calculation of the MVPF is as follows:

costs - I take the average annual treatment which is 840 USD. The direct cost to the government is the discounted sum of annual benefits for 10 years which is the average treatment period. The government recoups some of its losses from future tax revenue. I use recent estimates on returns to matriculation (Lavy et al. 2022) which imply that every one percentage point increase yields additional 75 USD in income. I assume this return to be constant over student's working lifetime which I assume start at $t = 16$ and end at $t = 59$. I use a 3% discount rate and a 20% tax rate.

Benefits - since this is a direct cash transfer and since incentives are unaffected across control and treatment group benefit to beneficiaries equals nominal costs.

caused parents or their children to adopt behaviors that are in accordance with Jewish ultraorthodox values. If it has, that implies the unintended consequences have been broader than otherwise. While the policy intended to change fertility and labor decisions - which were presumably distorted because of incentives - it caused a group on the religiosity margins to adopt more religious values, which possibly include the undesired labor and fertility patterns. I note that this potential effect on behaviors and values would invalidate the cost-benefit analysis in the previous paragraph since we would have to account for the adverse impact on labor income and fertility and the inter-generational consequences of this policy.

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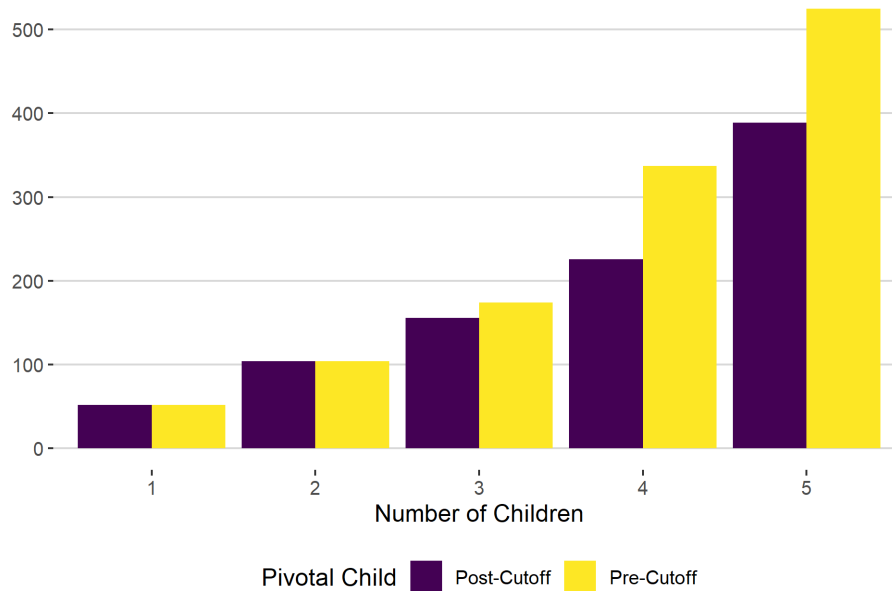Toledano, E., R. Frish, N. Zussman, and D. Gottlieb (2011). The effect of child allowances on fertility.

Figures

Figure 1: Child Allowances on August 2003

(A) Child Allowances Per Child by Child Order and Reform Status (2020 USD)
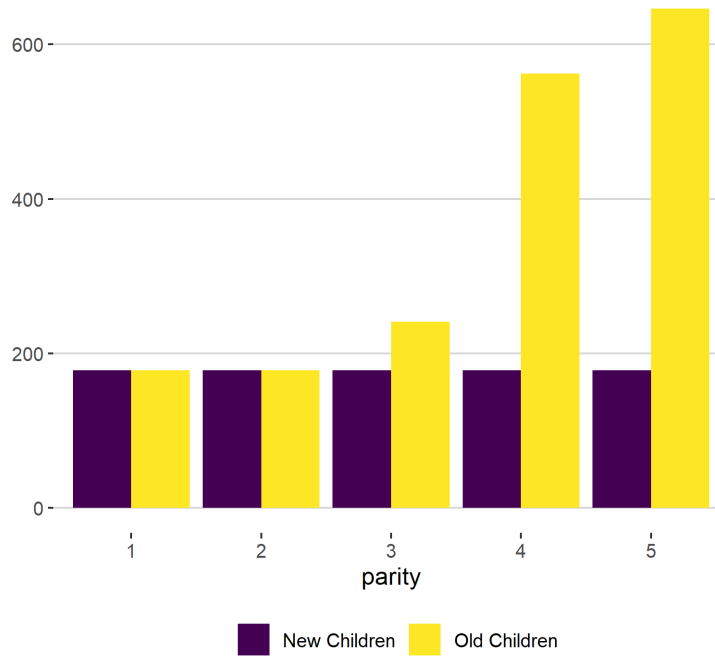
*(B) Total Family Child Allowances Income by Family Size and Birthday of the Last Child (2020 USD)*

Note: Panel A shows the monthly per-child child allowances payment on August 2003 by child parity separately for pre- and post-cutoff children. Panel B shows the overall family income from child allowances by family size separately for families with last birth right after the cutoff and right before the cutoff.
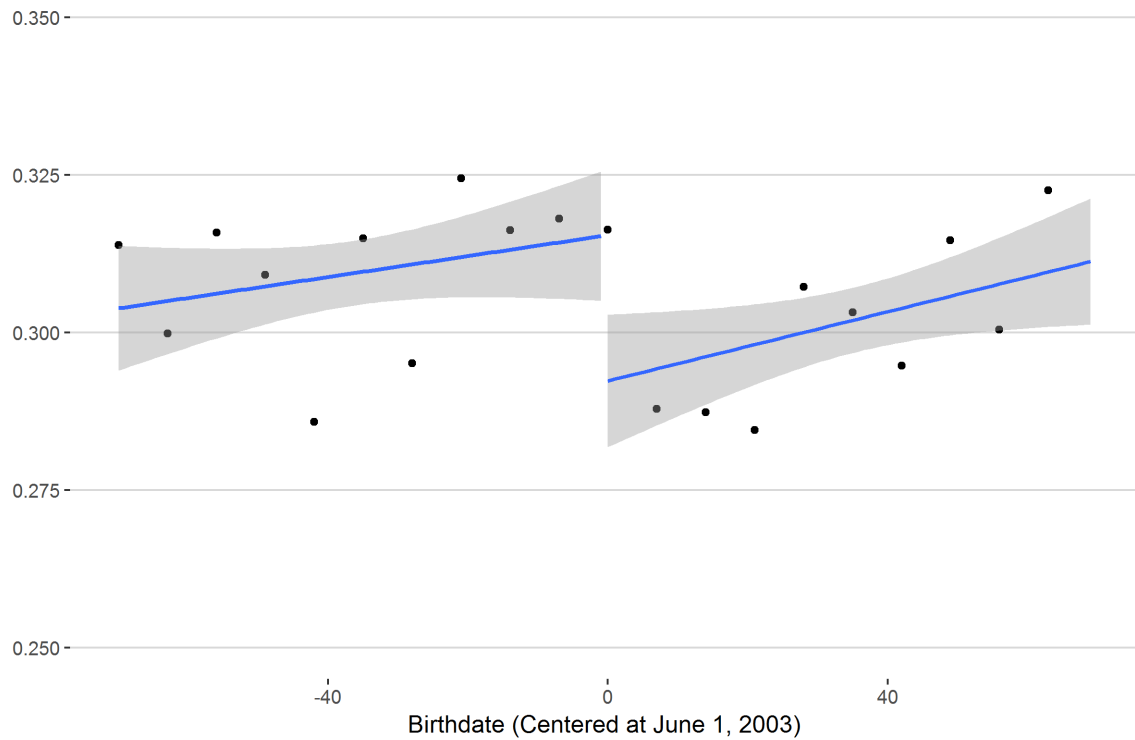
Figure 2: Distribution of High-Order Births Around June 1, 2003
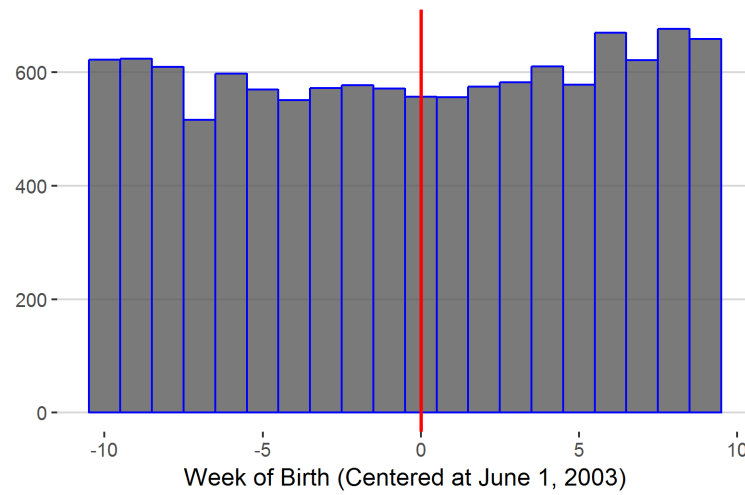
Notes: The figure plots the distribution of high-order births (parity four and up) in a 10 week window around the cutoff day. Each bar represents a week.

Figure 3: The Effect of the 2003 Reform on High School matriculation

Notes: The figure plots high school matriculation rate, on the vertical axis, against the birth date of the pivotal child, on the horizontal axis, centered at June 1, 2003. Each point represents the matriculation rate over a seven-days bin. Linear lines of the underlying data are fitted on either side of the cutoff date. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. The shaded area shows the 95% confidence interval.

Figure 4: Effect of the 2003 Reform on Child Allowances Exposure by Age 18 (2020 USD)
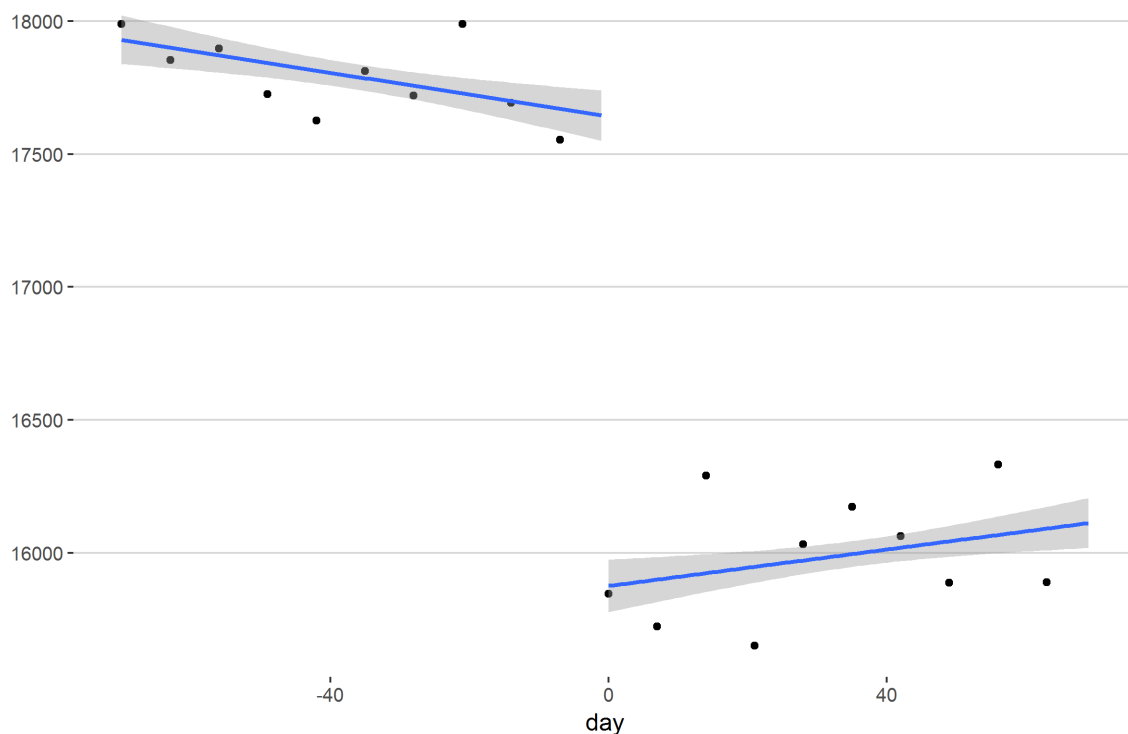
Notes: The figure plots total exposure to child allowances by the age of 18, on the vertical axis, against the birth date of the pivotal child, on the horizontal axis, centered at June 1, 2003. Each point represents an average over a seven-days bin. Linear lines of the underlying data are fitted on either side of the cutoff date. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. The shaded area shows the 95% confidence interval.

Figure 5: The Effect of USD 1,000 in Child Allowances on High School Matriculation, by age in 2003 (2SLS Estimates)
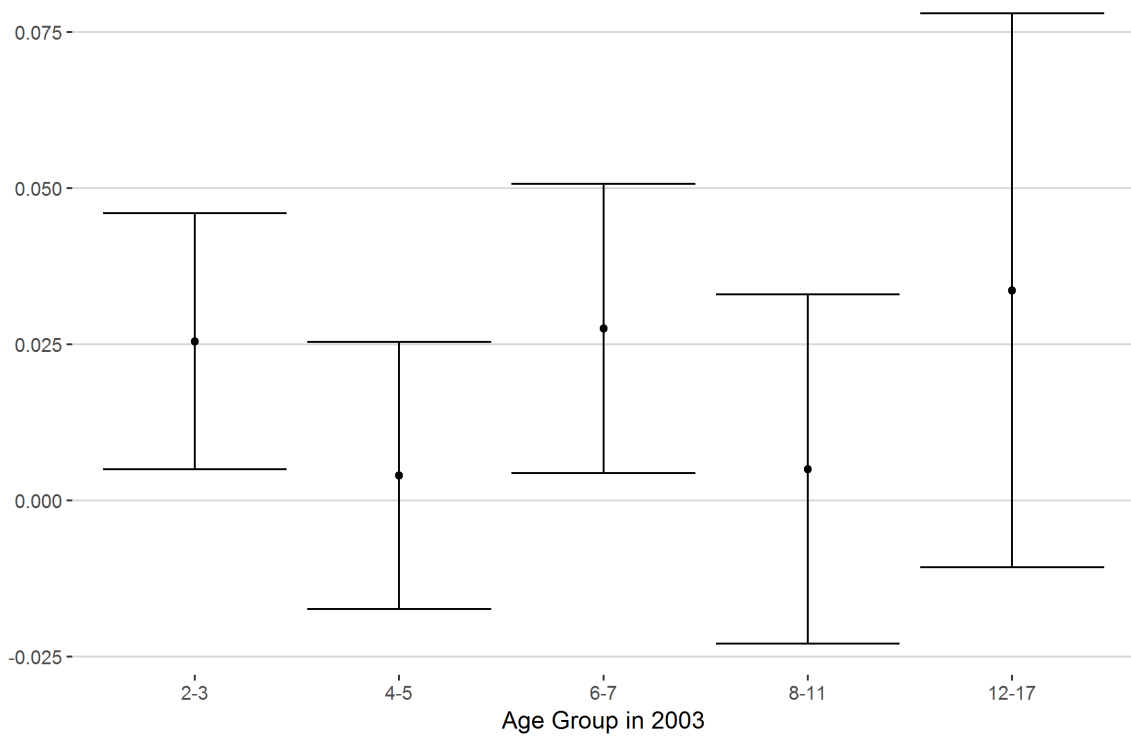
Notes: the figure plots results from estimating a version of the 2SLS model separately for each age group. The dependent variable is an indicator for a high school matriculation certificate. The sample contains only children who have turned 18 by the end of 2019. The sample includes only students from families with a high-order birth in a 10 weeks window around the cutoff day. Bars indicate 95% confidence intervals. Standard errors clustered at the family level.

Figure 6: Histogram Of Treatment Size
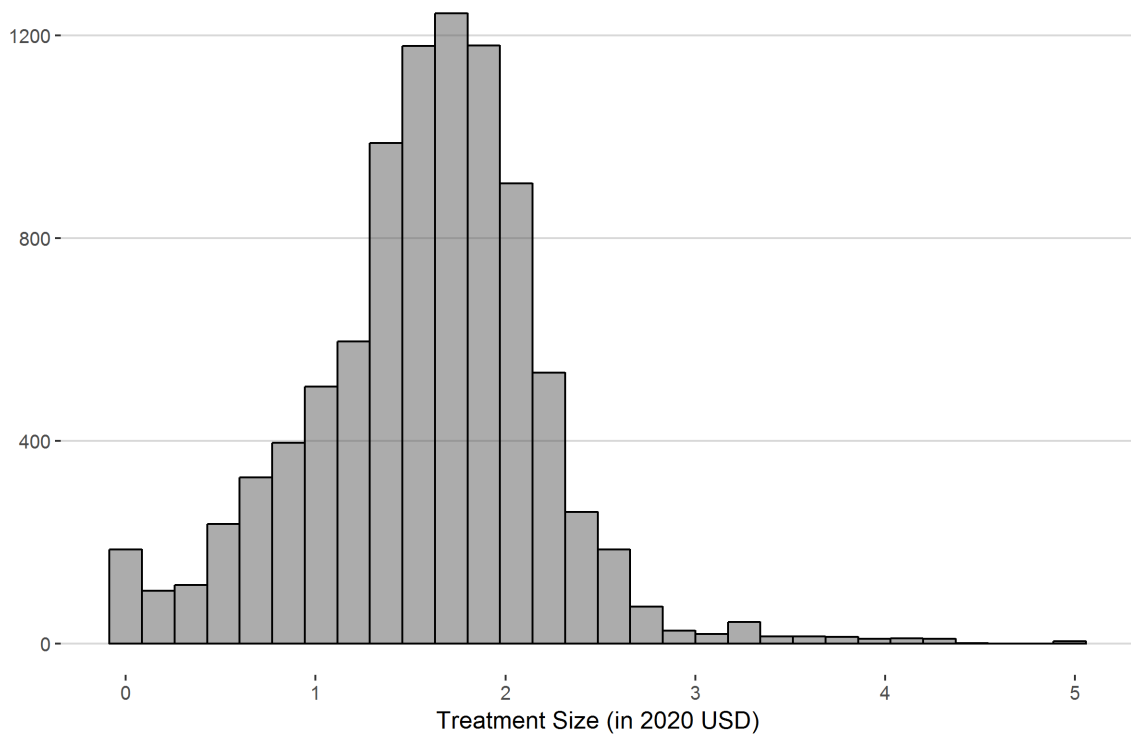
Notes: The figure shows the distribution of treatment size, as defined in the text, in the sample of students with a high order-birth in a three weeks window around the cutoff date.

Figure 7: Child Allowances and Family Size

(A) Child Allowances vs. Family Size in 2003
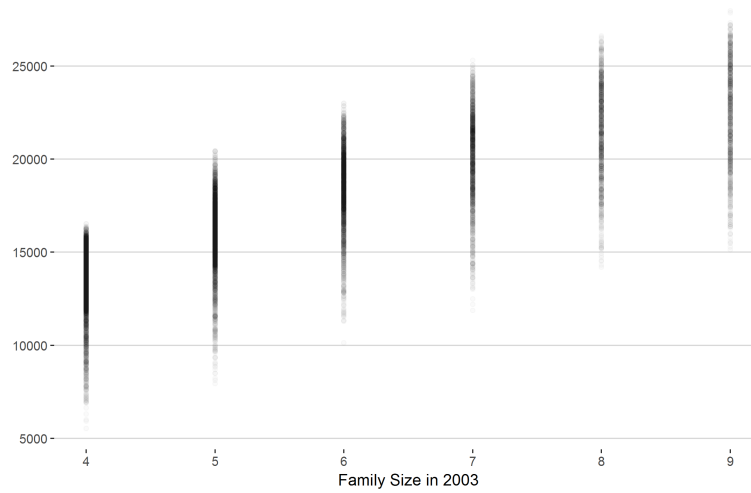
(B) Recentered Child Allowances vs. Family Size in 2003
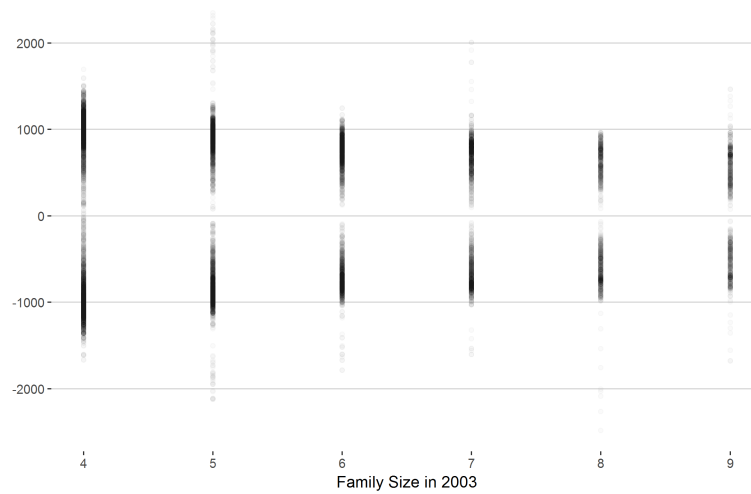
Notes: Panel A shows the distribution of family size in 2003 against $CA_{ia=18}$ (defined in equation 1). Panel B shows the distribution of family size in 2003 against recentered $CA_{ia=18}$. The sample is restricted to students from families with a high-order birth within a three weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019.

Figure 8: Recentered Child Allowances vs. Expected Child Allowances
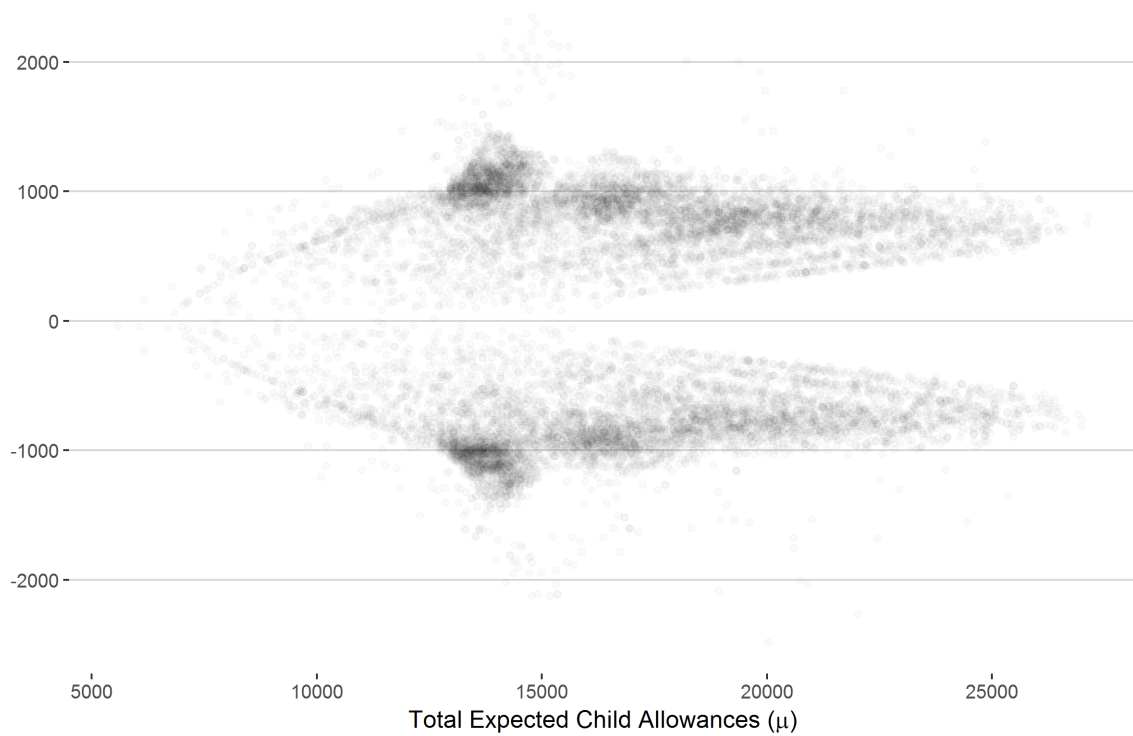
Notes: The figure shows the distribution of $\mu_{ia=18}$ (defined in equation 7) against recentered $CA_{ia=18}$. The sample is restricted to students from families with a high-order birth in a three weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019.

Figure 9: The Effect of USD 1,000 in Child Allowances on High School Matriculation, by age in 2003 (BH Estimates)
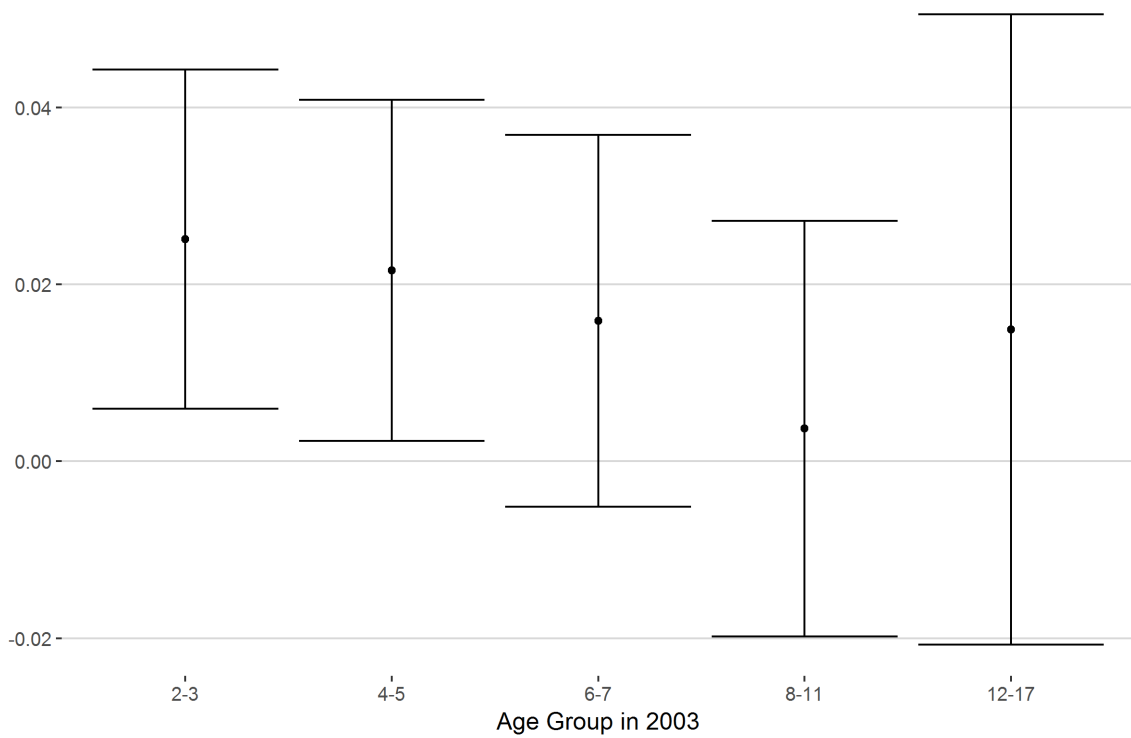
Notes: the figure plots results from estimating a 2SLS model where child allowances ($CA_{ia=18}$) are instrumented with recentered child allowances ($CA_{i\tilde{a}-18}$). The model is estimated separately for each age group. The dependent variable is an indicator for a high school matriculation certificate. The sample contains only children who have turned 18 by the end of 2019. The sample includes only students from families with a high-order birth in a three weeks window around the cutoff day. Bars indicate 95 percent confidence intervals. Standard errors clustered at the family level.

Tables

Table 1: Summary Statistics

	Experimental Sample	General Population
	(1)	(2)
Arab	0.34	0.24
	(0.47)	(0.42)
Birth Order	3.12	2.44
	(1.99)	(1.59)
Boy	0.50	0.48
	(0.50)	(0.50)
Father Schooling	11.96	12.19
	(4.15)	(3.69)
Mother Schooling	11.51	12.14
	(3.60)	(3.49)
Immigrant Parent	0.26	0.34
	(0.44)	(0.47)
High School Quality	0.30	0.50
	(0.34)	(0.34)
Diploma	0.31	0.50
	(0.46)	(0.50)
Total Credits	11.49	17.19
	(13.62)	(13.21)
Advanced Math	0.13	0.25
	(0.34)	(0.43)
Advanced English	0.26	0.49
	(0.44)	(0.50)

Note: The first column in the table reports summary statistics for the main analysis sample which contains children from families who had a high-order birth between January 1st, 2003 and October 31, 2003. The main sample contains only children who have turned 18 by the end of 2019. The second column shows weighted summary statistics for the entire population of Israelis students. Weights are chosen to reflect the distribution of birth cohorts in the main analysis sample in Column 1.

Table 2: Balance of Baseline Characteristics

	Arab	Mother Schooling	Father Schooling	Family Size	Birth Spacing (in days)	Immigrant Parent	Ultraorthodox	Birth Order
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born after June 1, 2003	0.013	0.022	−0.123	0.046	−3.173	0.012	−0.006	−0.036
	(0.019)	(0.132)	(0.148)	(0.070)	(14.771)	(0.017)	(0.020)	(0.037)
Mean	0.348	11.58	12.02	5.66	673	0.261	0.41	2.82

Note: The table shows estimates of β in equation 2 (with no controls) for predetermined variables which are listed in the column header. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. Standard errors clustered at the family level are shown in parentheses.

Table 3: The Effect of 2003 Reform on High School Matriculation

	(1)	(2)
RDD on Matriculation	−0.014	−0.024**
	(0.014)	(0.011)
First Stage	−1,745***	−1,627***
	(141)	(29)
2SLS-RDD (in 1,000 USD)	0.008	0.014**
	(0.008)	(0.007)
Number of Students		44,484
Number of Families		11,302
Background controls	No	Yes

Note: The first row in the table shows estimates of β in equation 2. The second row shows estimates of α_1 in equation 3. The third row shows estimates of θ in equation 4. The first Column show estimate of the model without any controls. The second column show results for a model that controls for the following pre-refrom controls: maternal and paternal education, birth cohort, month of birth, birth order, birth order of the pivotal child, average spacing between siblings, sex, parental immigration status, ethinicity and municipality fixed effects. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. Standard errors clustered at the family level are shown in parentheses.

Table 4: Heterogeneity in the Effect of The 2003 Reform

	Arabs	Jews	Boys	Girls	Jewish Boys	Non-Jewish Boys
	(1)	(2)	(3)	(4)	(5)	(6)
RDD for Matriculation	−0.009	−0.029**	−0.024*	−0.018	−0.038***	−0.013
	(0.018)	(0.013)	(0.013)	(0.015)	(0.015)	(0.013)
First Stage	−1,664***	−1,622***	−1,667***	−1,596***	−1,634***	−1,647.824***
	(53)	(40)	(38)	(37)	(50)	(33)
2SLS-RD (in 1,000 USD)	0.005	0.018**	0.014*	0.011	0.023**	0.008
	(0.010)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)
P-value	0.24		0.85		0.18	
Diploma Mean	0.35	0.31	0.23	0.42	0.23	0.37
Number of Students	16,117	28,401	22,381	22,137	14,557	29,961
Number of Families	4,160	7,184	10,875	10,802	6,900	10,551

Note: The first row in the table shows estimates of β in equation 2. The second row shows estimates of α_1 in equation 3. The third row shows estimates of θ in equation 4. Each column presents estimation results for a separate population which is indicated in column header. All results are from a model that includes background controls which are listed in the notes to Table 3. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. Standard errors clustered at the family level are shown in parentheses.

Table 5: RDD Estimates of the Effect of the 2003 Reform on Additional Outcomes, By Sex and Ethnicity

		Dependent variable:				
	Diploma Quality (Age 18)			School Progress (Age 15)		
	Credits	Adavanced Math	Adavanced Eng.	High School Quality	Ever 10th grade	Ever Repeat
	(1)	(2)	(3)	(4)	(5)	(6)
Entire Sample	−0.694**	−0.010	−0.022**	−0.015*	0.005	0.011
	(0.306)	(0.008)	(0.011)	(0.008)	(0.007)	(0.009)
Jews	−0.971**	−0.016	−0.034***	−0.023**	0.005	0.024**
	(0.377)	(0.010)	(0.013)	(0.011)	(0.006)	(0.012)
Arabs	0.050	0.002	0.005	0.003	0.009	−0.005
	(0.506)	(0.014)	(0.018)	(0.010)	(0.015)	(0.012)
P-value	0.13	0.33	0.04	0.12	0.80	0.04
Boys	−0.911**	−0.020**	−0.025**	−0.026***	0.012	0.011
	(0.357)	(0.009)	(0.012)	(0.009)	(0.010)	(0.014)
Girls	−0.319	0.003	−0.014	−0.001	−0.0001	0.014
	(0.410)	(0.012)	(0.015)	(0.010)	(0.008)	(0.009)
P-value	0.05	0.03	0.18	0.004	0.418	0.900
Jewish Boys	−1.463***	−0.023*	−0.041***	−0.040***	0.004	0.034*
	(0.479)	(0.012)	(0.014)	(0.012)	(0.009)	(0.019)
Non Jewish Boys	−0.279	−0.002	−0.011	−0.001	0.006	−0.001
	(0.374)	(0.010)	(0.013)	(0.008)	(0.009)	(0.008)
P-value	0.026	0.114	0.072	0.010	0.564	0.096

Note: The table shows estimates of β in equation 2 from separate regressions where the column header denotes the outcome variable. Rows denote the population which is included in the regression. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample in the first three columns contains only children who have turned 18 by the end of 2019. The sample in the last three columns contain both the pivotal children and their older siblings. All results are from a model that includes background controls which are listed in the notes to Table 3. Standard errors clustered at the family level are shown in parentheses. P-Values are reported for testing no heterogeneity.

Table 6: 2SLS Estimates of the Effect of Additional Child Allowance Income (in USD 1,000) on Education Outcomes, By Sex and Ethnicity

	Dependent variable:					
	High School Diploma Outcomes (Age 18)			School Progress Outcomes (Age 15)		
	Credits	Advanced Math	Advanced Eng.	High School Quality	Ever 10th grade	Ever Repeat
	(1)	(2)	(3)	(4)	(5)	(6)
Entire Sample	0.425**	0.006	0.014**	0.009*	−0.007	−0.005
	(0.197)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)
Jews	0.595**	0.010	0.021***	0.015**	−0.007	−0.014*
	(0.249)	(0.006)	(0.008)	(0.007)	(0.004)	(0.007)
Arabs	−0.049	−0.002	−0.003	−0.003	−0.010	0.007
	(0.302)	(0.009)	(0.011)	(0.006)	(0.010)	(0.008)
P-value	0.071	0.180	0.067	0.085	0.738	0.092
Boys	0.547**	0.012**	0.015**	0.012**	−0.008*	−0.003
	(0.230)	(0.006)	(0.007)	(0.005)	(0.005)	(0.007)
Girls	0.205	−0.001	0.009	0.003	−0.002	−0.007
	(0.253)	(0.007)	(0.009)	(0.006)	(0.005)	(0.005)
P-value	0.200	0.114	0.652	0.099	0.203	0.691
Jewish Boys	0.886***	0.014*	0.025***	0.026***	−0.009	−0.022*
	(0.290)	(0.007)	(0.009)	(0.008)	(0.006)	(0.012)
Non Jewish Boys	0.167	0.002	0.008	0.0005	−0.005	−0.0002
	(0.225)	(0.006)	(0.008)	(0.005)	(0.005)	(0.005)
P-value	0.015	0.115	0.083	0.005	0.848	0.140

Note: The table shows estimates of θ in equation 4 from separate regressions where the column header denotes the outcome variable. Rows denote the population which is included in the regression. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample in the first three columns contains only children who have turned 18 by the end of 2019. The sample in the last three columns contain both the pivotal children and their older siblings. All results are from a model that includes background controls which are listed in the notes to table 3. Standard errors clustered at the family level are shown in parentheses.

44

Table 7: 2SLS Estimates of the Effect of Additional Child Allowance Income (in USD 1,000) on Education Outcomes, By Family Size and Ethnicity

	Dependent variable:						
	High School Diploma Outcomes (Age 18)				School Progress Outcomes (Age 15)		
	Diploma	Credits	Pro Math	Pro Eng.	High School Quality	Ever 10th grade	Ever Repeat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel (a): All Students						
Parity 4 Pivotal Birth	0.028***	0.756***	0.017**	0.024**	0.016**	0.004	−0.008
	(0.010)	(0.277)	(0.008)	(0.010)	(0.007)	(0.005)	(0.007)
Parity 5 and Higher Pivotal Birth	0.002	0.101	−0.001	0.002	0.003	−0.008	−0.004
	(0.008)	(0.251)	(0.006)	(0.008)	(0.006)	(0.005)	(0.007)
P-value	0.085	0.052	0.030	0.110	0.670	0.082	0.150
	Panel (b): Jewish Students						
Parity 4 Pivotal Birth	0.032**	1.073***	0.027**	0.040***	0.027**	0.004	−0.015
	(0.014)	(0.389)	(0.012)	(0.014)	(0.010)	(0.005)	(0.011)
Parity 5 and Higher Pivotal Birth	0.005	0.200	−0.002	0.006	0.004	−0.005	−0.012
	(0.010)	(0.322)	(0.006)	(0.009)	(0.008)	(0.005)	(0.009)
P-value	0.120	0.113	0.086	0.075	0.122	0.181	0.6475
	Panel (c): Arab Students						
Parity 4 Pivotal Birth	0.020	0.173	−0.001	−0.0001	0.002	0.004	−0.002
	(0.013)	(0.360)	(0.012)	(0.013)	(0.007)	(0.009)	(0.009)
Parity 5 and Higher Pivotal Birth	−0.005	−0.216	−0.004	−0.007	−0.003	−0.010	0.010
	(0.014)	(0.390)	(0.010)	(0.013)	(0.007)	(0.012)	(0.009)
P-value	0.027	0.07	0.018	0.165	0.864	0.064	0.260

Note: The table shows estimates of θ in equation 4 from separate regressions where the column header denotes the outcome variable which is included in the regression. Rows denote the population variable. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample in the first three columns contains only children who have turned 18 by the end of 2019. The sample in the last three columns contain both the pivotal children and their older siblings. All results are from a model that includes background controls which are listed in the notes to table 3. Standard errors clustered at the family level are shown in parentheses.

Table 8: RDD and 2SLS Estimates of the Effect on Attending an Ultraorthodox School

	Middle School	High School	High School with 0 Matriculation
	Dependent variable: Ever Attending Ultraorthodox		
	(1)	(2)	(3)
	Panel A. RDD		
All Jewish Students	−0.00001	0.018	0.023*
	(0.013)	(0.011)	(0.011)
Mean	0.56	0.59	0.52
Boys	0.001	0.020	0.030**
	(0.016)	(0.014)	(0.015)
Mean	0.58	0.61	0.58
Girls	−0.003	0.017	0.007
	(0.014)	(0.014)	(0.016)
Mean	0.54	0.57	0.44
P-value	0.880	0.820	0.291
	Panel B. 2SLS		
All Jewish Students	0.0003	−0.011	−0.014*
	(0.007)	(0.008)	(0.009)
Boys	−0.0001	−0.010	−0.021**
	(0.009)	(0.009)	(0.010)
Girls	0.0005	−0.012	−0.008
	(0.008)	(0.010)	(0.012)
P-value	0.825	0.970	0.342

Note: Panel A in the table shows estimates of β in equation 2 from separate regressions where the column header denotes the outcome variable. Rows denote the population which is included in the regression. Panel B shows estimates of θ in equation 4 from separate regressions where the column header denotes the outcome variable. The samples are restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. Only Jewish students are included in the sample. All results are from a model that includes background controls which are listed in the notes to Table 3. Standard errors clustered at the family level are shown in parentheses

Table 9: Balance Test Of Child Allowances and Recentered Child Allowances

	Dependent variable:	
	Child Allowance	Recentered Child Allowance
	(1)	(2)
Arab	400***	−32
	(74)	(44)
Mother Schooling	−6	2
	(11)	(6)
Father Schooling	13	−1
	(9)	(5)
Family Size	2,190***	12
	(32)	(15)
Birth Spacing	−152***	11
	(24)	(14)
Immigrant Parent	87	−4
	(67)	(39)
Ultraorthodox	260***	−28
	(84)	(46)
Birth Order	−164***	−1
	(26)	(11)
Year of Birth	326***	4
	(10)	(5)
Boy	−16	3
	(40)	(19)
Constant	−645,303***	−7,435
	(19,371)	(9,594)
Joint F-test	3428	0.74
p-value	0	0.64
Number of Students	9,869	
Number of Families	2,998	

Note: The table shows coefficients from regressing child allowances ($CA_{ia=18}$) and recentered child allowances ($CA\tilde{}_{ia=18}$) on a set of pre-reform variables indicated in the rows. The sample is restricted to students from families with a high-order birth in a three weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. Standard errors clustered at the family level are shown in parentheses. Child Allowances are in USD 1,000.

Table 10: The Effect of Child Allowances Income on High School Matriculation (BH Estimates)

	Dependent Variable: High School Matriculation				
	OLS			2SLS	
	(1)	(2)	(3)	(4)	(5)
CA by Age 18 (in 1,000 USD)	-0.024^{***}	-0.024^{***}	0.005	0.015^{**}	0.018^{***}
	(0.0005)	(0.001)	(0.004)	(0.006)	(0.007)
Controls	No	No	Yes	Yes	No
Adjusted R^2	0.045	0.044	0.311	0.311	0.050
Bandwidth (in days)	150	21	21	21	21

Note: Columns 1-3 show the coefficient from regressing $CA_{ia=18}$ on an indicator for high school matriculation. In Column 1- 2 no controls are added and in Column 3 the set of controls listed in the notes to Table 3 is included. Columns 4-5 show estimates of θ^{BH} from IV estimates of equation 5. Column 2-5 are restricted to students from families with a high-order birth in a three weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. Standard errors clustered at the family level are shown in parentheses. Child allowances are in USD 1,000.

Table 11: The Effect of Child Allowances Income on Additional Outcomes, By Sex and Ethnicity (BH Estimates)

Dependent variable:

| | High School Diploma Outcomes (Age 18) | | | | School Progress Outcomes (Age 15) | | |
| | Diploma | Credits | Advanced Math | Advanced Eng. | High School Quality | Ever 10th grade | Ever Repeat |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Entire Sample	0.014**	0.448***	0.006	0.009	0.012***	−0.0004	−0.006
	(0.006)	(0.171)	(0.004)	(0.006)	(0.004)	(0.004)	(0.005)
Jewish	0.019***	0.666***	0.011**	0.019***	0.018***	0.003	−0.010*
	(0.007)	(0.218)	(0.005)	(0.007)	(0.006)	(0.004)	(0.006)
Arabs	0.003	−0.052	−0.002	−0.011	0.001	−0.007	−0.002
	(0.009)	(0.265)	(0.007)	(0.009)	(0.005)	(0.008)	(0.006)
P-value	0.264	0.036	0.107	0.006	0.016	0.489	0.291
Boys	0.014**	0.485**	0.008	0.011*	0.016***	−0.006	−0.008
	(0.007)	(0.210)	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)
Girls	0.010	0.294	0.002	0.005	0.008	0.002	−0.005
	(0.008)	(0.218)	(0.006)	(0.008)	(0.006)	(0.005)	(0.005)
P-value	0.873	0.511	0.166	0.862	0.235	0.159	0.470
Jewish Boys	0.022***	0.854***	0.013**	0.023***	0.027***	−0.002	−0.017*
	(0.008)	(0.261)	(0.006)	(0.008)	(0.007)	(0.005)	(0.010)
Non Jewish Boys	0.012**	0.044	0.001	0.006	0.007	0.002	−0.003
	(0.005)	(0.021)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
P-value	0.481	0.070	0.060	0.072	0.004	0.319	0.015

Note: The table shows estimates of θ^{BH} in equation 5 for various outcomes and subsamples from a 2SLS model where CA_{ia} is instrumented with $\tilde{CA}_{ia}$. All regressions include controls for predetermined background controls listed in the notes for Table 3. Column header indicates the outcome and the rows indicate the population included in the sample. The sample in the first four columns contains only children who have turned 18 by the end of 2019 and uses exposure to $CA_{ia=18}$. The sample in the last three columns contain both the pivotal children and their older siblings and uses $CA_{ia=15}$ as the regressor. The sample is restricted to students from families with a high-order birth in a three weeks window around the cutoff day. Standard errors clustered at the family level are shown in parentheses. Child allowances are in USD 1,000.

49

Table 12: The Effect of Child Allowances Income on Attending an Ultraorthodox School (BH Estimates)

	Dependent variable: Ever Attending Ultraorthodox		
	Middle School	High School	High School with 0 Matriculation
	(1)	(2)	(3)
All Jewish Students	−0.001	−0.011	−0.015**
	(0.007)	(0.007)	(0.008)
Boys	−0.003	−0.017*	−0.019**
	(0.008)	(0.009)	(0.009)
Girls	0.001	−0.006	−0.011
	(0.008)	(0.009)	(0.010)
P-value	0.696	0.217	0.458

Note: The table shows estimates of θ^{BH} in equation 5 from a 2SLS model where CA_{ia} is instrumented with $C\tilde{A}_{ia}$. Column headers denote the outcome variable and rows denote the population which is included in the regression. The sample is restricted to students from families with a high-order birth in a three weeks window around the cutoff day. Standard errors clustered at the family level are shown in parentheses. Child allowances are in USD 1,000.

A Appendix Tables and Figures

Appendix Figure A1: Child Allowances in Israel 1997-2003 (2020 USD)
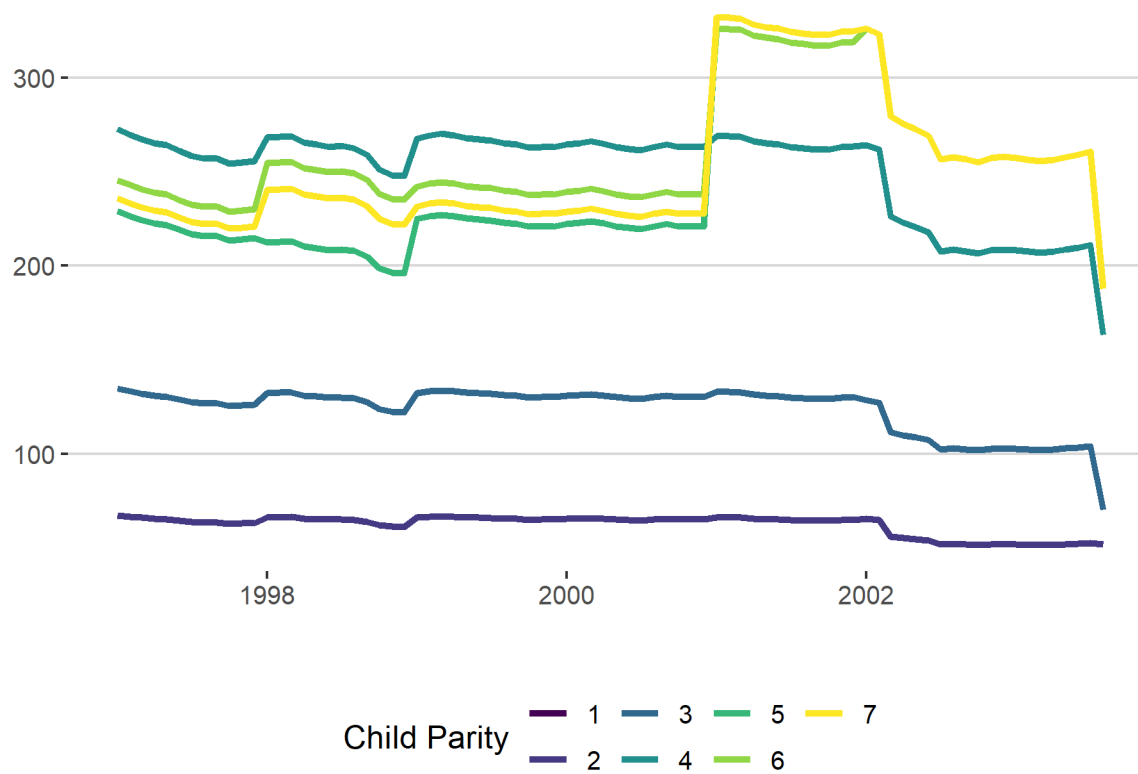
Note: The figure plots the monthly per child child allowances payment in 2020 USD by child birth order among children below the age of 18 in the household.

Appendix Figure A2: Difference Overtime in Child Allowances between Pre- and Post-Cutoff Children, by Child Parity (2020 USD)
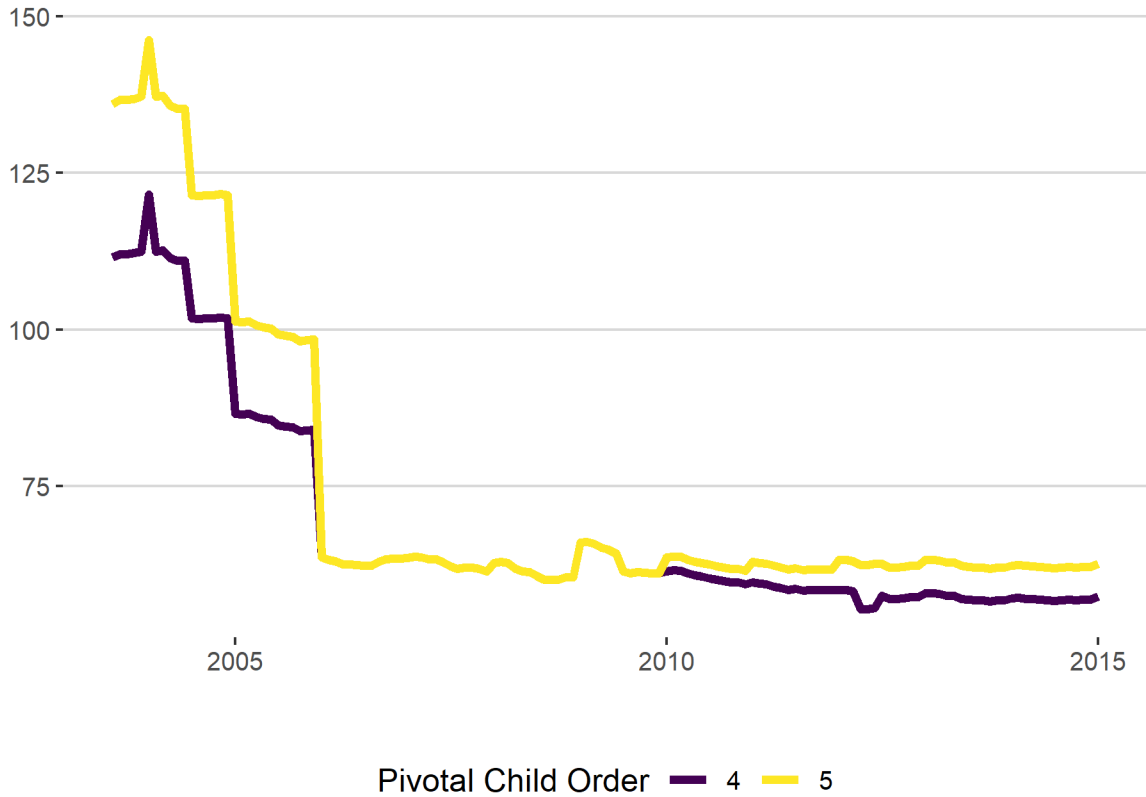

Note: The figure plots the monthly difference in child allowances payments between a family with a birth right before the cutoff and right after the cutoff, separately for a fourth order birth and a fifth order birth.

Appendix Figure A3: Births Density Manipulation Test

p=0.17
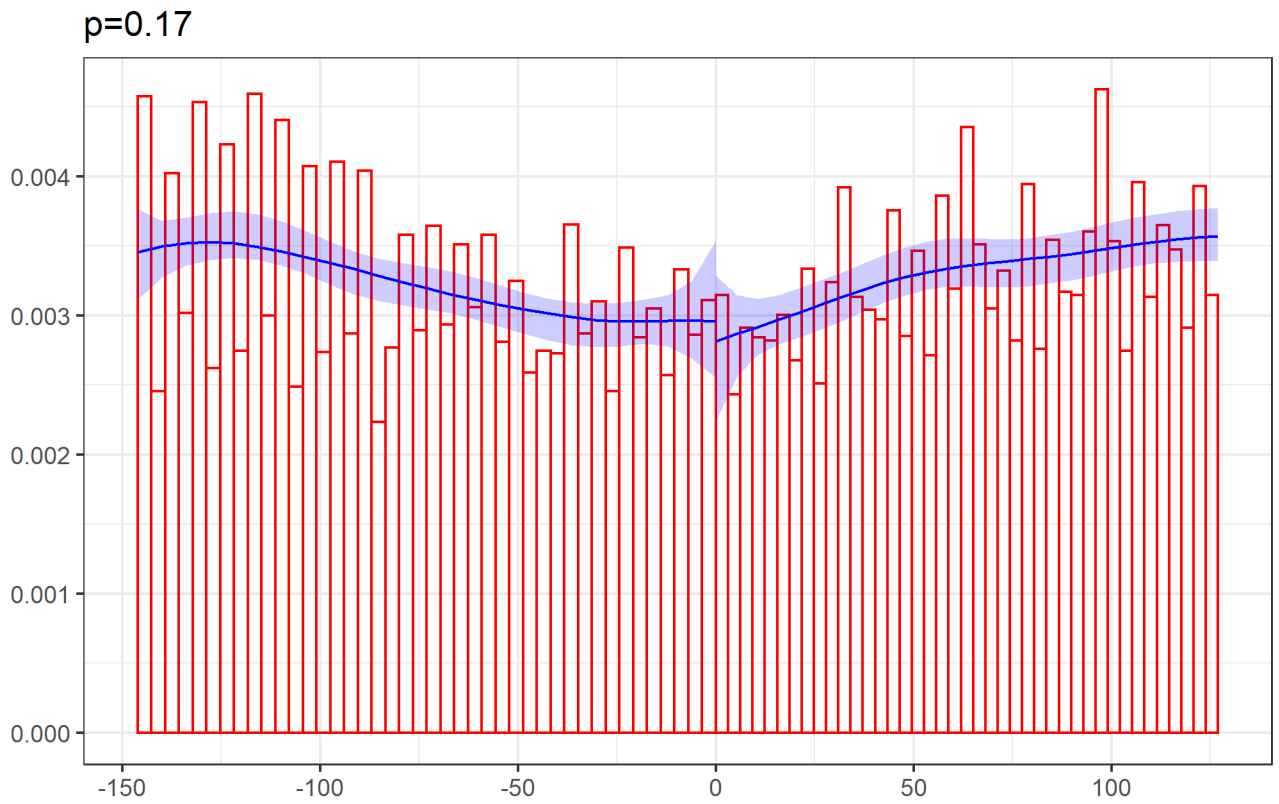
Note: The figure plots density of all parity four or higher births in Israel in a five months window around the cutoff (June 1, 2003). The figure uses *rddensity* package in R which implements the manipulation test of density discontinuity based on local polynomial density estimation in Cattaneo et al. (2018).

Appendix Figure A4: Empirical Cumulative Distribution Function of High School Matriculation Placebo RDD estimates
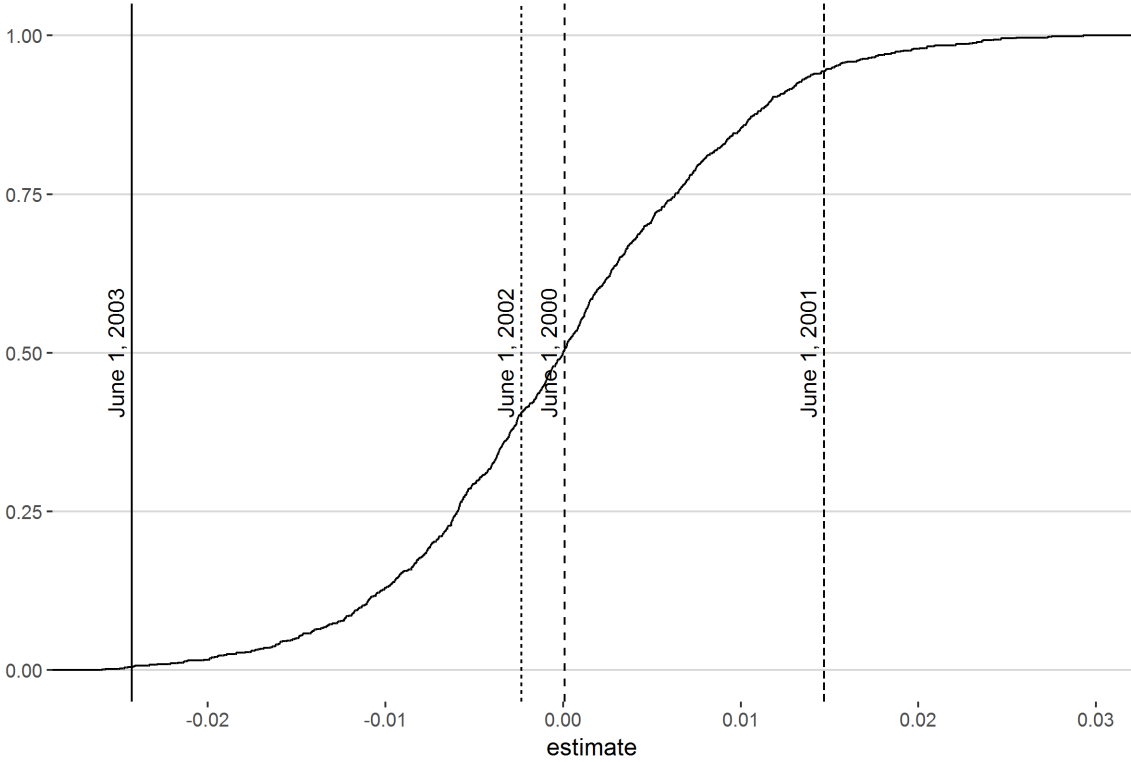
Notes: The figure plots empirical cumulative distribution function of high school matriculation RD placebo estimates, where placebo cutoff dates are all days between March 1, 2000 to March 1, 2003
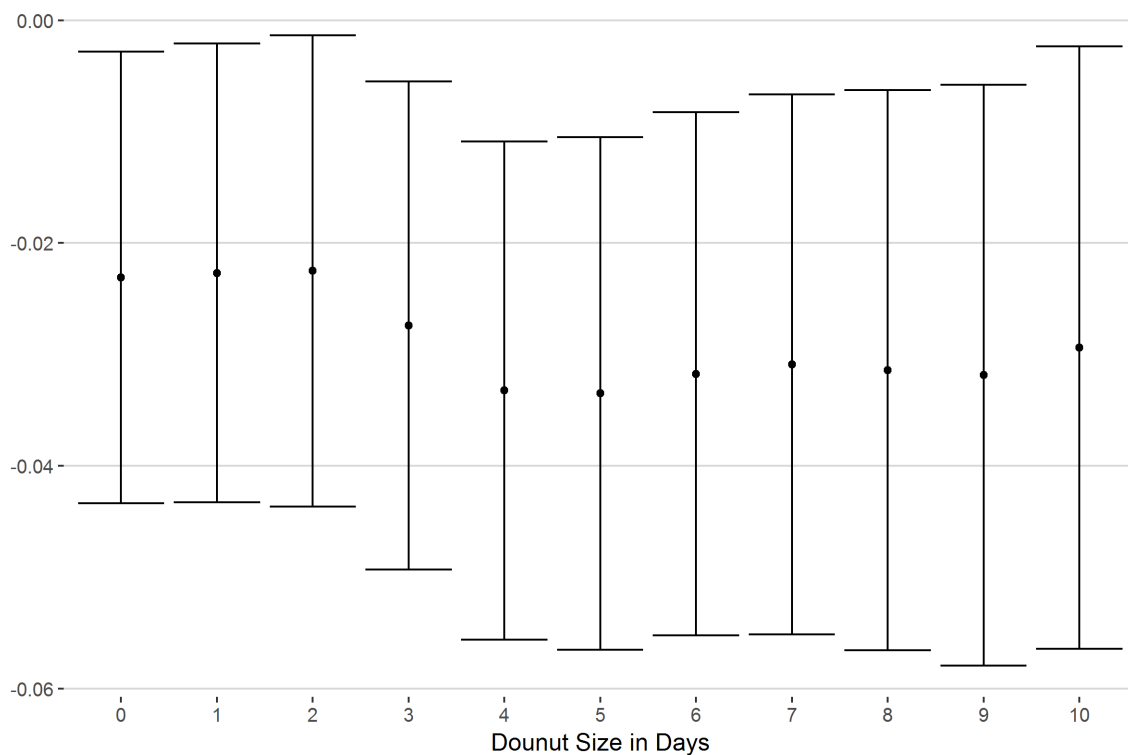
Appendix Figure A5: Robustness of matriculation RDD Estimate to Bandwidth Selection
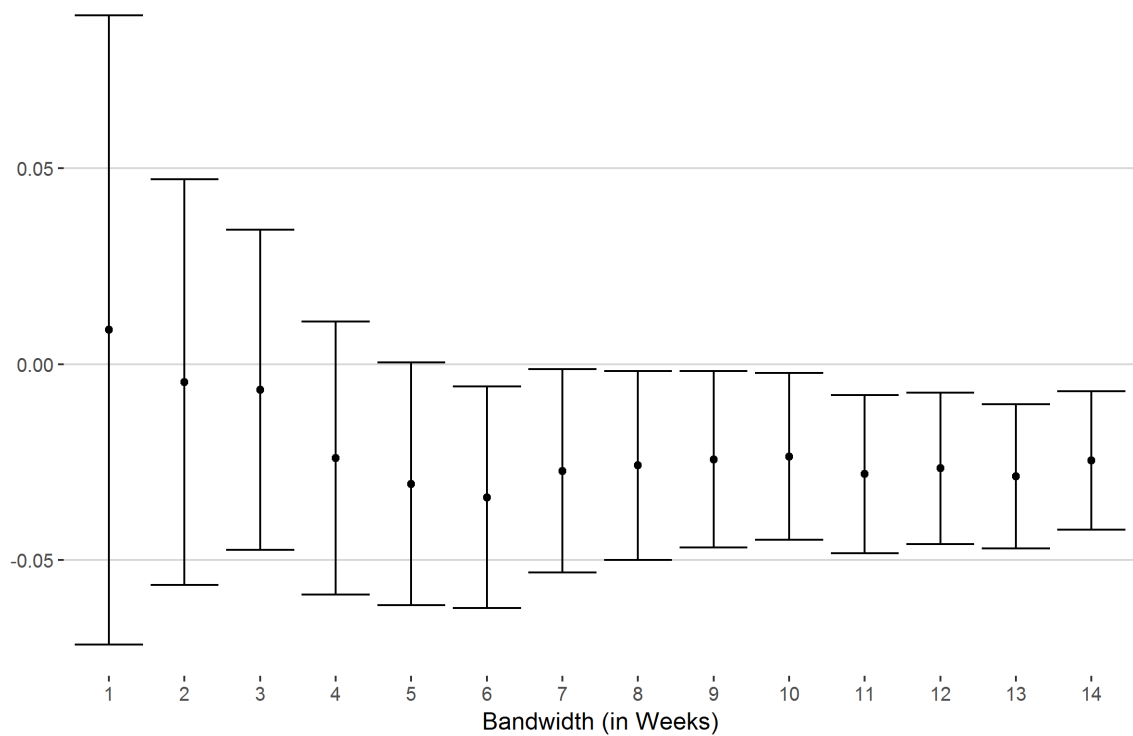
Notes: The figure presents estimates of β in equation 2 on high school matriculation for various bandwidth sizes (in weeks on either side of the cutoff day). Bars indicate 95 percent confidence intervals.The sample is restricted to students from families with a high-order birth. The sample contains only children who have turned 18 by the end of 2019.

Appendix Figure A6: Robustness of matriculation RDD Estimates to Donut Hole Specifications

Notes: The figure presents estimates of β in equation 2 on high school matriculation for various of donut hole sizes. The x-axis shows the number of days removed on either side of the cutoff. Bars indicate 95 percent confidence intervals. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019.

Appendix Figure A7: Balance on Baseline Characteristics

(a) Arab

(b) Mother Schooling

(c) Father Schooling

(d) Family Size

(e) Birth Spacing

(f) Immigrant Parent

## (g) Ultraorthodox

(h) Birth Order

Notes: Each panel plots a baseline covariate, on the vertical axis, against the birth date of the pivotal child, on the horizontal axis, centered at June 1, 2003. Each point represents the respective baseline mean over a seven-days bin. Linear lines of the underlying data are fitted on either side of the cutoff date. The sample is restricted to students from families with a high-order birth in a 10 weeks window around the cutoff day. The sample contains only children who have turned 18 by the end of 2019. The shaded area shows the 95% confidence interval.

Appendix Figure A8: Seasonality in Student Characteristics

(a) Share of Students With a High School matriculation Certificate   *(b) Share of Mothers With at Least 12 Years of Education*
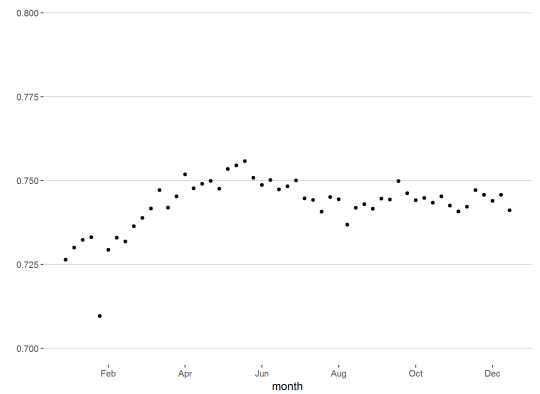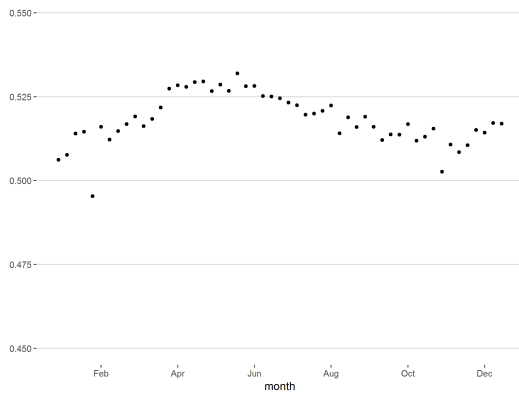

(c) Student's Birth Order
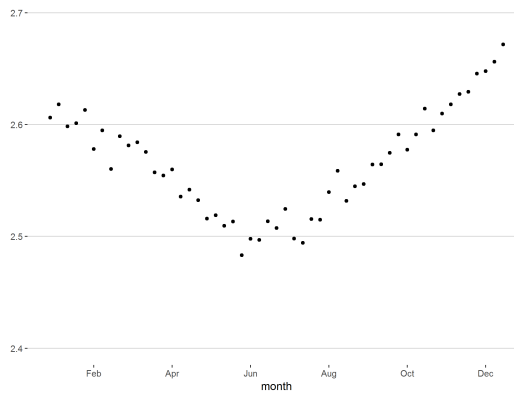
Note: This figure presents birthdays seasonality in mother and child characteristics. Panel A plots seasonality in high school matriculation rates, panel B plots seasonality in mothers education, and panel C shows seasonality in child parity. Each point represents a mean in a calendar week. The sample includes all births from 1990 to 2000.

Appendix Figure A9: Seasonality in High School Matriculation by Student's Birth Order
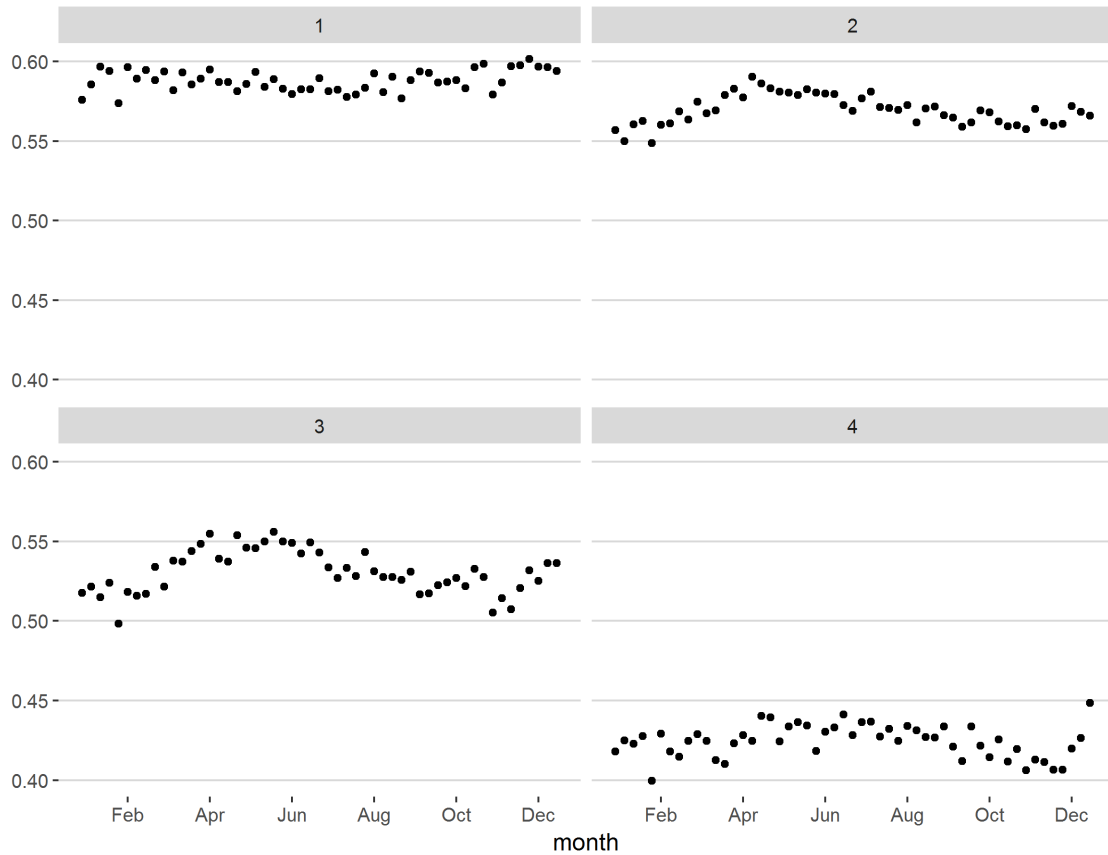
Note: the figure presents seasonality in high school matriculation rates by birth order. Birth order are indicated in the panel header. Each point represents a mean in a calendar week. The sample includes all births from 1990 to 2000.

Appendix Table A1: Robustness of the RDD Estimates to Data-Driven Bandwidth (Calonico et al. 2014)

	Dependent variable:						
	High School Diploma Outcomes (Age 18)				School Progress Outcomes (Age 15)		
	Matriculation	Credits	Advanced Math	Advanced Eng.	High School Quality	Ever 10th grade	Ever Repeat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Entire Sample	−0.029*	−1.112**	−0.012	−0.028**	−0.025**	−0.002	0.016
	(0.015)	(0.462)	(0.011)	(0.014)	(0.011)	(0.009)	(0.011)
Jews	−0.040**	−1.405**	−0.028**	−0.044**	−0.035**	−0.001	0.032**
	(0.019)	(0.596)	(0.014)	(0.018)	(0.016)	(0.008)	(0.016)
Arabs	−0.019	−0.253	0.014	−0.004	−0.005	0.002	−0.003
	(0.020)	(0.530)	(0.018)	(0.020)	(0.011)	(0.020)	(0.017)
Boys	−0.039**	−1.252**	−0.031**	−0.036**	−0.041***	0.003	0.008
	(0.018)	(0.576)	(0.012)	(0.016)	(0.014)	(0.013)	(0.017)
Girls	−0.014	−0.252	0.007	−0.010	−0.001	−0.010	0.012
	(0.021)	(0.564)	(0.016)	(0.020)	(0.013)	(0.010)	(0.010)
Jewish Boys	−0.045**	−1.849**	−0.040**	−0.055***	−0.055***	−0.003	0.041
	(0.023)	(0.745)	(0.017)	(0.021)	(0.019)	(0.012)	(0.025)

Note: The table shows estimates of β in equation 2 from separate regressions where the column header denotes the outcome variable. Rows denote the population which is included in the regression. The sample is restricted to students from families with a high-order birth. Bandwidth size is computed for each group-outcome using the R progrm *rdrobust* (Calonico et al. 2014). The sample in the first three columns contains only children who have turned 18 by the end of 2019. The sample in the last three columns contain both the pivotal children and their older siblings. All results are from a model that includes background controls which are listed in the notes to table 3. Standard errors clustered at the family level are shown in parentheses. P-Values are reported for testing no heterogeneity.